



The impact of school desegregation on White individuals' racial attitudes and politics in adulthood

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In this paper I study how school desegregation by race following *Brown v. Board of Education* affected White individuals' racial attitudes and politics in adulthood. I use geocoded nationwide data from the General Social Survey and differences-in-differences to identify causal impacts. Integration significantly reduced White individuals' political conservatism as adults in the U.S. South but not elsewhere. I observe similar geographic impact heterogeneity for individuals' attitudes towards Blacks and policies promoting racial equity, but positive effects emerge less consistently across specifications. Results suggest that this heterogeneity may depend on the effectiveness of integration policies. In the south, Black-White exposure was greater following desegregation, and White disenrollment was lower. My study provides the first causal evidence on how different theories concerning intergroup contact and racial attitudes (i.e., the contact and racial threat hypotheses) may have applied to school contexts following historic court mandates to desegregate.

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Abstract

In this paper I study how school desegregation by race following *Brown v. Board of Education* affected White individuals' racial attitudes and politics in adulthood. I use geocoded nationwide data from the General Social Survey and differences-in-differences to identify causal impacts. Integration significantly reduced White individuals' political conservatism as adults in the U.S. South but not elsewhere. I observe similar geographic impact heterogeneity for individuals' attitudes towards Blacks and policies promoting racial equity, but positive effects emerge less consistently across specifications. Results suggest that this heterogeneity may depend on the effectiveness of integration policies. In the south, Black-White exposure was greater following desegregation, and White disenrollment was lower. My study provides the first causal evidence on how different theories concerning intergroup contact and racial attitudes (i.e., the contact and racial threat hypotheses) may have applied to school contexts following historic court mandates to desegregate.

Keywords: desegregation, diversity, educational policy, race, quasi-experimental analysis

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Black and White youth in the U.S. are largely educated in racial isolation. About 75% of Black students in K-12 public schools attend a school serving a majority of students from racially minoritized backgrounds, and 16% attend a school with almost no White students at all (Orfield, Kucsera, and Siegel-Hawley 2012). This racial segregation may contribute to persistent racial inequality in the country, as research finds that youth in schools serving larger shares of Black students experience worse educational outcomes (e.g., Billings, Deming, and Rockoff 2014). Diverse schools may also benefit all students—and society more broadly—by encouraging the development of skills necessary for healthy civic engagement in multiethnic communities (for a review, see Ayscue, Frankenberg, and Siegel-Hawley 2017). And school integration policies specifically have improved Black students' educational attainment and adulthood outcomes without negative consequences for White youth (Angrist and Lang 2004; Anstreicher, Fletcher, and Thompson 2022; Bergman 2018; Guryan 2004; Jackson 2009; Johnson 2011; Weiner, Lutz, and Ludwig 2009). Several factors can explain these particular positive effects (Reardon and Owens 2014), including changes to Black students' peer groups (Billings, Deming, and Rockoff 2014), and increases in the quantity (Johnson 2011) and quality (Jackson 2009) of their schools' resources.

Integration also affects the in-school experiences of White students, which no other mainstream educational reform accomplishes explicitly by design. Specifically, intergroup interactions may increase in schools serving a more racially diverse population of students. The contact hypothesis (Allport 1954) predicts that, in some instances, this Black-White contact will improve outgroup attitudes and decrease negative outgroup bias—measures that predict Black-

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White inequalities in educational and socioeconomic outcomes (e.g., Bertrand and Mullainathan 2004; Charles and Guryan 2008; Chetty et al. 2020; Chin et al. 2020; Riddle and Sinclair 2019). Indeed, the theoretical benefits of improved racial attitudes was one major motivating factor for reducing racial isolation put forth by social scientists when the Supreme Court decided in *Brown v. Board of Education* that *de jure* racial school segregation was unconstitutional (Stephan 1978).

In contrast, the racial threat hypothesis (Key 1949) suggests that as formerly segregated schools become more racially balanced, White educational stakeholders may develop more negative racial prejudices and subsequently take actions to maintain their power because they perceive resources to be scarce. Yet most investigations of intergroup contact cannot make causal claims on whether and how racial attitudes shift, especially as time from contact elapses (Paluck, Green, and Green 2018). Nor does extant work provide causal evidence on how the attitudes of White students were affected by the substantial educational change caused by *Brown v. Board* and subsequent court mandates to desegregate across the country.

In this study, I thus set out to address this gap in the literature by answering the following question: What is the causal impact of historic court-mandated school desegregation on White individuals' racial attitudes and politics as adults?

To answer this question, I use nationwide data from the General Social Survey (GSS) geocoded to White adult respondents' county of residence as teenagers. I focus on responses to questions related to racial attitudes and politics in counties where districts racially integrated schools following court mandates. Using information on respondents' age relative to when major desegregation plans were implemented, I identify whether White adults in my sample were exposed as youth to desegregated schools (i.e., they were not yet 18 at the time of integration) or were not exposed (i.e., they were 18 and/or lived where districts were not mandated by courts to

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integrate). I then employ a differences-in-differences approach to recover the causal effect of school desegregation on outcomes. Specifically, I compare the racial attitudes and politics of those exposed and not exposed to integration, after controlling for county (to account for time-invariant differences in outcomes across contexts) and cohort fixed effects (to account for contemporaneous trends across cohorts in attitudes and politics).

Results from this study may provide guidance to policymakers considering efforts to reduce racial isolation across schools. Though efforts may help address racial inequality, historic school integration often met intense opposition (see the Boston busing crisis) which, according to the racial threat hypothesis and the necessary conditions outlined in Allport's contact theory (1954), meant that desegregation could actually exacerbate existing negative attitudes and bias. Present day efforts face additional institutional barriers. The *Miliken v. Bradley* Supreme Court ruling in 1974 established that school systems were not responsible to address one of the largest contributors to racial segregation—segregation across district lines—unless the evidence proved this segregation was established with racist intent; a more recent Supreme Court decision similarly limited districts' options for how to voluntarily integrate their own schools (*Parents Involved in Community Schools v. Seattle School District No. 1*).

Other research finds negative effects of integration on Black students and their communities. To be clear, these results do not necessarily outweigh the substantial positive evidence in its favor, but historic efforts to address racial isolation across schools led to Black teachers losing jobs, schools serving predominantly Black students being closed, and increases in disciplinary inequality by race (Chin 2021; Schofield 1991; Thompson 2020). More contemporarily, Bergman (2018) finds that racially minoritized students participating in an inter-district integration program in California were more likely to be arrested; in Florida, the racial

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composition of schools affects whether Black and Hispanic students are identified as having a disability (Elder, Figlio, Imberman, and Persico 2021). Finally, Black youth may be more at risk to exposure to racial microaggressions (or even macroaggressions) and stereotype threat in contexts with more White students and teachers (e.g., Fries-Britt and Turner 2002; Steele 1997). Without rigorous evidence confirming the theoretical benefits of school integration for attitudes, educational leaders might thus seek to implement other beneficial reforms that face fewer barriers or that lead to fewer unintended negative consequences.

After discussing the history and prior research relevant to this study, I provide here an overview of the data and method that allow me to answer my research question. I then describe the results from my analyses before discussing the implications of my findings for future research and policy efforts to integrate schools by race.

Background

School Segregation by Race after *Brown v. Board*

In a landmark unanimous decision, the U.S. Supreme Court ruled in *Brown v. Board* that the *de jure* racial segregation of public schools by districts across the country was unconstitutional. The decision effectively overturned the Supreme Court precedent set decades earlier in *Plessy v. Ferguson*, which permitted “separate but equal” public facilities. Yet despite the judicial significance of the *Brown v. Board* ruling, integration was not immediate. The decision did not describe how schools were to desegregate and subsequent decisions (i.e., “*Brown II*”) delegated responsibility for integrative efforts to lower courts. Furthermore, the Supreme Court did not establish a clear timeline for desegregation to occur (i.e., “with all deliberate speed”). Perhaps unsurprisingly, early attempts by districts to reduce racial isolation across schools were glacial in pace and largely ornamental.

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A series of judicial actions over a decade after *Brown v. Board* finally forced districts to begin racially desegregating schools in earnest. In *Green v. Board of Education of New Kent County*, the Supreme Court established that “freedom of choice” integration plans did not adequately meet the standards for non-discriminatory school admission. These plans, which relied on students voluntarily transferring schools, did little to challenge racial isolation (Welch and Light 1987). In *Swann v. Charlotte-Mecklenburg Board of Education*, the Supreme Court upheld the use of districtwide busing for school desegregation. This decision, like the *Green v. Board* decision before it, urged proactive efforts to integrate. Finally, in *Keyes v. School District No. 1, Denver*, the Supreme Court deemed the *de facto* segregation of schools to be unconstitutional as well. This decision forced districts, many outside of the U.S. South, to overhaul school admission policies that implicitly promoted racial isolation.

These rulings—in combination with legislation that empowered the U.S. Department of Justice to bring legal action against school districts resisting integration (i.e., the 1964 Civil Rights Act) and that supplied additional federal funds to schools serving disadvantaged students (i.e., Title I of the Elementary and Secondary Education Act in 1965)—resulted in a palpable change in the rate and manner that districts racially integrated schools. Johnson (2011) shows that, prior to 1965, major plans for integration could take nearly a decade to implement following a local court mandate to do so. After 1965, the period of focus for several studies (including this one) that identify the causal impacts of school desegregation on outcomes, districts’ major plans to integrate were implemented with much higher efficacy and fidelity (e.g., Chin 2021; Gordon and Reber 2018; Guryan 2004; Reber 2005; Shen 2018).

Finally, a series of more recent court decisions have made it easier for schools to perpetuate racial isolation. In *Miliken v. Bradley*, the U.S. Supreme Court essentially determined

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that school systems bore no responsibility for inter-district racial integration unless this segregation was proven intentional. Contemporary research shows that racial isolation between school districts in the same locale contribute significantly to continued segregation (Fiel 2013). Further weakening efforts to integrate were a series of court decisions in the 1990s allowing districts previously under court mandate to integrate to be released from this oversight (Reardon et al. 2012). With even voluntary efforts to remedy racial isolation within school districts facing judicial scrutiny (*Parents Involved in Community Schools v. Seattle School District No. 1*), the demographic shifts in schools following the considerable integrative efforts beginning in the 1950s appear by some measures to have receded (Reardon and Owens 2014).

