The Effect of Interracial Peers on Political Preferences: Evidence from Schools in the United States*

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Abstract

This study examines the long-term influence of Black peers in school on their classmates' future political preferences. Exploiting idiosyncratic variation in the share of Black students across cohorts within the same school in a longitudinal, nationally representative survey of US adolescents, I find that exposure to same-gender Black peers reduces conservatism among non-Black classmates in adulthood: a one within-school standard deviation increase in the Black peer share lowers the probability of identifying as conservative by 4 percent and as very conservative by 14 percent of the respective means, with no corresponding increase in strongly liberal identification. The effect emerges in 2008–2009, during the election and first year of Barack Obama's presidency, when racial attitudes bore most directly on political choices. Reconstructing the schools' friendship networks, I show that the effect operates through the shared school environment rather than personal contact and concentrates among students who report belonging to their school, consistent with a re-categorization of racial groups into a common school identity. To generalize my findings, I measure the childhood exposure to Black schoolmates of every US county and birth cohort schooled between 1968 and 2008 and link it to national political outcomes. I find that a ten percentage point increase in exposure to Black peers is associated with a 2.2 percentage point increase in the probability that White Americans identify as Democrats, a 3.0 percentage point increase in voter turnout, and a 3.2 percentage point increase in the probability of voting for the Democratic candidate for president.

Keywords: Political Economy, Political Preferences, Peer Effects, Racial Identity, Education

JEL Codes: P16, D72, I21, J15

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1 Introduction

Political preferences are fundamental determinants of individual behavior, interpersonal relations, and professional decisions. Particularly in light of rising partisan polarization in the United States (Boxell et al., 2020; Callander & Carbajal, 2022) and growing support for populist politics around the world (Funke et al., 2023), it is of great interest to understand how these political preferences are shaped.

A large body of research traces political preferences to historical and economic circumstances, among them slavery (Acharya et al., 2016), trade (Autor et al., 2020), and acts of terrorism (Gould & Klor, 2010). Political preferences also respond to the immediate social environment, a person's geographical and interpersonal surroundings (Nathan & Sands, 2023). Among these environmental determinants, exposure to racial diversity stands out (Billings et al., 2021; Brown et al., 2021; Calderon et al., 2023; Schindler & Westcott, 2021), as it brings together in a single setting two of the main dimensions along which American polarization runs: race and politics (Kuziemko & Washington, 2018).

In this paper, I study the causal effect of exposure to racial diversity in school on the political preferences students hold in adulthood. Schools bring together adolescents of different races during their formative years (Moody, 2001). I measure this exposure as the share of Black peers in a student's cohort and examine whether these peers lead their non-Black classmates to identify as more conservative or more liberal. The closest evidence on this question comes from the end of court-ordered desegregation in Charlotte-Mecklenburg (Billings et al., 2021). I draw instead on the variation in racial composition that arises naturally across cohorts of the same school, in a national sample of American students. I observe these preferences at four points between 2001 and 2024, and take as the main outcome those expressed in 2008–2009, during the election and first year of Barack Obama's presidency, when racial attitudes bore most directly on political choices (Parker, 2016; Tesler & Sears, 2010).

To overcome the problem of endogenous selection into schools, I exploit idiosyncratic variation in the share of Black students across different cohorts of students within a school, an approach first proposed by Hoxby (2000) and subsequently used extensively to study causal peer effects (see, for example, Carrell et al., 2018; Lavy & Schlosser, 2011; Merlino et al., 2019, 2024). I rely on data from the National Longitudinal Study of Adolescent to Adult Health (Add Health), a nationally representative survey of US adolescents in grades 7–12 that started in 1994–1995.[1] Add Health recorded the race of all students in 134 schools across the country and has followed a core sample of around 20,000 of them for more than 20 years. The main outcome is the political preferences that non-Black respondents reported in Wave IV, at ages 25 to 34, an age range by which most

[1] See Harris (2013) for an overview of the study design and accomplishments of Add Health.

people have formed their political views (Chan & Clayton, 2006; Trevor, 1999).

I find that the racial composition of a student's cohort shapes the political preferences of their non-Black classmates decades later, and that the effect comes from same-gender Black peers. A one within-school standard deviation increase in this share (2.7 percentage points) moves their preferences by 0.023 points toward the liberal end of the five-point scale, 2.6 percent of a standard deviation of the outcome. This movement comes from the conservative side of the scale. The probability of identifying as conservative falls by 1 percentage point, 4 percent of the mean, and that of identifying as very conservative by 0.62 percentage points, 14 percent of the mean. Strongly liberal identification does not increase, consistent with Billings et al. (2021). The influence of these peers also depends on the political moment, appearing in 2008–2009, when racial attitudes weighed most heavily on the vote (Tesler & Sears, 2010), and not in 2001, 2016, or 2022–2024. These results are stable under the full set of predetermined controls and formal sensitivity bounds (Cinelli & Hazlett, 2020), are specific to Black peers rather than minority peers in general, and survive randomization inference over cohort composition within schools.

Regarding possible mechanisms underlying these results, I first show that the effect of Black peers on future political attitudes cannot be explained by an effect of these peers on future outcomes such as education, income, criminal history, or political participation. Second, I reconstruct the friendship networks of the sampled schools from the nomination rosters of the in-school questionnaire, with their interaction items, club and team memberships, and grade-level ability tracking, and show that personal contact with Black peers does not carry the effect. The effect is concentrated among students without a Black same-gender friend, does not depend on close friendships, shared clubs and teams, or classroom contact in tracked grades, and is stronger in cohorts whose Black members are more central in the network, measured by their Bonacich centrality. Third, I present evidence that my results can be explained under the main predictions of the Common Ingroup Identity Model (Gaertner et al., 1993), which posits that a shared social environment can lead individuals to re-categorize members of different racial groups as part of a common ingroup. In the contemporaneous census of around 65,000 non-Black students in the sampled schools, larger Black cohorts reported no more prejudice or insecurity and a stronger sense of belonging to their school in 1994–1995, and the long-run effect is concentrated among the students who report belonging to it.

In the last part of the paper, I show that these effects extend to political behavior at the national scale. From the school-level surveys of the Office for Civil Rights and the administrative universe of the Common Core of Data, I reconstruct the childhood exposure to Black schoolmates of every US county and birth cohort schooled between 1968 and 2008, and validate it against the Black peer shares observed in Add Health. I take this measure to the data in two exercises, one at the individual level and one at the level of county electorates. At the individual level, I link the measure to the

719,000 respondents of the Cooperative Election Study, with turnout validated against voter files, and to the 8.9 million registration records of the North Carolina voter file. I find that, among White respondents, a ten percentage point increase in exposure predicts a 2.2 percentage point higher probability of Democratic identification, a 3.0 percentage point higher probability of turnout, and a 3.2 percentage point higher probability of a Democratic presidential vote, and that the registration records reproduce the gradient. At the aggregate level, I measure the exposure carried by each county's White electorate in each election, averaging the childhood exposures of its birth cohorts with their predetermined shares of the White voting-age population, and estimate an event study of the county Democratic vote share on this electorate exposure over the 1980–2024 presidential elections. I find that counties whose electorates carry more exposure become more Democratic from 2008 onward.

My work contributes to three fields in current literature. First, it relates to the formation of political preferences through the social environment (Nathan & Sands, 2023). This literature has documented environmental drivers as different as population ageing (Ahlfeldt et al., 2022; Cattaneo & Wolter, 2009), social media (Enikolopov et al., 2020; Levy, 2021), and the local presence of refugees and migrants, which raises the support of natives for conservative and anti-immigration parties in most settings (Alesina & Tabellini, 2022; Dustmann et al., 2019; Halla et al., 2017; Otto & Steinhardt, 2014). Within this field, my work belongs to the literature on exposure to racial and ethnic minorities within one's own country. Economists have shown that randomly assigned exposure changes intergroup attitudes, among college students with Black roommates (Boisjoly et al., 2006; Corno et al., 2022) and in integrated military units and teams (Cáceres-Delpiano et al., 2021; Dahl et al., 2021), and that childhood exposure to Black Americans shapes politics in the long run, with early-life contact leading White Americans toward less conservative and more Democratic positions (Billings et al., 2021; Brown et al., 2021), a result that extends to the British regions that hosted African American soldiers (Schindler & Westcott, 2021), while Polipciuk et al. (2021) find effects on participation but not on party identity. My contribution to this literature is the direction of political preferences as the outcome, identified from cohort variation within schools in a national sample, together with the conditions under which the effect appears, concentrated in same-gender peers, surfacing when race is politically salient, and transmitted through the shared environment rather than through personal contact.

A second field to which this work contributes is the political economy of race and identity in American elections. A central finding of this literature is that racial attitudes reshaped the party system itself, as the defection of racially conservative White Southerners accounts for the Democratic losses in the South after the Civil Rights era (Kuziemko & Washington, 2018), and that the local presence of Black Americans has moved White voting directly, as during the Great Migration (Calderon et al., 2023). A theoretical branch studies when identity matters for politics,

with voters whose racial, religious, or class identities gain weight in their beliefs and votes once political conflict turns on them (Bonomi et al., 2021; Shayo, 2020). The elections I study are a central case, as racial attitudes predicted vote choice in 2008 more strongly than in previous elections (Kinder & Dale-Riddle, 2012; Tesler & Sears, 2010), and Black candidates raise the turnout of White voters (Washington, 2006). I contribute to this field on the formation side. The racial composition of the school cohort shapes the racial dimension of political preferences decades later, and this dimension remains latent until elections make race salient, in the timing of the individual effect and in the county returns of Section 5.

Finally, my work adds to the literature on the long-run effects of school peers (Bifulco et al., 2011; Giulietti et al., 2022; Lavy & Schlosser, 2011; Merlino et al., 2019; Sacerdote, 2011). With the same Add Health design, cohort composition has been shown to move academic performance (Bifulco et al., 2011; Patacchini et al., 2017), labor force participation (Olivetti et al., 2020), mental health (Fruehwirth et al., 2019; Giulietti et al., 2022; Kiessling & Norris, 2023), and obesity (Brunello et al., 2020; Trogdon et al., 2008). For the share of Black peers, the treatment of this paper, Merlino et al. (2019) find more interracial romantic relationships in adulthood, Merlino et al. (2024) find residence in neighborhoods with more Black residents twenty years later, and Bifulco et al. (2011) find no effect of the combined Black and Hispanic share on the post-secondary outcomes they examine. I add political preferences to the outcomes this variation moves, with two features that distinguish them from the earlier ones. The effect runs through the composition of the shared environment rather than through the ties students form, and it surfaces only when national politics makes race salient.

The rest of this paper is organized as follows. Section 2 describes the data and the empirical strategy. Then, Section 3 presents the main results. In Section 4, I discuss possible mechanisms underpinning the effect. Section 5 examines whether the effect extends from stated preferences to political behavior and is large enough to register in aggregate electoral outcomes. Finally, Section 6 concludes.

2 Data and Empirical Strategy

2.1 Add Health Data

For my analysis I rely on data from Add Health, a nationally representative longitudinal survey designed to study the social and health environments of a sample of adolescents in grades 7th to 12th in the US (Harris et al., 2009). The schools selected to participate came from a stratified sample frame, aiming to be representative of US schools regarding region, urbanicity, school type, school size, and ethnic composition (Harris, 2013). The project currently consists of six rounds,

starting in 1994–1995 with Wave I, in which more than 90,000 students from 134 schools (80 high schools and 54 matched feeder schools) were administered an in-school questionnaire collecting general information on the participants, including gender, race, ethnicity, friendship ties, and basic household background; school administrators filled a parallel questionnaire on the schools. A core sample of these students, randomly selected and stratified by gender and grade, was then followed longitudinally through in-home surveys: around 20,700 adolescents were interviewed in the Wave I in-home survey on their social environment, and re-interviewed in 1996 (Wave II), 2001–2002 (Wave III), 2008–2009 (Wave IV), 2016–2018 (Wave V), and 2022–2024 (Wave VI).

The combination of numerous school-wide respondents in the in-school questionnaire and the longitudinal structure of the subsequent parts of Add Health proves to be optimal for the study of long-term peer effects, as it allows researchers to construct accurate school and grade shares of peers,[2] and then to follow a representative subgroup of the students for nearly 30 years. I construct three shares of Black students: the share of all Black peers in the same grade at the same school, and the divided shares of same-sex and opposite-sex Black peers,[3] a distinction that is especially relevant because the transmission of attitudes varies according to the peers' gender (Kalmijn, 2002). The shares are based on self-reported race; because respondents could identify with more than one race, I define the Black share as the share of students who uniquely identified as Black/African American.

The political preferences outcome for the main analysis is measured in the Wave IV in-home survey (i.e., 12 years later, in 2008–2009), when respondents were between 25 and 34 years old and therefore able to vote and exercise their political preferences. In particular, I use the responses to the question "[i]n terms of politics, do you consider yourself very conservative, conservative, middle-of-the-road, liberal, or very liberal?". For most of the analysis I exploit the categorical characteristic of this question, but I additionally define indicator variables for overall conservative ("conservative" or "very conservative"), overall liberal ("liberal" or "very liberal"), and for the two extreme responses separately. The same item was administered in Waves III, V, and VI, which allows me to trace the within-person evolution of political preferences from 2001 to 2024.[4]

Beyond the core survey, I draw on two further components of Add Health. First, the in-school questionnaire asked each respondent to nominate up to five male and five female friends, which I match to the school rosters to measure each student's own interracial friendships—on the extensive margin, as a share of valid nominations, and restricted to reciprocated ties—as well as the realized integration of entire school cohorts.[5] Second, I use the restricted-use contextual database,

[2]Constructing reliable shares from the in-school survey minimizes measurement error in cohort composition differences and thus lowers the biases arising from this error (Bifulco et al., 2011).

[3]See Appendix Table A.1 for a detailed description of all variables used in this work.

[4]The Wave VI wording differs slightly from the earlier waves; all wave-specific estimations use the corresponding Add Health cross-sectional weights.

[5]Nominations are top-coded at five friends of each gender; cohort-level measures are computed over non-Black nom-

which links each Wave I respondent to pre-treatment characteristics of their local environment: the county's presidential vote margins in 1992, the racial composition and racial dispersion of the home census tract in 1990, and Black–White dissimilarity indices of the school district. Because Add Health geography is pseudo-coded, these contextual measures are the only channel through which local conditions enter the within-Add Health analysis.

Starting from over 20,700 respondents who answered all parts of the survey in Wave I, I exclude approximately 5,000 individuals with no information in Wave IV, around 1,000 who did not report their political preferences, 241 cases that cannot be matched to a school, 308 who cannot be assigned to a school cohort, 663 for whom the Black peer shares cannot be constructed, and 429 without Wave IV sample weights, leaving 13,073 individuals. The focus of this work lies on students who are not Black, because variation in the share of Black peers has opposite implications regarding interraciality for Black students—for whom a higher share represents a larger concentration of same-race peers—than for the rest of the sample. I restrict the definition of Black students to those who exclusively identify as Black/African American in the Wave I in-home questionnaire, and show in Table A.2 that my results are robust to instead using the interviewer-assigned definition of Merlino et al. (2019). This drops 2,716 individuals, leaving a final sample of 10,357 non-Black individuals from 130 schools and 434 school cohorts.

The fact that many individuals surveyed in Wave I do not appear in my sample could raise concerns about attrition bias; in particular, cohort composition might have a causal effect on attrition levels (Bifulco et al., 2011). Following standard procedure, all parts of the analysis use the individually assigned probability weights constructed by Add Health for the respective outcome wave. I further test whether the likelihood of participating in Wave IV is correlated with the share of all Black peers and with the gender-divided shares (Appendix Table A.3, columns 1 and 2), and find no evidence for this,[6] in line with Bifulco et al. (2011). Because non-response to the political preferences question could reflect unobserved underlying factors, I also test whether the probability of not answering it correlates with cohort racial composition (columns 3 and 4) and, reassuringly, report no statistically significant correlations.

Table 1 presents summary statistics for the outcome and treatment variables; a full set of descriptive statistics for the 10,357 non-Black participants in my sample is reported in Appendix Table A.4. Regarding the political outcomes in Wave IV, around 4% of the sample report very conservative political views, and together with the moderate conservatives they make up 25.6% of the responses. This is mirrored on the other side of the spectrum, where liberal and very liberal respondents make up 25.9% of the sample, while the remaining half consider themselves politi-

inators and compared to the share expected under random mixing given cohort composition.

[6]Merlino et al. (2019) run a series of additional tests and likewise find no evidence that Wave IV attrition correlates with within-cohort minority shares.

cally "middle-of-the-road". The share of Black peers in the same cohort is on average around 9%, with a standard deviation of 14.1 percentage points; the averages of the gender-divided shares are nearly identical, with the opposite-sex share slightly higher (9.4%). More relevant for the empirical design is the within-school variation in these shares: net of school-gender and grade-gender fixed effects, the standard deviation of the same-gender share is 2.7 percentage points (2.1 and 2.8 for the overall and opposite-gender shares), which is the variation exploited by the identification strategy. The appendix table further shows that the sample is composed mainly of students who self-identify as White (86.6%), aged 12 to 21 in Wave I and 25 to 34 in Wave IV, distributed almost equally across grades 7 to 12, and that its religious composition is in line with nationally representative polling from those years (see, for example, Newport, 2009).

Table 1: Summary Statistics: Outcome and Treatment Variables

	Observations	Mean	Std. dev.	Within-school std. dev.
Political preferences (Wave IV):				
Political preferences (1–5)	10,357	3.020	0.911	
very conservative	10,357	0.044	0.205	
conservative	10,357	0.212	0.409	
middle-of-the-road	10,357	0.485	0.500	
liberal	10,357	0.198	0.399	
very liberal	10,357	0.061	0.239	
Cohort composition (Wave I):				
% Black peers, all	10,357	0.092	0.141	0.021
% Black peers, same gender	10,357	0.092	0.141	0.027
% Black peers, opp. gender	10,357	0.094	0.145	0.028

Notes: Descriptive statistics for the outcome and treatment variables; 10,357 non-Black students in grades 7–12 from 130 schools and 434 school cohorts, Wave IV cross-sectional weights. The within-school standard deviation residualizes the share on school-gender and grade-gender fixed effects—the variation the design exploits. Full descriptives in Appendix Table A.4; variable construction in Appendix Table A.1. Source: Add Health (Harris et al., 2009).

2.2 Other Data Sources

The analysis in Section 5 draws on four additional, publicly available data sources, which I use to construct a measure of school-age exposure to Black schoolmates for every US county and birth cohort, and to relate it to adult political outcomes at the national level.

School racial composition, 1968–1992. To measure the school environments of cohorts educated before the Add Health sample, I rely on the Elementary and Secondary School Civil Rights Surveys conducted by the Office for Civil Rights (U.S. Department of Education, Office for Civil

Rights, 2024), which report school-level enrollment by race. The surveys constitute a near-census in 1968–1972, covering between 71,529 and 72,164 public schools per year, and sample schools thereafter, between 32,378 (1984) and 54,087 (1978) per survey year; the 1976 round covers the near-universe of 14,873 school districts. I assign schools to counties through county names and district identifiers, matching between 96% and 99% of schools per year, and aggregate to county-level Black enrollment shares, relying on the district files with their sampling weights in the sample years. The resulting series covers 1,500 to 2,700 counties per survey year at the school level and the full set of counties in the weighted district panel.[7]

Common Core of Data, 1987–2008. From 1987 onwards I measure school racial composition with the Common Core of Data of the National Center for Education Statistics (Urban Institute, 2026), an administrative universe of 86,000 to 97,000 public schools per year. Combining both sources yields an annual panel of county-level Black enrollment shares for 3,168 counties over 1968–2008, from which I construct, for every county and birth cohort, the average Black share of schoolmates a student growing up in that county experienced over grades 1 to 12. A cohort born in year b attends grades 1 to 12 in the school years $b + 6$ to $b + 17$; since the panel starts in 1968, all twelve school years are observed for cohorts born in 1962 or later, and the exposure measure is available for 3,120 counties for the cohorts born between 1962 and 1991. For earlier cohorts part of the schooling falls before 1968, so they are excluded from the main estimations. Reassuringly, the weighted distribution of this exposure measure among non-Black students in the 1994–1995 school years replicates the distribution of the Add Health treatment variable: the means are 0.091 and 0.092, the medians 0.026 in both, and the 90th percentiles 0.282 and 0.300.

