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

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Abstract

In this paper I study the impact of court-mandated school desegregation, which began in the late 1950s, on White individuals' racial attitudes and politics in adulthood. Using geocoded nationwide data from the General Social Survey, I compare outcomes between respondents living in the same county who were differentially exposed to desegregated schools, based on respondent age and the year of court-mandated integration. With this differences-in-differences approach, I find that exposure to desegregated schools increased White individuals' conservatism and negatively impacted their racial attitudes and support for policies promoting racial equity, such as affirmative action. Heterogeneity analyses indicate that effects are particularly pronounced in counties where opposition to integration was strongest: Southern counties desegregating after the passage of the Civil Rights Act of 1964 and counties where support for the Democratic presidential candidate between the 1960 and 1968 elections substantially decreased. My study provides causal evidence for key tenets of the contact hypothesis, which theorizes that Black-White contact in integrated schools can improve outgroup racial attitudes only under certain conditions, including when this intergroup contact has institutional support.

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

Black and White youth in the U.S. are 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, & Siegel-Hawley, 2012). Research finds that racial segregation harms Black students in particular and may thus be contributing to persistent racial inequality in the country. Youth in schools serving larger shares of Black students experience worse educational outcomes (Billings, Deming, & Rockoff, 2014). Conversely, school integration policies have increased Black students' educational attainment and socioeconomic outcomes in adulthood without negative consequences for White youth (Angrist & Lang, 2004; Bergman, 2018; Guryan, 2004; Johnson, 2011; Weiner, Lutz, & Ludwig, 2009). Several factors can explain these effects (Reardon & Owens, 2014), including changes to Black students' peer groups (Billings et al., 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—something that no other educational policy 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 improves outgroup attitudes and decreases negative outgroup bias—measures that predict Black-White inequalities in educational and socioeconomic outcomes (e.g., Bertrand & Mullainathan, 2004; Charles & Guryan, 2008; Chetty, Hendren, Jones, & Porter, 2020; Chin, Quinn, Dhaliwal, & Lovison, 2020; Riddle & 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* (1954) that *de jure* racial school segregation was unconstitutional (Stephan, 1978).

However, most investigations of the contact hypothesis cannot causally show that contact improves racial attitudes (Paluck, Green, & Green, 2018). Furthermore, these studies typically do not measure the long-term impacts of intergroup contact on outcomes nor do they test Allport's set of conditions necessary for promoting positive outgroup attitudes (i.e., equal status between groups, shared goals, cooperation, and support for contact from authorities, laws, or customs; 1954). Allport hypothesized that, without meeting these baseline conditions, Black-White contact could actually exacerbate existing negative attitudes and bias. In this study, I thus attempt to address the gap in the literature and ask:

1) What is the relationship between White individuals' racial attitudes and politics as adults and the educational and socioeconomic outcomes of Black adults?
2) What is the impact of school desegregation on White individuals' racial attitudes and politics as adults?

To answer these questions, I use geocoded nationwide data from the General Social Survey (GSS). I link White adults' responses to GSS questions related to their racial attitudes and politics to the counties where schools were mandated by court order to racially integrate in the 1950s through the 1980s. Using information on respondents' age and the year of these court orders, 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 the court order) or were not exposed. 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 White adults who were exposed to school desegregation to those who were not exposed, 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).

Correlations show that Black adults attain more education and earn more when they live in counties where White adults: have more liberal politics; possess more positive attitudes towards Blacks and policies promoting racial equity; and are more likely to support protections for racist speech. However, I find that school integration increased Whites' political conservatism as adults, negatively impacted their racial attitudes towards Blacks, and decreased their support for policies promoting racial equity. School integration had no effect on White adults' support for protecting racist speech. These results are robust to several different modeling approaches, including those addressing concerns of bias in difference-in-differences estimates due to variation in treatment timing (Goodman-Bacon, 2018). I uncover little impact heterogeneity of school desegregation by baseline county-level characteristics that might predict differential impacts (i.e., proportion of school-aged children that are White; private school enrollment; initial levels of school racial segregation).

Conversely, increased conservatism and negative impacts on racial attitudes and support for policies promoting racial equity appear particularly concentrated in Southern counties that desegregated after the Civil Rights Act of 1964 was passed. This heterogeneity may be explained by contemporary legal and political shifts which made integration policies particularly unpopular in the South. Supreme Court cases decided in the years after passage of the Civil Rights Act made it more difficult for school districts to employ voluntary school integration policies. Furthermore, the Republican Party around this time began to embrace the "Southern strategy", which engaged White Southerners' racism towards Blacks to win elected political seats. Results from additional investigation support the importance of political context in explaining findings:

impacts of integration are greater in counties where support for the Democratic presidential candidate decreased significantly between the 1960 and 1968 elections.

Schools in my sample counties experienced substantial racial integration after being mandated by courts to do so. Because I do not find strong evidence of county-level White flight or increases in private school enrollment, I ascribe observed effects to integration itself and not to other potential contributors. Relatedly, results do not appear to be driven by individual mobility nor by impacts to White adults' labor market experiences. Like prior studies (Guryan, 2004; Johnson, 2011), I find no differences in income, educational attainment, and perceptions of class between Whites exposed to desegregated schools as youth and those that are not.

My results contribute to several strands of research. First, my study builds on a substantial body of work exploring the impacts of K-12 school racial diversity and policies promoting this diversity. Much of this research focuses on the educational and socioeconomic consequences of integration for Black youth (e.g., Angrist & Lang, 2004; Bergman, 2018; Billings et al., 2014; Card & Rothstein, 2007; Guryan, 2004; Jackson, 2009; Johnson, 2011; Weiner, Lutz, & Ludwig, 2009) and finds positive effects. I instead investigate the attitudinal outcomes of White youth and find effects that suggest potential negative consequences of school integration for racial equity.

A much smaller subset of this literature identifies causal impacts on proxies for racial attitudes as my study does. For example, Merlino, Stenhardt, and Wren-Lewis (2019) find that Whites with more same-gender Black peers in school have more relationships with Blacks as adults and score higher on proxies for positive racial attitudes. Billings, Chyn, and Haggag (2020) find that White students enrolled in schools with more peers from racial minority backgrounds are substantially less likely to be registered as a Republican in adulthood. The

authors highlight prior work demonstrating associations between racial attitudes and partisan identity (Valentino & Sears, 2005). In a study conducted outside of K-12, Boisjoly and colleagues (2006) find that White students randomly assigned to African-American roommates in college are more likely to support affirmative action. Contrastingly, Gordon and Reber (2018) find small, insignificant effects of school desegregation on one particular proxy for improved racial attitudes—mixed-race births. In the discussion, I describe in more detail how my exploration speaks to existing studies with conflicting results.