School Desegregation and Racial Attitudes and Politics

Many scholars have tested how desegregation affects the educational and socioeconomic outcomes of youth (for a review, see Ayscue, Frankenberg, and Siegel-Hawley 2017; Reardon and Owens 2014). Reardon and Owens (2014) presents a stylized model arguing that, by changing the distribution and overall level of resources in schools, integration can improve racial equity. But increasing Black students' academic achievement was just one argument used by the Supreme Court to support their decision in *Brown v. Board* (Stephan 1978). Another key motivating factor for reducing racial isolation was reducing the outgroup racial prejudice of both Whites and Blacks.¹

Allport's seminal piece on contact theory (1954) explains why many believed in the potential for desegregation to cause attitudinal changes in individuals. According to the theory, increasing Black-White contact can reduce prejudice, and such contact would substantially

¹ Importantly, social scientists at the time also argued that racial isolation negatively affected the moral and character development of White youth, but this rationale was largely ignored by the Supreme Court in its decision (Bell 2005).

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increase in integrated school settings—especially in a society where deeply entrenched residential segregation may have limited it otherwise. However, in his hypothesis, Allport also stressed that improved intergroup relations would most likely follow in contexts meeting certain conditions (1954): equal status between Blacks and Whites, shared goals and cooperation, and societal, legal, and cultural support for Black-White contact. Whether or not the formally segregated schools met these conditions after *Brown v. Board* is debatable (Gerard 1983), especially given variation in the capacity (or desire) of school leaders, teachers, and other stakeholders to establish classrooms conducive to successful intergroup racial interaction. And though Pettigrew and Tropp's (2006) influential meta-analysis of studies of contact theory suggests that Black-White interactions can lead to prejudice reduction even when Allport's conditions (1954) are not met, more recent research highlight the need for more systematic evaluation (Paluck, Green, and Green 2018).

The racial threat hypothesis further supplies reason to believe that intergroup contact in desegregated schools may not have improved White youth's racial attitudes (Key 1949). The theory broadly posits that in the U.S., as Black individuals represent a larger share of a population, White individuals will feel threatened and take actions to reduce that threat. Much research has applied the racial threat hypothesis to residential integration, with some also focusing on its implications for educational contexts (see Fiel 2015). In integrating schools, the hypothesis suggests worsened racial attitudes among White students due to perceived increases in competition for educational resources. Perceived threat might also lead individuals to react in ways that reinforce their power. With regards to the *Brown v. Board* context in particular, these reactions could be reflected in the political realignment of White individuals. Specifically, many Republican party politicians at the time—especially in the U.S. South—attempted to win votes

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by appealing to White racial conservatives. With prior research also suggesting a link between racial attitudes and partisan identity (e.g., Kuziemko and Washington 2018; Valentino and Sears 2005), school integration might have thus also affected White youths' political behaviors.

Other explanations exist besides those put forth by the contact and racial threat theories for why and how the racial attitudes of White individuals may change—or remain stable—over time. White flight from integrated school districts (Reber 2005; Welch and Light 1987) or increases in private school enrollment could have limited actual contact between Black and White youth and, thus, hindered changes in attitudes. Within-school segregation—due to tracking or self-selection of individuals into racially isolated social groups—similarly impedes contact (Moody 2011; Oakes 1985). When measuring the racial attitudes of White *adults* who attended integrated schools, mobility may bias estimates from a surveyed sample (e.g., Gordon and Reber 2018; Shen 2018). Finally, experiences in adulthood may counteract attitudinal shifts resulting from reduced racial isolation. With prior research showing positive effects of desegregation for the educational and socioeconomic outcomes of Black youth (Anstreicher, Fletcher, and Thompson 2022; Guryan 2004; Johnson 2011), White adults who attended desegregated schools may have developed negative outgroup attitudes due to increased labor market competition.

Schofield (1991) describes that much of the empirical research in the decades following *Brown v. Board* investigating the impact that integration had on racial attitudes could not establish causality, covered a constrained geographic scale, and suffered from weak measurement instruments. These limitations contribute to the early literature's overall inconclusiveness on desegregation's impact on attitudes and their proxies. More rigorous investigations of recent educational contexts, however, demonstrate the importance of the racial

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composition of students' peers. For example, Merlino, Stenhardt, and Wren-Lewis (2019) find that Whites with more same-gender Black peers in schools have more relationships with Blacks as adults and score higher on proxies for positive racial attitudes. Billings, Chyn, and Haggag (2021) find that White students enrolled in schools with more peers from racially minoritized backgrounds are substantially less likely to be registered as a Republican in adulthood. Shen (2018) finds that historic school desegregation outside the U.S. South increased biracial births among Black mothers, another potential proxy for improved racial attitudes (cf. Gordon and Reber 2018). Finally, in a postsecondary study, Boisjoly and colleagues (2006) find that White students randomly assigned to African-American roommates in college are more likely to support affirmative action.

In summary, many explanations can account for the inconclusiveness of historic school desegregation's impact on racial attitudes. Early empirical studies were limited methodologically, but theory and studies of more recent contexts also suggests that positive, negative, and null effects are possible. I next describe the data, sample, and empirical strategy that allow me to address the methodological concerns of prior work.

Method

Data

White Adults' Racial Attitudes and Politics

To investigate the impact of school desegregation on White individuals' racial attitudes and politics in adulthood, I use responses to the GSS, a nationwide survey of adults first administered in 1972 by the National Opinion Research Center.² Since 1994, the survey has been conducted in every even-numbered year. I focus on surveys conducted between the years 1993

² For other sub-national analyses of racial attitudes using the GSS, see for example, Charles and Guryan (2008), Cutler, Glaeser, and Vigdor (1999), and Card, Mas, and Rothstein (2008).

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and 2018, when restricted-use data contain geographic information on respondents' county of residence. The items I use from these surveys fall into three primary categories, measures of: respondents' background, socioeconomic outcomes, and racial attitudes and politics.

Respondents' background data include their age, their race, their current county of residence, whether they were living in the same city at the age of 16, and the year they responded to the GSS survey. With this information, I identify whether White individuals lived in a county that underwent school desegregation and, if so, I use survey year and age to determine if the respondent was of plausible school age (i.e., 17 years old or younger) when the county's earliest major desegregation plan was implemented.

I use measures of respondents' socioeconomic outcomes to test potential mechanisms for any observed effects of integration. Specifically, I explore whether desegregation impacts White individuals' educational attainment, earnings, or perceptions of class as adults. I conduct this "placebo" test because White individuals' racial attitudes and politics may shift as a response to the labor market they face, and not just because of exposure to less racially isolated schools. As noted above, prior research shows that integration improved the educational and socioeconomic outcomes for Black youth (Anstreicher, Fletcher, and Thompson 2022; Guryan 2004; Johnson 2011), which would have led to more competitive labor markets. For educational attainment I use respondents' number of years of schools completed and for earnings I use reported family income (in 1986 dollars). To assess perceptions of class, I create a composite score using responses to three GSS items querying respondents' self-reported social class, satisfaction with his or her financial situations, and opinion of family income relative to "American families in general". In Appendix A I describe in more detail how I estimate composite scores; Appendix Table 1 provides summary statistics for these three GSS items.

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Finally, the primary outcomes in my analyses are respondents' answers to questions regarding their racial attitudes and politics. I consider both sets of items because prior work on school integration and racial attitudes explore political outcomes and show the association between the two. As noted above, for the period following *Brown v. Board* in particular, the link between racial attitudes and partisan identity may be even more salient, as racially conservative White adults in the South who historically voted for Democratic political candidates realigned with the Republican party (Kuziemko and Washington 2018). More concretely, the racial threat hypothesis indeed predicts that White individuals might use their political influence to maintain power in reaction to the perception of increased competition for resources in integrated schools.

I identify 19 items on GSS surveys that both plausibly relate to these topics and are also administered to a substantial number of survey respondents. However, the number of items and the relatively small size of my sample (described below) suggests that multiple inference may be an issue in analyses. As such, I use factor analyses to reduce the GSS data on White adults' attitudes and politics into a set of three composites. Scores on these composites capture the conservatism of respondents' politics (e.g., identifies as a Republican), their attitudes towards Blacks and policies promoting racial equity (e.g., feels close to Blacks relative to Whites; favors affirmative action in hiring and promotions), and their support for protecting racist speech (e.g., believes individuals with racist points of view should be allowed to teach in a college or university).

In Appendix A, I provide details on the exploratory and confirmatory factor analyses informing the creation of these three composite scales and my method for estimating composite

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scores for White GSS respondents.³ I also include in Appendix Table 1 the item text for all survey questions I consider as well summary statistics.

Most respondents lacking data on specific questions are missing this information completely at random (i.e., MCAR) due to the structure of the GSS. Specifically, though many items regarding individuals' racial attitudes and politics are asked across survey administrations, not every item appears in all years or on all survey forms. As such, I also detail in Appendix Table 1 the rates of MCAR and other missingness for items I analyze. To account for missingness without dropping observations, I use full information maximum likelihood to compute the covariance matrix used in factor analyses (Graham 2009) and to estimate composite scores for those in my sample. As a sensitivity check, I also employ multiple imputation to account for missingness and estimate composite scores from imputed datasets. Internal reliability estimates for the three composites based on multiply imputed data were acceptable, ranging from .66 to .76. I also find that scores for individuals' conservatism of politics, attitudes towards Blacks and policies promoting racial equity, and support for protecting racist speech estimated using full information maximum likelihood predict scores from imputed data almost one-to-one.

County- and District-Level Characteristics

I leverage data from several other sources collected prior to the *Brown v. Board* decision to characterize the counties and school districts in my analytic sample. Data from the 1950 Decennial Census provide county-level details on demographics and socioeconomic outcomes. I

³ I opt to reduce the 19 GSS items into these three separate scales as opposed to a single scale (e.g., with principal component analysis), which would further help address multiple inference issues, for several reasons. First, despite their observed association in prior research, face validity would argue that political identity and racial attitudes are distinct constructs. Second, in Appendix Table A1, I show that exploratory factor analysis of the 19 items results in few obvious cross-loadings across the three scales, suggesting multidimensionality. Finally, GSS respondent-level correlations in my sample for the three composite scores are low to moderate, i.e., below .1 and up to .5 for White individuals' conservatism and their attitudes towards Blacks and policies promoting racial equity.

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augment this data with county-level information on U.S. presidential voting in the 1952 election from Dave Leip's Atlas of U.S. Presidential Elections (2016).

To further explore mechanisms behind any observed effects of integration on racial attitudes and politics, I use district-level data linked to U.S. counties from the Office of Civil Rights surveys (OCR), which were administered to school districts across the country beginning in the 1960s.⁴ I specifically focus on measures of district-level school segregation and the proportion of districts' students that are White. With these data I investigate whether major desegregation plans "worked" and, if so, the extent to which they worked (i.e., the magnitude of the decrease in racial isolation across schools and/or White disenrollment following integration). My preferred measure of school segregation is the White-Black exposure index, which captures the proportion of Black students in the average White student's school. Another commonly studied measure, the dissimilarity index, is not appropriate to test theories of intergroup contact because of its specific focus on how evenly groups are distributed across schools.