Cooperative Election Study. Adult political outcomes at the national scale come from the cumulative Cooperative Election Study common content for 2006–2025 (Kuriwaki, 2025), covering around 719,000 respondents with their county of residence, seven-point party identification, and turnout validated against voter files. Because the cumulative file does not carry residential tenure, I extract the tenure items from the ten year-specific common content files and merge them at the respondent level, which allows me to restrict the sample to long-term residents.[8]

[7] In the overlap years 1988–1992, the county Black shares from these surveys correlate at 0.95–0.97 with the administrative universe described below, with an enrollment-weighted mean absolute difference of 2–3 percentage points.

[8] A respondent is classified as a long-term resident if they report having lived five or more years at their present address (the top category of the address-tenure item, available in 2008–2024 except 2018) or fifteen or more years in their current city (the city-tenure item, available in 2006, 2010, 2016, and 2018); 45% of White respondents meet this definition. The restriction limits the misclassification that arises from assigning childhood exposure by the county of current residence.

County presidential returns, 1980–2024. County-level presidential election results for 2000–2024 come from the MIT Election Data and Science Lab (MIT Election Data and Science Lab, 2018), and for 1980–1996 from the collection assembled by Amlani and Algara (2021), together covering roughly 3,100 counties in each of twelve elections. From these returns I compute each county's two-party Democratic vote share, the outcome of the county-level analysis. In 2000, the one election covered by both sources, the county shares from Amlani and Algara (2021) and the MIT Election Data and Science Lab are virtually identical, and in every election the implied national two-party shares reproduce the official results.

North Carolina voter registration records. As an administrative complement to the survey outcomes, I use the statewide voter registration file of the North Carolina State Board of Elections (North Carolina State Board of Elections, 2026), a public snapshot of the 8.9 million registration records of the state, which carry the registrant's self-reported race and ethnicity, party affiliation, county, and birth year. I retain the 1.9 million registered White non-Hispanic voters of the 1962–1991 birth cohorts and collapse them to county-by-cohort registration counts by party.

Census population by age and race. Finally, I use county population counts by age, sex, and race from the 1980 through 2020 decennial censuses, provided by IPUMS NHGIS (Schroeder et al., 2026), to construct the White voting-age population of each county by birth cohort at every election year. Each cohort's county size is measured once, in the 1980 census or, for cohorts born later, in the first census after their birth, and is moved forward with the national survival ratio of the cohort. These predetermined counts serve as the weights that aggregate cohort-level childhood exposure into the exposure composition of each county's White electorate.

2.3 Estimation Strategy

The effect of cohort composition on an individual's future preferences is very likely confounded by the effect of various other correlated factors (Lavy & Schlosser, 2011). In this regard, one of the biggest challenges is the possibility of individual self-selection into schools, thus correlating individual attributes and school composition (Moffitt et al., 2001). Further, selection into classes can also arise, as some parents may try to influence the class within a cohort of a school to which their children go—if for example they think one teacher is better—or school administrators may group different types of students in different classes of a cohort (Hoxby, 2000).

To mitigate these concerns, I employ an empirical approach proposed by Hoxby (2000), attempting to exploit idiosyncratic variation in the share of Black peers across cohorts of students within the same school. More intuitively, this means that I assume that, while families to some degree choose the school their children go to, they do not base this decision on the difference

between average school composition and cohort-specific composition, and that these differences are not correlated with omitted variables. This approach has been widely used in research on peer effects (see, for example Bifulco et al., 2014; Merlino et al., 2019; Olivetti et al., 2020).

The empirical model I follow can be written as:

$$Y_{i,2008} = \alpha BlackShare_{sc,1995} + I_{gf} + I_{sf} + \epsilon_i, \tag{1}$$

where i denotes individuals, c cohorts, s schools, g grades, and f stands for gender. The main dependent variable, $Y_{i,2008}$, represents the political preferences individual i holds in 2008 (i.e., Wave IV). $BlackShare_{sc,1995}$ is the share of Black students from all students in a specific cohort c at school s in Wave I (i.e., 1995). I_{sf} denotes school-gender fixed effects, and I_{gf} are grade-gender fixed effects. Finally, ϵ_i is a random error term. The coefficient of main interest is α, which arguably measures the causal effect of Black peers on the outcome of interest.

Because part of this analysis is based on the idea that the transmission of social attitudes during childhood and adolescence has gendered differences (Kalmijn, 2002), I extend the model by separating the share of Black peers into the shares of same-gender Black students and the opposite-gender Black students in the cohort, so that I estimate the following regression:

$$Y_{i,2008} = \beta BlackShare^{same}_{sc,1995} + \gamma BlackShare^{opp.}_{sc,1995} + I_{gf} + I_{sf} + \epsilon_i, \tag{2}$$

where $BlackShare^{same}_{sc,1995}$ is the share of Black peers in school s and cohort c of the same gender, and $BlackShare^{opp.}_{sc,1995}$ of opposite gender in Wave I. The causal effects of the peers in focus are measured in this specification by β for the same-gender, and by γ for the opposite-gender Black peers.

For all specifications of my analysis, standard errors are clustered at the school level—as is commonly done in the school peer-effects literature (see Bifulco et al., 2011; Imberman et al., 2012)—and Add Health Wave IV cross-sectional weights are used.

2.4 Evidence on the Identification Strategy

Before moving on to the results, it is first important to investigate more closely the validity of my empirical strategy by performing two examinations suggested by Lavy and Schlosser (2011). First, I check if the model has enough variation in the cohort share of Blacks, an important verification since my estimates' precision depends on this. To do so, I examine how much of the raw variation in the share of Black students remains after removing school-gender and grade-gender fixed effects, and then also school-specific trends. Appendix Table A.5 shows the results of this review. Each panel shows the same test for a different share: in panel A I examine the variation in the all-sex

share of Black peers, in panel B the share of same-sex, and in panel C the share of opposite-sex Black peers. For all panels, I report the corresponding standard deviations accompanied by further descriptive statistics. Panel A shows that removing school-gender and grade-gender fixed effects reduces around 85% of the standard deviation of the share of Black peers, and further removing school-specific trends eliminates nearly 90%. For the cohort's same-gender share, reported in Panel B, the remaining variation after controlling for school and grade fixed effects is 19,15% and 16,31% of the variation of the raw shares after removing school trends. In panel C, the reported residual variation for the share of opposite-sex Black peers is similar, with 19,31% and 15,86% residual variation, respectively. Although the residual variation is relatively small after controlling for these fixed effects and trends, there is sufficient variation left to estimate—with reasonable precision—the effects of small changes in racial composition of cohorts within a school. Other work by Bifulco et al. (2011) or Olivetti et al. (2020) also rely on these relatively small, but sufficient, residual variations for their analyses.

The second check tests the assumption that the variation in the composition of cohorts within a school is as good as random, once I control for grade-gender fixed effects. Table 2 reports "balancing tests" that examine whether the share of Black peers across cohorts within a school is correlated with predetermined individual characteristics commonly used in the peer-effects literature (see, for example Bifulco et al., 2011; Olivetti et al., 2020).[9] Following Merlino et al. (2019), I also include the size of the grade within the school. Each variable is regressed first on the share of all Black peers in the same cohort (column 2) and then simultaneously on the same-sex and opposite-sex shares (columns 3 and 4). All regressions include school-gender and grade-gender fixed effects, and column 1 reports the number of observations available for each dependent variable. None of the correlations between the individual characteristics and the shares is significant. This matters for the main assumption of my empirical strategy because, as Altonji et al. (2005) argue, the amount of selection on observable variables provides good guidance to the selection on unobserved characteristics, so the absence of significant correlations supports the idea that the specification exploits an exogenous source of variation. In line with these results, Merlino et al. (2019) regress all pre-treatment variables available in Add Health on the share of Black peers and find, for both the overall and the gender-divided specifications, that less than 10% of the variables are significantly correlated with these shares. The only variable with a correlation significant at the 5%-level is the size of the grade. I suspect this correlation to be spurious, especially because I control for school fixed effects, but I add grade size as a control variable to all parts of my analysis.

Further checks of the identification assumption proposed by Lavy and Schlosser (2011) include

[9]The variables are: Hispanic heritage, age, being born in the US, parental age, parental education, parental religion, number of older siblings, family income, language spoken at home, living in a single-parent home, and living with both biological parents. A description of these variables is presented in Appendix Table A.1.

Table 2: Balancing Tests for Cohort Composition Measures

		Independent variable(s):		
Dep. variable:	Observations (1)	% Black peers, all (2)	% Black peers, opp. gender (3)	% Black peers, same gender (4)
Hispanic	10,327	-0.025	0.014	0.021
		(0.162)	(0.137)	(0.140)
Age	10,354	-0.199	0.022	-0.298
		(0.646)	(0.423)	(0.416)
Born in the United States	10,357	-0.070	0.051	-0.138
		(0.165)	(0.137)	(0.160)
Parent's age	9,036	-0.022	-2.800	2.337
		(2.951)	(3.477)	(2.872)
Parent's education	9,890	0.657	0.451	0.021
		(0.970)	(0.742)	(1.032)
Parent's religion = Catholic	8,371	0.094	0.166	-0.112
		(0.249)	(0.207)	(0.197)
Number of older siblings	10,346	-0.473	-0.201	-0.272
		(0.725)	(0.505)	(0.507)
Log of family income	9,938	0.752	0.448	0.268
		(0.607)	(0.379)	(0.398)
Non-English language at home	10,357	-0.214	-0.127	-0.048
		(0.137)	(0.112)	(0.150)
Single parent family	9,029	-0.247	-0.066	-0.221
		(0.381)	(0.240)	(0.251)
Live w/both biological parents	8,953	0.243	0.134	0.156
		(0.329)	(0.201)	(0.265)
Grade size	10,357	262.598**	127.697**	131.395**
		(118.235)	(60.087)	(61.007)

Notes: Each row reports two separate regressions of the row variable on the all-Black cohort share (column 2) and jointly on the same- and opposite-gender shares (columns 3 and 4); column 1 reports the available observations. School-gender and grade-gender fixed effects, Wave IV weights; standard errors (in parentheses) clustered at the school level. Sample: non-Black students with full Wave I and IV information. Source: Add Health (Harris et al., 2009).

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

testing for non-random clustering of Black students across grades within schools and examining whether within-school variation in the Black share across cohorts is symmetric. First, if variation in the share of Black peers across grades within a school is plausibly exogenous, a student's race should be uncorrelated with the racial composition of their cohort once school fixed effects are included. The results in Appendix Figure A.1 are consistent with this implication and provide support for the identifying assumption. Second, Appendix Figure A.2 examines whether differences in Black shares across grades are symmetric. If changes in grade-level Black shares were driven by selective attrition—such as Black students disproportionately dropping out in higher grades—one would expect systematically lower Black shares in later grades. Reassuringly, the distributions of changes in overall and gender-specific Black shares are centered around zero, suggesting no

systematic trend.

3 Results

3.1 Main Results

Table 3 reports the benchmark results of models (1) and (2), that study the effect of the share of Black peers on future political outcomes from their non-Black peers.[10] All specifications include school-gender and grade-gender fixed effects, and control for grade size (as a consequence of the findings from the balancing test). This table shows a set of results for three different groups of outcomes. First, columns 1 and 2 display the effect of the share of Black peers on political preferences, which is a 1-to-5 categorical variable going from very conservative to very liberal. The share used in column 1 includes all Black peers, while in column 2 this is separated into peers of same and opposite gender. In order to understand more precisely which peers are driving the effect, the rest of the results presented in the table follow this second specification. In columns 3 and 4, I concentrate on the effect of this group of peers on self-identifying as overall conservative or very conservative, respectively. The last set of results, in columns 5 and 6, report the effect of exposure to Black students in the same cohort on being either overall liberal, or very liberal. For columns 2 to 6, I additionally present the p-value of the statistical examination assessing the equality of coefficients for the same-gender and opposite-gender shares.

For model (1), the coefficient for the effect of the share of all Black peers shows a positive correlation with the political outcomes, however, not significantly different from zero at conventional levels. On the other hand, in column 2, when regressing the political preferences on both shares of Blacks, I find a significant and, once again, positive effect of the same-gender share. In terms of magnitude, the coefficient expresses that an increase of 2.7 percentage points in this variable (a within-school standard deviation in the same-gender share) would shift the reported political preferences by 0.023 points toward the liberal end of the 1-to-5 scale, which corresponds to about 2.6% of a standard deviation of the outcome. For the share of opposite-sex peers, the reported effect is negative but very small in size, and statistically insignificant. The fact that the transmission of socio-political preferences seems to occur from the same-gender peers goes in line with findings from related literature (see, for example Giulietti et al., 2022; Kalmijn, 2002; Merlino et al., 2024).

I now turn to the benchmark results on the effect of Black peers on their non-Black cohort classmates on the likelihood of politically self-identifying as a conservative. For both definitions

[10] Appendix Table A.2 shows that these results are robust to different definitions of the sample. Column 1 looks at the main results for only the White students, and column 2 defines as Black those participants who were perceived as Black by the Add Health interviewers, and excludes them from the sample (to focus on non-Black students). The coefficients remain very similar in size and direction.

Table 3: Effect of Black Peers on Future Political Preferences – Main Results

	Political preferences		Conservative		Liberal	
			Overall	Very	Overall	Very
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Baseline specification						
% Black peers, all	0.763					
	(0.468)					
% Black peers, same gender		0.868**	-0.378**	-0.231**	0.118	0.141
		(0.333)	(0.182)	(0.095)	(0.150)	(0.098)
% Black peers, opp. gender		-0.076	0.145	0.016	0.181	-0.096
		(0.353)	(0.182)	(0.073)	(0.152)	(0.107)
P-value (same = opp. gender)		0.07	0.09	0.05	0.78	0.06
Mean of dep. var.	3.020	3.020	0.256	0.044	0.259	0.061
Std. dev. of dep. var.	0.911	0.911	0.437	0.205	0.438	0.239
Individual and family controls	No	No	No	No	No	No
Cohort and peer controls	No	No	No	No	No	No
Observations	10,357	10,357	10,357	10,357	10,357	10,357
Panel B: With extended pre-determined controls						
% Black peers, all	0.645					
	(0.500)					
% Black peers, same gender		0.854**	-0.388**	-0.213**	0.148	0.105
		(0.347)	(0.196)	(0.095)	(0.160)	(0.093)
% Black peers, opp. gender		-0.168	0.206	0.018	0.166	-0.111
		(0.373)	(0.193)	(0.078)	(0.158)	(0.104)
P-value (same = opp. gender)		0.06	0.06	0.07	0.94	0.09
Individual and family controls	Yes	Yes	Yes	Yes	Yes	Yes
Cohort and peer controls	Yes	Yes	Yes	Yes	Yes	Yes
Observations	10,357	10,357	10,357	10,357	10,357	10,357

Notes: OLS estimates; standard errors (in parentheses) clustered at the school level. All specifications include school-gender and grade-gender fixed effects and control for grade size. Panel B adds pre-determined individual controls (race, Hispanic origin, born in the US), family controls (non-English home language, parental age and education, household size, log family income), cohort size by gender and gender ratio, and same-gender peer shares (born in the US, parents born in the US, college-educated parents, working parents, Asian, Hispanic), with missing-value indicators so the sample is identical across panels. Sample: non-Black students with full Wave I and IV information, Wave IV weights. The dependent variable in columns (1)–(2) is the 1-to-5 scale from very conservative to very liberal; in columns (3)–(6), an indicator for the preference in the column title. The p-value tests equality of the same- and opposite-gender coefficients. Net of the fixed effects, the within-school standard deviations of the Black share are 0.021 (all), 0.027 (same gender), and 0.028 (opposite gender). Source: Add Health (Harris et al., 2009).
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

of conservative, I find negative effects of the same-gender share, that in both cases are significant at the 5% level. In the first specification, the coefficient is -0.378. Scaled to the ten percentage point increase in the cohort Black share studied by Billings et al. (2021), it implies a reduction of approximately 3.8 percentage points in the probability of having conservative preferences, which resembles closely in scale their estimated decrease in the probability of being affiliated with the Conservative Party. This comparison is an out-of-sample extrapolation and is to be taken with caution: the within-school variation that identifies the coefficient has a standard deviation of 2.7

percentage points, so a ten percentage point change lies well outside the range of cohort differences the design exploits.[11] The preferred interpretation for this coefficient is to look at an increase of one within-school standard deviation in the share of same-sex Black peers, which would lead to a reduction in the likelihood of self-identifying as overall conservative of around 1.0 percentage points (or 4.0% of the mean of the outcome). For the more extreme definition of this view, the coefficient can be interpreted as a 0.62 percentage point decrease in the likelihood of being very conservative, associated with an increase of a within-school standard deviation of the share of Black peers. Here the effect is relatively sizable and represents 14% of the mean. For the share of opposite-gender Black students in the same cohort, the coefficient is positive in both cases, suggesting this group of peers would have the opposite effect of the same-gender Black students on the likelihood of being conservative, but these coefficients are, once again, insignificantly different from zero and small, especially for the very conservative outcome (0.016). These results, together with the alternative specifications in Appendix Table A.2 (columns 4 and 5), are all in line with the hypothesis that exposure to Black peers leads to a weakening of the conservative ideology expressed.[12]

The last group of results reported in Table 3 is for the likelihood of having overall liberal, or very liberal political preferences. Columns 5 and 6 show that the effect of the same-gender Black peers in both specifications is positive, but statistically insignificant. These results go in line with previous research on political preferences, for example by Billings et al. (2021), who find no effects of the racial changes in cohort's composition on Democratic Party affiliation in the US.

Additionally, Table 4 replicates the analysis from columns 2–4, taking advantage of the fact that political preference questions were also asked in Add Health's Waves III (2001), V (2016), and VI (2022–2024). The table shows that the effect of Black peers on political preferences varies over time; the coefficient is statistically significant only for the 2008–2009 survey period. This pattern also holds for binary outcomes related to conservative and very conservative preferences. These results suggest that the influence of Black peers on political preferences may be context-dependent, with the 2008 presidential election being particularly influenced by racial dynamics (Tesler, 2016; Tesler & Sears, 2010).

[11] Billings et al. (2021) exploit a different source of variation, i.e., the abolition of school segregation policies, which led to big changes in the racial composition of school cohorts.

[12] Essentially, the alternative definitions try to capture possible *anti-sentiments* to the political views, meaning that the *anti*-liberals are defined as those conservative, or middle of the road, and the *anti*-conservative the other way around. I find a positive and statistically significant effect for the share of same-sex Black peers on the expressed *anti*-conservative feeling. The table also reports a negative correlation of the cohort composition with the *anti*-liberal sentiment, but this is not significant at conventional levels.