Second, my results add to the limited body of causal evidence exploring key tenets of the contact hypothesis (Paluck et al., 2018). Though these few studies generally show that contact affects attitudes, they also find that the context matters for how attitudes change (Lowe, 2020; Mousa, 2020). As noted earlier, most research specifically focusing on Black-White intergroup contact is observational. Even fewer studies track attitudinal changes over time. In my investigation, I rely on a credibly causal quasi-experimental design and data collected from White adults several years after exposure to racially integrated schools to overcome the limitations of prior studies. In finding negative impacts of school desegregation on Whites' attitudes towards Blacks and the policies and politics that promote racial equity, I provide suggestive evidence of the importance of Allport's required conditions for successful intergroup contact (1954). He specifically highlights that improved outgroup attitudes will most likely follow from Black-White interaction when social and institutional authorities support this interaction. That school integration was externally mandated (i.e., court-ordered) and faced resistance from both political leaders and the general public may thus ultimately explain my surprising main findings.

Finally, by investigating the impacts of changes to the schooling experiences of White youth on their attitudes and politics, I build on a growing causal literature identifying how different educational interventions influence desired outcomes besides the test scores and labor market success of youth. Some of these studies, for example, look at how reforms like school choice influence students' civic engagement (e.g., McEachin, Lauen, Fuller, & Perera, 2020). The most relevant studies to my work in this strand of research have found that: a school voucher program increased students' altruism towards charitable organizations but not towards their peers (Bettinger & Slonim, 2006); attending preschool leads children to develop more egalitarian views of fairness (Cappelen, List, Samek, & Tungodden, 2020); and that socioeconomic integration in Indian schools makes rich students more prosocial and less likely to discriminate against poor students (Rao, 2019). Like my study on the impact of desegregation on White adults' racial attitudes and politics, these studies specifically identify how changes to experiences in school influence individuals' attitudes and behaviors towards others.

Leaders have long considered the integration of schools by race as one potential solution to stubborn Black-White disparities. Attempts to integrate, however, have always faced substantial legal, political, and social challenges. The successes of historic court-mandated school desegregation were stymied by "White flight", or the substantial outmigration of White families from school districts subject to these orders (Reber, 2005; Welch & Light, 1987). Surveys show that White families strongly oppose the policies most effective at integrating schools (i.e., forced between-school busing; Orfield, 1995), evident in past violent protests against busing (i.e., the Boston busing crisis). The *Miliken v. Bradley* Supreme Court ruling in 1974 established that one of the largest contributors to racial segregation—segregation across district lines—could not be remedied through legislation or policy; 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*, 2007).

Some studies also show that integration may lead to unintended, undesirable consequences for Black students. Bergman (2018), for example, finds that minority students participating in an inter-district integration program in California were more likely to be arrested. In higher education, researchers have compared the outcomes of African-American college students attending historically Black colleges or universities (HBCUs) to those enrolled at predominantly White institutions. Many find more positive academic and social outcomes for those attending HBCUs (e.g., Allen, 1992), potentially because Black students may be less at risk to exposure to racial microaggressions (or even macroaggressions) and stereotype threat in contexts with fewer White students and faculty (e.g., Fries-Britt & Turner, 2002; Steele, 1997). In her review of the research on historic school desegregation, Schofield highlights that Black communities often experienced disproportionate adverse impact in the service of integration (1991): busing primarily involved Black youth, Black schools were often closed, and Black staff and teachers lost jobs.

The results from my study, in combination with the existing set of challenges to (and potential unintended, undesirable consequences of) racial integration, thus suggest that policymakers should be more hesitant in desegregating schools. Policymakers interested in leveraging education to address Black-White disparities might instead opt to implement less controversial reforms such as universal access to early childhood education, which do not necessarily introduce intergroup contact but still improve the outcomes for youth from disadvantaged backgrounds (Heckman, 2006). For those intent on reducing racial isolation in schools, garnering stakeholder support for these efforts should take precedent first and foremost.

I organize the rest of the paper as follows. In Section I, I describe court-mandated school desegregation beginning in the 1950s and the channels through which desegregation may lead to changes in individuals' racial attitudes and politics. In Section II, I outline the data sources and measures used in my analyses. In Section III, I detail the differences-in-differences empirical strategy I use to recover causal effects. In Section IV, I document the effects of school desegregation. Finally, in Section V, I summarize these results and discuss the implications of my findings for future research and for policy.

I. Background

A. School Desegregation after Brown v. Board of Education

In a landmark unanimous decision, the U.S. Supreme Court ruled in *Brown v. Board of Education* (1954) that the *de jure* racial segregation of public schools by districts across the country was unconstitutional. The decision effectively overturned the Supreme Court precedent made decades earlier in *Plessy v. Ferguson* (1896), which allowed for "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 largely ornamental.

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* (1968), 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 & Light, 1987). In *Swann v. Charlotte-Mecklenburg Board of Education* (1971), 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* (1973), 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 legislative action 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, implementation was nearly immediate. Notably, though the timing of a district's effort towards desegregation following a court decree may have depended on confounding factors such as resistance to integration (especially prior to 1965), when and how challenges to school admissions policies emerged appears more idiosyncratic. This allows for plausible identification of the causal impacts of school desegregation by leveraging variation in the timing of initial court orders (Johnson, 2011).

Finally, a series of more recent court decisions have made it easier for schools to remain racially isolated. In *Miliken v. Bradley* (1974), the U.S. Supreme Court essentially determined 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 voluntary efforts to remedy racial isolation even within school districts facing judicial scrutiny (*Parents Involved in Community Schools v. Seattle School District No. 1*, 2007), the demographic shifts in schools following the considerable integrative efforts beginning in the 1950s appear by some measures to have receded (Reardon & Owens, 2014).

B. School Desegregation and Racial Attitudes and Politics

Many scholars have tested how desegregation affects the educational and socioeconomic outcomes of Black youth (for a review, see Reardon & Owens, 2014). Reardon and Owens (2014) presents a stylized model arguing that, by changing the distribution and overall level of resources in schools serving Black students, integration can improve racial equity. But as Stephan (1978) highlights, increasing Black students' academic achievement was just one argument used by the Supreme Court to support their decision in *Brown v. Board* (1954). Another key motivating factor for reducing racial isolation was reducing outgroup racial prejudice by both Whites and Blacks. However, Schofield (1991) describes that much of the research in the decades following *Brown v. Board* investigating this additional goal of integration could not establish causality, covered a constrained geographic scale, and suffered from weak measurement instruments. These limitations likely contribute to the literature's overall inconclusiveness on desegregation's impact on racial attitudes.