Finally, I use information on county-level voting results for U.S. presidential elections from 1912 onwards (Leip 2016) to test the robustness of my main results. Specifically, I explore whether patterns in the proportion of votes won by Democratic presidential candidates before and after school integration mirror contemporaneous changes in White individuals' politics in the GSS data.

Sample

⁴ The OCR surveys administered from the 1960s through the 1980s did not occur every year. School districts received surveys in the fall semester for every school year from 1968 to 1974, then again in 1976, 1978, 1980, 1984, 1986, and 1988. Not all districts were surveyed every year but once a district was surveyed, data for all schools in the district were included. Furthermore, across administrations, the OCR survey leveraged different sampling approaches though, in all instances, efforts were made to maintain national representativeness. Finally, larger districts tended to be sampled more frequently, as well as those that were of "high interest" to the OCR, i.e., those under school integration orders. Importantly, nearly all desegregating districts that I consider in analyses have full representation across OCR surveys during this entire time period.

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To identify the impact of school desegregation on White individuals' racial attitudes and politics in adulthood, I focus on these outcomes for respondents from 342 U.S. counties represented in the GSS. 74 of these counties contain 77 school districts of interest that implemented major school integration plans between 1960 and 1990 following a court mandate (Welch and Light 1987). I focus on these districts as several prior studies have investigated the effects of integration in their contexts (e.g., Chin 2021; Guryan 2004; Reber 2005). Furthermore, these districts were sampled for targeted analyses in the past because they represented a substantial proportion of school enrollment—especially of youth from racially minoritized backgrounds—in the U.S. at the time of measurement (Welch and Light, 1987). Finally, I focus on these districts because they are among the largest in the country, which helps to ensure that the geographic level at which integration efforts occur (at the district level and not the county level) is less of a concern for my analyses. In Appendix Table 2 and Appendix Figure 1, I identify these districts, their counties, and the years of their earliest major plans, which are compiled by Welch and Light (1987). When a county comprises multiple integrating districts, in analyses I focus on the earliest year of integration.

The other 268 counties I include in my sample are “comparison” counties where districts were never mandated by court orders to integrate. To identify these counties, I link districts to counties using the OCR survey data and cross-reference these data to the most comprehensive list of districts ever under court mandate, collated by ProPublica (2017) (see also Fiel and Zhang 2019).

Among the White GSS respondents in these sample counties, I restrict the sample further. First, I exclude the extremely small proportion of individuals missing data on age, which I use in combination with survey year to identify exposure to desegregated schools. Second, I focus on

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respondents that report that they currently live in the same city as they did at the age of 16.

Investigations of the impacts of school integration on long-term outcomes often cannot fully address potential bias resulting from the mobility of individuals over time (i.e., movement into or away from counties with desegregating schools) due to reliance on cross-sectional data (e.g., Gordon and Reber 2018; Shen 2018). By including just those who report living in the same city at a school-going age, I thus hope to assuage serious concerns of mobility biasing my results.

Finally, I include only GSS respondents who turned 18 within 12 years of implementation of their county of residence's first major integration plan. For those living in comparison counties, I select only those who turned 18 within 12 years of any sample county's earliest integration efforts (e.g., 1961 through 1982, see Appendix Table 2; I make additional sample exclusions as I note in my description of estimation models below). This restriction reflects my preference to compare the racial attitudes and politics of individuals who are relatively closer in age but differentially exposed to desegregated schools, as they may be more similar on unobservable characteristics. In tests of the robustness of results, I further restrict my sample's age range to strengthen this assumption.

In Table 1, I provide baseline summary statistics for the counties and GSS respondents in my analytic sample, split by desegregation status.

[Insert Table 1 about here.]

From the table, I conclude that desegregating and comparison observations vary significantly from one another at baseline. For example, comparison counties are less populated, whiter, and less urban. They are also less Democratic and experience less unemployment despite lower levels of median household income. These patterns for socioeconomic outcomes are reflected in the individual-level comparisons as well. As I describe below, however, baseline

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equivalence between desegregating and comparison counties is not necessary to draw causal conclusions because I use a differences-in-differences approach. Furthermore, for two of the three outcomes of interest capturing racial attitudes and politics, differences are not significant. Finally, I test the robustness of my main findings when using only as comparison counties those whose districts have not yet desegregated as a result of court mandates but do so eventually.

Because the GSS is nationally representative, the sample size of respondents within any given county may concern some. However, of the desegregating counties, 26 have 10 to 19 respondents, nine have 20 to 29 respondents, and seven have more than 30 respondents across survey administrations. In the average desegregating county, 62% of respondents are exposed to racially integrated schools, ensuring sufficient variation in the “treatment” variable of interest (for a similar county-level study in education using nationally representative data with comparable sample sizes, see Thompson, 2018).

Empirical Strategy

To identify the causal impact of school desegregation on White individuals’ racial attitudes and politics in adulthood, I use a differences-in-differences approach (DID). For the first difference, I compare outcomes between those living in the same county but, based on age and time of major desegregation plan implementation, experience credibly exogenous differences in exposure to integration. Specifically, I identify those who turn 18 after the year of his or her county’s earliest plan to be exposed and those who turn 18 the year of or earlier (i.e., no longer of traditional K-12 school age) to not be exposed. For the second difference, I compare the outcomes of GSS respondents living in desegregating counties to those living in comparison counties who turned 18 in the same year. The first difference thus accounts for persistent contextual differences in racial attitudes and politics among White adults, whereas the second

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difference accounts for contemporaneous shifts in outcomes over time across age cohorts.

Notably, with my particular DID approach (i.e., a “stacked” difference-in-differences; see Baker, Larcker, and Wang 2022; Cengiz et al. 2019; Chin 2021; Deshpande and Li 2019), I never use counties that have already integrated as comparison observations for other integrating contexts. This restriction, also commonly used in synthetic control strategies (Abadie, Diamond, and Hainmueller 2010), ensures that estimates are robust to concerns of bias raised by recent advances in the literature on DID (Goodman-Bacon 2021).

To operationalize this DID, I first create a unique dataset for the 74 “treated” counties—those implementing major school desegregation plans—in my analytic sample. For each of these datasets, I include the desegregating county’s outcome data as well as the same data for all 268 comparison counties that are never mandated by court order to integrate. I then append all 74 individual datasets into a single dataset and estimate the following model:

$$Y_{ijkt} = \beta \left(D_j \mathbf{1}(t > T_j^*) \right) + \gamma \left(D_j \mathbf{1}(t > T_j^*) \times Deseg_j \right) + \delta_j + \theta_k + \eta_t + \varepsilon_{ijkt} \quad (1)$$

Where Y_{ijkt} captures the score on one of the three composites capturing racial attitudes and politics (rescaled as a z -score to facilitate interpretation) for White GSS respondent i who lives in county j and who turned 18 in year t . In addition to fixed effects for county δ_j and age 18 cohort year η_t , I also include fixed effects for each unique stacked dataset k , θ_k , where $\{k \in \mathbb{Z} \mid 1 \leq k \leq 74\}$. I cluster standard errors at the most conservative level—the dataset level.

The main coefficient of interest is γ , which captures for counties with desegregating school districts the relationship between the outcome and a dichotomous variable that indicates whether the year that the White GSS respondent turned 18, t , occurred after the implementation year, T_j^* , of the earliest integration plan for districts in the desegregating county in the dataset.

The regression coefficient β captures this same relationship for those in the same age 18 cohorts,

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t , but who live in comparison counties in the dataset. As noted above, I restrict my analytic sample based on age, including only GSS respondents whose age 18 cohort year is specifically within 12 years of the year of the earliest integration effort for the desegregating county of the unique stacked dataset, i.e., $\{(t - T_j^*) \in \mathbb{Z} \mid -11 \leq (t - T_j^*) \leq 12\}$. Finally, I do not include a main effect for living in a desegregating county, $Deseg_j$, as variation in this variable is subsumed by the county fixed effects, δ_j .

The key assumption necessary for my DID approach to recover the causal impact of integration on racial attitudes and politics is that the average outcome on GSS composites for White individuals in both desegregating and comparison counties would have followed parallel trends absent school integration. However, because it is impossible to determine what GSS respondents' outcomes in desegregating counties would have been without “treatment”—the counterfactual of interest—the parallel trends assumption is inherently unobservable. In some DID investigations, researchers provide suggestive evidence that the parallel trends assumption is tenable by showing no differences in outcomes between treated and comparison observations in the same age cohorts leading up to treatment. Given the relatively small sample size of GSS respondents, I provide the results from this “event study” approach but do not rely on the event study model as my preferred specification as cohort-specific impacts are estimated less precisely (see also Guryan 2004; Thompson 2018).

I do additionally show in sensitivity tests below that my results are robust when I restrict my sample to GSS respondents whose age 18 cohort year is within eight or four years (instead of 12 years, my preferred analytic sample) of the earliest implementation year of major integration plans for desegregating counties. This increases the likelihood of similarity on unobservable characteristics for analyzed cohorts and reduces the chance that any observed DID impacts for

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integration are capturing pre-trends across cohorts. Second, I show that results are robust when I slightly weaken the parallel trends assumption. Specifically, I estimate the model represented by Equation (1) above but additionally include a linear age 18 cohort variable interacted with county fixed effects. Finally, I leverage the near-perfectly measured outcome of county-level Democratic U.S. presidential candidate vote share in event study regressions. I estimate whether changes in this measure of politics approximates changes in GSS measures of politics in desegregating counties compared to comparison counties, leading up to and after the implementation of major school integration plans. That my findings remain qualitatively the same across these sensitivity tests—in addition to extensive prior evidence of parallel trends for different outcomes in other DID papers investigating the impacts of integration for the same districts I consider (e.g., Chin 2021; Reber 2005; Shen 2018)—lends credence to the robustness of my DID results.

Finally, for all my analyses investigating the impact of school desegregation on White individuals' racial attitudes and politics in adulthood, I investigate whether results vary by geographic region. Specifically, I estimate the model represented by Equation (1) but interact exposure to integration variables with a dichotomous variable indicating if the desegregating county in the stacked dataset is located in the U.S. South, based on Census designation.⁵ I investigate geographic heterogeneity because, as laid out clearly by Shen (2018), how Black and White students were educated prior to the era of racial school desegregation—and, consequently, how integration unfolded—varied substantially across regions. For example, the U.S. South exhibited higher levels of segregation at baseline and, unlike in other parts of the U.S., Black and White students were separated by law. One recent study also found positive impacts of

⁵ Results from models estimated separately by geography (as opposed to single models interacting exposure to integration with geography indicators) are essentially the same and available upon request.