Table 4: Effect of Black Peers on Political Preferences across Waves

	Wave III 2001–02 (1)	Wave IV 2008–09 (2)	Wave V 2016–18 (3)	Wave VI 2022–25 (4)
Panel A: Political preferences (1–5)				
% Black peers, same gender	-0.367	0.868**	0.183	-0.274
	(0.416)	(0.333)	(0.481)	(0.636)
% Black peers, opp. gender	-0.528	-0.076	0.297	-0.218
	(0.439)	(0.353)	(0.423)	(0.506)
P-value (same = opp. gender)	0.81	0.07	0.87	0.95
Mean of dep. var.	2.967	3.020	2.928	2.836
Observations	8,035	10,357	7,125	6,740
Panel B: Overall conservative				
% Black peers, same gender	0.229	-0.378**	-0.142	0.039
	(0.260)	(0.182)	(0.251)	(0.266)
% Black peers, opp. gender	0.017	0.145	-0.212	0.087
	(0.264)	(0.182)	(0.244)	(0.272)
P-value (same = opp. gender)	0.64	0.09	0.85	0.91
Mean of dep. var.	0.230	0.256	0.299	0.343
Observations	8,035	10,357	7,125	6,740
Panel C: Very conservative				
% Black peers, same gender	-0.045	-0.231**	0.074	0.120
	(0.071)	(0.095)	(0.175)	(0.198)
% Black peers, opp. gender	0.049	0.016	-0.074	0.391**
	(0.070)	(0.073)	(0.139)	(0.171)
P-value (same = opp. gender)	0.35	0.05	0.52	0.32
Mean of dep. var.	0.023	0.044	0.065	0.096
Observations	8,035	10,357	7,125	6,740

Notes: OLS estimates of the political preferences measured in each wave on the Wave I cohort composition; column samples differ because the outcome is observed per wave, and wave-specific cross-sectional weights are used. School-gender and grade-gender fixed effects, grade-size control, non-Black students; standard errors (in parentheses) clustered at the school level. The Wave VI wording differs slightly from earlier waves. The p-value tests equality of the same- and opposite-gender coefficients. Source: Add Health (Harris et al., 2009).
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

3.2 Robustness

Next, I present a series of robustness checks to the benchmark results. A first concern is that the estimates could be driven by omitted characteristics of the students or their cohorts. To address this, in Panel B of Table 3 I re-estimate the benchmark with the full set of pre-determined individual, family, and cohort-composition controls and find nearly unchanged coefficients; following Altonji et al. (2005), this stability under observable controls suggests that the potential bias from unobservables is likely small as well. To quantify this argument, I compute the omitted-variable sensitivity bounds of Cinelli and Hazlett (2020): a confounder would need a partial R^2 of 2.5 percent with both the within-school share variation and the outcome to drive the estimate to zero, and

a confounder up to three times as strong as the strongest observed cohort-level control—the share of peers with college-educated mothers—moves the coefficient only from 0.859 to 0.845. The corresponding sensitivity contours are reported in Appendix Figure A.3. Appendix Table A.6 collects further specification checks—using the same-gender share on its own, adding post-treatment Wave IV controls (with the caution that these are potentially "bad controls", Angrist and Pischke, 2009), and, following Olivetti et al. (2020), controlling for the within-grade standard deviation of the Black share. Across these specifications, the same-gender coefficients lie between 0.808 and 0.810 for political preferences, -0.372 and -0.387 for overall conservative, and -0.187 and -0.196 for very conservative. In Appendix Table A.2 I show that the results are also robust to restricting the sample to White students and to the interviewer-assigned definition of Black respondents of Merlino et al. (2019).

A second concern is that the share of same-gender Black peers could proxy for the cohort's minority composition more broadly, in which case the estimates would reflect exposure to diversity in general rather than to Black peers specifically. To separate the two, I re-estimate the benchmark entering the Hispanic and Asian same- and opposite-gender shares as placebo treatments, with the Black shares kept in the model. Appendix Table A.7 shows that neither placebo reproduces the benchmark pattern: the few placebo coefficients significant at the ten percent level are isolated and differ in sign across outcomes, while the Black same-gender coefficients remain close to their benchmark values (0.937, -0.415, and -0.220 conditional on the Hispanic shares; 0.791, -0.354, and -0.222 conditional on the Asian shares). Because these shares exhibit less within-school variation than the Black share, the table also reports the minimum detectable effect for each placebo coefficient.

A third concern is the cardinal treatment of the 1-to-5 political preferences scale, since respondents need not perceive the distances between the response categories as identical. To account for this, as first proposed by McCullagh (1980), I estimate ordered logit models; the results, reported in Appendix Table A.8, show negative effects on the two conservative response categories, positive effects on the two liberal categories, and no effect on the middle category. Appendix Table A.9 confirms that the implied marginal effects are close to the linear estimates.

Fourth, with 130 school clusters, the analytic clustered standard errors could overstate the precision of the estimates. To check this, I compute wild cluster bootstrap p-values with 9,999 Rademacher draws and report them in Appendix Table A.10: 0.013 for political preferences, 0.048 for overall conservative, and 0.025 for very conservative, against analytic values of 0.010, 0.040, and 0.016. The bootstrap confidence intervals exclude zero for all three outcomes. Two related checks complete the inference analysis. Since the benchmark tests the same treatment on five outcomes, I adjust the family of estimates for multiple hypothesis testing with the stepdown procedure of Romano and Wolf (2005), which yields adjusted p-values of 0.007, 0.039, and 0.012. And

since any variance estimator presumes the validity of the within-school design itself, I also conduct randomization inference in the spirit of Young (2019), reassigning the grade-level profiles of male and female Black shares at random across the sampled grades within each school—holding grade sizes, controls, and fixed effects at their actual values—and comparing the observed cluster-robust t-statistics with their distribution over 1,000 such reassignments. The resulting permutation p-values, reported in Appendix Table A.11, are 0.013, 0.038, and 0.017, so that effects of the estimated size arise in fewer than four percent of random reassignments of cohort composition within schools.

Fifth, the benchmark imposes a particular functional form: the outcome is linear in the share of Black peers, so that the mere presence of a Black peer has no separate effect and the response neither flattens nor steepens at higher shares. I examine the two margins separately; the results for these considerations are reported in Panels A and B of Appendix Table A.12, respectively. To test whether the effect reflects the presence of any Black peer rather than their number, I add to the benchmark an indicator for having at least one same-gender Black cohort peer; the indicator is small and statistically insignificant, while the coefficient on the share remains close to the benchmark. To test the linearity of the response, I replace the share with a two-piece linear spline, i.e., a piecewise-linear function in which the effect of the share is allowed to take one slope below a given threshold and a different slope above it, with the two segments joined at that threshold (the knot), set here at the median of the positive shares (7.6 percent). The two slopes are statistically indistinguishable for all three outcomes, so a single linear slope is not rejected. The effect thus operates through the intensity of exposure, and the linear specification describes it well.

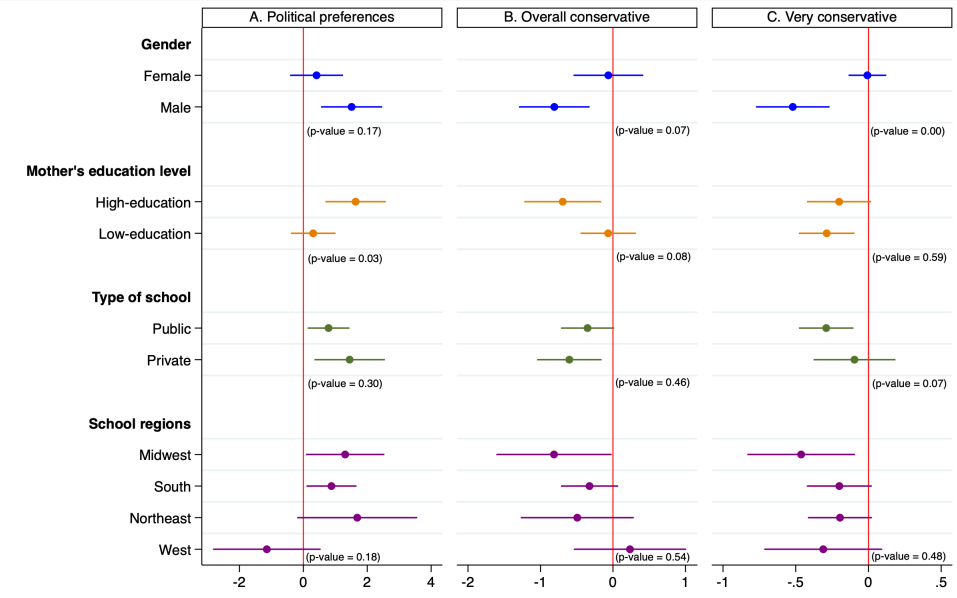
Finally, self-reported political preferences could respond to the race of the Add Health interviewer. To rule out that such reporting drives the results, I interact the peer share with an indicator for a Black Wave IV interviewer. Appendix Table A.13 shows that the effect is present among respondents interviewed by non-Black interviewers, with an insignificant interaction for political preferences and overall conservative, and a somewhat stronger very-conservative response under Black interviewers.

3.3 Heterogeneity Analysis

To gain further insights on the extent to which the cohort's racial composition affects future political preferences, I explore different possible sources of heterogeneity in the effect, by dividing the full sample into sub-groups. The results of this analysis are presented in Figure 1. Each marker represents the coefficient point for the share of same-sex Black peers in each separate regression, for the indicated outcome (i.e., either political preferences, overall conservative, or very conservative) and the indicated subgroup, and is accompanied by its respective 90%-confidence interval.

All regressions control for school-gender fixed effects, grade-gender fixed effects, and grade size. The figure also reports, in parentheses below each comparison group, the p-value for the hypothesis test of equality of coefficients within each comparison group.

Figure 1: Effect of Black Peers on Future Political Preferences – Heterogeneity



Notes: The figure reports the coefficients of the effect of same-gender Black peers on future political outcomes from OLS regressions. Standard errors are clustered at the school level. All model specifications include school-gender and grade-gender fixed effects, and control for grade size. The dep. variable in panel A corresponds to the categorical variable political preferences, in panel B to the overall conservative indicator variable, and in panel C to the very conservative indicator variable (see more in detail the construction of these dep. variables in Appendix Table A.1). The sample corresponds to non-Black students in Add Health that had full information for Waves I and IV. For each category, the sample is divided into the different sub-categories specified in the y-axis. Each coefficient is accompanied by its respective 90% confidence interval. The reported p-value below each category corresponds to the hypothesis test that the coefficients in that group are equal. Add Health cross-sectional weights for Wave IV are used. Source: National Longitudinal Survey of Adolescent Health (Add Health), Harris et al. (2009).

First, in blue, the results are split by the gender of the respondents. For the three sets of outcomes, the results are consistent in direction with the estimates from the full-sample analysis. Interestingly, the cohort's effect remains only statistically significant for the group of men in the sample, and is—for both the overall and very conservative outcomes—significantly different than the effect in the group of women. I interpret these results not as a general statement that the effect of Black peers is stronger for men, because related work, e.g., by Merlino et al. (2024) finds that the effect of exposure to Black peers on other outcomes are also significant for women, but rather as a specific pattern that arises when looking at the development of political preferences. The literature presents many possible explanations for this. It could be driven, for example, by gendered differences in the socialization and construction of political views (Trevor, 1999; Van Ditmars, 2022), or by the degree to which male and female adolescents participate in political discussion and activities (Hooghe & Stolle, 2004; Pfanzelt & Spies, 2019).

Then, in orange, I show the heterogeneity of the effect by comparing those non-Black students whose mothers have at least a college degree, with those with mothers with lower educational

achievements.[13] This analysis can provide insights into how the parents' education level (which has important implications on the political preferences of their children, see Jennings et al., 2009) impacts the process of transmission of political beliefs by exposure to Black peers. Comparing these sub-samples shows that both for the political preferences and the likelihood of being overall conservative, the effects are significantly different at conventional levels, and in both cases they are higher in magnitude for those with high-education mothers. In part, these findings go in line with studies showing how (maternal) education is an important determinant of political preferences (Czarnek et al., 2021; Dunn, 2011; Zingher, 2022). It is plausible that higher levels of maternal education may foster an environment that is more conducive to the operation of the effect of Black peer effects on their children's political attitudes. For the last outcome of interest, the effect of the exposure to Black peers in school points directionally to the same side for both subgroups, but the hypothesis that the two coefficients are the same cannot be rejected.

Third, the comparison of the effects by the type of school the students attended in Wave I—either private or public—is presented in green. The reported p-values for both political preferences and overall conservative outcomes do not reject the hypothesis that the coefficients for the two sub-groups are the same. In both cases, the direction of the Black peers' effect is consistent with the benchmark results, albeit with slightly greater magnitude for students from private schools. However, the effects differ significantly for the outcome of very conservative preferences, and the reduction in the likelihood of identifying as one is stronger among students attending public schools. This difference in the effects may be attributable to contextual differences between Black students in private and public schools (see also Appendix Figure A.4). Additionally, since a vast majority of the students in the sample attended public schools, it is possible that the analysis of the effect of Black peers in private schools lacks sufficient variation.

Lastly, the figure presents in purple the estimates for the impact of Black peers, divided into the four broad Census Bureau Regions where the schools were located in Wave I (i.e., Midwest, South, Northeast, and West). This analysis is relevant, as it may have valuable implications for the interpretation of my work. For example, there are many studies that show important regional differences in the impact of race (see, for example, Kousser, 2010) and the long-lasting legacy of slavery (Acharya et al., 2016) in determining political behavior. Similar to the work by Merlino et al. (2019), the results show no statistically significant differences of the effect among the different regions for any of the studied outcomes. The effect seems to be the strongest in all cases for the Midwest region, where the coefficient remains at the 10%-level significant in all specifications, followed by the schools located in the South and the Northeast. For the political preferences, and the probability of being overall conservative in the West subgroup, the direction of the coefficient is opposed to the main results, which raises the question of whether there are structural differences

[13]The results remain almost unchanged when separating the groups by the level of paternal education.

in this sector leading to different effects of the level of interraciality in schools.[14]

4 Mechanisms

Next, I analyze the mechanisms underlying the finding that the school cohort's racial composition shapes the political preferences individuals express in adulthood. Two canonical theories guide this analysis, and they make opposite predictions in my setting. Contact theory holds that attitudes change through personal relationships with members of the other group (Allport, 1954; Pettigrew et al., 2011), so the effect should be concentrated among students who formed interracial friendships. Group-threat theory holds that a larger outgroup presence provokes hostility and defensive reactions (Key & Heard, 1949), so exposure should have worsened the climate of the schools and pushed preferences in the opposite direction from the one I find. The following subsections test these predictions in turn, and neither theory holds in its strict form. The effect is concentrated among students without personal cross-race ties, it is stronger where the Black peers are central in the school's social life, the climate of more exposed cohorts shows no hostility and, if anything, greater social acceptance, and it is driven by the students who feel part of the shared environment. These results suggest that exposure works through the school environment itself and that it changes the preferences of the students who identify with it. The effect is also not driven by intermediate outcomes such as political participation, education, or employment, which I document in the appendix.[15] I close by tracing the interracial ties formed in school into adulthood.

Contact versus exposure

Intergroup contact theory states that positive contact experiences can lead, under appropriate conditions, to a reduction of prejudice, stereotyping, and discrimination, and a large empirical literature supports it (Allport, 1954; Cáceres-Delpiano et al., 2021; Pettigrew et al., 2011; Rao, 2019). I test this channel using the friendship nominations from the in-school questionnaire. In Table 5 I first show that cohort composition translated into personal ties. A higher share of same-gender Black peers significantly increases the probability of nominating a Black same-gender friend, the share

[14] Because of data limitations, each subgroup has a relatively low number of observations (ranging from 1,581 in Northeast to 3,345 in the South) so that I cannot broaden my analysis into this question, as there may be too little variation for the model to calculate accurate estimates. Further research should focus more on these regional differences in the transmission of political preferences.

[15] The effect is not driven by intermediate outcomes. The Black peer shares are uncorrelated with party identification, turnout in the 2000 presidential election, and voter registration at Wave III, and with the voting frequency reported in Waves IV and V (Appendix Table A.14). They are equally uncorrelated with college attendance, employment, income, voter registration and turnout, and criminal behavior at Wave IV (Appendix Figure A.5). Voting frequency varies with the extremity of political views rather than their direction (Appendix Table A.15), so participation-based mediation of a directional effect is in any case weakly identified.

of Black friends among all nominations, and the probability of a reciprocated Black friendship.

Table 5: First Stage: Cohort Composition and Own Interracial Friendships

	Has Black friend (1)	Share Black friends (2)	Reciprocated Black friend (3)
Grade black share, same gender	0.902***	0.452***	0.536***
	(0.172)	(0.113)	(0.191)
Grade black share, opposite gender	0.492*	0.308**	0.199
	(0.256)	(0.152)	(0.233)
P-value same = opp.	0.22	0.44	0.22
Mean of dep. var.	0.079	0.036	0.028
Observations	6,688	6,688	6,688

Notes: OLS estimates. Dependent variables: the respondent's own same-gender Black friendship measures from the in-school nominations, top-coded at five friends per gender; sample restricted to respondents with valid nomination data. School-gender and grade-gender fixed effects, grade-size control, Wave IV weights; standard errors (in parentheses) clustered at the school level. Source: Add Health (Harris et al., 2009).
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

If the political effect operated through personal friendships, it should be stronger among the students who formed them. In Table 6 I therefore add to the benchmark an indicator for having a same-gender Black friend, together with its interaction with the same-gender share, estimated on the full sample with valid nomination data. The interactions, however, offset the main effects for all three outcomes, and do so significantly for the overall conservative outcome, where the interaction coefficient is 0.299. The conservatism-reducing effect is therefore concentrated among respondents without a Black same-gender friend, and the implied effect for those with one is close to zero. A Gelbach-style accounting leads to the same conclusion, attributing only between 4% and 6% of the benchmark coefficient to own friendship ties.[16]

The friendship indicator is a coarse measure of contact, and the in-school questionnaire contains two finer margins. Each nomination carries five interaction items—whether the friends visited each other, met after school, spent the weekend together, discussed a problem, or spoke on the phone in the last seven days—which identify close ties, and the activity roster records the clubs and teams in which contact is cooperative and sustained, the type of contact that Lowe (2021) shows to be decisive in a field experiment on intergroup integration. If the effect operated through actual interaction with Black classmates rather than through mere acquaintance, it should be concentrated

[16]The friendship measures are an equilibrium outcome, so this accounting decomposes the coefficient rather than identifying a causal mediation; the subsample split is subject to the same caveat. The friendship information is available for the subsample of respondents with valid in-school nomination data.

Table 6: Exposure With and Without a Black Friend: Full-Sample Interaction

	Political preferences (1)	Overall conservative (2)	Very conservative (3)
% Black peers, same gender	0.731	-0.248	-0.064
	(0.540)	(0.287)	(0.106)
% Black same gender × Black friend	-0.322	0.299**	0.074
	(0.314)	(0.142)	(0.130)
Has same-gender Black friend	0.105	-0.064	-0.024
	(0.091)	(0.044)	(0.018)
% Black peers, opp. gender	-0.521	0.262	0.012
	(0.574)	(0.271)	(0.120)
Observations	6,688	6,688	6,688

Notes: OLS estimates on the full sample with valid nomination data; an indicator for a same-gender Black friend and its interaction with the same-gender share are added to the benchmark, so the same-gender coefficient is the effect for respondents without such a friend, and the opposite-gender share enters uninteracted. The moderator is an equilibrium outcome: the estimates decompose the effect and do not identify causal mediation. School-gender and grade-gender fixed effects, grade-size control, Wave IV weights; standard errors (in parentheses) clustered at the school level. Source: Add Health (Harris et al., 2009).
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

among respondents with such close ties, which is not the case.[17] For the cooperative margin, in Appendix Table A.16 I split the same-gender share into two components with a common denominator, the Black peers who share at least one club or team with the respondent and those who share none, so that the two coefficients add up to the benchmark effect. If cooperative contact drove the effect, the coefficient on the co-participating peers should be the larger one. The two coefficients are statistically indistinguishable, for team sports as for other activities.