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

increase in integrated school settings—especially in a society where deeply entrenched residential segregation 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 racially segregated schools of the 1950s, 1960s, and 1970s met these conditions is debatable (see Gerard, 1983), and variation in the capacity of school leaders and teachers to establish classrooms conducive to successful intergroup racial interaction may also explain the inconclusive literature.

It is worth noting that other explanations exist besides those put forth by contact theory 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 & Light, 1987) or increases in private school enrollment could have limited actual contact between Black-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 (as few studies do), mobility may bias estimates from the surveyed sample (e.g., Gordon & Reber, 2018; Shen, 2018). Finally, experiences in adulthood may counteract changes in attitudes resulting from reduced racial isolation. For example, prior research has shown that desegregation improved the educational and socioeconomic outcomes of Black youth (Guryan, 2004; Johnson, 2011). This may have led White adults who attended desegregated schools to develop negative outgroup attitudes due to increased labor market competition.

In summary, many explanations can account for why research on historic school desegregation's impact on racial attitudes is mixed. Early empirical studies were limited

methodologically, but theory also suggests that positive, negative, and null effects are possible. Below I describe my efforts to address the methodological concerns of prior work and test the numerous channels outlined above.

II. Data

A. Data and Measures Overview

I use data from three primary nationwide sources in my analyses identifying the impact of desegregation on attitudes and politics and assessing the extent to which these measures predict the outcomes of Black adults. Outcome data for Black adults come from the first main source, the American Community Survey (ACS). I specifically use county-level estimates of educational attainment and median household income from the 2005-2009 5-year ACS.

B. School Desegregation Case Data

The second main dataset I use in analyses comes from the American Communities Project at Brown University. This dataset contains information on every district ever subject to a court mandate to desegregate schools from the 1950s through the 1980s. Using this data, I identify for each district every litigation related to the segregation of schools by race, whether litigations resulted in a mandate to integrate, and the year each court case was decided.

Following *Brown v. Board,* districts could face multiple litigations related to school segregation (if, for example, districts implemented desegregation plans too slowly or not at all). Furthermore, with some exceptions (particularly in the U.S. South), school district boundaries are not typically coterminous with county boundaries. Because my primary unit of analysis is the county, I aggregate the desegregation court case data from the case-district level to the county level. I specifically identify across cases and districts, the year of each county's earliest court case mandating school integration. My empirical strategy (described in more detail below) for

evaluating the impacts of school desegregation depends on plausible exogeneity of individuals' exposure to desegregated schools. By focusing on each county's earliest observed litigation, I avoid ascribing effects to subsequent court decisions within counties that are potentially endogenous to initial rulings (see also Shen, 2018). Of the 594 counties with districts ever under court order to integrate, 9% of them were subject to multiple mandates across years, with a median difference for these counties of four years between the first and last order to desegregate.

C. Data on White Adults' Racial Attitudes and Politics

The final key data source I leverage is the General Social Survey (GSS), a nationwide survey of adults first administered in 1972 by the National Opinion Research Center at the University of Chicago. Since 1994, the survey has been conducted in every even-numbered year. I focus on GSS responses for surveys conducted between the years 1993 and 2018, when restricted-use data specifically contain geographic information on respondents' county of residence. The items I use from these surveys fall into three primary categories: measures of respondents' background, measures of respondents' socioeconomic outcomes, and measures of respondents' racial attitudes and politics.

Respondents' background data include their age, their race, their current county of residence, whether or not 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 White individuals who lived in a county that underwent school desegregation and, using survey year and age, whether or not the respondent was of plausible school age (i.e., 17 years old or younger) when the county's earliest court mandate to integrate was put into place.

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 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. Indeed, prior research shows that integration improves the educational and socioeconomic outcomes for Black youth (Guryan, 2004; Johnson, 2011), which would make labor markets more competitive. For educational attainment I use respondents' number of years of schools completed and for earnings I use reported family income (in 1986 dollars). Finally, to measure 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 this composite score; Appendix Table 1 provides summary statistics for these three GSS items.

Finally, the primary outcomes in my analyses are respondents' answers to questions regarding their racial attitudes and politics. 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 liberalness of respondents' politics (e.g., identification as a Democrat), their attitudes towards Blacks and policies promoting racial equity (e.g., feeling 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 scores for White GSS respondents. 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. Also in Appendix Table 1, I detail 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' liberalness 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.

While I use respondent-level GSS composite scores when investigating the impact of school desegregation on White individuals' racial attitudes and politics as adults, I also use scores collapsed to the county level in other analyses. To arrive at aggregated scores, I estimate the following multilevel model:

$$Y_{ijt} = \beta_t + \alpha_j + \varepsilon_{ijt} \quad (1)$$

Where Y_{ij} captures scores on one of the composites, rescaled as a z-score, for White adult i living in county j who was administered the GSS survey in year t. I include in this model fixed effects for survey year, β_t, to account for potential differences in outcomes over the range of years of GSS data I employ. I use from this model the predicted random effects for each county, $\widehat{\alpha_j}$, in correlational analyses with Black adults' county-level outcomes from the ACS. These random effects are shrunken using empirical Bayes to the mean in counties with fewer White GSS respondents to account for more uncertainty.

D. Other Data Sources

I leverage data from several other minor sources in analyses. Data from the 1950 Decennial Census provide county-level detail on demographics and socioeconomic outcomes. I supplement this baseline 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).

Finally, for heterogeneity analyses and to begin exploring mechanisms behind any observed effects, I use county-level data from the 1970, 1980, and 1990 Decennial Censuses, county-level information on U.S. presidential voting in the 1960 and 1968 elections (Leip, 2016), and district-level data again from the American Communities Project. With Census data, I track county-level baseline and changes in the proportion of school-aged (i.e., five to 14) children that are White, and changes in the proportion of individuals enrolled in private school.[1] With the presidential election data, I measure county-level changes in vote share for the Democratic

[1] The available county-level data on private school enrollment from the Decennial Census varies decade to decade. No county-level data is available in 1950. In 1960, the Census tracks the "Percentage of Children in Elementary School Attending Private School". With 1970 data, I can compute the percentage of the population aged *three to 34* enrolled in private school from grades one through eight. With 1980 data, I can compute the percentage of the population *three years and over* enrolled in private school from grades one through eight. With 1990 data, I can only compute the percentage of the population *three years and over* enrolled in private school in *elementary or high school*. Finally, with 2000 data, I can compute the percentage of the population three years and over enrolled in private school in grades one through eight.

presidential candidate over a period of time when many voters in the U.S. South switched political party allegiance due to antagonism with the Democratic party's embrace of the Civil Rights Movement. With the American Communities Project data, I track county-level baseline and changes in districts' school desegregation.[2]

E. Sample

My main analytic sample includes 159 U.S. counties where districts were ever under court mandate to integrate and the 10,987 White GSS respondents living in these counties. To arrive at this sample, I exclude respondents from counties where districts were never under court mandate to integrate. I make this exclusion because counties that either voluntarily desegregated schools or whose school admission policies were never deemed racially discriminatory are likely to be substantially different from those where courts determined racial integration was a necessary legal remedy. However, I was also forced to exclude 435 counties where racial integration was deemed necessary because no White GSS respondents lived in these counties.