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integration on Black students' adulthood outcomes but only in the south (Anstreicher, Fletcher, and Thompson 2022), with the authors theorizing that differential resistance among White stakeholders to desegregation efforts may explain heterogeneity across regions.

Results

Main Impacts of School Desegregation

I first describe my main results for the impact of integration on White individuals' racial attitudes and politics as adults. In Table 2, I present coefficient estimates from estimation of the model represented by Equation (1) with the three GSS composite scores—political conservatism, having a positive attitude towards Blacks and policies that promote racial equity, and support for protecting racist speech—as outcomes of the model. I focus on impacts for GSS respondents who lived in desegregating counties and were still school-aged when the first major integration plan was implemented for their district.

[Insert Table 2 about here.]

I find that school integration by race had no overall effect on White individuals' racial attitudes and politics in adulthood. Those exposed to desegregation on average were .03 *SDs* less conservative and scored .01 *SDs* higher on the composite capturing positive racial attitudes. Further, there was no average difference in support for protecting racist speech between those exposed and not exposed. For all three point estimates, I cannot reject the null hypothesis that average impacts of integration are zero.

However, the table also depicts substantial heterogeneity in the effects of historic desegregation on outcomes by geography. Specifically, I find that White adults in the U.S. South experienced significant decreases in conservatism when exposed to integration in contrast to small, insignificant increases in conservatism for those in other geographic regions. Efforts to

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reduce racial isolation across schools lowered conservatism by .18 *SDs* in the South and increased conservatism by .038 *SDs* in non-Southern counties. The difference in these point estimates are statistically significant ($p < .05$). I observe a similar pattern when focusing on the composite score capturing White individuals' positive attitudes towards Blacks and policies that promote racial equity. Impacts on this measure are positive in the South and slightly negative elsewhere, though the geographic-specific estimates and the difference between them are insignificant by conventional standards ($p \approx .14$). Finally, I observe no evidence of impact heterogeneity for integration on individuals' support for protecting racist speech.^{6,7,8}

Because there is no obvious benchmark in the literature for the composites in my analyses, interpreting the magnitude of these impacts of integration is challenging. Thus, to facilitate understanding, I focus on the impact of desegregation on a more transparent item of the composites. For example, when I estimate the model represented by Equation (1) but include as my outcome whether the GSS respondent voted (or intended to vote) for the Republican candidate in the most recent presidential election, I find that those who were exposed to schools desegregating by race in the South were 5.85 percentage points less likely to have voted for the

⁶ In Appendix Table 3, I test the sensitivity of my results to other plausible specifications, including: interacting age 18 cohort fixed effects with Census region fixed effects; including survey year fixed effects; and using General Social Survey weights. Results are qualitatively similar across all models.

⁷ An alternative explanation for these main results might be that GSS respondents unexposed to integrating schools may be "treated" by efforts to reduce racial isolation and have become more conservative. However, there would still need to be some treatment (e.g., successful intergroup contact) happening in integrated schools that prevent commensurate shifts in outcomes for those exposed. It is thus important to stress that this interpretation of these DID effects focus on how outcomes change relative to the trend observed in these comparison groups.

⁸ As noted above, results from the factor analysis of the 19 GSS items describing racial attitudes and politics I consider may explain this distinct result. Specifically, though some items cross-load onto both composites capturing political conservatism and positive attitudes towards Blacks and policies that promote racial equity, there are almost none that also cross-load onto the composite capturing individuals' support for protecting racist speech, indicating a weaker association (Appendix Table A1). Exploratory analyses (available upon request) suggest that respondents' support for protecting racist speech aligns more closely with attitudes towards the protection of free speech more generally.

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Republican candidate. Those exposed in non-Southern counties were instead 1.37 percentage points more likely to have voted for the Republican candidate.

Robustness Tests

I next provide in Table 3 evidence supporting the robustness of my findings regarding the impact of school desegregation on White individuals' racial attitudes and politics as adults.

[Insert Table 3 about here.]

In the table, I share the coefficient estimates from estimation of variants of the model represented by Equation (1), again with the three GSS composite scores as outcomes. In Panels A and B, I test whether the results in Table 2 hold after changing the sample of White GSS respondents included in my sample. Specifically, in my preferred specification, I focus on those who turned 18 within 12 years of the earliest implementation year of major integration plans for desegregating counties. When I restrict this age range further—to include only those who turned 18 within eight or four years of integration—and re-estimate my models, results remain qualitatively the same. Those in the south exposed to school integration by race are significantly less conservative and more likely to hold positive attitudes towards Blacks and policies that promote racial equity. These causal effects are more attenuated for those living elsewhere and, in some cases, are in the opposite direction (though still insignificant). I also continue to observe no impact of efforts to reduce racial isolation on individuals' support for the protection of racist speech. The findings in Panels A and B in Table 3 thus suggest that my results are consistent when I compare the racial attitudes and politics of those more likely to share unobservable characteristics but who are differentially exposed to the racial desegregation of schools.

In Panel C of Table 3, I present further evidence of the robustness of my main findings. In this panel, I provide the coefficient estimates from estimation of the model represented by

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Equation (1), but additionally interact county fixed effects with a linear term for age 18 cohort. The inclusion of these interactions results in a conservative specification that can disentangle potential impacts of integration from underlying trends in racial attitudes and politics within county. But even with these controls I continue to find the same result. White individuals exposed to integrated schools are less conservative and hold more positive racial attitudes as adults, and these effects are stronger in magnitude for those living in the U.S. South.

Finally, in Panel D of Table 3, I test the robustness of my results when I compare each county with desegregating school districts to only the other counties that are eventually mandated by courts to desegregate and do so in the future. I conduct this sensitivity test because, as shown in Table 1, there may be some concern over the baseline differences between districts under court order and never under court order (though baseline equivalence is not a necessary condition to recover causal estimates using DID). But I continue to find the same pattern as my main results when I focus on respondents who turned 18 within eight years of the earliest implementation year of major integration plans for desegregating counties and difference out the contemporaneous trends observed in counties that desegregate at least eight years later.⁹

[Insert Figure 1 about here.]

Event study estimates reflect the main results. Specifically, I plot in Figure 1 regression coefficients that differentiate the impacts of desegregation by time exposed to integrated schools. Conservatism (positive attitudes towards Blacks and policies promoting racial equity)

⁹ I focus on those that turned 18 within eight years of desegregation as opposed to 12 years because this expands the number of counties with districts integrating schools in my sample that contribute to estimates (i.e., counties with districts integrating in later years essentially have fewer or even no comparison units as this restriction is further loosened), while also maintaining a reasonable sample size of respondents within counties (i.e., there would be far fewer composite scores to analyze when focusing on respondents turning 18 within four years of desegregation). Comparing politics and racial attitudes with those from counties with already desegregated districts may lead to bias in DID estimates (Goodman-Bacon 2021). These issues contribute additional justification as to why my preferred DID specification relies on making comparisons with counties with districts that are never under court order to reduce racial isolation across schools.

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significantly decreases (increases) post-integration in the U.S. South but not elsewhere. The event study results do not indicate significant differential pre-trends in outcomes, further strengthening my identification approach, nor do they depict substantial heterogeneity by length of exposure. However, the point estimates are (as expected) less precisely estimated, which cautions against over concluding.

Finally, in Figure 2, I corroborate my findings for the impact of school desegregation on the GSS composite outcomes with analyses leveraging population data on politics: county-level voting outcomes for U.S. presidential elections. I specifically estimate the model represented by Equation (1) and focus on as an outcome the proportion of votes in a county over time that went to the Democratic presidential candidate. Instead of considering results from just the elections immediately before and after school integration (as I do in my preferred specification), I also include in analyses the data from those occurring several years prior to and after the implementation of districts' earliest major integration plans to estimate an event study model.

[Insert Figure 2 about here.]

In the figure, I plot the regression coefficients of desegregation's impact on average Democratic vote share for each leading and lagging election. I find that for integrating counties, the Democratic vote share increased after efforts to reduce racial isolation across schools, with larger discernable impacts that persisted over time in the U.S. South. These results confirm patterns I observe using the GSS data on White individuals' political conservatism: As those in desegregated schools became less conservative and began to vote, election results slowly reflected this change in the electorate more broadly. I also find that, for the election two cycles prior to integration (i.e., -1), there was no impact of integration on the Democratic vote share in

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desegregating contexts, as would be expected. This was particularly true for all elections leading up to integration for county-level votes outside the U.S. South.

I do show some evidence that Democratic presidential candidates were losing vote share in counties in the U.S. South in the elections prior to school integration, but I am not overly concerned that this “pre-trend” violates the parallel trends assumption necessary to recover causal effects for my DID approach. Elections before the two held immediately prior to integration could have occurred over eight years earlier (because they occur every four years) and, as noted above, I observe no significant difference in the Democratic vote share for elections held up to seven years before integrative efforts. Second, the primary reason to be concerned of the decreasing Democratic vote share prior to integration is that observed “impacts” following integration may capture mean reversion (i.e., an “Ashenfelter Dip”). But post-integration impacts are sustained, and the pattern observed converges with historic trends in voting across the U.S. South and may not result from random cyclicity. As the Democratic party embraced the Civil Rights movement beginning with Democratic President Harry Truman in 1948, White southerners began to leave the party (Kuziemko and Washington 2018) and Republican politicians began to embrace policies appealing to White voters’ racism (i.e., the “Southern strategy”). Finally, the downward pre-trend and significant and sustained positive Democratic vote share gain post-integration suggests an even stronger positive impact of integration on politics once the trend is accounted for—which matches qualitatively the findings in Panel C of Table 3. Thus, because presidential voting outcomes are arguably measured without error, Figure 2 lends additional plausible evidence to other event study estimates of the impact of desegregation on White individuals’ racial attitudes and politics in adulthood—an approach that suffers when using GSS data due to concerns regarding imprecision resulting from

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small sample sizes. That standard errors are increasing for the regression coefficients in Panels A, B, and D of Table 3 reinforces this concern, as these estimates emerge from models that further restrict the sample of GSS respondents analyzed.

Mechanisms

In Table 4, I present results from an exploratory investigation of potential mechanisms explaining why I observe greater effects of integration for White individuals in the U.S. South. I test two primary mechanisms. First, I identify the extent to which efforts to reduce racial isolation across schools worked. Second, I consider whether integration affected White individuals' socioeconomic outcomes as adults. This latter investigation also serves as a placebo test for my DID approach, i.e., if socioeconomic outcomes changed for White individuals exposed to integration, it might signal some other policy occurring concurrently with school desegregation leading to shifts in racial attitudes and politics as well.