A further test uses variation in contact that does not depend on the students' choices. School administrators report, for each grade, whether English classes are grouped by ability, a practice that decouples classroom composition from cohort composition. Because this indicator varies at the school-grade level, it is identified within schools, and it is uncorrelated with the shares themselves. If classroom-level contact carried the effect, it should weaken in tracked grades. In Appendix Table A.17 I interact the shares with the tracking status of the respondent's own grade and find no such weakening. The interaction is statistically insignificant, and the same-gender effect in tracked

[17] I repeat the interaction exercise of Table 6 with an indicator for having a same-gender Black tie with at least two of the five interaction items in place of the friendship indicator. The interaction coefficients are close to zero and statistically insignificant for all three outcomes, although close ties respond strongly to cohort composition, and conditioning on them accounts for at most 9 percent of the benchmark coefficient.

grades, at 0.965, is if anything larger than in untracked ones.

Exposure and contact can also be separated by where the effect is strongest. If ambient exposure drives it, the effect should increase with how central the Black peers are in the school's social life, independently of the respondent's own ties, and the friendship network allows me to measure this centrality. In Table 7 I interact the same-gender share with the mean within-school-standardized Bonacich centrality of the same-gender Black cohort members, on cohorts with at least three Black peers. The moderator itself does not respond to the share. The interactions are 0.380 for political preferences and -0.226 for overall conservative, significant at the ten and five percent levels, and both carry the same sign as the benchmark effects, so that the effect is stronger where the Black peers are more central in the school's social life. The respondent's own centrality, in contrast, does not moderate the effect, with small and statistically insignificant interactions for all three outcomes. When the centrality of the Black peers and the socioeconomic composition of this group are included jointly, the centrality interaction remains significant at the 5 percent level, while the interaction with the share of peers with college-educated mothers is statistically insignificant.

Table 7: The Social Centrality of the Black Peers and the Exposure Effect

	Political preferences (1)	Overall conservative (2)	Very conservative (3)
Grade black share, same gender	0.372	-0.182	-0.208*
	(0.404)	(0.202)	(0.114)
% Black same gender × Black-peer centrality	0.380*	-0.226**	-0.045
	(0.220)	(0.113)	(0.042)
Black-peer centrality (std.)	-0.015	0.010	0.003
	(0.032)	(0.013)	(0.006)
Grade black share, opposite gender	-0.290	0.246	-0.022
	(0.425)	(0.206)	(0.098)
Min. detectable interaction (80% power)	0.617	0.317	0.118
Mean of dep. var.	3.024	0.260	0.045
Observations	5,133	5,133	5,133

Notes: OLS estimates. The same-gender share is interacted with the mean within-school-standardized Bonacich centrality of the same-gender Black cohort members, measured in the Wave I in-school network, on cohorts with at least three Black peers. Minimum detectable interaction effects (5% level, 80% power) are reported. The moderator is an equilibrium feature of the cohort; the estimates are heterogeneity, not a causal interaction. School-gender and grade-gender fixed effects, grade-size control, Wave IV weights; standard errors (in parentheses) clustered at the school level. Source: Add Health (Harris et al., 2009).
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

These results indicate that the effect is not primarily carried by students who formed interracial

friendships, but by exposure to Black peers in the shared school environment, an effect that is stronger where those peers are more central in the school's social life. This evidence is more consistent with an information channel than with contact in the strict sense of Allport (1954), and implies that the relevant treatment is the composition of the environment rather than the realized friendship network.

The school environment and identification with it

If the relevant treatment is the environment, a natural question is what that environment looked like while exposure was taking place. The in-school questionnaire allows me to answer this question contemporaneously, as the full census of students in the sampled schools—around 65,000 non-Black respondents—rated the climate of their school in 1994–95, thirteen years before the political outcome is measured. Competing theories make opposite predictions for these items. Group-threat accounts predict that non-Black students in more exposed cohorts perceive more prejudice and feel less safe, while normalization through everyday exposure predicts a stable or better climate. I use the strongest answer to each item, strongly agreeing and, for the prejudice item, strongly disagreeing, so that all seven indicators point toward a better climate and capture emphatic rather than mild assent, the margin exposure should move most clearly. I summarize them in the first principal component of their polychoric correlations, scored for every respondent who answers at least one item.

In Table 8 I estimate the benchmark specification on the census, with the index in the first row and the seven items below. The index increases with the same-gender share, with a coefficient of 0.345 that is significant at the 5 percent level, while the opposite-gender coefficient is small and negative. One within-school standard deviation in the share raises the index by about 1 percent of a standard deviation. The item rows show where this effect comes from and give no support to the threat predictions. The indicators for perceived prejudice and safety, the two margins on which threat accounts predict deterioration, have coefficients close to zero with small standard errors. The effect is concentrated in feeling part of the school, being happy at school, and feeling socially accepted, the belonging dimensions of the battery and the ones closest to the identification channel examined below. For these three items, the equality of the same- and opposite-gender coefficients is rejected at the ten or five percent level, the same gender asymmetry as in the benchmark results. The broader agree-or-strongly-agree versions of the outcomes, reported in Appendix Table A.18, show the same pattern, with feeling socially accepted increasing by about half a percentage point per within-school standard deviation on a mean of 67 percent and the equality of the two coefficients rejected at the 5 percent level. I read these results as the climate counterpart of the benchmark findings. Larger Black cohorts left the threat-related dimensions of the school climate

unchanged and moved its belonging dimensions toward inclusion.

Table 8: School Climate at the Moment of Exposure: Strong Answers and Summary Index

Outcome (strongly agrees)	% Black peers, same gender	% Black peers, opp. gender	Observations	P-value (same = opp.)
Climate index (polychoric PCA, std.)	0.345**	-0.056	59,706	0.100*
	(0.163)	(0.190)		
Rejects that students are prejudiced	-0.032	-0.025	58,049	0.889
	(0.053)	(0.056)		
I feel safe in my school	0.041	-0.004	58,395	0.621
	(0.067)	(0.065)		
Close to people at this school	0.058	-0.002	59,319	0.598
	(0.070)	(0.072)		
Feel part of this school	0.146**	-0.022	58,921	0.071*
	(0.071)	(0.065)		
Happy to be at this school	0.162*	-0.053	58,634	0.046**
	(0.083)	(0.093)		
Teachers treat students fairly	0.005	0.057	58,502	0.531
	(0.052)	(0.061)		
Feel socially accepted	0.193**	-0.073	58,175	0.036**
	(0.079)	(0.082)		

Notes: Each row is a separate OLS regression on the Wave I in-school census. The dependent variables equal one only for the strongest answer to each item (S62): strongly agreeing with the statement, and for the prejudice item strongly disagreeing, so that all items are oriented toward a better climate. The climate index is the first principal component of the polychoric correlation matrix of the seven indicators, computed from pairwise correlations and scored for every respondent answering at least one item, standardized. School-gender and grade-gender fixed effects, grade-size control, non-Black students; unweighted, as the questionnaire is a census. Standard errors (in parentheses) clustered at the school level. The last column reports the p-value of the test of equality of the same- and opposite-gender coefficients. Source: Add Health (Harris et al., 2009).
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

If the treatment is the shared school rather than personal ties, the effect should be concentrated among students attached to that school. This is the prediction of the common ingroup identity model (CIIM) of social psychology. The CIIM proposes that intergroup bias can be reduced via the re-categorization of pre-established separate groups—"us" and "them"—into an inclusive ingroup sharing a common superordinate identity, in this case belonging to the same school (Gaertner & Dovidio, 2005; Gaertner et al., 1993). This framework is relevant in my setting because racial in- and out-groups form precisely during childhood and adolescence (De Franca & Monteiro, 2013; Hagerman, 2020), so that a shared school identity can become more salient than racial categories and, potentially, lead to long-term changes in political preferences. I test this mechanism in two complementary ways. First, as a proxy for a formed common identity, I use four Wave I in-home questions on the students' feeling of belonging to their school—whether they get along with their peers, feel close to them, feel part of the school, and are happy there. Under the CIIM, the effect of exposure to Black peers should be concentrated among students with high scores on these

variables, as they are more likely to have undergone the re-categorization.[18] In Table 9 I estimate the benchmark separately on the high- and low-scoring half of each item and find that the effect remains statistically significant in the high-scoring groups of all four items—at the ten percent level for the feeling-close item—and in the low-scoring group only for the get-along split. Similar conclusions hold for the likelihood of identifying as conservative (Appendix Table A.19). These splits rely on endogenous, self-reported measures, and they cannot distinguish whether non-Black students include their Black peers in the reported "we".

Table 9: Heterogeneity in the Effect by School-Belonging Categories

	Gets along with others (1)	Feels close to people (2)	Part of the school (3)	Happy at school (4)
High belonging	6.086**	0.851*	1.251***	0.788**
	(2.454)	(0.440)	(0.400)	(0.398)
Low belonging	0.715**	0.526	0.274	0.954
	(0.316)	(0.715)	(0.738)	(0.784)
Mean of dep. var., high	3.092	3.015	3.009	3.008
Mean of dep. var., low	3.033	3.083	3.111	3.094
Observations, high	725	6,978	7,527	6,889
Observations, low	9,627	3,372	2,823	3,464

Notes: Each cell reports the same-gender coefficient from the benchmark estimated separately on the high- and low-belonging half of each item; the outcome is Wave IV political preferences (1-to-5, very conservative to very liberal). Items: getting along with other students (H1ED18), feeling close to people (H1ED19), feeling part of the school (H1ED20), and being happy at school (H1ED22); the high/low classification follows the original figure. All specifications include the opposite-gender share, school-gender and grade-gender fixed effects, and grade size; non-Black students, Wave IV weights; standard errors (in parentheses) clustered at the school level. The items are self-reported during exposure, so the splits are suggestive heterogeneity. Source: Add Health (Harris et al., 2009).
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Second, I construct a measured counterpart from the friendship nominations, a cohort-level integration index defined as the observed share of same-gender cross-race ties among non-Black nominators minus the share expected under random mixing given the cohort's composition. If re-categorization through realized ties drove the effect, it should be stronger in cohorts where composition translated into ties. In Appendix Table A.20 I interact the benchmark specification with this index and find no such amplification. The interactions are statistically insignificant, with the exception of a positive coefficient for overall conservative that, if anything, points to a weaker

[18] See Appendix Table A.1 for the construction of these variables. For the items on feeling close to people, feeling part of the school, and being happy at school, respondents answering "strongly agree" or "agree" are classified as high-scoring; the get-along item is asked as a frequency and is dichotomized as described there.

response in better-integrated cohorts. Merlino et al. (2024) interact their residential effect with school-level friendship segregation and likewise report no amplification. Belonging to the school thus moderates the effect, while the realized integration of the cohort does not.

Taken together, the results of this subsection provide positive evidence that the school environment itself is the operative treatment. Exposure improved the belonging dimensions of the school climate while it was taking place, and the long-run political effect is concentrated among the students who report belonging to that environment, in both cases with the same-gender pattern of the benchmark results. The contact-based alternatives find no support, as friendships carry little of the effect and realized integration does not amplify it. This reading also fits the timing documented in Section 3, as an attitude formed through the environment remains latent until national politics makes race salient and only then surfaces in expressed preferences.

The persistence of interracial ties

A last exercise asks whether the interracial ties formed in school leave a trace in adult life, as the residential findings of Merlino et al. (2024) would suggest. In Table 10 I estimate the benchmark specification on four behavioral outcomes that follow such ties across waves: the in-school friendships of Wave I, current friendships with former schoolmates at Wave III, romantic partners at Wave III, and the racial composition of the census tract of residence at Wave IV. The first-stage relationship on the Wave I friendships remains strong, while the adult outcomes respond less. The probability of a current Black romantic partner increases with the same-gender share, with a coefficient of 0.239 that is significant at the 10 percent level. The cumulative partner measure and the schoolmate-friendship measure—the latter available only for respondents in grades 7 and 8 at Wave I, with 714 observations and correspondingly large standard errors—do not respond, and the Black share of the Wave IV residential tract is unaffected. Exposure thus shifted attitudes without inducing residential sorting, and its trace in adult relationships is suggestive at best. The next section examines whether the attitude effect nevertheless registers in political behavior at scale.

5 From Preferences to Electoral Outcomes

The estimates so far establish that exposure to Black peers in adolescence shifts political preferences, measured as attitudes within one cohort study. Whether this effect carries economic and political significance depends on two further questions: whether it extends from stated preferences to political behavior, and whether it is large enough to register in aggregate outcomes. The desegregation of American schools offers a setting in which both questions can be studied, as it created large differences in childhood exposure to Black schoolmates across counties and birth cohorts,

Table 10: The Behavioral Chain: Interracial Ties from Adolescence to Adulthood

Outcome	% Black peers, same gender	% Black peers, opp. gender	P-value (same = opp.)	Mean of dep. var.	Observations
Same-gender Black friend, in-school (W1)	0.902***	0.492*	0.22	0.078	6,688
	(0.172)	(0.256)			
Current Black friend among former schoolmates (W3)	-0.094	0.055	0.88	0.174	714
	(0.644)	(0.698)			
Any Black romantic partner since 1995 (W3)	0.100	-0.247	0.25	0.108	7,276
	(0.186)	(0.188)			
Current Black romantic partner (W3)	0.239*	-0.105	0.17	0.043	7,276
	(0.127)	(0.166)			
Proportion Black, tract of residence (W4)	-0.052	0.064	0.28	0.093	10,353
	(0.086)	(0.071)			

Notes: Each row is a separate benchmark OLS regression of the behavioral outcome on the cohort shares (school-gender and grade-gender fixed effects, grade size, non-Black students; standard errors, in parentheses, clustered at the school level). Wave I friendship is measured from the in-school nominations. The Wave III schoolmate outcome comes from a module asked only of Wave I 7th–8th graders, who indicated which of ten former schoolmates are current friends; race comes from the Wave I roster, and the outcome is defined when at least one offered name is Black. Partner outcomes cover all relationships reported since summer 1995; the Wave IV outcome is the tract Black share of the Wave IV residence (ACS 2005–2009). Wave III outcomes use Wave III weights, Wave I and IV outcomes Wave IV weights. These are equilibrium outcomes, not causal mediation. Source: Add Health (Harris et al., 2009).
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

and these differences entered the electorate as the affected cohorts reached voting age. This section traces them through survey partisanship, validated turnout, and county presidential returns. I find that cohorts with higher childhood exposure to Black peers identify more Democratic, vote more, and shift the vote shares of their counties, but only after race became a central dimension of partisan competition.

I measure the childhood exposure of birth cohort b in county c, denoted $e(c, b)$, as the average Black enrollment share in the county's schools over the twelve years in which the cohort attends grades one through twelve, using the school-level OCR surveys for 1968–1986 and the CCD universe for 1987–2008 described in Section 2. The analysis covers the 1962–1991 birth cohorts in 3,120 counties, those whose entire school window falls inside the enrollment panel, and by 2024 these cohorts constitute 62 percent of the White voting-age population, computed from the census county populations by age (Schroeder et al., 2026). The differences in exposure are large. Among the cohorts born 1976–1983, and weighting county-cohort cells by their White population, the gap between the 90th and the 10th percentile of childhood exposure is 34.0 percentage points. The measure also aligns closely with Add Health. For the 1994/95 school year, the mean, median, and 90th percentile of the Black schoolmate share of non-Black students are 0.091, 0.026, and 0.282 in the CCD, compared with 0.092, 0.026, and 0.300 in Add Health. Appendix Figures A.6 and A.7 show the geography of the measure and the distributional comparison.

In a first step, I take this measure to the individual level and relate it to the political outcomes of White CES respondents of these cohorts, as Calderon et al. (2023) relate the Black inflows of the Great Migration to the stated preferences of White survey respondents. The model I estimate can be written as:

$$y_{icb} = \beta\, e(c, b) + \alpha_c + \gamma_b + \varepsilon_{icb}, \tag{3}$$

where y_{icb} is the political outcome of respondent i of birth cohort b living in county c, an indicator for identifying as a Democrat or for validated general-election turnout, $e(c, b)$ is the childhood exposure defined above, α_c and γ_b are county and birth-cohort fixed effects, and ε_{icb} is an error term. I use CES survey weights and cluster standard errors at the county level. Exposure is assigned by the county of residence at the time of the survey, so β is a descriptive gradient rather than a causal effect, and residential mobility attenuates it toward zero. Table 11 reports the estimates. A ten percentage point increase in childhood exposure predicts a 2.2 percentage point higher probability of Democratic identification, a 3.0 percentage point higher probability of validated turnout, and a 3.2 percentage point higher probability of a reported Democratic vote in presidential elections, all significant at least at the 5 percent level.[19]

Table 11: Childhood Exposure, Partisanship, and Turnout in the CES

	P(Democrat)		P(validated vote)		P(Dem. pres. vote)	
	All	Stayers	All	Stayers	All	Stayers
	(1)	(2)	(3)	(4)	(5)	(6)
Childhood exposure	0.222**	0.256**	0.297***	0.314**	0.323***	0.254
	(0.086)	(0.110)	(0.097)	(0.128)	(0.115)	(0.165)
Mean of dep. var.	0.413	0.394	0.534	0.587	0.491	0.460
Observations	170,820	67,536	134,011	69,725	116,342	49,000

Notes: Estimates of county-cohort childhood exposure to Black schoolmates on Democratic identification (seven-point party identification of 3 or below) and validated general-election turnout, for White CES respondents of the 1962–1991 birth cohorts with fully covered exposure windows (Kuriwaki, 2025); exposure is assigned by county of residence and birth year. County and birth-cohort fixed effects, CES survey weights; standard errors (in parentheses) clustered by county. Stayers: five or more years at the present address or fifteen or more in the current city. Turnout is estimated on even survey years. The presidential vote outcome is an indicator for a reported Democratic vote among two-party voters, estimated in presidential survey years.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Administrative registration records provide further evidence on these gradients. Among the 1.9 million registered White non-Hispanic voters of these cohorts in the North Carolina voter file, the same specification estimated on county-by-cohort registration shares yields a Democratic share of two-party registration that is 5.7 percentage points higher for a ten percentage point increase in childhood exposure, and a Democratic registration gradient of 0.220 that matches the CES estimate. The gradient reflects movement between the parties, as the Republican share falls by a corresponding amount while the unaffiliated share does not change, and it is largest among the

[19] Residential mobility attenuates these gradients, since movers carry the exposure of their current county. Restricting to respondents with five or more years at their current address yields $\beta = 0.256$ (s.e. 0.110) for Democratic identification and $\beta = 0.314$ (s.e. 0.128) for turnout; the subsample reporting fifteen or more years in their current city is too small to be informative; these estimates are reported in Appendix Table A.21.

youngest cohorts, who registered long after the Southern realignment. Appendix Table A.22 reports the estimates.