In Table 1, I provide summary statistics on the 159 in-sample and 435 out-of-sample counties ever under court mandate to desegregate schools.

[Insert Table 1 about here.]

[2] As is described in Logan, Zhang, and Oakley (2017), the district-level data from the American Communities Project itself is collated from several sources. The earliest time period for this data on elementary schools spans the 1969 and 1972 school years and was initially collected by the Office of Civil Rights. Over 80% of this district data come from the 1968-1969 school year specifically. American Communities Project information on district-level school segregation between the 1980 and 1982 school years again come from the Office of Civil Rights. Finally, data from the National Center for Education Statistics Common Core of Data provide school segregation data in 1989-1990 and 1999-2000. Due to how the Office of Civil Rights sampled districts for data collection, not all school districts have information in earlier years. 99 of the 159 counties that are ever under court mandate to desegregate in my sample (described next) have an earliest case year for integration after 1968 (i.e., when the earliest school segregation data is available). Of these 99 counties, 75 of them have school segregation data for all districts with court mandates to desegregate for at least one time point before and after the county's earliest case year. 16 of the 99 counties have districts who are all missing some school segregation data. Thus, for the 83 remaining counties, to arrive at measures of county-level school segregation I collapse data from the American Communities Project on district-level dissimilarity indices (one commonly used measure of racial segregation) for each time period.

From the table, I conclude that included and excluded observations vary significantly from one another. In-sample counties are more populated, Whiter, more urban, more Democratic, and have marginally better socioeconomic outcomes. Some of these differences are unsurprising; a number of urban, highly populated areas are included with certainty in the GSS survey sample. Notably, though the 159 counties I analyze only account for 5% of counties in the entire country as of the 1950 Decennial Census, they account for 33% of non-White youth aged 5 to 14 in the entire country, and 50% of this population among counties that desegregated. Still, the dissimilarity between the in-sample and out-of-sample counties suggests that my results may not generalize beyond the observations in immediate consideration.

III. Empirical Strategy

I use differences-in-differences (DID) to identify the causal effect of school desegregation on White individuals' racial attitudes and politics as adults. For the first difference, I compare outcomes between those living in the same county but, based on age and time of initial court mandate, experience credibly exogenous differences in exposure to integration. Specifically, I identify those who turn 18 after the year of his or her county's first mandate 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 outcomes between those who turned 18 in the same year but who live in different counties that also underwent court-mandated school integration, but in a different year. The first difference thus accounts for persistent contextual differences in racial attitudes and politics among White adults, whereas the second difference accounts for contemporaneous shifts in outcomes over time across age cohorts.

To operationalize this DID, I estimate variants of the following model:

$$Y_{ijt} = \alpha Exposed_i + \delta_j + \gamma_t + \varepsilon_{ij} \quad (2)$$

Where Y_{ijt} 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 turned age 18 in year t and who lives in county j. δ_j and γ_t are fixed effects for county and age 18 cohort, respectively. $Exposed_i$ is a dummy variable that indicates if the respondent turned 18 after the year of his or her county's first court mandate to desegregate schools. α thus captures the parameter of interest: the impact of integration on outcomes, after accounting for contextual and age cohort trends. Standard errors are clustered at the county level.

Many recent papers have investigated the properties of DID models where observations are treated at different times—the approach I employ in my study. Most notably, Goodman-Bacon (2018) highlights that, unlike estimates recovered from the canonical DID model where there is a single pre-period and a single post-period, estimates from DID models with multiple treatment times may be biased if treatment effects change monotonically over time across treated groups. To account for this, I estimate several variations of my primary DID model represented by equation (2) above to test the robustness of my findings.

First, I use an event study to directly identify treatment effect changes by time exposed to desegregated schools using the same data. Representing this event study is the following model:

$$Y_{ijt} = \sum_y \beta_y D_i 1(t - T_j^* = y) + \delta_j + \gamma_t + \varepsilon_{ij} \qquad (3)$$

Which replicates the model seen in equation (2) except that I replace the indicator variable $Exposed_i$ with a vector of indicator variables that summarize how many years y remained before GSS respondent i turned 18 and the year of his or her county j's first court mandate to integrate, T_j^*. Those turning 18 after this year ($y \geq 1$) are again considered exposed to desegregation. To improve precision, in place of individual indicator variables for each value of y, D_i, I estimate equation (3) using year groups, D_i^g, where D_i^g is equal to one if individual i is

in year group g, where g is a category for $y \leq -12$, $-11 \leq y \leq -9$, $-8 \leq y \leq -6$, $-5 \leq y \leq -3$, $1 \leq y \leq 3$, $4 \leq y \leq 6$, $7 \leq y \leq 9$, $10 \leq y \leq 12$, and $y \geq 13$ ($-2 \leq y \leq 0$ is omitted). Standard errors are clustered at the county level.

As another robustness check on results estimated from my primary DID model depicted in equation (2), I reshape my analytic dataset and create a set of control observations for each of the 159 "treated" counties in my sample. These control observations include counties that, at the time of each treated county's initial court mandate to desegregate, had not yet integrated or never do. This restriction reduces concerns raised by Goodman-Bacon (2018) about treatment effect changes biasing DID estimates because earlier observations that are treated are never considered in comparisons for later treated observations.

Following Deshpande and Li (2019), I first create unique datasets for each of the 159 counties in my analytic sample. In each dataset, only one county is considered the treated group. Of the remaining 158 counties, those who have already integrated before the treated county's initial court order for desegregation are dropped. Counties whose initial court orders occur between zero and six years immediately after the treated county's initial order are also dropped. Finally, only White GSS respondents who turned 18 between eight years before to six years after the year of the treated county's initial order are kept in the dataset. By making these exclusions, I necessarily estimate impacts on racial attitudes and politics for only those exposed for a short period of time—up to six years. To estimate impacts resulting from longer exposure, I would need to further shrink the control group and exclude more counties (i.e., those with initial court orders even further in the future), decreasing precision. On the other hand, making these exclusions ensures that comparison groups for treated observations do not also experience school integration.