To conduct these investigations, I re-estimate the model represented by Equation (1) but include as outcomes: school segregation, the proportion of students that are White, years of education, real income (logged), and a composite for perceptions of class. The first two outcomes are measured at the district level (using OCR survey data from the 12 years before and after integration); the final three are captured by GSS respondents' survey answers.

[Insert Table 4 about here.]

I first find that school integration worked “better” in the U.S. South, which might explain the stronger effects on White individuals' racial attitudes and politics. School segregation, as measured by the White-Black exposure index, in desegregating districts in the south dropped by .14; elsewhere it dropped by .12. In layman's terms, the average White student in the south attended a school that served a student population with 14 percentage points more Black students

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after integration. These results closely mirror those seen in prior studies (Reber 2005). Though the difference between the regression coefficient for the impact of desegregation in the South is not statistically significant from the same coefficient for non-Southern contexts ($p \approx .29$), it is when considering the impact of integration on the district-level percent enrollment of White students. Districts in the U.S. South saw significantly less White disenrollment following integration, further suggesting that White individuals who were exposed to less racially isolated schools were more likely to experience White-Black contact as a result.

Conversely, I find weaker evidence that the greater decreases in political conservatism (and increases in positive attitudes towards Blacks and policies that promote racial equity) could be driven by differential experiences in the labor market for White adults in the U.S. South as compared to other regions. Specifically, though I show that White adults exposed to integrating schools outside the U.S. South on average attained approximately half a year less of education, I observe no commensurate impact on their income or perceptions of class, nor do I observe significant geographic heterogeneity for impacts on these socioeconomic outcome measures.

Discussion

The court-mandated desegregation of schools by race starting in the 1950s left an indelible impact on education in the U.S. Research largely finds that these changes improved the life outcomes of Black youth across the country (Anstreicher, Fletcher, and Thompson 2022; Guryan 2004; Johnson 2011). But stubborn resistance to integration over the decades, primarily by White families, and judicial precedent eventually slowed and even reversed the rate of desegregation.

In recent years, school integration by race has yet again become a topic of conversation in education policy circles. Given its largely positive effects on the educational and socioeconomic

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outcomes of Black youth and its potential importance in the moral and character development of White youth, this interest is unsurprising. However, some research also finds negative unintended consequences resulting from desegregation. Because other educational interventions have helped advance equity, without additional evidence of integration's positive contributions, it may be more politically (and legally) feasible to pursue these other programs.

But integration advocates have long cited its unique potential to improve the racial attitudes of White youth, as contact theory broadly predicts (Allport 1954). These theoretical changes can have major implications for the long-term opportunities of Black individuals across the U.S. However, limited causal evidence exists that supports advocates' views and alternative theories suggest that negative attitudinal shifts (i.e., racial threat hypothesis) appear just as applicable.

In this study, I address this limitation of existing research by leveraging a credibly causal design, in combination with nationwide data on White individuals' racial attitudes and politics, to identify these theoretical benefits of integration. I find evidence that though historic court-mandated school integration by race did not positively affect racial attitudes (and the political views that correlate with these attitudes) on average across the country, significant heterogeneity exists. Specifically, White individuals exposed to desegregated schools in the U.S. South exhibit in adulthood weaker conservative politics, and more positive attitudes towards Blacks and policies promoting racial equity. I find suggestive evidence that the stronger positive effects of reduced racial isolation in the south may be stemming from more effective desegregation plans, i.e., those that led to relatively larger increases in White-Black exposure in public schools and smaller decreases in White public school enrollment. Divergences in the effectiveness of these plans may themselves proxy for heterogeneity across contexts in terms of resistance to

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integrative efforts. My findings converge with the most relevant study to my work, which is conducted by Billings and colleagues (2021) in Charlotte-Mecklenburg in North Carolina. The authors find that exposure to more racially diverse schools decreased the propensity for White youth to register as Republicans in adulthood.

Any investigation of how substantial school reforms affect individuals' outcomes measured decades later is subject to limitations. Studies on the long-term impacts of historic efforts to reduce racial isolation across schools following *Brown v. Board* are no different. For example, one of the most pressing concerns is that the richest data available to explore racial attitudes and proxies for attitudes in the present day—the General Social Survey—is not collected for the same individuals longitudinally. My study is generally able to address this issue by focusing only on data from GSS respondents that report living in the same city in both adulthood and in their teenage years.

Yet by restricting my analytic sample to nationwide data collected from a subset of GSS respondents, I cannot leverage (arguably) more rigorous methodological approaches to assess the impacts of school desegregation on White individuals' racial attitudes and politics in adulthood. Specifically, I cannot estimate precisely the event study models that would allow me to predict the impact of reduced racial isolation on individual cohorts of White youth exposed to integrated schools. I thus supplement the GSS data with population data proxying for individuals' politics that do allow me to use an event study approach: county-level results for presidential voting. However, because the election data itself is cross-sectional, it is important to confirm that the results using this data converge with my main findings for the GSS data. Reassuringly, when I consider both data sources (and leverage their associated strengths), I do not find any differences

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in results: school integration by race led to particularly strong decreases in political conservatism in the U.S. South.

Finally, the generalizability of study may be limited geographically and temporally. Though I investigate the impacts of efforts to reduce racial isolation across schools in the largest districts across the country, how these efforts translate to shifts in racial attitudes may vary in contexts that are dissimilar to the 77 focal districts of my study. Furthermore, the educational landscape in the decades leading up to and following the *Brown v. Board* decision is substantially different from the present landscape. As such, whether my results will replicate in districts currently pursuing desegregation is uncertain. With increasingly racially homogenous school districts and schools, and with between-district school segregation accounting for more and more of the overall observed racial isolation, integrative efforts may be less effective at actually expanding outgroup contact by race.

Future research should thus investigate how contemporary school desegregation policies impact the long-term racial attitudes of White youth. Because the demographics of students attending U.S. public elementary and secondary schools are changing, this work should also test how White individuals' attitudes towards other groups are changing. My study, for example, cannot test whether school desegregation might be able to remedy prejudice towards Hispanic or Asian individuals—the largest growing populations in the country. Furthermore, because the GSS sample is composed predominantly of White respondents, I cannot investigate how the politics and attitudes of Black youth (or youth from other racial/ethnic backgrounds) change in integrated settings—something that psychological theories would also predict.

With these important caveats in mind, how should policymakers currently interested in reducing racial isolation across schools leverage the findings of my study? It is clear that the

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context in which desegregation occurs matters for how the policy change can impact racial attitudes. I find strong positive effects of historic integration in the U.S. South but not elsewhere. I argue that my exploratory analyses on mechanisms suggest this heterogeneity because desegregation was more effective in the south. As such, if new policies are only marginally effective—or counteracted by other within-school sorting of students by race (e.g., tracking)—the positive attitudinal effects I observe may not be realized. Furthermore, I would encourage school and district leaders to heed Allport's (1947) original concerns that increasing Black-White contact in schools might actually exacerbate negative outgroup prejudices if not pursued intentionally and with stakeholder support.

School integration has worked to improve racial equity in the past and it can do so again. Given the social and legal barriers to reducing racial isolation, policymakers should of course continue to explore adopting educational programs that improve the outcomes of Black youth but are neutral on how they change the racial composition of schools. But desegregation's theoretical effects on improving intergroup relations did accrue in certain contexts following *Brown v. Board*. The potential benefits of integrated schools to support the development of youth's attitudinal outcomes and, subsequently, their ability to engage in a diverse, multiethnic society thus compels leaders to identify how best to foster integrated spaces in education.

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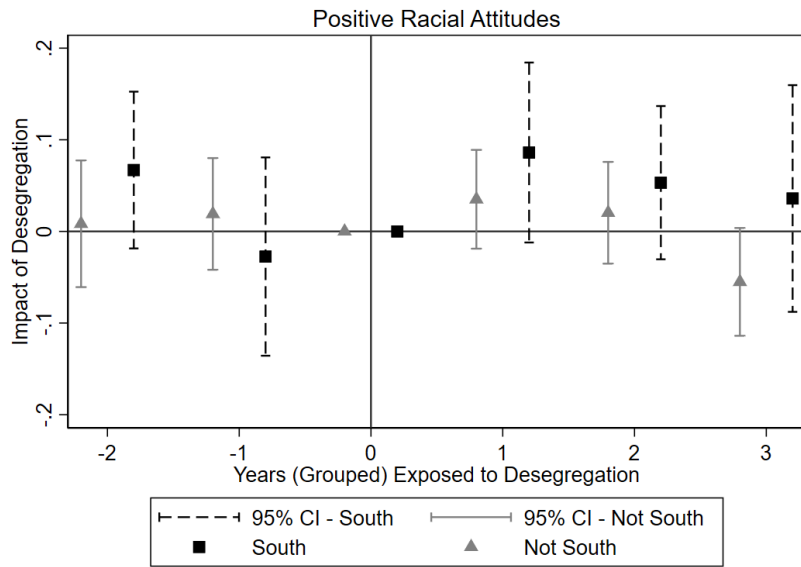
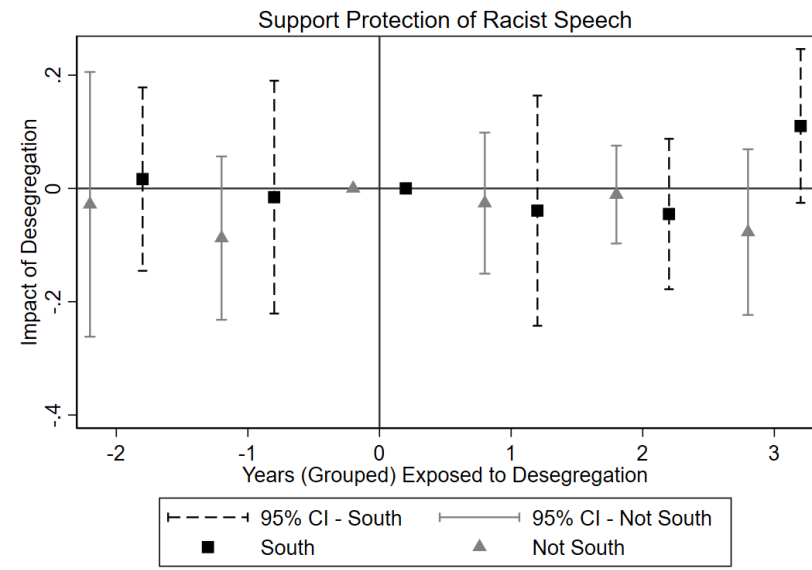
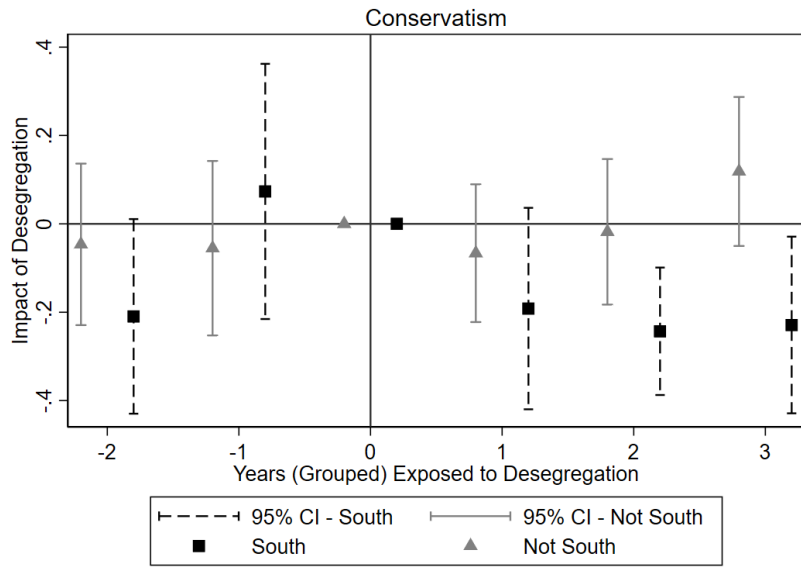
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Figure 1. Average impacts of school desegregation by race on White racial attitudes and politics with 95% confidence intervals.