In a second step, I ask when exposure begins to predict partisanship. If the attitude surfaces only once race divides the parties, as the Add Health results indicate, the association should be recent rather than constant. The association is indeed absent in the 2006–2010 surveys, which predate the sorting of racial attitudes into party identification documented by Tesler (2016). It emerges in 2012–2014, as race moved to the center of partisan competition over Barack Obama's presidency, and is largest in 2020–2024, once that division was entrenched (Appendix Figure A.8). The cohorts covered are the same in every survey year, so this timing reflects the political environment rather than a change in who is observed.

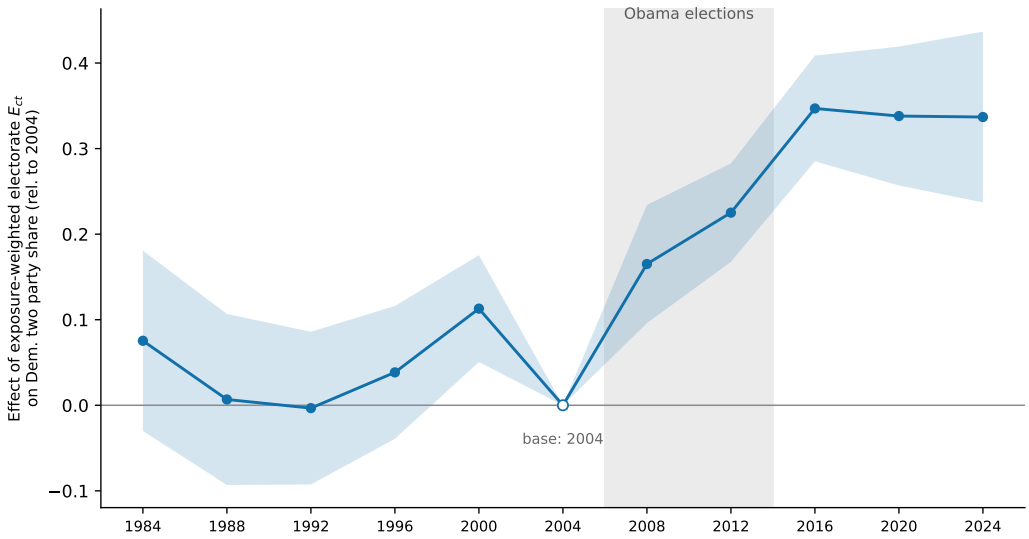
Next, I aggregate the exposure of the electorate to the level of county presidential returns. I define the exposure of the White electorate as $E_{ct} = \sum_b w_{bct}\, e(c, b)$, where the weight w_{bct} is the share of birth cohort b in the White voting-age population of county c in election year t, normalized over the cohorts with exposure data. These weights are built from predetermined quantities. Each cohort's size in each county is measured once, in the 1980 census or, for later cohorts, in the first census after their birth, and is moved forward over time with the national survival ratio of the cohort, in the spirit of the simulated instruments of Currie and Gruber (1996). Migration after the baseline census does not enter the weights, so within a county E_{ct} changes only through cohort replacement, rising where later cohorts were schooled with larger Black enrollment shares and falling where they were not. With this measure, I estimate the event-study model:

$$D_{ct} = \sum_{k \neq 2004} \theta_k \, \mathbf{1}\{t = k\} \, E_{ct} + \alpha_c + \gamma_{st} + X_{ct}'\delta + \varepsilon_{ct}, \tag{4}$$

where D_{ct} is the Democratic two-party vote share of county c in presidential election t, α_c are county fixed effects, γ_{st} are state-by-election-year fixed effects, and X_{ct} contains the coverage of the exposure measure, the share of the county's White voting-age population that belongs to the cohorts with exposure data. This share rises from about one quarter of the electorate in 1980 to about 70 percent in 2016 as the covered cohorts age in, at different speeds across counties, so the control separates the arrival of exposure from the arrival of young voters. Because E_{ct} combines predetermined cohort exposures with demographic cohort shares, the design is a shift-share regression, and identification requires that counties whose electorates gained exposure at different rates would otherwise have followed parallel trends in the Democratic share, which can be justified either by exogeneity of the cohort shares (Goldsmith-Pinkham et al., 2020) or by quasi-random variation in the cohort exposures (Borusyak et al., 2022). Counties are weighted by their total votes and standard errors are clustered at the state level. Figure 2 reports the estimates. The coefficients for 1984 through 1996 are within 0.075 of zero and statistically insignificant, and the

2000 coefficient is 0.113, significant at the 1 percent level. From 2008 the coefficients rise to 0.165, reach 0.225 in 2012 and 0.347 in 2016, and remain at 0.337 through 2024, all significant at the 1 percent level. I assess the parallel-trends assumption with three tests, a joint Wald test of the pre-period coefficients, a linear trend fitted through them, and, since a pretest alone cannot settle the assumption (Roth, 2022), the sensitivity analysis of Rambachan and Roth (2023). First, the Wald test rejects parallel trends, with an F statistic of 91.[20] Second, a linear trend fitted to the pre-period coefficients has a slope of -0.001 per election, with a standard error of 0.008, and the steepest trend this allows produces 0.05 in 2016, against the estimated 0.347. The rise after 2004 is therefore not the continuation of a pre-existing trend. Third, the sensitivity analysis allows parallel trends to fail after 2004 by up to $\bar{M}$ times the largest pre-period change, the 0.113 swing between 2000 and 2004. At $\bar{M} = 0.5$ the robust confidence intervals, $[0.07, 0.43]$ in 2012 and $[0.10, 0.63]$ in 2016, exclude zero; at $\bar{M} = 1$ they include it. The estimates therefore survive violations of half the size seen in the pre-period, but not larger ones.

Figure 2: White-Electorate Exposure and County Democratic Vote Share



Notes: The figure reports the coefficients on the interaction of White-electorate exposure E_{ct} with election-year indicators, relative to 2004, from a regression of the county Democratic two-party vote share on these interactions with county and state-by-election-year fixed effects, a control for electorate coverage, and county total votes as weights. The sample covers the 1980–2024 elections; the 1980 coefficient, 0.046 with standard error 0.053, is not displayed. The specification includes no control for racial composition; the composition-controlled version is reported in the text. Standard errors are clustered at the state level; the shaded band shows 95% confidence intervals. Sources: MIT Election Data and Science Lab (2018), Amlani & Algara (2021), Schroeder et al. (2026).

[20]The validity of this test should be taken with caution, as the clustered covariance matrix of the pre-period coefficients is near singular, with pairwise correlations of up to 0.99 across 51 state clusters, so the statistic loads on directions of the coefficient vector that are estimated with almost no variance.

A concern with these estimates is that the model contains no control for the racial composition of the electorate, so part of the rise in the Obama elections could reflect the mobilization of Black voters in high-exposure counties rather than the preferences of White voters. I address this concern in two ways, re-estimating the model with a control for racial composition, and testing whether turnout, the margin on which mobilization operates, moves with exposure in the counties where Black voters live. First, I re-estimate the model, including the main effect of E_{ct}, with election-year fixed effects and the contemporaneous Black share of the voting-age population. This control is demanding, as current racial composition is itself an outcome of the same desegregation histories, so it removes part of the effect of interest together with the confound. The uncontrolled estimates of Figure 2 therefore overstate the White-electorate effect and the controlled estimates understate it, and the effect lies between the two. In the controlled version the coefficients halve, with 0.040 in 2012 and 0.101 in 2016, significant at the 5 and the 1 percent level, and the 2008 coefficient is zero, which matches the individual-level survey evidence, where the association among White respondents appears only from 2012 (Appendix Figure A.9). In both versions the association appears in 2012 and peaks in 2016, so it survives the control, if at half its size; the controlled 2020 and 2024 coefficients are 0.076 and 0.067, significant at the 5 and the 10 percent level. Second, if Black mobilization drove the rise, the additional Black ballots would raise turnout itself, and they would do so in the counties where Black voters live. I therefore replace the vote share with the county turnout rate, total votes cast over the Black-plus-White voting-age population, as the outcome of the event-study model. Exposure predicts a turnout rise of 0.077 in 2008 and 0.112 in 2012, both significant at the 1 percent level, which fades after 2016, in the elections in which Black turnout rose nationally (Fraga, 2018). Estimated separately by the counties' 1980 Black share of the voting-age population, however, the turnout response is largest in the counties below 5 percent, at 0.176 in 2008 and 0.317 in 2012, where Black voters are too few to produce it, and smallest in the counties above 20 percent. In the nearly all-White counties, a ten percentage point increase in electorate exposure predicts a 3.2 percentage point higher turnout rate in 2012, against the 3.0 percentage point individual-level turnout gradient of Table 11. The aggregate surge is therefore consistent with the turnout response of exposed White voters to the first Black presidential candidacies, in line with the evidence that Black candidates raise the turnout of White voters (Washington, 2006), rather than with Black mobilization alone. Appendix Table A.23 reports the estimates.

Taken together, the results of this section place the within-school estimates in the national electorate. White Americans of the cohorts schooled with more Black peers identify more Democratic, register Democratic rather than Republican, turn out more, and report more Democratic presidential votes, with gradients that agree between the survey and the administrative records and amount to three fifths of the within-school causal estimate over the same exposure gap, consistent with

attenuation from residential mobility. The counties whose electorates carry this exposure moved toward the Democratic party as these cohorts entered them, and the composition-controlled estimates account for over half of the 2004–2016 divergence in Democratic share between high- and low-exposure counties. Every outcome shows the same timing, absent while race is peripheral to partisan competition, arriving on the turnout margin with the Obama candidacies, and strongest once the parties divide over race. The racial composition of school cohorts thus predicts not only the attitudes their students express but the returns of the elections they later vote in.

6 Conclusion

This work studies the effect of Black peers on the political preferences their classmates hold in adulthood. To do so, I draw upon data from Add Health, a longitudinal study that has followed a representative group of high-school students in the US for over two decades, and exploit variation in the proportion of Black students within schools across different cohorts aiming to identify causal effects. I concentrate on the expressed political preferences in 2008–2009, a time frame in American politics in which race was especially salient in determining political opinions, with the Presidential election of Barack Obama, the first Black president in US history.

I find that a higher share of Black peers—and in particular those of the same gender—has a significant impact in shifting these political views away from conservative, to more liberal tendencies. The findings suggest that the racial composition of a cohort is a significant factor in reducing the probability of individuals' identifying as highly conservative or conservative, without resulting in a substantial increase in extremely liberal beliefs.

This study examines three channels that potentially underlie the effect of Black peers on their classmates' future political preferences: First, I find suggestive evidence that the re-categorization of racially divided groups into a shared school-belonging identity may be one of the forces behind the results. Then, looking at the mediating role of increased political participation in shaping future political preferences, I conclude that this is unlikely to be a major force behind the results. However, due to data limitations, I focus on the third mechanism—i.e., the role of information updating and increased awareness of racial discrimination—from a theoretical perspective, and argue that this may play an important role in explaining the observed patterns of political preferences.

Regarding policy implications, my analysis suggests that promoting interracial schools and increasing the share of Black students could be an effective policy to prevent the development of the polarizing and far-right political tendencies that have emerged in recent years. Furthermore, this work highlights how interracial peer groups in schools facilitate the development of shared identities, which may play a crucial role in raising the awareness of issues related to racial justice and promoting political consensus that addresses racial inequality in society.

There are numerous avenues to expand the findings presented in this work. For instance, future work could delve deeper into the composition of cohorts, as the racial identification of Black captures a highly heterogeneous group. Examining the interplay between race and socio-economic status, school location, and teachers may provide a more comprehensive understanding of the observed patterns. Such an investigation could shed more light on the nuanced mechanisms that drive the transmission of political preferences among high school students. Also, the setting of the study in 2008—a year marked by the salience of race in American politics—gives rise to two important questions. Firstly, it is unclear whether the effect of Black peers on political preferences is a one-time occurrence or whether it has long-lasting implications across multiple election periods. Secondly, if the effect is indeed stronger due to the particular political climate of President Barack Obama's election year, it would be worthwhile to investigate whether other characteristics of a school cohort's composition become relevant in different election cycles.

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Appendix

Table A.1: Variables Definition

Variable	Wave	Description	Range
		Main outcomes & cohort variables:	
Political preferences	IV	In terms of politics, do you consider yourself (1) very conservative; (2) conservative; (3) middle of-the-road; (4) liberal; or (5) very liberal?	1, ..., 5
Overall conservative	IV	Indicator variable: (1) if the political preference is conservative or very conservative; (0) otherwise	0, 1
Very conservative	IV	Indicator variable: (1) if the political preference is very conservative; (0) otherwise	0, 1
Overall liberal	IV	Indicator variable: (1) if the political preference is liberal or very liberal; (0) otherwise	0, 1
Very liberal	IV	Indicator variable: (1) if the political preference is very liberal; (0) otherwise	0, 1
Anti conservative	IV	Indicator variable: (1) if the political preference is middle-of-the-road, liberal or very liberal; (0) otherwise	0, 1
Anti liberal	IV	Indicator variable: (1) if the political preference is middle-of-the-road, conservative or very conservative; (0) otherwise	0, 1
% Black peers, all	I	Share of students in an individual's cohort who self-identify as Black/African American	[0, 1]
% Black peers, same sex	I	Share of same-gender students in an individual's cohort who self-identify as Black/African American	[0, 1]
% Black peers, opp. sex	I	Share of opposite-gender students in an individual's cohort who self-identify as Black/African American	[0, 1]
		Balancing outcomes:	
Hispanic	I	Are you of Hispanic origin?	0, 1
Age	I	Age of individual	12, ..., 21
Parent's age	I	Age of the parent who responded the parental survey	20, ..., 81
Parent Catholic	I	Indicator variable: (1) if the parent who responded the survey is Catholic; (0) otherwise	0, 1
Number of older siblings	I	Respondent's number of older siblings	0, ..., 11
Family income	I	Annual family income of respondent (in thousand USD)	0, ..., 999
English spoken at home	I	Indicator variable: (1) if language spoken at home is primarily English; (0) otherwise	0, 1
Single patent family	I	Indicator variable: (1) if only one parent in household; (0) otherwise	0, 1
Live w/both biological parents	I	Indicator variable: (1) if respondent lives with both biological parents; (0) otherwise	0, 1
Grade size	I	Number of students in the school cohort of the respondent	2, ..., 695

(Continues on next page)

Table A.1: Variables Definition (Continued)

Variable	Wave	Description	Range
		School-belonging variables:	
Get along	I	How much do you agree or disagree with the following statement: You get along with other students: (1) strongly agree; (2) agree; (3) neither agree nor disagree; (4) disagree; (5) strongly disagree	1, ..., 5
Close to people	I	How much do you agree or disagree with the following statement: You feel close to people at your school: (1) strongly agree; (2) agree; (3) neither agree nor disagree; (4) disagree; (5) strongly disagree	1, ..., 5
Part of school	I	How much do you agree or disagree with the following statement: You feel like you are part of your school: (1) strongly agree; (2) agree; (3) neither agree nor disagree; (4) disagree; (5) strongly disagree	1, ..., 5
Happy at school	I	How much do you agree or disagree with the following statement: You are happy to be at your school: (1) strongly agree; (2) agree; (3) neither agree nor disagree; (4) disagree; (5) strongly disagree	1, ..., 5
		Political participation outcomes:	
Party identification	III	Do you identify with a specific political party? (1) yes; (0) no	0, 1
Voted in election of 2000	III	Did you vote in the most recent Presidential election: (1) yes; (0) no	0, 1
Registered to vote	III	Are you registered to vote: (1) yes; (0) no	0, 1
Vote frequency	IV & V	How often do you usually vote in local or statewide elections: (1) never; (2) sometimes; (3) often; (4) always	1, ..., 4

Notes: The table defines the variables used in the main analysis, and the outcomes variables for the different parts of the thesis. Source: National Longitudinal Survey of Adolescent Health (Add Health), Harris et al. (2009).

Table A.2: Effect of Black Peers on Future Political Preferences – Definitions

	Alternative samples:			Anti-sentiment:	
	Alternative Definition (1)	Only Whites (2)	Small sample (3)	Anti conservative (4)	Anti liberal (5)
% Black peers, same gender	0.870***	0.825**	0.815**	0.378**	-0.118
	(0.330)	(0.377)	(0.339)	(0.182)	(0.150)
% Black peers, opp. gender	-0.039	-0.094	-0.051	-0.145	-0.181
	(0.350)	(0.398)	(0.373)	(0.182)	(0.152)
P-value	0.07	0.08	0.09	0.08	0.78
Mean dep. var.	3.018	3.009	3.022	0.744	0.741
Std. dev. dep. var.	0.912	0.909	0.909	0.437	0.438
Observations	10,108	8,275	10,297	10,357	10,357

Notes: The table reports estimate results from OLS regressions. Standard errors (in parentheses) are clustered at the school level. All model specifications include school-gender and grade-gender fixed effects, and control for grade size. The sample in column (1) is conformed by all participants, who were not categorized as "Black/ African American" by Add Health's interviewers, in column (2), by those who self-identified exclusively as White, and in column (3) by non-Black students, in cohorts with at least five other students in Add Health. The sample in columns (4) and (5) corresponds to non-Black students in Add Health that had full information for Waves I and IV. In columns (1) to (3), the dep. variable is a 1-to-5 categorical variable ranging from "very conservative" to "very liberal". For columns (4) and (5) the dep. variable is an indicator variable coded as 1 if the political preference corresponds to the title (see more in detail the construction of these dep. variables in Appendix Table B.1). Add Health cross-sectional weights for Wave IV are used. The reported p-value in the table corresponds to the hypothesis test that the coefficients for same-gender and opposite-gender Black peers are equal. Source: National Longitudinal Survey of Adolescent Health (Add Health), Harris et al. (2009).
*** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$

Table A.3: Attrition and the Likelihood of Responding about Political Preferences

Dep var.:	Interviewed in Wave IV		Missing political preferences	
	(1)	(2)	(3)	(4)
% Black peers, all	-0.003		-0.077	
	(0.206)		(0.106)	
% Black peers, same gender		-0.042		0.048
		(0.113)		(0.081)
% Black peers, opp. gender		-0.140		-0.103
		(0.116)		(0.081)
Mean dep. var.	0.760	0.761	0.061	0.061
Std. dev. dep. var.	0.427	0.426	0.239	0.239
Observations	15,013	14,897	11,416	11,341

Notes: The table reports estimate results from OLS regressions. Standard errors (in parentheses) are clustered at the school level. All model specifications include school-gender and grade-gender fixed effects. The dependent variable in columns (1) and (2) is an indicator variable, which equals to 1 if the person was interviewed in Wave IV, and to 0 otherwise. The sample in these columns is conformed by all non-Black students in the in-home survey of Wave I from Add Health. The dependent variable in columns (3) and (4) is an indicator variable, which equals to 1 if the respondent did not reply to the question about his/her political preferences, and to 0 otherwise. The sample in these columns is conformed by all non-Black students who were interviewed in Wave IV from Add Health. Source: National Longitudinal Survey of Adolescent Health (Add Health), Harris et al. (2009).
*** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$