I then append all 159 datasets into a single dataset and estimate variations of the following model, based on Deshpande and Li (2019):

$$Y_{ijkt} = \alpha Treated_{jk} + \sum_y \beta_y D_i 1(t - T^*_{jk} = y) + \sum_y \psi_y (D_i 1\big(t - T^*_{jk} = y\big) \times Treated_{jk}) + \delta_j + \gamma_t + \phi_k + \varepsilon_{ijkt} \quad (4)$$

Where Y_{ijkt} again captures the racial attitudes or politics of White GSS respondent i who turned 18 in year t and who lives in county j. In addition to fixed effects for county δ_j and age 18 cohort γ_t, also included in the model are fixed effects for dataset for treated county k, ϕ_k where $1 \leq k \leq 159$. Like the event study I estimate represented by equation (3), I include a vector of indicator variables that summarize how many years y remained before GSS respondent i turned 18 and the year of the first court mandate to integrate, but specifically for the treated county k, T^*_j. Those turning 18 after this year ($y \geq 1$) are again considered exposed to desegregation. In place of individual indicator variables for each value of y, D_i, I estimate equation (4) using year groups, D_i^g, where D_i^g is equal to one if individual i is in year group g, where g is a category for $-8 \leq y \leq -6$, $-5 \leq y \leq -3$, $1 \leq y \leq 3$, and $4 \leq y \leq 6$ ($-2 \leq y \leq 0$ is omitted). Finally, I include a main effect for being the treated county in the dataset, $Treated_{jk}$, and the interaction between this effect and individuals' year groups. These interactions are the parameters of interest.

As one final robustness check, I again create unique datasets for each of the 159 counties in my analytic sample where just one county is considered treated. But instead of considering other counties who have ever been under court mandate to integrate as comparison groups, I flag as control observations the 45 counties with GSS respondents and school districts that went through litigation for segregation but ultimately were not mandated by courts to integrate. Again, only White GSS respondents who turned 18 between eight years before and six years after the

year of the treated county's initial order are kept in the dataset. After appending all 159 of these datasets into a single dataset, I re-estimate the event study model represented by equation (4) to recover causal impact estimates of desegregation on White adults' racial attitudes and politics.

IV. Results

A. White Adults' Racial Attitudes and Politics and Black Adults' Outcomes

I find that Black adults experience more positive educational and socioeconomic outcomes in counties where White adults: have more positive racial attitudes towards Blacks and support policies promoting racial equity; report being more politically liberal; and express more support for protecting racist speech. I show these relationships in Figure 1.

[Insert Figure 1 about here.]

In the figure, I plot county-level estimates of the three composites (rescaled as *z*-scores), recovered after estimating equation (1), against the proportion of Black individuals 25 years or older with bachelor's degrees and the income of the median Black household across counties. I also provide the correlation coefficient for the bivariate relationship depicted in each plot. I find moderate correlations, ranging from .16 to .38, across outcomes and predictors. Though not the sole motivator for my investigation of the impact of desegregation on racial attitudes and politics for White adults, the observed relationships seen here in Figure 1 provide some descriptive evidence that my particular measures matter for racial equity.

B. Impacts of Desegregation on White Adults' Racial Attitudes and Politics

In Table 2, I present impact estimates of desegregation on the composites (rescaled as *z*-scores) capturing White adults' racial attitudes and politics.

[Insert Table 2 about here.]

All estimates come from variations of the model represented by equation (2). In column (1) I display my preferred estimates which highlight that exposure to desegregated schools increases individuals' conservatism, negatively impacts their racial attitudes and support for policies promoting racial equity, and has no effect on their support for protecting racist speech.

My preferred model controls for cohort fixed effects which, as described above, account for contemporaneous trends in outcomes across birth cohorts. The two other sets of fixed effects in my preferred model require additional explanation as they are not typically included in DID models. First, I control for county-by-GSS sample fixed effects. County fixed effects by themselves are standard, and account for time invariant differences in outcomes across contexts. But I further interact county with indicator variables that identify the sample frame of the GSS survey administration each individual respondent was a part of. Including these interactions ensures that comparisons in outcomes are made only between those in the same county who are sampled for the survey using the same procedures. I also include age fixed effects in the model represented by equation (2). Because the GSS is not administered to individuals of a particular age, I use these to account for potential differences in outcomes at various points of adulthood.

However, in Table 2, I show that impact estimates of desegregation on White adults' racial attitudes and politics are robust to different modeling decisions. Under column (2), I show results using only standard DID fixed effects—those for county and cohort. Under column (3), I report estimates using my controls from my preferred model, but weight respondents by their individual weights provided by the GSS that account for the sampling design of the survey. For estimates shown in column (4), I add context-specific trends to my preferred model to address potential linear shifts in outcomes over time within counties. Finally, under column (5), I present results that allow cohort-specific effects to vary by U.S. Census region. Across models,

conclusions are the same: desegregation increases conservatism and negatively impacts racial attitudes and support for policies promoting racial equity. Notably, I observe fairly little movement in the magnitudes and the statistical significance of these effects across the columns. In all cases, absolute impacts on conservatism and racial attitudes and support for equity policies are substantial: between .06 to .11 SDs.

Goodman-Bacon (2018) investigates properties of the DID approach I employ to identify impacts of school desegregation and suggests that my estimates shown in Table 2 may be biased if treatment effects of integration vary within counties over time. To assuage concerns of this bias, I estimate different event study models and show the robustness of my main DID results across models. First, in Figure 2, I plot estimates from the model represented by equation (3). Specifically, I plot desegregation's impact on outcomes for individuals grouped by how much exposure they had to integrated schools. Time of exposure is based on the year that individuals turned 18 relative to the year of their county's earliest court case mandating desegregation. In the event study model whose results I plot, I include the fixed effects from my preferred DID equation: cohort fixed effects, county-by-GSS sample fixed effects, and age fixed effects.

[Insert Figure 2 about here.]

From Figure 2, two patterns are worth highlighting. First, I do not find strong evidence of differential pre-treatment trends in outcomes, though estimates are noisy. Individuals from different counties but who are born in the same cohort do not have observably divergent predicted outcomes (i.e., the plotted impact of desegregation approximates zero) if the year that they turn age 18 occurs at least three years before their county's first court case mandating integration (i.e., time period is negative). On the other hand, individuals from different counties but who are born in the same cohort do have observably divergent predicted outcomes if the year

that they turn age 18 occurs after integration (i.e., time period is positive). Notably, these effects appear fairly consistent over time. Those exposed to desegregated schools for longer periods of time demonstrate similar outcome levels as those who are exposed for shorter periods of time. The relatively low variance in event study estimates helps assuage concerns that changing effects over exposure time may be biasing the main DID estimates shown in Table 2 (Goodman-Bacon, 2018). I display the estimates plotted in Figure 2 in Appendix Table 2.

Figures 3 through 6 further support the robustness of my main results in Table 1 to potential biasing factors of DID models leveraging differential timing of treatment across observations to identify causal impacts.