Note: Regression coefficients come from estimation of γ for the model represented by Equation (1), interacted with dichotomous variables indicating the number of years (grouped to improve precision) passed since desegregation. All models include fixed effects for cohort, county, and dataset. Years passed are grouped as such: -2 = -11 to -8 years passed; -1 = -7 to -4; 0 (reference period) = -3 to 0; 1 = 1 to 4; 2 = 5 to 8; 3 = 9 to 12. Number of desegregating counties is 74. Number of comparison counties is 268. Outcomes are rescaled as z-scores for ease of interpretation. Standard errors used to generate confidence intervals clustered at the dataset level.

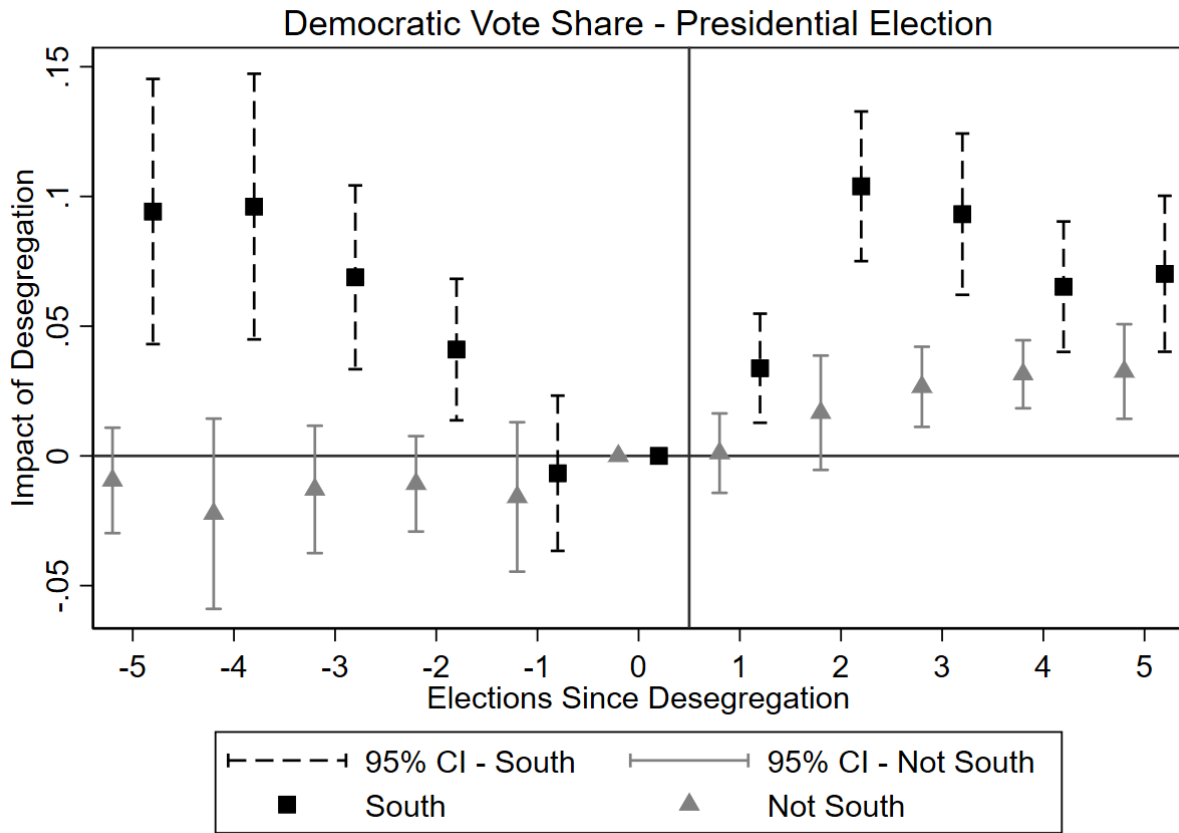


Figure 2. Average impacts of school desegregation by race on county-level proportion of votes won by the Democratic candidate across presidential elections with 95% confidence intervals.

Note: Regression coefficients come from estimation of γ for the model represented by Equation (1), interacted with dichotomous variables indicating the number of elections passed since desegregation. Estimates are weighted by the total county-level vote count. The reference period is the election immediately prior to desegregation. Number of desegregating counties is 73. Number of comparison counties is 268. Data from all counties in the study sample are included except for the Anchorage Municipality in Alaska. Standard errors used to generate confidence intervals clustered at the dataset level.

Table 1. Summary statistics for sample counties and General Social Survey respondents

	Desegregating Sample		Comparison Sample		Difference	
	Mean	SD	Mean	SD		
<i>Panel A. County level</i>						
N	74		266			
Total population in 1950	561013.30	764254.10	79084.20	104284.90	481929.10	***
Proportion White in 1950	0.87	0.10	0.94	0.10	-0.06	***
Proportion unemployed in 1950	0.05	0.02	0.04	0.02	0.01	***
Median income in 1950	3236.11	530.56	2730.70	725.78	505.42	***
Proportion urban in 1950	72.08	12.04	45.73	27.03	26.35	***
Percent population growth 1940 to 1950	39.72	29.26	18.69	26.24	21.03	***
Proportion voted Eisenhower in 1952	0.45	0.07	0.41	0.13	0.04	*
South	0.46	0.50	0.30	0.46	0.15	*
<i>Panel B. General Social Survey respondent level</i>						
N	1126		4077			
Proportion male	0.47	0.50	0.49	0.50	-0.02	
Conservatism composite	-0.02	0.74	0.05	0.70	-0.07	
Positive racial attitudes composite	-0.02	0.28	-0.03	0.25	0.01	
Support protection of racist speech composite	0.04	0.53	-0.03	0.55	0.06	**
Years of education	13.67	2.64	13.05	2.53	0.62	***
Real income (logged)	10.15	1.01	10.01	0.97	0.15	*
Perceptions of class composite	-0.02	0.52	-0.09	0.48	0.07	**

Note: All composite scores are rescaled as z-scores. Two counties are dropped from the county-level summary statistics because they did not exist at the time of data collection: Chesapeake, VA (independent city) and Anchorage, AK (municipality). The significance levels of differences between counties of the desegregating and comparison samples come from two-sample t-tests. The significance of differences between respondents of the desegregating and comparison samples comes from two-sample t-tests where standard errors are adjusted for clustering at the county level. * $p < .1$, ** $p < .05$, *** $p < .01$.

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Table 2. Impacts of school desegregation by race on White individuals' racial attitudes and politics

	Conservatism	Positive racial attitudes	Support protection of racist speech
<i>Panel A. Main effects</i>			
Impact of desegregation	-0.0325 (0.0513)	0.0117 (0.0174)	0.000694 (0.0363)
<i>Panel B. Heterogeneous effects by geography</i>			
Impact of desegregation - not South	0.0377 (0.0591)	-0.00549 (0.0190)	0.000498 (0.0431)
Impact of desegregation - South	-0.180** (0.0727)	0.0475 (0.0302)	0.00120 (0.00120)
Wald test: Not South vs. South, p-value	0.0232	0.143	0.993

Note: Regression coefficients come from estimation of γ for the model represented by Equation (1). Number of desegregating counties is 74. Number of comparison counties is 268. All models include fixed effects for cohort, county, and dataset. Outcomes are rescaled as z -scores for ease of interpretation. The Wald test tests for significant differences in the regression coefficient for the impact of desegregation in the south against the impact of desegregation in non-southern Census regions. Standard errors clustered at the dataset level reported in parentheses. ** $p < .05$.

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Table 3. Robustness tests for the impacts of school desegregation by race on White individuals' racial attitudes and politics

	Conservatism	Positive racial attitudes	Support protection of racist speech
<i>Panel A. Years since desegregation: -7 to 8</i>			
Impact of desegregation - not South	-0.000591 (0.0652)	0.0159 (0.0211)	0.0275 (0.0404)
Impact of desegregation - South	-0.243** (0.0979)	0.0850** (0.0346)	-0.0341 (0.0886)
Wald test: Not South vs. South, p-value	0.0427	0.0930	0.529
<i>Panel B. Years since desegregation: -3 to 4</i>			
Impact of desegregation - not South	-0.107 (0.0764)	0.0476* (0.0281)	-0.0181 (0.0670)
Impact of desegregation - South	-0.393*** (0.141)	0.176*** (0.0471)	0.00607 (0.108)
Wald test: Not South vs. South, p-value	0.0790	0.0225	0.850
<i>Panel C. County-level linear cohort trends</i>			
Impact of desegregation - not South	-0.118 (0.0799)	0.0913*** (0.0318)	0.0376 (0.0684)
Impact of desegregation - South	-0.307** (0.154)	0.128** (0.0547)	-0.151 (0.141)
Wald test: Not South vs. South, p-value	0.280	0.560	0.234
<i>Panel D. Panel A with only comparison counties that desegregate 8+ years later</i>			
Impact of desegregation - not South	0.109 (0.123)	-0.00873 (0.0907)	-0.213* (0.108)
Impact of desegregation - South	-0.138 (0.142)	0.303** (0.124)	-0.0691 (0.179)
Wald test: Not South vs. South, p-value	0.185	0.0435	0.481

Note: Regression coefficients come from estimation of γ for the model represented by Equation (1), interacted with Census region. All models include fixed effects for cohort, county, and dataset. Outcomes are rescaled as z-scores for ease of interpretation. The Wald test tests for significant differences in the regression coefficient for the impact of desegregation in the south

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against the impact of desegregation in non-southern Census regions. Standard errors clustered at the dataset level reported in parentheses. County-level sample sizes are as follows: Panel A, 74 desegregating counties and 267 comparison counties; Panel B, 71 desegregating counties and 260 comparison counties; Panel C, 74 desegregating counties and 268 comparison counties; and Panel D, 74 desegregating counties. $*p < .1$, $**p < .05$, $***p < .01$.