Table A.4: Summary Statistics: Full Set of Variables

	Observations	Mean	Std. dev.
Political preferences (Wave IV):			
Political preferences (1–5)	10,357	3.020	0.911
Political preference = very conservative	10,357	0.044	0.205
Political preference = conservative	10,357	0.212	0.409
Political preference = middle-of-the-road	10,357	0.485	0.500
Political preference = liberal	10,357	0.198	0.399
Political preference = very liberal	10,357	0.061	0.239
Main explanatory variables (Wave I):			
% Black peers, all	10,357	0.092	0.141
% Black peers, same gender	10,357	0.092	0.141
% Black peers, opp. gender	10,357	0.094	0.145
Other variables (Wave I):			
Female	10,357	0.492	0.500
Age	10,354	15.863	1.799
Race = White	10,357	0.866	0.340
Race = Asian	10,357	0.040	0.195
Race = American Indian	10,357	0.015	0.121
Race = other	10,357	0.078	0.268
Hispanic	10,327	0.135	0.342
Family income	8,002	48.515	49.439
Religion = Catholic	10,218	0.281	0.449
Religion = Protestant	10,218	0.423	0.494
Religion = atheist (no religion)	10,218	0.131	0.337
Lives in urban area	10,357	0.253	0.435
Grade size in school	10,357	214.346	144.004
Grade in school = 7	10,357	0.183	0.387
Grade in school = 8	10,357	0.170	0.376
Grade in school = 9	10,357	0.168	0.374
Grade in school = 10	10,357	0.161	0.368
Grade in school = 11	10,357	0.152	0.359
Grade in school = 12	10,357	0.165	0.371
Avg. school grades	10,357	4.783	1.278
Other variables (Wave IV):			
Age (Wave IV)	10,354	28.858	1.804
Attends college	10,356	0.667	0.471
Employed	10,354	0.652	0.476
No health insurance	10,345	0.210	0.407
Military participation	10,357	0.069	0.253
Ever arrested	10,317	0.279	0.448
Voting regularity	10,332	2.308	1.155

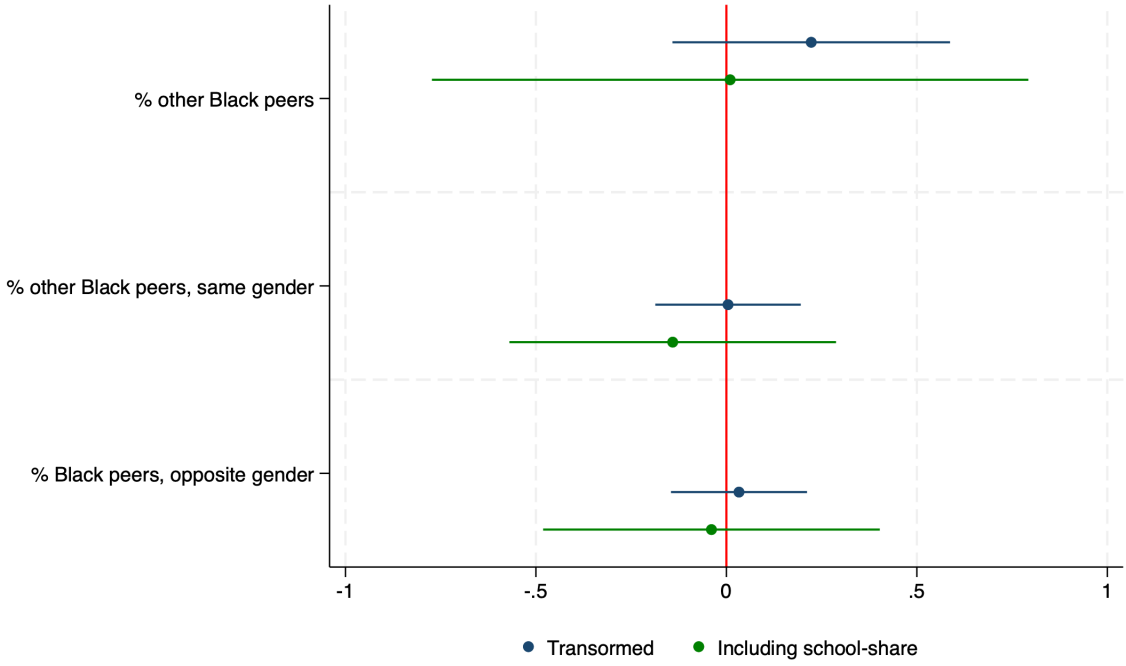
Notes: The table reports descriptive statistics for the full set of variables used in the analysis. The sample includes non-Black students in grades 7 through 12. Wave IV cross-sectional weights are used. The outcome and treatment variables are also reported in Table 1 in the main text, together with their within-school standard deviations. Detailed information on variable construction can be found in Appendix Table A.1. Source: National Longitudinal Survey of Adolescent Health (Add Health), Harris et al. (2009).

Table A.5: Raw and Residual Variation in the Share of Black Peers

	Observations (1)	Mean (2)	Std. dev. (3)	Min (4)	Max (5)
% Black peers, all:					
Raw cohort variable	10,357	0.092	0.141	0.000	0.942
Residuals: net of school-gender and grade-gender FE	10,357	0.000	0.021	-0.345	0.130
Residuals: net of school-gender, grade-gender FE and school trends	10,357	0.000	0.015	-0.215	0.111
% Black peers, same gender					
Raw cohort variable	10,357	0.092	0.141	0.000	1.000
Residuals: net of school-gender and grade-gender FE	10,357	0.000	0.027	-0.334	0.383
Residuals: net of school-gender, grade-gender FE and school trends	10,357	0.000	0.023	-0.221	0.427
% Black peers, opp. gender					
Raw cohort variable	10,357	0.094	0.145	0.000	0.962
Residuals: net of school-gender and grade-gender FE	10,357	0.000	0.028	-0.356	0.335
Residuals: net of school-gender, grade-gender FE and school trends	10,357	0.000	0.023	-0.299	0.335

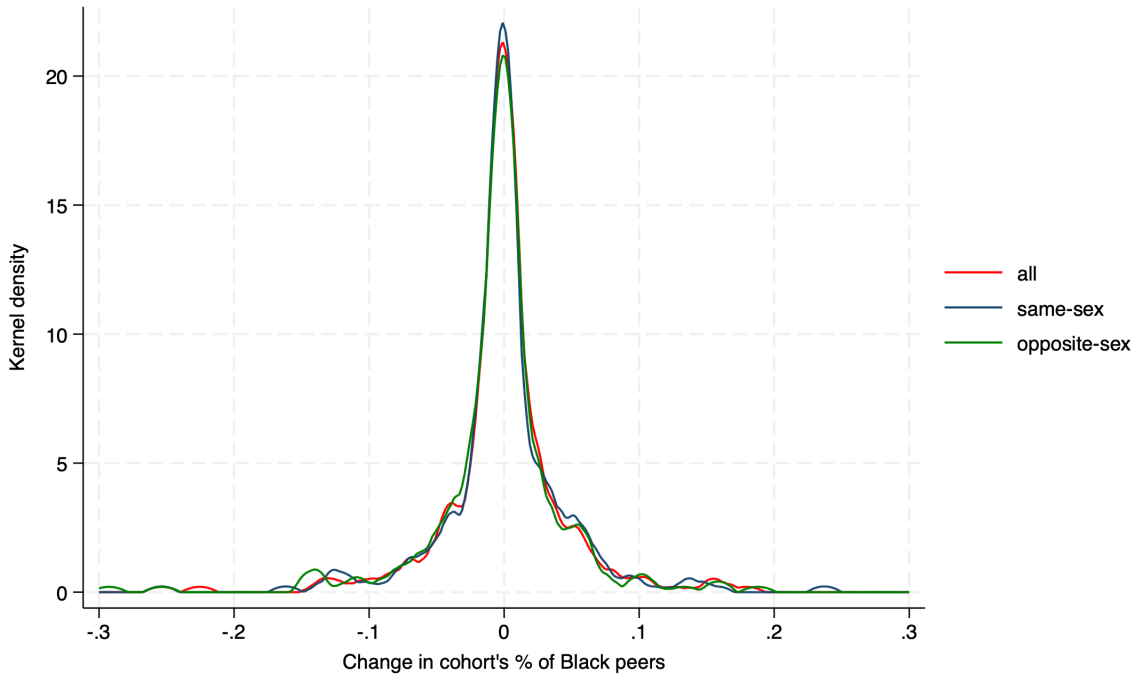
Notes: The table reports descriptive statistics for the share of all Black peers, same-gender Black peers, and opposite-gender Black peers at the cohort level before and after removing school-gender and grade-gender fixed effects, and then also after removing school specific trends. The sample includes non-Black students in grades 7 through 12. Wave IV cross-sectional weights are used. Source: National Longitudinal Survey of Adolescent Health (Add Health), Harris et al. (2009).

Figure A.1: Test for Nonrandom Clustering



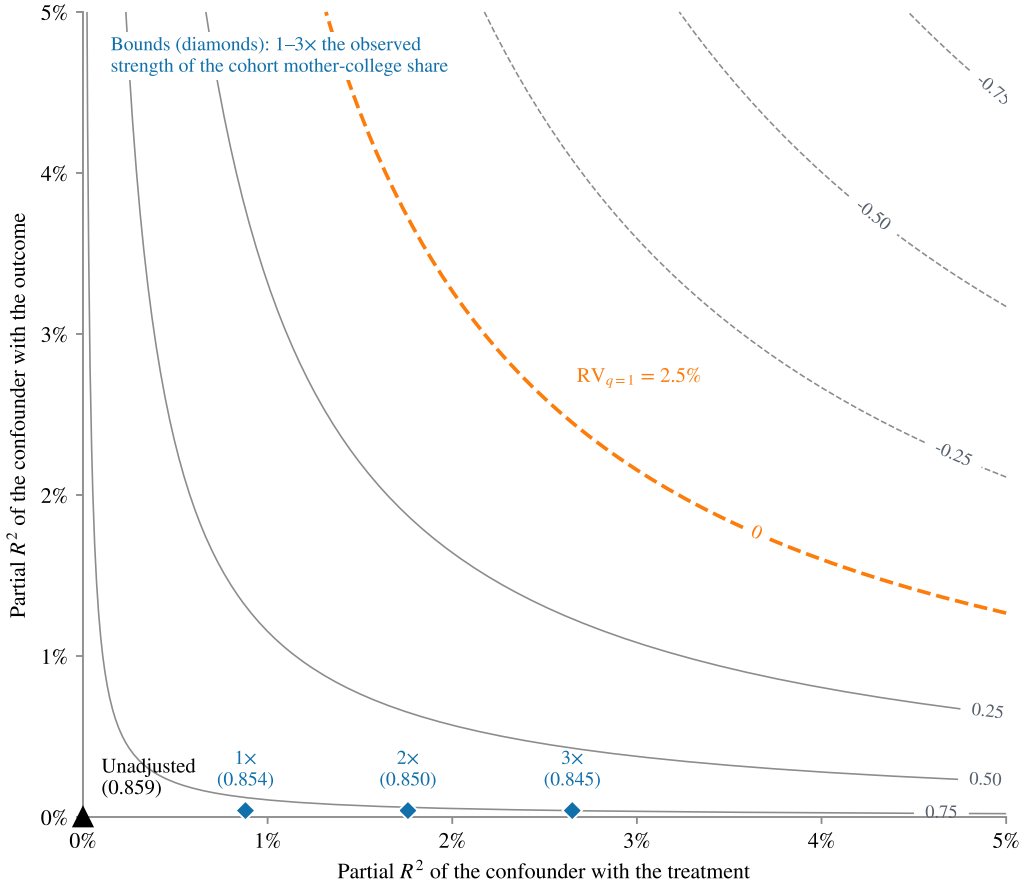
Notes: The figure reports the coefficients of the effect of same-gender Black peers on future political outcomes from OLS regressions. Standard errors are clustered at the school level. All model specifications include school-gender and grade-gender fixed effects, and control for grade size. The dep. variable in panel A is the overall liberal indicator variable, and in panel B the very liberal indicator variable (see more in detail the construction of these dep. variables in Appendix Table A.1). The sample corresponds to non-Black students in Add Health that had full information for Waves I and IV. Each coefficient is accompanied by its respective 90% confidence interval. The reported p-value below each category corresponds to the hypothesis test that the coefficients in that group are equal. Add Health cross-sectional weights for Wave IV are used. Source: National Longitudinal Survey of Adolescent Health (Add Health), Harris et al. (2009).

Figure A.2: Variation of the Share of Black Students in a School



Notes: The figure reports the coefficients of the effect of same-gender Black peers on future political outcomes from OLS regressions. Standard errors are clustered at the school level. All model specifications include school-gender and grade-gender fixed effects, and control for grade size. The dep. variable in panel A is the overall liberal indicator variable, and in panel B the very liberal indicator variable (see more in detail the construction of these dep. variables in Appendix Table A.1). The sample corresponds to non-Black students in Add Health that had full information for Waves I and IV. Each coefficient is accompanied by its respective 90% confidence interval. The reported p-value below each category corresponds to the hypothesis test that the coefficients in that group are equal. Add Health cross-sectional weights for Wave IV are used. Source: National Longitudinal Survey of Adolescent Health (Add Health), Harris et al. (2009).

Figure A.3: Sensitivity of the Benchmark Estimate to Omitted Variables



Notes: The figure reports the sensitivity contours of Cinelli & Hazlett (2020) for the same-gender benchmark coefficient on political preferences, estimated with the extended pre-determined controls of Panel B of Table 3. The axes measure the partial R^2 of a hypothetical confounder with the treatment and with the outcome; contours give the adjusted coefficient. The dashed line marks the robustness value $RV_{q=1}$, at which the estimate would be driven to zero. Diamonds benchmark a confounder one to three times as strong as the cohort share of peers with college-educated mothers, the strongest observed cohort-level control. Source: National Longitudinal Survey of Adolescent Health (Add Health), Harris et al. (2009).

Table A.6: Effect of Black Peers on Future Political Preferences – Robustness Checks

	(1)	(2)	(3)	(4)	(5)
Panel A – Dep. variable: Political preferences					
% Black peers, same gender	0.858**	0.810**	0.825**	0.808**	0.801**
	(0.331)	(0.340)	(0.342)	(0.356)	(0.351)
Mean dep. var.	3.020	3.020	3.020	3.020	3.020
Std. dev. dep. var.	0.911	0.911	0.911	0.911	0.911
Panel B –Dep. variable: Overall conservative					
% Black peers, same gender	-0.359**	-0.372**	-0.372*	-0.387*	-0.383*
	(0.174)	(0.184)	(0.200)	(0.198)	(0.202)
Mean dep. var.	0.256	0.256	0.256	0.256	0.256
Std. dev. dep. var.	0.437	0.437	0.437	0.437	0.437
Panel C – Dep. variable: Very conservative					
% Black peers, same gender	-0.229**	-0.218**	-0.187**	-0.196**	-0.203**
	(0.094)	(0.094)	(0.094)	(0.095)	(0.095)
Mean dep. var.	0.044	0.044	0.044	0.044	0.044
Std. dev. dep. var.	0.205	0.205	0.205	0.205	0.205
Baseline controls	Y	Y	Y	Y	Y
Individual controls		Y	Y	Y	Y
School, peer controls			Y	Y	Y
Wave IV controls				Y	Y
Std. dev. control					Y
Observations	10,357	10,357	10,357	10,357	10,357

Notes: The table reports estimate results from OLS regressions. Standard errors (in parentheses) are clustered at the school level. All model specifications include school-gender and grade-gender fixed effects, and control for grade size. Individual controls are: race, Hispanic, born in the US, age, language spoken at home, parental age, household size, parental education, log-family income, and a dummy variable that equals 1 if the student lives in a single-parent household. School peer controls refers to: cohorts gender ratio, and number of same-gender peers, average school grade, dummy if ever repeated a grade, and the share of same-gender peers born in the US, Asian, Hispanic, with parents born in the US, with parents with a college degree, and with parents in the labor force. Wave IV controls are: log-earnings, home-ownership, and health insurance status in 2008-2009. Missing dummies were used for missing information in the following variables: number of older siblings, parental education, log-family income, average school grade, age in Wave IV, log-earnings in Wave IV, house-ownership in Wave IV, and health insurance in Wave IV. Std. dev. control is the within cohort's standard deviation of the share of same-gendered Black peers. The sample corresponds to non-Black students in Add Health that had full information for Waves I and IV. Add Health cross-sectional weights for Wave IV are used. Source: National Longitudinal Survey of Adolescent Health (Add Health), Harris et al. (2009).
*** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$

Table A.7: Placebo Treatments: Hispanic and Asian Same-Gender Peer Shares

	Political preferences (1)	Overall conservative (2)	Very conservative (3)
Panel A: Hispanic peer shares as the treatment			
% Hispanic peers, same gender	0.528	-0.271*	0.065
	(0.338)	(0.150)	(0.077)
% Hispanic peers, opp. gender	-0.693**	0.264*	0.044
	(0.273)	(0.136)	(0.054)
% Black peers, same gender	0.937***	-0.415**	-0.220**
	(0.335)	(0.187)	(0.094)
% Black peers, opp. gender	-0.162	0.174	0.025
	(0.360)	(0.187)	(0.074)
P-value (placebo same = opp.)	0.01	0.01	0.81
MDE, placebo same-gender	0.946	0.421	0.214
Mean of dep. var.	3.020	0.256	0.044
Observations	10,357	10,357	10,357
Panel B: Asian peer shares as the treatment			
% Asian peers, same gender	-0.464	0.320	0.241*
	(0.575)	(0.233)	(0.127)
% Asian peers, opp. gender	1.114	-0.267	-0.072
	(0.883)	(0.276)	(0.103)
% Black peers, same gender	0.791**	-0.354*	-0.222**
	(0.339)	(0.189)	(0.090)
% Black peers, opp. gender	-0.021	0.124	0.004
	(0.362)	(0.187)	(0.074)
P-value (placebo same = opp.)	0.11	0.13	0.06
MDE, placebo same-gender	1.611	0.652	0.356
Mean of dep. var.	3.020	0.256	0.044
Observations	10,357	10,357	10,357

Notes: The table reports estimates from OLS regressions. In each panel, the same-gender and opposite-gender cohort shares of the placebo group are entered as treatments while the Black peer shares remain in the model; a Black-specific mechanism predicts null placebo coefficients. Standard errors (in parentheses) are clustered at the school level. All specifications include school-gender and grade-gender fixed effects, and control for grade size. The sample corresponds to non-Black students in the main estimation sample; Add Health Wave IV cross-sectional weights are used. Net of the fixed effects, the within-school standard deviations of the same-gender shares are 0.035 (Hispanic), 0.020 (Asian), and 0.027 (Black); the MDE row reports the minimum detectable effect on the placebo same-gender coefficient (80% power, 5% two-sided test). Source: National Longitudinal Survey of Adolescent Health (Add Health), Harris et al. (2009).