[Insert Figure 3 about here.]

[Insert Figure 4 about here.]

[Insert Figure 5 about here.]

[Insert Figure 6 about here.]

In Figures 3 and 4, I plot raw averages of White adults' racial attitudes and politics by time of exposure to desegregated schools. I further differentiate outcomes by "treatment" or "control" group status, which are determined as described above in the Empirical Strategy section. From these two figures, I find additional evidence of no pre-treatment differences in outcome trends between treated and control individuals. This is true both for when I construct control groups for each county ever under court mandate to desegregate using: counties that are also ever under orders to integrate, but only in the future (Figure 3); or when the constructed control group includes the 45 counties with GSS respondents that underwent litigation for their school assignment policies but were not ultimately mandated by courts to desegregate (Figure 4).

In addition to no pre-treatment differences in trends for raw average outcomes, I again also observe divergent trends in outcomes post-treatment between treatment and control counties in both Figures 3 and 4. In particular, for White adults in treatment counties exposed between one to three years to desegregated schools (i.e., time period one), average conservatism is much higher than observed conservatism for those in cohorts at time period zero. For the comparable individuals in control counties, I observe no raw difference in conservatism. On the other hand, in terms of racial attitudes and support for policies promoting racial equity, White adults in treatment counties exposed between one to three years to desegregated schools (i.e., time period one) demonstrate lower outcomes compared to those in cohorts at time period zero. For comparable individuals in control counties, I observe the opposite—steady or slightly more positive outcomes for those in post-treatment cohorts.

In Figures 5 and 6, I plot the formal event study treatment effects from the model represented by equation (4). The estimates themselves can be found in Appendix Table 2. Estimates represented in the figures generally mirror those found in Figure 2: no strong evidence of differential pre-treatment trends, with strong observed impacts of desegregation on White adults' conservatism and racial attitudes and support for racial equity policies—in particular for cohorts of individuals with the shortest exposure time to desegregated schools (i.e., between one to three years of exposure, based on the year they turn 18 relative to the year of their county's first court mandate to integrate).[3]

C. Impact Heterogeneity

[3] Though Figures 5 and 6 suggest that effects of desegregation attenuate in time period two, I observe a similar pattern in the main event study estimates plotted in Figure 2. However, in Figure 2, in which I can leverage a longer panel of data (see Empirical Strategy), integration appears to have an impact on conservatism and racial attitudes/racial equity policies over exposure time.

I next investigate the extent to which estimates from my preferred DID model represented in Table 2 column (1) vary across contexts or time. As I detail next, there are several theoretical reasons to expect variation in the effect of integration on White adults' racial attitudes and politics. Impact heterogeneity may also help uncover mechanisms explaining the main results showing that exposure to school desegregation increases White individuals' conservatism and negatively impacts their racial attitudes and support for policies promoting racial equity.

I perform six tests of heterogeneity. For the first three tests, I explore whether results differ by baseline characteristics of counties that potentially predict differential impact. First, I test whether results vary by counties' baseline proportion of school-aged individuals that are White. Next, I test whether results vary by counties' baseline level of school segregation. These tests explore the possibility that the impact of school desegregation may be magnified in contexts where initial intergroup contact was lower (i.e., due to the particularly high representation overall of White youth in school systems or due to greater levels of school segregation). Third, I explore whether counties with higher baseline private school enrollment experience weaker effects of integration. I posit that contexts with higher baseline private school enrollment have more capacity to absorb youth from White families opting out of the public school system following court mandates to racially integrate. This would predict mitigated impact estimates.

For the final three tests of impact heterogeneity, I investigate the importance of the geography of counties and the timing of their earliest court case mandating desegregation. I explore whether results vary for counties located in the U.S. South versus for those in other parts of the country. School districts in the U.S. South are unique in that most court cases mandating their racial desegregation focused on remedying existing *de jure* segregation; districts outside of the U.S. South were also racially segregated, but usually the racial isolation of students was less

intense and resulted from *de facto* segregation. Next, I explore whether results across counties vary by when each experienced its first court case mandating school desegregation. As stated earlier, the nature of school desegregation following these orders varied over time; districts could be more (or less) proactive in efforts to integrate due to the existing legislative and judicial context. Finally, I look for heterogeneity by geography and time together. Specifically, I compare impacts of desegregation between: Southern counties who desegregated before passage of the Civil Rights Act of 1964 (when they were not yet legally bound to implement involuntary integration plans and when the Republican Party embrace of the Southern Strategy had not yet fully taken root); Southern counties who desegregated after passage of the Civil Rights Act; and non-Southern counties. I do not distinguish between non-Southern counties who desegregate before and after the Civil Rights Act because nearly all do so after its passage.

In Tables 3 and 4, I show the results of these six investigations of impact heterogeneity.

[Insert Table 3 about here.]

[Insert Table 4 about here.]

When considering baseline county characteristics (Table 3), I find surprisingly little evidence of impact heterogeneity. Effects do not appear to vary strongly by the racial composition of youth across counties, by private school enrollment, or by initial school segregation. Results similarly do not appear to vary substantially by geography (Table 4). I do observe weaker effects of desegregation on White adults' conservatism in counties that desegregated earlier (i.e., before 1965, the first year following passage of the Civil Rights Act), but I do not find parallel patterns by court case timing for my outcome capturing racial attitudes or support for policies promoting racial equity.

However, I do observe that effects of desegregation are most pronounced for White individuals living in the South whose counties desegregated after the Civil Rights Act. Conversely, effects are weakest for individuals living in the South whose counties desegregated before passage of the law. Because of the noisiness of estimates, I cannot convincingly establish that differences overall are statistically significant. However, this pattern—where White adults exposed to racial school segregation in contexts most likely to oppose integration demonstrate more conservatism in politics and greater negative impacts on racial attitudes and support for policies promoting racial equity—does converge with theory. To further test this theory, I investigate whether estimates vary by support for the Civil Rights Movement, leveraging as a proxy the county-level changes between the 1960 and 1968 elections in vote share for the Democratic presidential candidate.[4] As seen in Table 5, counties with the least support for this movement experienced the most negative effects of school segregation. I highlight the significance of these particular findings of impact heterogeneity in more detail in the discussion.[5]

[Insert Table 5 about here.]

D. Other Mechanisms for Impacts

Given the lack of strong evidence for impact heterogeneity besides the joint importance of geography and time and other proxies for opposition to integration, I investigate other potential mechanisms that may explain my findings. First, I explore whether the data show that

[4] In results available on request, I find that county-level opposition for federal involvement in school integration, measured using data from the 1956, 1958, and 1960 American National Election Studies (ANES) surveys, explains approximately 30% of the variance in county-level changes in the Democratic presidential candidate vote share between 1960 and 1968. I do not use these data in analyses because just under 100 counties are represented in the ANES survey over these years, with only about 30 counties also appearing in my GSS county sample.