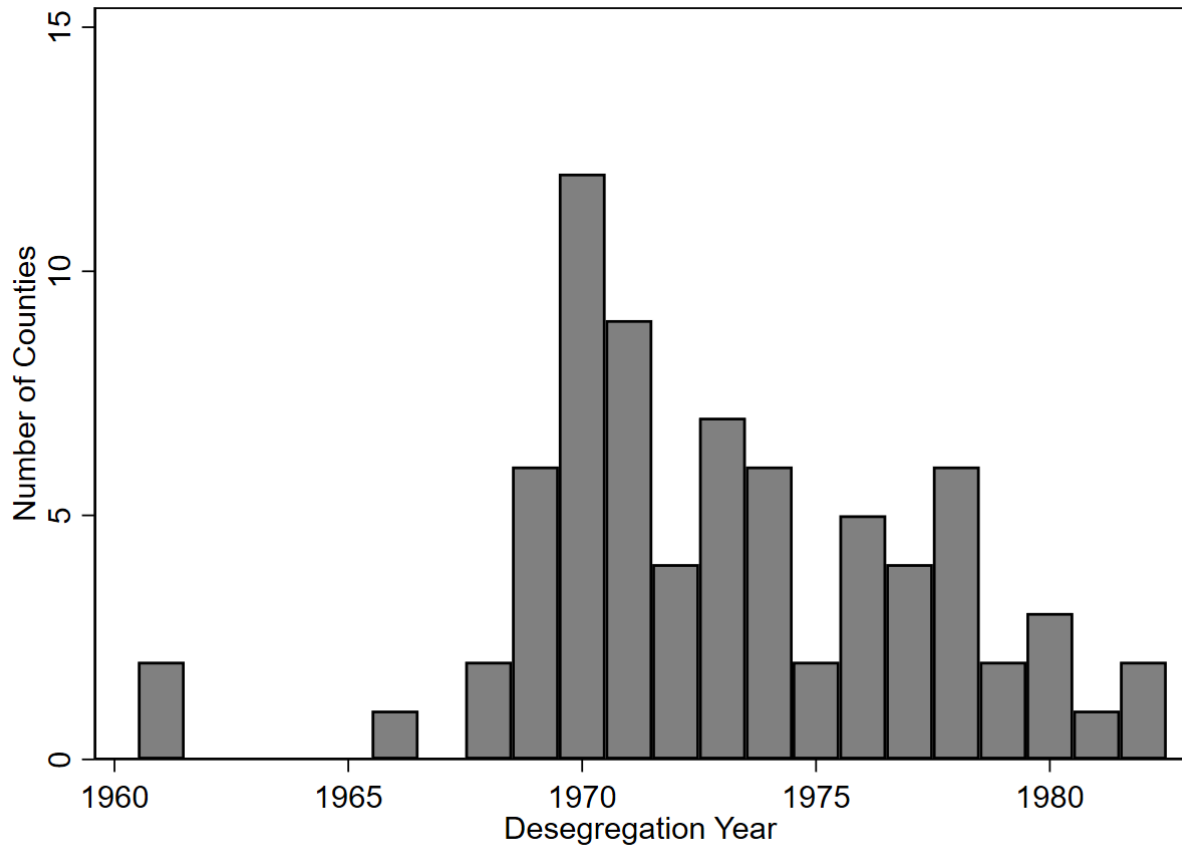
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Table 4. Impacts of school desegregation by race on mechanisms for impacts on White individuals’ racial attitudes and politics

	White-Black Exposure Index	Percent Enrollment - White	Years of education	Real income (logged)	Perceptions of class composite
Impact of desegregation - not South	0.115*** (0.0223)	-0.112*** (0.00978)	-0.434* (0.249)	0.0427 (0.0852)	0.0205 (0.0477)
Impact of desegregation - South	0.144*** (0.0154)	-0.0806*** (0.0127)	0.314 (0.342)	-0.0996 (0.129)	-0.0461 (0.0517)
Wald test: Not South vs. South, p-value	0.287	0.0533	0.0813	0.361	0.346
Analysis level	District	District	Individual	Individual	Individual

Note: Regression coefficients come from estimation of γ for the model represented by Equation (1), interacted with Census region. Number of desegregating counties is 74. Number of comparison counties is 268. For district-level analyses, the following counties are dropped because of missing data: Teller County, CO, and New Castle County, DE. For analyses of real income, Somerset, NJ county is dropped due to missing data. When White-Black Exposure is the outcome, estimates are weighted by total district enrollment of White students. When Percent Enrollment – White is the outcome, estimates are weighted by total district enrollment. All models include fixed effects for cohort, county, and dataset. Perceptions of class composite scores are rescaled as z-scores for ease of interpretation. The Wald test tests for significant differences in the regression coefficient for the impact of desegregation in the south against the impact of desegregation in non-southern Census regions. Standard errors clustered at the dataset level reported in parentheses. * $p < .1$, *** $p < .01$.

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Appendix Figure 1. Discrete histogram of the years of earliest major desegregation plan implementation across sample counties (N=74).

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Appendix Table 1. GSS item summary statistics and text

GSS item	Mean	SD	Range	Total prop. missing	Total prop. MCAR	Item text
<i>Panel A. Conservatism composite</i>						
partyid	3.05	1.97	[0,6]	2.76	2.13	Generally speaking, do you usually think of yourself as a Republican, Democrat, Independent, or what?
polviews	3.18		[0,6]	11.08	7.87	We hear a lot of talk these days about liberals and conservatives. I'm going to show you a seven-point scale on which the political views that people might hold are arranged from extremely liberal--point 1--to extremely conservative--point 7. Where would you place yourself on this scale?
pres?? if??who	0.44		[0,1]	6.54	6.28	In [YEAR], you remember that [NAME] ran for President on the Democratic ticket against [NAME] for the Republicans. Do you remember for sure whether or not you voted in that election? 1. IF VOTED: Did you vote for [NAME] or [NAME]? 2. IF DID NOT VOTE: Who would you have voted for, for President, if you had voted?

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eqwlth 3.08 1.98 [0,6] 41.54 40.76

Some people think that the government in Washington ought to reduce the income differences between the rich and the poor, perhaps by raising the taxes of wealthy families or by giving income assistance to the poor. Others think that the government should not concern itself with reducing this income difference between the rich and the poor. Here is a card with a scale from 1 to 7. Think of a score of 1 as meaning that the government ought to reduce the income differences between rich and poor, and a score of 7 meaning that the government should not concern itself with reducing income differences. What score between 1 and 7 comes closest to the way you feel?

wlthblk-
wlthwht 4.76 1.33 [0,12] 50.10 48.43

Now I have some questions about different groups in our society. I'm going to show you a seven-point scale on which the characteristics of people in a group can be rated. In the first statement a score of 1 means that you think almost all of the people in that group are "rich." A score of 7 means that you think almost everyone in the group are "poor." A score of 4 means you think that the group is not towards one end or another, and of course you may choose any number in between that comes closest to where you think people in the group stand. Blacks relative to Whites?

Panel B. Positive Attitudes Towards Blacks and Policies Supporting Racial Equity Composite

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natrace	1.12	0.70	[0,2]	57.17	52.05	<p>We are faced with many problems in this country, none of which can be solved easily or inexpensively. I'm going to name some of these problems, and for each one I'd like you to name some of these problems, and for each one I'd like you to tell me whether you think we're spending too much money on it, too little money, or about the right amount. Are we spending too much, too little, or about the right amount on Improving the conditions of Blacks?</p>
discaff	1.14	0.70	[0,2]	52.68	50.75	<p>What do you think the chances are these days that a White person won't get a job or promotion while an equally or less qualified Black person gets one instead? Is this very likely, somewhat likely, or not very likely to happen these days?</p>
racliv	0.66		[0,1]	16.55	11.74	<p>Are there any Blacks living in this neighborhood now?</p>
affrm	0.61	0.87	[0,3]	48.87	46.01	<p>Some people say that because of past discrimination, Blacks should be given preference in hiring and promotion. Others say that such preference in hiring and promotion of Blacks is wrong because it discriminates against Whites. What about your opinion -- are you for or against preferential hiring and promotion of Blacks? IF FAVORS: A. Do you favor preference in hiring and promotion strongly or not strongly? IF OPPOSES: B. Do you oppose preference in hiring and promotion strongly or not strongly?</p>

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wrkwayup	1.00	1.16	[0,4]	46.77	46.01	Do you agree strongly, agree somewhat, neither agree nor disagree, disagree somewhat, or disagree strongly with the following statement (HAND CARD TO RESPONDENT): Irish, Italians, Jewish and many other minorities overcame prejudice and worked their way up. Blacks should do the same without special favors.
closeblk-closewht	6.46	2.31	[0,16]	56.93	57.86	In general, how close Do you feel to Blacks (relative to Whites)?
workblk-workwht	5.27	1.42	[0,12]	49.47	47.49	Now I have some questions about different groups in our society. I'm going to show you a seven-point scale on which the characteristics of people in a group can be rated. In the first statement a score of 1 means that you think almost all of the people in that group are "rich." A score of 7 means that you think almost everyone in the group are "poor." A score of 4 means you think that the group is not towards one end or another, and of course you may choose any number In between that comes closest to where you think people in the group stand. B. The second set of characteristics asks if people in the group tend to be hard-working or if they tend to be lazy. Blacks relative to Whites?