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

Table A.8: Effect of Black Peers on Future Political Preferences – Ordered Logit Regressions

	Independent variable: % Black peers		
	all	opp. gender	same gender
Dep. var.: Political preferences	(1)	(2)	(3)
Very conservative	-0.065*	0.002	-0.069**
	(0.039)	(0.031)	(0.030)
Conservative	-0.220*	0.007	-0.234**
	(0.131)	(0.103)	(0.101)
Middle of-the-road	-0.001	0.000	-0.001
	(0.001)	(0.000)	(0.001)
Liberal	0.198*	-0.006	0.210**
	(0.118)	(0.093)	(0.091)
Very liberal	0.089*	-0.003	0.094**
	(0.053)	(0.042)	(0.040)
Mean dep. var.	3.020	3.020	
Std. dev. dep. var	0.911	0.911	
Observations	10,357	10,357	

Notes: The table reports average marginal effects of ordered logit regressions. Standard errors (in parentheses) are clustered at the school level. All model specifications include school-gender and grade-gender fixed effects, and control for grade size. Column (1) reports the results from using the overall cohort share of Black peers. Column (2) and (3) report results from the same regression, in (2) the opposite-gender Black share, and (3) for the same-gender share. The sample corresponds to non-Black students in Add Health that had full information for Waves I and IV. Source: National Longitudinal Survey of Adolescent Health (Add Health), Harris et al. (2009).
*** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$

Table A.9: Effect of Black peers on Future Political Preferences – Nonlinear Binary Specifications

Dep. var:	Overall conservative			Very conservative		
	Probit (1)	Logit (2)	Poisson (3)	Probit (4)	Logit (5)	Poisson (6)
Panel A — Effect of all Black peers						
% Black peers, all	-0.224	-0.229	-0.211	-0.322	-0.323	-0.240
	(0.193)	(0.195)	(0.181)	(0.205)	(0.203)	(0.149)
Panel B — Effect of Black peers, divided by gender						
% Black peers, same gender	-0.345**	-0.354**	-0.308**	-0.316***	-0.319***	-0.241***
	(0.170)	(0.170)	(0.157)	(0.121)	(0.121)	(0.091)
% Black peers, opp. gender	0.130	0.126	0.116	-0.058	-0.068	-0.052
	(0.171)	(0.172)	(0.161)	(0.121)	(0.117)	(0.086)
Mean dep. var.	0.257	0.257	0.256	0.058	0.058	0.044
Std. dev. dep. var.	0.439	0.439	0.437	0.234	0.234	0.205
Observations	10,287	10,287	10,357	8,260	8,260	10,357

Notes: The table reports average marginal effects of Probit, Logit, and Poisson regressions for the dependent variables overall conservative and very conservative (see more in detail the construction of these dep. variables in Appendix Table B.1). Standard errors (in parentheses) are clustered at the school level. All model specifications include school-gender and grade-gender fixed effects, and control for grade size. The sample corresponds to non-Black students in Add Health that had full information for Waves I and IV. Missing observations in columns (1), (2), (4), and (5) are observations omitted by Stata (version 17) because of collinearity. Add Health cross-sectional weights for Wave IV are used. Source: National Longitudinal Survey of Adolescent Health (Add Health), Harris et al. (2009). *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$

Table A.10: Benchmark Inference: Wild Cluster Bootstrap

	Coefficient	Analytic p	Wild bootstrap p	Bootstrap 95% CI
Political preferences	0.868	0.010	0.013	[0.201, 1.544]
Overall conservative	-0.378	0.040	0.048	[-0.741, -0.004]
Very conservative	-0.231	0.016	0.025	[-0.438, -0.030]
Overall liberal	0.118	0.435	0.439	[-0.186, 0.432]
Very liberal	0.141	0.151	0.173	[-0.059, 0.348]

Notes: Benchmark specification per outcome (school-gender and grade-gender fixed effects, grade size, Wave IV weights, standard errors clustered at the school level). Analytic p-values use the cluster-robust t with 129 degrees of freedom; wild cluster bootstrap p-values and confidence intervals use 9,999 Rademacher draws (boottest). Romano–Wolf stepdown p-values across the five-outcome family are reported in the text (rwolf2, 1,000 cluster-bootstrap replications). Source: National Longitudinal Survey of Adolescent Health (Add Health), Harris et al. (2009).

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

Table A.11: Randomization Inference: Permuting Cohort Composition within Schools

	Coefficient	Observed t	Analytic p	Permutation p
Political preferences	0.868	2.60	0.010	0.013
Overall conservative	-0.378	-2.07	0.040	0.038
Very conservative	-0.231	-2.44	0.016	0.017
Overall liberal	0.118	0.78	0.435	0.441
Very liberal	0.141	1.44	0.151	0.164

Notes: Each row reports the benchmark same-gender coefficient (school-gender and grade-gender fixed effects, grade size, Wave IV weights, standard errors clustered at the school level) together with a randomization-inference p-value in the spirit of Young (2019). In each of 1000 permutations, the grade-level profiles of male and female Black shares are randomly reassigned across the sampled grades within each school, holding grade sizes, all controls, and the fixed effects at their actual values; the permutation p-value is the share of permutations in which the absolute cluster-robust t-statistic on the same-gender share is at least as large as the observed one. Schools contributing a single grade are unchanged by construction. Source: National Longitudinal Survey of Adolescent Health (Add Health), Harris et al. (2009).
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A.12: The Distribution of the Exposure Effect

	Political preferences (1)	Overall conservative (2)	Very conservative (3)
Panel A — Extensive and intensive margins			
Any Black same-gender peer	0.086	-0.013	-0.003
	(0.053)	(0.029)	(0.014)
% Black peers, same gender	0.761**	-0.362*	-0.227**
	(0.338)	(0.184)	(0.095)
Grade black share, opposite gender	-0.084	0.146	0.016
	(0.354)	(0.181)	(0.073)
P-value (joint)	0.01	0.11	0.05
Observations	10,357	10,357	10,357
Panel B — Linear spline (knot at median positive share)			
Share slope below 0.076	1.339	-0.748	-0.637*
	(1.586)	(0.732)	(0.365)
Share slope above 0.076	0.804**	-0.328	-0.177*
	(0.335)	(0.198)	(0.097)
Grade black share, opposite gender	-0.082	0.149	0.021
	(0.352)	(0.180)	(0.073)
P-value (equal slopes)	0.75	0.60	0.23
Mean of dep. var.	3.020	0.256	0.044
Observations	10,357	10,357	10,357

Notes: The table reports OLS estimates of two parameterizations of exposure dose. Panel A adds an indicator for any same-gender Black cohort peer alongside the share; Panel B is a two-piece linear spline in the share with the knot at the weighted median of the positive shares. Standard errors (in parentheses) are clustered at the school level. All specifications include school-gender and grade-gender fixed effects and control for grade size. Add Health Wave IV cross-sectional weights are used. Source: National Longitudinal Survey of Adolescent Health (Add Health), Harris et al. (2009).

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

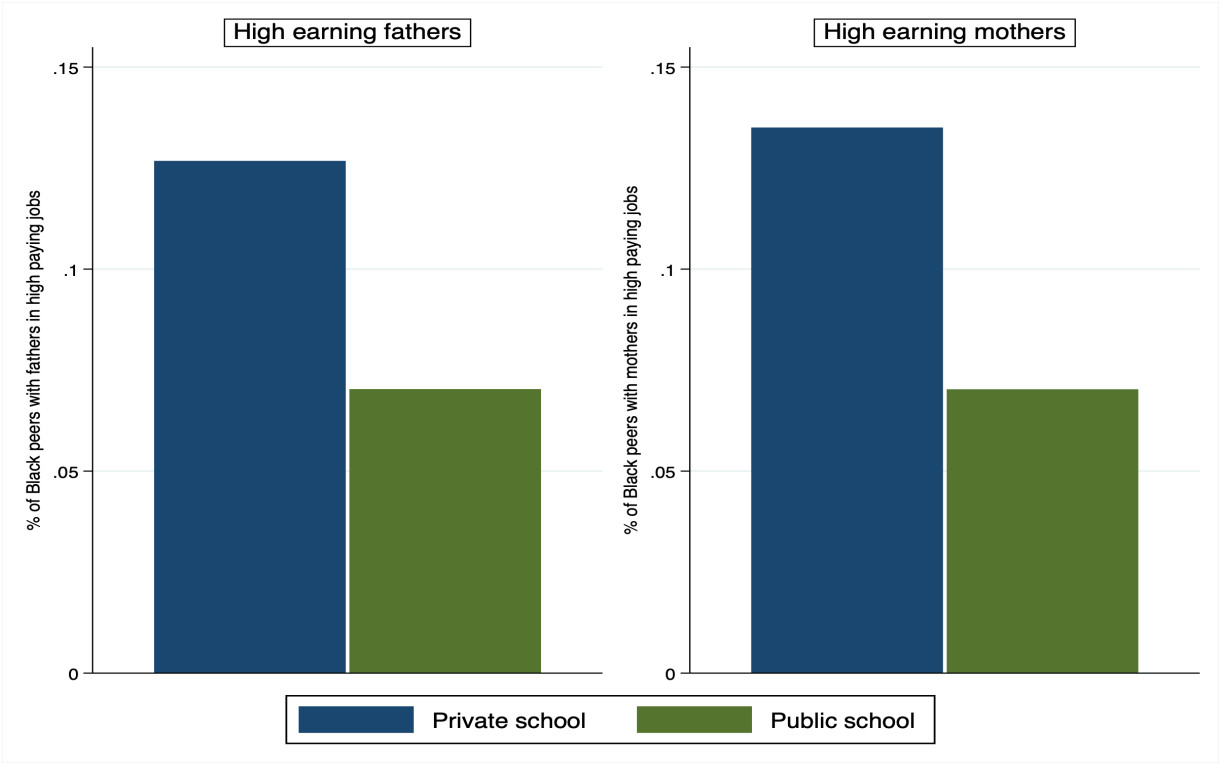
Table A.13: Interviewer Race and Reported Political Preferences

	Political preferences (1)	Overall conservative (2)	Very conservative (3)
Grade black share, same gender	0.697*	-0.435**	-0.196*
	(0.389)	(0.200)	(0.102)
% Black same gender × Black interviewer	0.358	0.099	-0.134**
	(0.280)	(0.140)	(0.064)
Black interviewer	-0.026	-0.012	0.026
	(0.057)	(0.026)	(0.018)
Grade black share, opposite gender	0.016	0.125	0.034
	(0.376)	(0.188)	(0.078)
Share effect, Black interviewer	1.055	-0.336	-0.329
(s.e.)	0.414	0.202	0.115
Min. detectable interaction (80% power)	0.784	0.391	0.180
Mean of dep. var.	3.008	0.259	0.043
Observations	9,210	9,210	9,210

Notes: The table reports estimates from OLS regressions. The specification interacts the same-gender Black peer share with an indicator for a Black Wave IV interviewer. Standard errors (in parentheses) are clustered at the school level. All specifications include school-gender and grade-gender fixed effects, and control for grade size. The sample corresponds to non-Black students in the main estimation sample. Add Health Wave IV cross-sectional weights are used. Source: National Longitudinal Survey of Adolescent Health (Add Health), Harris et al. (2009).

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

Figure A.4: High-Earning Black Families in Private and Public Schools



Notes: The figure reports the share of Black peers with mothers and fathers in high-paying positions from all Black peers in the cohort, for all public and private schools in the sample. The following professions are listed as high-earning jobs: doctor, lawyer, scientist, and manager. Source: National Longitudinal Survey of Adolescent Health (Add Health), Harris et al. (2009).

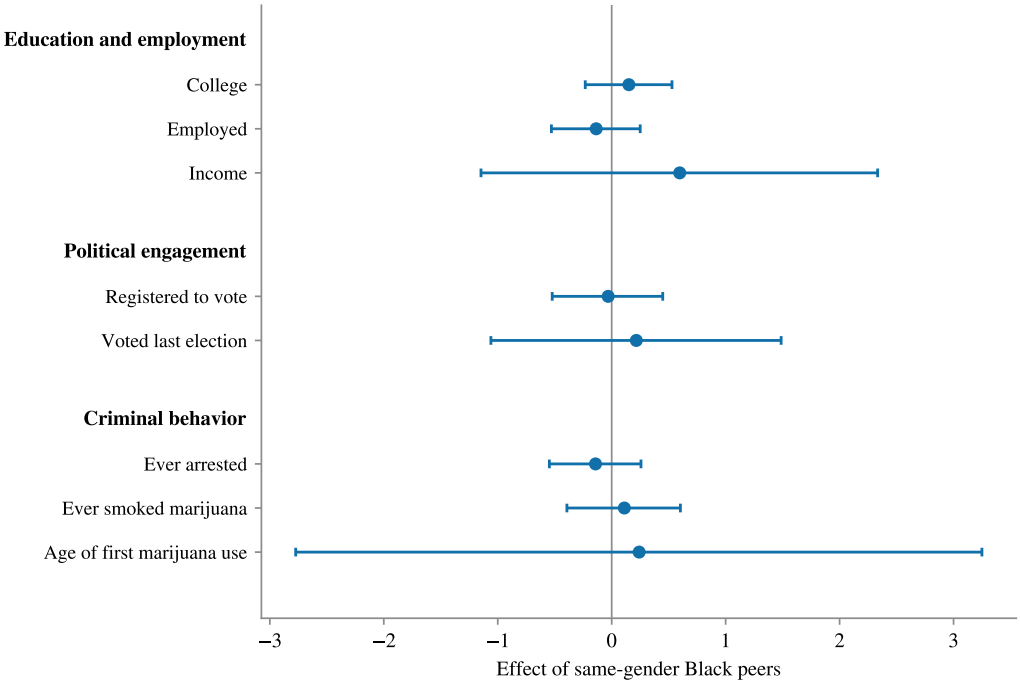
Table A.14: Effect of Black Peers on Future Political Participation

Dep. variable:	Participation in Wave III			Vote frequency	
	Party	Election 2000	Registered	Wave IV	Wave V
	(1)	(2)	(3)	(4)	(5)
% Black peers, same gender	0.284	0.160	-0.092	0.212	0.242
	(0.249)	(0.257)	(0.216)	(0.646)	(0.757)
% Black peers, opp. gender	0.235	0.377	0.241	0.884*	0.696
	(0.225)	(0.296)	(0.218)	(0.514)	(0.604)
Mean of dep. var.	0.329	0.426	0.721	2.308	2.464
Std. dev. of dep. var.	0.470	0.495	0.448	1.155	1.145
Observations	8,595	8,621	8,644	10,332	7,328

Notes: OLS estimates. School-gender and grade-gender fixed effects, grade-size control, non-Black students; standard errors (in parentheses) clustered at the school level. Columns (1)–(3) use Wave III, column (4) Wave IV, and column (5) Wave V cross-sectional weights. Wave VI contains no voting-frequency item. Source: Add Health (Harris et al., 2009).

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

Figure A.5: Effect of Black Peers on Candidate Mediator Outcomes



Notes: The figure reports the coefficient on the share of same-gender Black peers from separate benchmark regressions on each Wave IV outcome, with its 95% confidence interval. All specifications include the opposite-gender share, school-gender and grade-gender fixed effects, and control for grade size; standard errors are clustered at the school level and Add Health Wave IV cross-sectional weights are used. Source: Add Health (Harris et al., 2009).

Table A.15: Effect of Strong Political Preferences on Political Participation

Dep. variable: Voting frequency	Wave IV			Wave V		
	(1)	(2)	(3)	(4)	(5)	(6)
Political preferences	0.004			-0.017		
	(0.020)			(0.024)		
Very conservative		0.082			0.219**	
		(0.083)			(0.109)	
Very liberal			0.100			0.091
			(0.071)			(0.080)
Mean dep. var.	2.308	2.308	2.308	2.464	2.464	2.464
Std. dev. dep. var.	1.155	1.155	1.155	1.145	1.145	1.145
Observations	10,332	10,332	10,332	7,328	7,328	7,328

Notes: The table reports estimate results from OLS regressions. Standard errors (in parentheses) are clustered at the school level. All model specifications include school-gender and grade-gender fixed effects, and control for grade size. The sample corresponds to non-Black students in Add Health that had full information for Waves I and IV. Add Health cross-sectional weights for Wave IV are used in columns (1) – (3). Add Health cross-sectional weights for Wave V are used in columns (4)–(6). See more in detail the construction of the dep. variable in Appendix Table B.1. Source: National Longitudinal Survey of Adolescent Health (Add Health), Harris et al. (2009).
*** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$

Table A.16: Decomposing Exposure by Shared Clubs and Teams

	Political preferences (1)	Overall conservative (2)	Very conservative (3)
Panel A: shared activity vs. no shared activity			
% Black same gender, shared activity	0.439 (0.485)	-0.175 (0.244)	-0.122 (0.098)
% Black same gender, no shared activity	0.473 (0.519)	-0.226 (0.257)	-0.116 (0.099)
% Black peers, opp. gender	-0.275 (0.493)	0.274 (0.247)	-0.054 (0.108)
P-value (shared = not shared)	0.76	0.33	0.65
Mean of dep. var.	3.014	0.262	0.042
Observations	8,177	8,177	8,177
Panel B: team sports vs. clubs vs. no shared activity			
% Black same gender, shared team sport	0.454 (0.488)	-0.177 (0.246)	-0.117 (0.100)
% Black same gender, shared club only	0.500 (0.592)	-0.176 (0.298)	-0.080 (0.111)
% Black same gender, no shared activity	0.505 (0.523)	-0.226 (0.262)	-0.093 (0.105)
% Black peers, opp. gender	-0.226 (0.483)	0.269 (0.241)	-0.037 (0.112)
P-value (shared team = not shared)	0.49	0.20	0.08
Observations	8,177	8,177	8,177

Notes: The table reports estimates from OLS regressions. The same-gender Black cohort share is decomposed, respondent by respondent, into Black same-gender cohort peers who share at least one club, organization, or team with the respondent (In-School Questionnaire, items S44A1–A33) and those who share none; both pieces have the same denominator as the benchmark share, so they sum to it mechanically. Panel B splits the shared-activity piece into team sports (cheerleading/dance and the twelve interscholastic sports) and all other activities. Participation is chosen by students, so the decomposition is reported as an accounting, not as causal mediation. All models control for the respondent's own participation. The p-value tests equality of the shared-activity (Panel B: shared-team) and the no-shared-activity coefficients. Standard errors (in parentheses) are clustered at the school level. All specifications include school-gender and grade-gender fixed effects, and control for grade size. The sample corresponds to non-Black students in the main estimation sample with in-school questionnaire data; Add Health Wave IV cross-sectional weights are used. Source: National Longitudinal Survey of Adolescent Health (Add Health), Harris et al. (2009).

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

Table A.17: Grade-Level Ability Grouping and the Exposure Effect

	Political preferences (1)	Overall conservative (2)	Very conservative (3)
% Black peers, same gender	0.696	-0.310	-0.258**
	(0.503)	(0.277)	(0.112)
% Black same gender × tracked grade	0.269	-0.040	0.110
	(0.622)	(0.328)	(0.165)
English ability-grouping in own grade (A23)	0.079*	0.000	-0.007
	(0.043)	(0.019)	(0.009)
% Black peers, opp. gender	-0.159	0.270	0.032
	(0.519)	(0.286)	(0.102)
% Black opp. gender × tracked grade	0.121	-0.071	-0.110
	(0.653)	(0.323)	(0.131)
Same-gender effect, tracked grades	0.965	-0.350	-0.147
standard error	0.434	0.229	0.155
p-value	0.028	0.129	0.344
Mean of dep. var.	3.021	0.255	0.044
Observations	10,232	10,232	10,232

Notes: The table reports estimates from OLS regressions. Tracked grade is an indicator for the school administrator reporting that English or language arts classes in the respondent's own grade are grouped by ability or achievement (School Administrator Questionnaire, item A23); because it varies at the school-grade level, it is identified within schools. The implied same-gender effect in tracked grades (benchmark coefficient plus interaction) and its p-value are reported below the coefficients. Standard errors (in parentheses) are clustered at the school level. All specifications include school-gender and grade-gender fixed effects, and control for grade size. The sample corresponds to non-Black students in the main estimation sample with tracking information; Add Health Wave IV cross-sectional weights are used. Source: National Longitudinal Survey of Adolescent Health (Add Health), Harris et al. (2009).