[5] Based on data availability, not all counties could be included in every test of impact heterogeneity (e.g., county-level private enrollment data begin in the 1960 Decennial Census; I thus exclude counties whose earliest court mandate occurred before 1960). Similarly, because of missing data, not all GSS respondents could be included in every test of mechanisms (described in more detail in the next section). In Appendix Table 3, I provide impact estimates for my main DID model when I exclude counties and individuals due to these data limitation. Main results are robust to all exclusions.

school desegregation following court mandate actually led to meaningful increases in Black-White contact in schools. In Figure 7, I provide evidence suggesting that intergroup contact did increase.

[Insert Figure 7 about here.]

In the figure, I plot the distributions of county-level changes in: school-level segregation (the dissimilarity index); proportion of school-aged children that are White; and proportion of students enrolled in private school. To calculate changes, I take the year of each county's earliest court mandate to racially integrate schools and subtract the closest available values for each of these three outcomes before and after this year.

From Figure 7, I show that schools in my sample counties experienced substantial racial integration following court mandates. The average county experienced a decrease on the dissimilarity index (which ranges from zero to 100, with 100 capturing complete segregation and zero complete integration) of approximately 37. Conversely, counties in my sample experienced very little change in the composition of students in public schools. The average county experienced almost no change to the proportion of school aged youth that were White, nor did it experience any change to the proportion of students enrolled in private schools. These findings together suggest that White flight from counties undergoing court-mandated racial integration do not contribute to explaining my main results.

Finally, in Table 6, I show results from two other investigations of potential mechanisms for my main effects of desegregation. First, I explore whether or not White adults exposed to desegregation experience worse educational or socioeconomic outcomes (columns [1] through [3]). To estimate these effects, I leverage the same DID model I use for my preferred results looking at impacts on racial attitudes and politics. As stated earlier, changes to these outcomes

for White individuals may not stem from experiences in integrated schools, but from their experiences as adults on the labor market. Specifically, school integration improved the educational and socioeconomic outcomes of Black youth, which would have increased the competition White individuals faced on the labor market. Yet, as seen in the table, I find no evidence that White adults attained less education, earned less income, or even had worse perceptions of their class if they were exposed to desegregated schools.

[Insert Table 6 about here.]

Also in Table 6 I explore whether or not results differ by the mobility of GSS respondents. Prior work (e.g., Gordon & Reber, 2018; Shen, 2018) looking at the impacts of historic desegregation on adulthood outcomes using datasets that do not track individuals longitudinally have had to grapple with the possibility of bias in estimates due to mobility in and out of integrating school districts. The GSS data allows me to partially address this concern. Specifically, I use information on whether White adult GSS respondents report living in the same city at the age of 16 (i.e., still during school age) to investigate whether or not my main results hold for this population, for which I can more reasonably assume was impacted by school integration. In Table 6, I show that my main results replicate for those reporting no mobility: desegregation for this group continues to significantly increase conservatism and negatively impact racial attitudes and support for policies promoting racial equity (columns [5] through [7]). Individuals who were exposed to desegregation were also not significantly more or less likely to report living in the same city at age 16 (column [4]).

V. Discussion

The court-mandated desegregation of schools by race starting in the 1950s left an indelible impact on education in the U.S. Research yields strong evidence that these changes

significantly improved the life outcomes of Black youth across the country (Guryan, 2004; Johnson, 2011). But stubborn resistance to integration over the decades, primarily by White families, 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 positive effects on the educational and socioeconomic outcomes of Black youth, this interest is unsurprising. However, research also shows the power of other educational interventions to have these same impacts. Thus, without additional evidence of its contributions to racial equity, it may be more politically (and legally) feasible to pursue these other programs.

Integration advocates have long cited the potential for desegregation 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 youth in the U.S. However, limited causal evidence exists that supports advocates' views.

In this study, I address this limitation of existing research and find evidence that historic court-mandated school integration by race did not improve racial attitudes. In fact, White individuals exposed to desegregated schools in adulthood exhibit stronger conservative politics, more negative racial attitudes towards Blacks, and weaker support for policies promoting racial equity. These findings suggest that the theoretical benefits of reduced racial isolation in particular may work to contribute to increase racial inequity—not decrease it.

My findings diverge from the most relevant study to my work, which is conducted by Billings and colleagues (2020). The authors find that exposure to more diverse schools decreased the propensity for White youth to register as Republicans in adulthood. Diving more deeply into the core tenets of contact theory, however, can explain this discrepancy. Allport (1954)

highlights that though the intergroup contact occurring in integrated schools can improve racial attitudes, the possibility of this happening depends on a set of conditions. Most importantly, he argues that there should be social support for this intergroup contact.

Much research has documented the intense resistance—by political leaders and White members of society more broadly—to historic school desegregation. It is perhaps reasonable then to expect that Allport's conditions (1954) for successful intergroup contact were not met in schools under mandate to integrate. This could then exacerbate existing negative racial attitudes among White youth, as I find in my study. Furthermore, I find suggestive evidence that negative effects were most pronounced in contexts with the least support for integration: in the U.S. South following the passage of the Civil Rights Act of 1964 and in counties opposing the Civil Rights Movement more broadly.

The context of historic school desegregation starting in the 1950s varies substantially from the context of Charlotte-Mecklenburg Schools (CMS) in 2002, where Billings et al.'s (2020) study takes place. In CMS, schools were more integrated at baseline (i.e., before 2002), not less. Afterwards, newly drawn school boundaries resulted in more racially isolated schools where some White students from the same neighborhoods ended up by chance in schools serving larger proportions of minority students. It is this variation that the authors leverage to identify causal evidence in education supporting contact theory.

It is worth stressing here the limitations of my analyses. As stated earlier, I exclude several counties who were under court mandate to integrate schools because they are not represented in the GSS data. These counties are observably different on many characteristics at baseline, which limits the generalizability of my findings. Another limitation of my study is that, unlike Billings et al. (2020), I cannot say for certain that the White adults in my sample went to

public schools in the districts and counties that I link them to. While I provide some results suggesting that they did (i.e., little White flight or movement to private schools after integration; main results holding for those reporting no mobility since the age of 16), I cannot be certain of individuals' school enrollments. Additionally, I cannot link individuals to their counties during primary and secondary education if they moved after K-12. Finally, though some research has used GSS responses aggregated more locally to track racial attitudes (e.g., Card, Mas, & Rothstein, 2008; Charles & Guryan, 2008; Cutler, Glaeser, & Vigdor, 1999), the GSS data is meant to be nationally representative. Though I am not concerned that the GSS sampling procedures would bias the internal validity of my estimates (because individuals in the same county that vary in exposure to integrated schools would need to have been differentially sampled), effects I observe for desegregation may again be generalizable only to the population of individuals in my sample.