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intlblk-intlwht	5.59	1.11	[0,12]	53.65	51.74	<p>Now I have some questions about different groups in our society. I'm going to show you a seven-point scale on which the characteristics of people in a group can be rated. In the first statement a score of 1 means that you think almost all of the people in that group are "rich." A score of 7 means that you think almost everyone in the group are "poor." A score of 4 means you think that the group is not towards one end or another, and of course you may choose any number in between that comes closest to where you think people in the group stand. Do people in these groups tend to be unintelligent or tend to be intelligent? Blacks relative to Whites?</p>
helpblk	1.29	1.16	[0,4]	42.78	40.77	<p>Some people think that (Blacks/Negroes/African-Americans) have been discriminated against for so long that the government has a special obligation to help improve their living standards. Others believe that the government should not be giving special treatment to (Blacks/Negroes/African-Americans). Where would you place yourself on this scale, or haven't you made up your mind on this?</p>
racdifl	0.34		[0,1]	44.14	41.36	<p>On the average (Negroes/Blacks/African-Americans) have worse jobs, income, and housing than White people. Do you think these differences are . . . Mainly due to discrimination?</p>

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racdif4	0.51		[0,1]	44.81	41.36	On the average (Negroes/Blacks/African-Americans) have worse jobs, income, and housing than White people. Do you think these differences are . . . Because most (Negroes/Blacks/African-Americans) just don't have the motivation or will power to pull themselves up out of poverty?
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Panel C. Support Protection of Racist Speech Composite

spkrac	0.64		[0,1]	42.36	41.35	Or consider a person who believes that Blacks are genetically inferior... If such a person wanted to make a speech in your community claiming that Blacks are inferior, should he be allowed to speak, or not?
colrac	0.49		[0,1]	43.31	41.35	Or consider a person who believes that Blacks are genetically inferior.... Should such a person be allowed to teach in a college or university, or not?
librac	0.69		[0,1]	42.91	41.35	Or consider a person who believes that Blacks are genetically inferior. If some people in your community suggested that a book he wrote which said Blacks are inferior should be taken out of your public library, would you favor removing this book, or not?

Panel D. Perceptions of Class Composite

class	2.48	0.67	[1,4]	4.46	3.87	If you were asked to use one of four names for your social class, which would you say you belong in: the lower class, the working class, the middle class, or the upper class?
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satfin	1.93	0.74	[1,3]	12.07	11.73	We are interested in how people are getting along financially these days. So far as you and your family are concerned, would you say that you are pretty well satisfied with your present financial situation, more or less satisfied, or not satisfied at all?
finrela	2.93	0.88	[1,5]	12.73	11.73	Compared with American families in general, would you say your family income is far below average, below average, average, above average, or far above average? (PROBE: Just your best guess.)

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Appendix Table 2. List of school districts desegregating by race in the sample

Desegregation Year	State	County Name	District Name
1970	AL	Jefferson	Jefferson County
1970	AL	Jefferson	Birmingham
1978	AZ	Pima	Tucson
1966	CA	Alameda	Oakland
1969	CA	Contra Costa	Richmond
1978	CA	Fresno	Fresno
1970	CA	Los Angeles	Long Beach
1970	CA	Los Angeles	Los Angeles
1970	CA	Los Angeles	Pasadena
1976	CA	Sacramento	Sacramento
1978	CA	San Bernardino	San Bernardino
1977	CA	San Diego	San Diego
1971	CA	San Francisco	San Francisco
1981	CA	Santa Clara	San Jose
1974	CO	Denver	Denver
1970	CT	Fairfield	Stamford
1978	DE	New Castle	New Castle County
1969	FL	Brevard	Brevard County
1970	FL	Broward	Broward County
1970	FL	Dade	Dade County
1971	FL	Duval	Duval County
1971	FL	Hillsborough	Hillsborough County
1969	FL	Lee	Lee County
1972	FL	Orange	Orange County
1970	FL	Palm Beach	Palm Beach County
1970	FL	Pinellas	Pinellas County
1969	FL	Volusia	Volusia County
1973	GA	De Kalb	Atlanta
1973	GA	Fulton	Atlanta
1982	IL	Cook	Chicago
1971	IN	Allen	Fort Wayne
1973	IN	Marion	Indianapolis
1977	KS	Wyandotte	Kansas City
1975	KY	Jefferson	Jefferson County
1971	LA	Jefferson	Jefferson Parish

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1961	LA	Orleans	New Orleans Parish
1976	MA	Bristol	New Bedford
1974	MA	Hampden	Springfield
1974	MA	Suffolk	Boston
1974	MD	Baltimore City	Baltimore City
1972	MI	Ingham	Lansing
1968	MI	Kent	Grand Rapids
1975	MI	Wayne	Detroit
1974	MN	Hennepin	Minneapolis
1977	MO	Clay	Kansas City
1980	MO	St. Louis City	St. Louis
1970	NC	Gaston	Gaston County
1970	NC	Mecklenburg	Mecklenburg County
1969	NC	New Hanover	New Hanover County
1961	NJ	Essex	Newark
1976	NJ	Hudson	Jersey City
1972	NV	Clark	Clark County
1976	NY	Erie	Buffalo
1970	NY	Monroe	Rochester
1979	OH	Cuyahoga	Cleveland
1979	OH	Franklin	Columbus
1973	OH	Hamilton	Cincinnati
1977	OH	Summit	Akron
1972	OK	Oklahoma	Oklahoma City
1974	OR	Multnomah	Portland
1980	PA	Allegheny	Pittsburgh
1978	PA	Philadelphia	Philadelphia
1970	SC	Charleston	Charleston County
1971	TN	Davidson	Nashville
1973	TN	Shelby	Memphis
1969	TX	Bexar	San Antonio
1971	TX	Dallas	Dallas
1982	TX	Ector	Ector County
1971	TX	Harris	Houston
1973	TX	McLennan	Waco
1973	TX	Tarrant	Fort Worth
1980	TX	Travis	Austin
		Arlington	
1971	VA	County	Arlington County
1970	VA	Norfolk	Norfolk

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1978	WA	King	Seattle
1968	WA	Pierce	Tacoma
1976	WI	Milwaukee	Milwaukee

Note: The list of desegregating school districts and the years of major desegregation plan implementation come from Welch and Light (1987).

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Appendix Table 3. Alternative model specifications for the impacts of school desegregation by race on White individuals' racial attitudes and politics

	Conservatism	Positive racial attitudes	Support protection of racist speech
<i>Panel A. Interacting age 18 cohort with Census region fixed effects</i>			
Impact of desegregation - not South	0.0290 (0.0571)	0.00320 (0.0181)	0.00389 (0.0426)
Impact of desegregation - South	-0.150** (0.0714)	0.0364 (0.0292)	-0.0405 (0.0709)
Wald test: Not South vs. South, p-value	0.0535	0.338	0.593
<i>Panel B. Including survey year fixed effects</i>			
Impact of desegregation - not South	0.0423 (0.0594)	-0.00576 (0.0191)	-0.00272 (0.0437)
Impact of desegregation - South	-0.185** (0.0737)	0.0473 (0.0304)	-0.000908 (0.0665)
Wald test: Not South vs. South, p-value	0.0188	0.144	0.982
<i>Panel C. Using General Social Survey weights</i>			
Impact of desegregation - not South	0.0416 (0.0664)	0.00658 (0.0201)	0.0209 (0.0504)
Impact of desegregation - South	-0.201** (0.0862)	0.0294 (0.0338)	0.0252 (0.0688)
Wald test: Not South vs. South, p-value	0.0289	0.564	0.960

Note: Regression coefficients come from estimation of γ for the model represented by Equation (1), interacted with Census region. All models include fixed effects for cohort, county, and dataset. Outcomes are rescaled as z-scores for ease of interpretation. The Wald test tests for significant differences in the regression coefficient for the impact of desegregation in the south against the impact of desegregation in non-southern Census regions. Standard errors clustered at the dataset level reported in parentheses. For all panels, county-level sample sizes are as follows: 74 desegregating counties and 267 comparison counties. * $p < .1$, ** $p < .05$, *** $p < .01$.

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Appendix A. Factor analyses and estimation of composites using GSS items

From GSS data starting in 1993 through 2018, I identify 19 items (see Appendix Table 1) that both plausibly relate to individuals' racial attitudes and politics and are also administered to a substantial number of survey respondents. However, the number of items and the relatively small size of my analysis sample (see main text) suggests that multiple inference may be an issue in analyses. As such, I use factor analyses to reduce the GSS data on White adults' attitudes and politics into a set of composites.

Before factor analyses, I rescale each individual item as z -scores to place them on comparable scales. I then first conduct exploratory factor analyses (EFA) to identify the number of composites to estimate scores for. In order to address missingness in my data, I employ maximum likelihood with the expectation-maximization (EM) algorithm to estimate the covariance matrix necessary for EFA (Graham, 2009). Three latent factors emerged with eigenvalues above one (Kaiser, 1960). I then use a promax rotation to identify the loading of each of the 19 items onto the three factors. These loadings can be found in Appendix Table A1. The patterns of factor loadings across items suggested three composites capturing individuals' political conservatism, attitudes towards Blacks and policies promoting racial equity, and support for protecting racist speech.

[Insert Appendix Table A1 about here.]

Next, I conduct confirmatory factor analyses (CFA) to determine the extent to which the three-factor structure identified in EFA fit the data. I use the *sem* package in Stata version 17.0 to conduct this CFA. I had each of the 19 items load onto the latent factor for which they had the strongest loading for in EFA. To account for missing data, I estimate the CFA using maximum likelihood with missing variables. In Appendix Table A2 I present the goodness-of-fit statistics

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from this CFA model. These statistics suggested sufficient fit for the three-factor solution (Hu & Bentler, 1999).

[Insert Table A2 about here.]

Finally, to estimate the three composite scores for each White GSS respondent in my sample, I use the *predict, latent* command after estimating the CFA model using the *sem* command, which uses regression scoring. I similarly use this process to arrive at the single composite used in my analyses capturing White adults' perceptions of class.

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Appendix A References

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Appendix Table A1. Factor loadings for three-factor solution from exploratory factor analyses on racial attitudes and politics

	Conservatism	Positive attitudes towards blacks and policies supporting racial equity	Support the protection of racist speech
partyid	-.7470595	.0615827	-.0528343
polviews	-.5789399	-.0899709	-.1179652
discaff	.085754	.316484	.0216477
pres?? If??who	.66682	-.0318458	.0581065
natrace	.2189277	.4209424	-.0694056
eqwlth	.4521691	.0410301	-.1130333
spkrac	.0325117	.033207	.7048334
colrac	.068528	.0021654	.6439528
librac	.0686851	.018859	.5746817
racliv	-.0038024	.1269491	.0301249
affrm	.2454374	.3144949	-.0836352
wrkwayup	.2022196	.5452267	.0217542
closeblk-closewht	-.0853869	.3481929	.0004857
wlthblk-wlthwht	-.2445578	.167634	-.0049636
workblk-workwht	-.2163679	.6268729	.0602189
intlblk-intlwht	-.1983152	.4415256	.0630868
helpblk	.3117067	.4412124	-.0891695
racdif1	.2505645	.3282159	-.0714217
racdif4	.0010794	.5359307	.0700332

Note: Promax rotation.

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Appendix Table A2. Fit statistics for three-factor solution from confirmatory factor analyses on racial attitudes and politics

Fit statistic	Value
RMSEA	0.041
CFI	0.88
TLI	0.86