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

Table A.18: Cohort Composition and the School Climate at the Moment of Exposure

Outcome (agrees)	% Black peers, same gender	% Black peers, opp. gender	P-value (same = opp.)	Mean of dep. var.	Observations
Students here are prejudiced	0.008	0.108	0.38	0.405	64,593
	(0.089)	(0.097)			
I feel safe in my school	0.054	0.048	0.96	0.633	65,108
	(0.089)	(0.072)			
Close to people at this school	0.094	0.043	0.58	0.590	66,215
	(0.083)	(0.071)			
Feel part of this school	0.033	-0.095	0.24	0.571	65,744
	(0.094)	(0.083)			
Happy to be at this school	0.132	-0.068	0.07	0.586	65,390
	(0.100)	(0.107)			
Teachers treat students fairly	0.138	-0.015	0.26	0.519	65,235
	(0.096)	(0.102)			
Feel socially accepted	0.165**	-0.088	0.05	0.666	64,828
	(0.083)	(0.081)			

Notes: Each row is a separate OLS regression, on the full Wave I in-school census of the sampled schools, of an indicator for agreeing or strongly agreeing with the row statement (item S62) on the cohort's same- and opposite-gender Black shares; the outcomes are measured in 1994–95, contemporaneously with exposure. School-gender and grade-gender fixed effects, grade-size control, non-Black students; unweighted, as the questionnaire is a census. Standard errors (in parentheses) clustered at the school level. The p-value tests equality of the same- and opposite-gender coefficients. Source: Add Health (Harris et al., 2009).

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

Table A.19: Heterogeneity in the Effect by School-Belonging Categories

	Get along		Close to people		Part of school		Happy at school	
	High	Low	High	Low	High	Low	High	Low
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A – Dep. variable: Overall conservative								
% Black peers, same gender	-0.895***	0.360	-0.379	-0.102	-0.680***	0.346	-0.334	-0.385
	(0.275)	(0.278)	(0.242)	(0.441)	(0.210)	(0.306)	(0.229)	(0.340)
Mean dep. var.	0.253	0.267	0.261	0.246	0.266	0.228	0.264	0.242
Std. dev. dep. var.	0.435	0.443	0.439	0.431	0.442	0.420	0.441	0.428
Panel B – Dep. variable: Very conservative								
% Black peers, same gender	-0.323**	0.002	-0.226**	-0.215	-0.225*	-0.140	-0.156	-0.301**
	(0.158)	(0.138)	(0.113)	(0.209)	(0.116)	(0.148)	(0.124)	(0.138)
Mean dep. var.	0.045	0.043	0.046	0.039	0.046	0.038	0.047	0.039
Std. dev. dep. var.	0.207	0.204	0.210	0.193	0.210	0.191	0.211	0.193
Observations	4,664	4,134	6,978	3,372	7,527	2,823	6,889	3,464

Notes: The table reports estimate results from OLS regressions. Standard errors (in parentheses) are clustered at the school level. All model specifications include school-gender and grade-gender fixed effects, and control for grade size. The sample corresponds to non-Black students in Add Health that had full information for Waves I and IV. For each category of school-belonging, the sample is divided into those with high-scoring and low-scoring answers to the respective question (see more in detail the construction of these categories in Appendix Table B.1). Add Health cross-sectional weights for Wave IV are used. Source: National Longitudinal Survey of Adolescent Health (Add Health), Harris et al. (2009).
*** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$

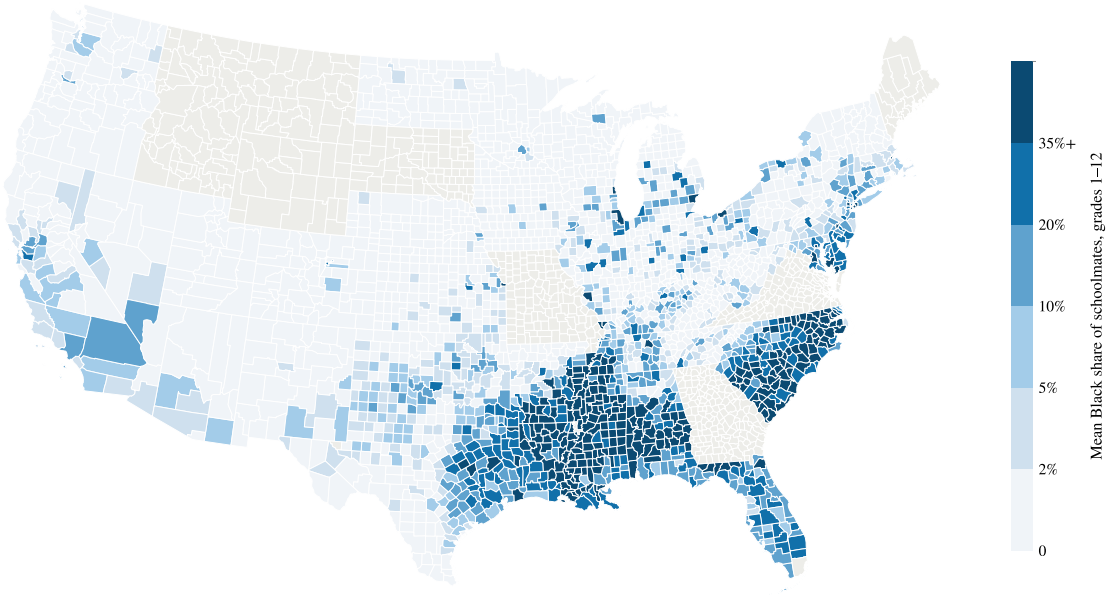
Table A.20: Cohort Network Integration as a Test of the Common Ingroup Identity Model

	Political preferences (1)	Overall conservative (2)	Very conservative (3)
Grade black share, same gender	0.730*	-0.365*	-0.210**
	(0.372)	(0.200)	(0.098)
% Black same gender × integration	-0.031	0.068**	-0.005
	(0.055)	(0.029)	(0.016)
Cohort integration, residualized (std.)	-0.002	-0.021*	0.007
	(0.024)	(0.012)	(0.007)
Grade black share, opposite gender	-0.060	0.164	-0.034
	(0.387)	(0.187)	(0.082)
Mean of dep. var.	3.039	0.253	0.041
Std. dev. of dep. var.	0.911	0.435	0.199
Observations	6,835	6,835	6,835

Notes: OLS estimates. The integration index is the observed share of same-gender cross-race ties among non-Black nominators minus the share expected under random mixing given cohort composition, standardized; it is an equilibrium outcome, and the estimates are suggestive heterogeneity. Sample: non-Black students in cohorts with positive expected cross-race exposure. School-gender and grade-gender fixed effects, grade-size control, Wave IV weights; standard errors (in parentheses) clustered at the school level. Source: Add Health (Harris et al., 2009).

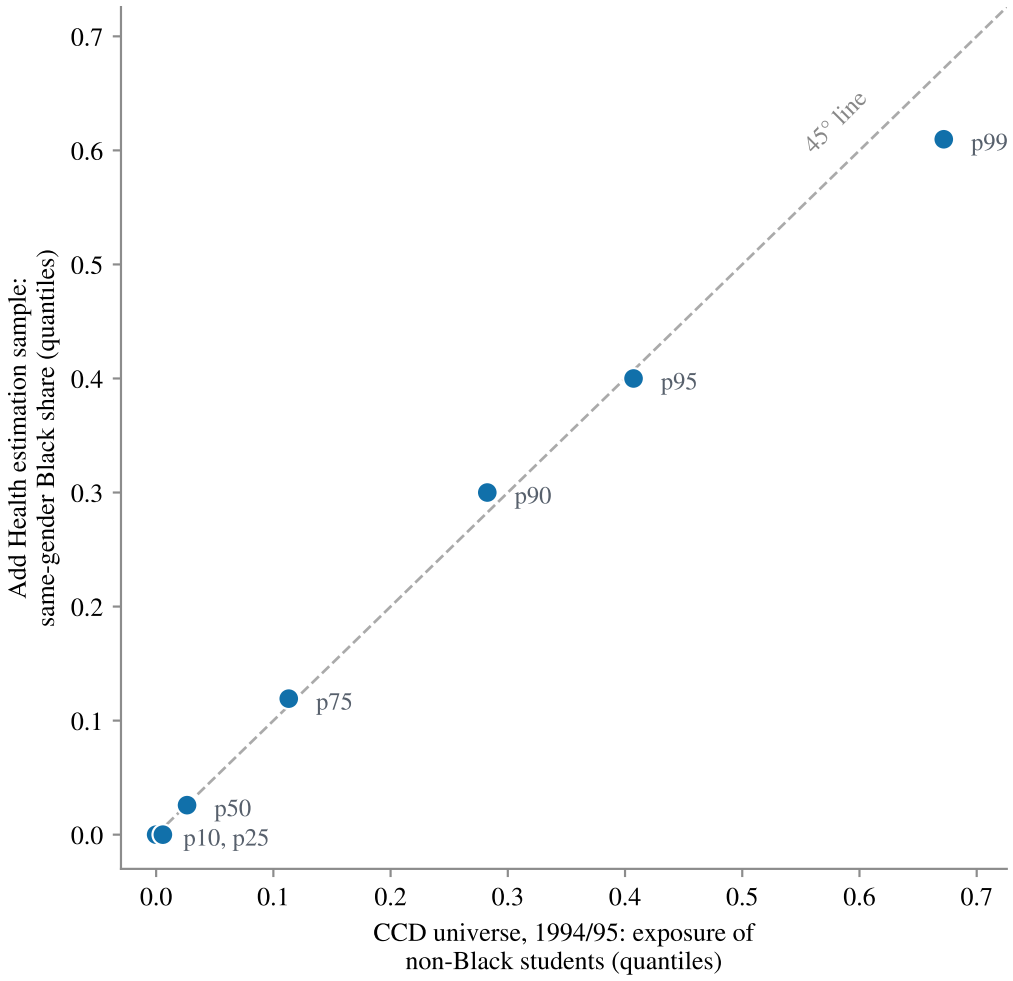
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Figure A.6: Childhood Exposure to Black Schoolmates by County



Notes: The figure maps $e(c, b)$, the average Black enrollment share in a county's schools over the twelve years in which a birth cohort attends grades one through twelve, averaged over the 1976–1983 birth cohorts. Enrollment comes from the Office for Civil Rights Elementary and Secondary School Surveys for 1968–1986 and from the Common Core of Data for 1987–2008. Counties outside the panel's coverage are left blank. Sources: U.S. Department of Education, Office for Civil Rights (2024), Urban Institute (2026).

Figure A.7: Validation of the Exposure Measure against Add Health



Notes: The figure compares the distribution of the Black schoolmate share of non-Black students in the 1994/95 school year, as measured in the Common Core of Data, with the same-gender Black peer share in the Add Health estimation sample. Quantiles of the two distributions are plotted against each other; the dashed line is the 45-degree line. Sources: Urban Institute (2026); National Longitudinal Survey of Adolescent Health (Add Health), Harris et al. (2009).

Table A.21: CES Gradients by Residence Tenure

	P(Democrat) (1)	P(validated vote) (2)
Full sample	0.222**	0.297***
	(0.086)	(0.097)
Observations	170,820	134,011
Five or more years at address	0.256**	0.314**
	(0.110)	(0.128)
Observations	67,536	69,725
Fifteen or more years in city	0.003	-0.051
	(0.184)	(0.224)
Observations	16,964	17,566

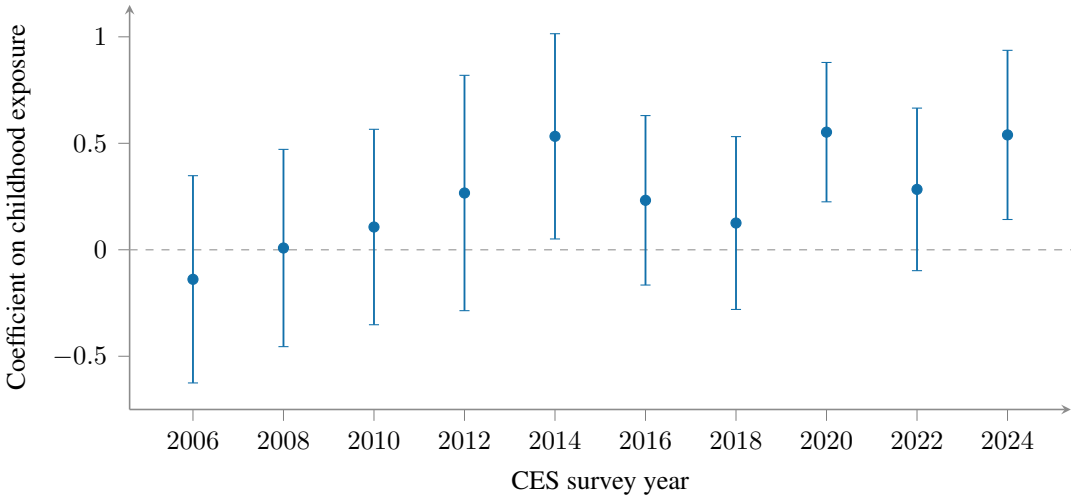
Notes: The table reports the pooled exposure estimates of Table 11 under increasingly strict residence-tenure restrictions. The address-tenure item (five or more years at the present address) is available in 2008–2024 except 2018; the city-tenure item (years in the current city) is available in 2006, 2010, 2016, and 2018 only, so the fifteen-year restriction retains a small subsample and its confidence intervals cover both zero and the full-sample estimate. All specifications include county and birth-cohort fixed effects and use CES survey weights; standard errors (in parentheses) are clustered at the county level (Kuriwaki, 2025).
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A.22: Childhood Exposure and the Party Registration of White North Carolinians

	Dem. share, two-party reg. (1)	Dem. share of registrants (2)	Rep. share of registrants (3)	Unaffiliated share (4)
Exposure	0.573***	0.220***	-0.272***	0.046
	(0.144)	(0.072)	(0.055)	(0.067)
Mean of outcome	0.297	0.170	0.403	0.418
County-cohort cells	2,133	2,133	2,133	2,133

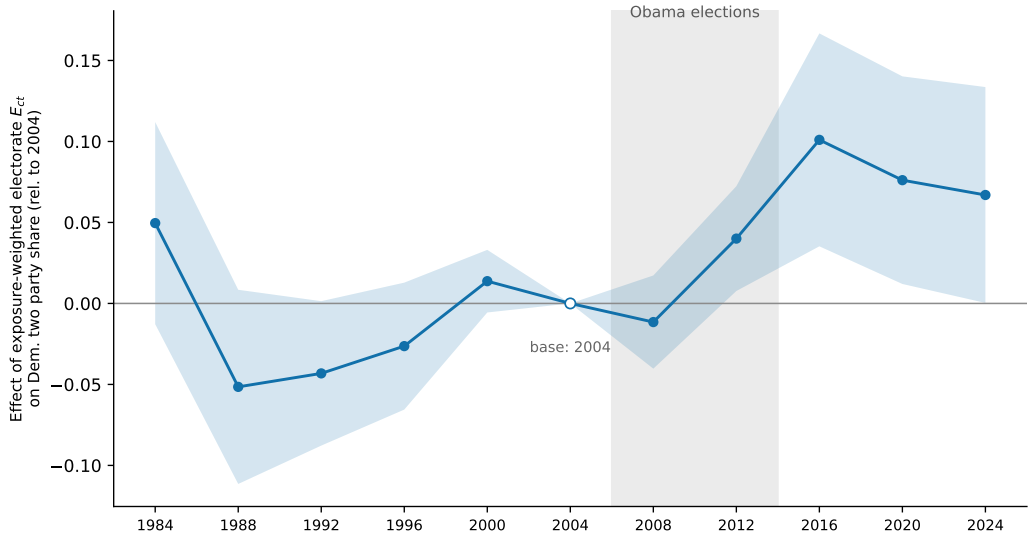
Notes: The table reports estimates of the Table 11 specification on administrative registration records: the North Carolina statewide voter file (North Carolina State Board of Elections, 2026), collapsed to county-by-birth-cohort cells of the 1,874,766 registered White non-Hispanic voters born 1962–1991, with race and ethnicity as self-reported on the registration form. Each column regresses the indicated registration share on childhood exposure with county and birth-cohort fixed effects, weighting cells by their registrant counts (two-party registrants in column 1) and clustering standard errors (in parentheses) by county. Estimating column 1 separately by cohort group yields 0.342 (s.e. 0.117) for the 1962–1971 cohorts, 0.244 (s.e. 0.163) for 1972–1981, and 0.523 (s.e. 0.153) for 1982–1991.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Figure A.8: Childhood Exposure and Democratic Identification by Survey Year



Notes: The figure reports year-by-year estimates of β from $y_{icb} = \beta\, e(c,b) + \alpha_c + \gamma_b + \varepsilon_{icb}$, estimated separately in each federal-election-year wave of the CES for White respondents of the 1962–1991 birth cohorts, with county and birth-cohort fixed effects, survey weights, and standard errors clustered by county. The outcome is an indicator for Democratic party identification; $e(c,b)$ is childhood exposure measured on the zero-to-one share scale, so the coefficient gives the change in the probability of Democratic identification for a one-unit change in exposure. Vertical bars are 95% confidence intervals. Because exposure is assigned by county of residence, these are descriptive gradients, not causal effects. Source: Kuriwaki (2025).

Figure A.9: White-Electorate Exposure and County Democratic Vote Share, Composition-Controlled



Notes: The figure reports the coefficients on the interaction of White-electorate exposure E_{ct} with election-year indicators, relative to 2004, from the composition-controlled event-study specification of Section 5, with county and election-year fixed effects, the main effect of E_{ct}, the contemporaneous Black share of the voting-age population, the coverage control, and county total votes as weights. The 1980 coefficient, 0.146 with standard error 0.043, is not displayed. Standard errors are clustered at the state level; the shaded band shows 95% confidence intervals. Sources: MIT Election Data and Science Lab (2018), Amlani & Algara (2021), Schroeder et al. (2026).

Table A.23: White-Electorate Exposure and County Turnout

	All counties	1980 Black VAP share		
		<5%	5–20%	>20%
	(1)	(2)	(3)	(4)
1984	0.075**	-0.059	0.014	0.146**
	(0.031)	(0.097)	(0.063)	(0.057)
1988	0.020	-0.150	-0.034	0.089*
	(0.028)	(0.098)	(0.061)	(0.048)
1992	-0.028	-0.117**	-0.029	0.071*
	(0.024)	(0.056)	(0.047)	(0.041)
1996	-0.024	-0.250***	-0.028	0.033
	(0.020)	(0.063)	(0.049)	(0.031)
2000	-0.024***	-0.126***	-0.014	0.026
	(0.009)	(0.044)	(0.028)	(0.027)
2008	0.077***	0.176***	0.079***	0.017
	(0.008)	(0.036)	(0.027)	(0.033)
2012	0.112***	0.317***	0.185***	0.078***
	(0.013)	(0.078)	(0.029)	(0.022)
2016	0.079***	0.228*	0.229***	0.041*
	(0.026)	(0.134)	(0.086)	(0.022)
2020	0.052	0.216	0.288	0.003
	(0.045)	(0.260)	(0.218)	(0.023)
2024	-0.040	-0.078	0.164	-0.119*
	(0.034)	(0.220)	(0.179)	(0.065)
Counties	3,110	1,957	545	462
Observations	36,891	23,460	6,492	5,440

Notes: The table replaces the Democratic vote share with the county turnout rate, total votes cast over the Black-plus-White voting-age population, as the outcome of the event-study model of Section 5, including the main effect of E_{ct}, county and state-by-election-year fixed effects, and the coverage control, with county total votes as weights and standard errors (in parentheses) clustered by state. Column (1) uses all counties; columns (2) to (4) split counties by their 1980 Black share of the voting-age population. Coefficients are relative to 2004. The 1980 coefficients are 0.005 (s.e. 0.039), -0.154 (s.e. 0.088), -0.055 (s.e. 0.077), 0.151 (s.e. 0.061) in columns (1) to (4). Returns from MIT Election Data and Science Lab (2018) and Amlani & Algara (2021); population counts from Schroeder et al. (2026).

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