With these important caveats in mind, how should policymakers interested in school integration leverage the findings of my study? It is clear that the context in which desegregation occurs matters for how the policy change can impact racial attitudes. Without community support, my results suggest negative consequences following efforts to reduce racial isolation across schools in a district. Any attempt to address segregation across districts lines—by some accounts an even greater contributor to racial isolation between schools (Fiel, 2013)—is likely to face even stronger opposition.

School integration has worked to improve racial equity in the past and it can do so again. The renewed recent interest in reducing racial isolation has been accompanied by increased support among White families for previously controversial policies aimed at addressing contributors to Black-White disparities (e.g., police reform). If the attitudes of White families

towards desegregation similarly shift, increasing Black-White contact in schools may lead to improved racial attitudes. But until such changes are evident, policymakers may want to continue to adopt educational programs that improve the outcomes of Black youth but are neutral on how they change the racial composition of schools.

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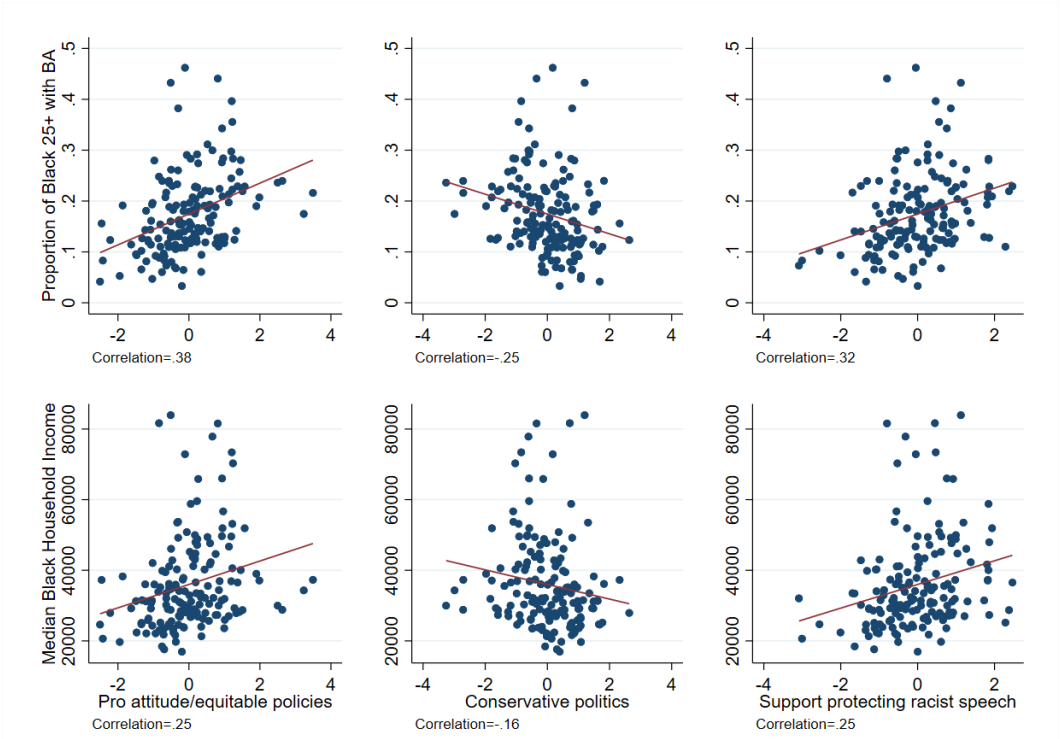

Figure 1. County-level bivariate relationship between White adults' racial attitudes and politics and Black adults' educational and socioeconomic outcomes.

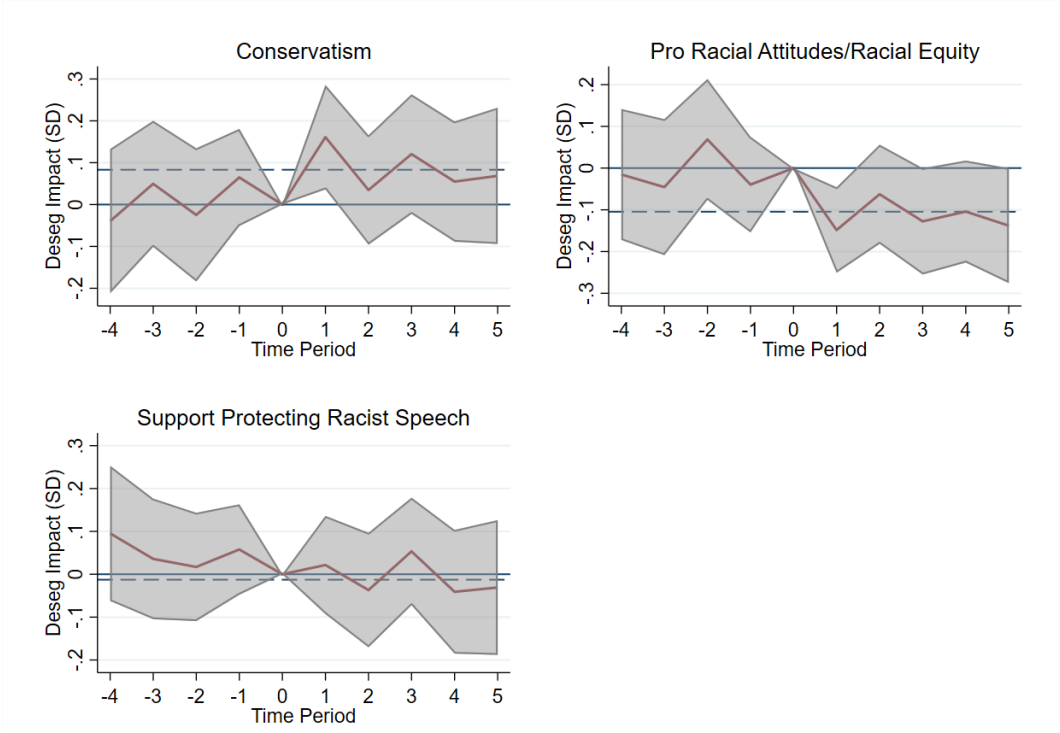

Figure 2. Plotted event study estimates of the impact of desegregation on White adults' racial attitudes and politics from equation (3). Time period reflect years until individuals' counties' earliest court mandate to integrate (or years of individuals' exposure to racially integrated schools), grouped to improve precision. The solid blue line represents the pre-treatment effect, and the dashed blue line represents the post-treatment effect.

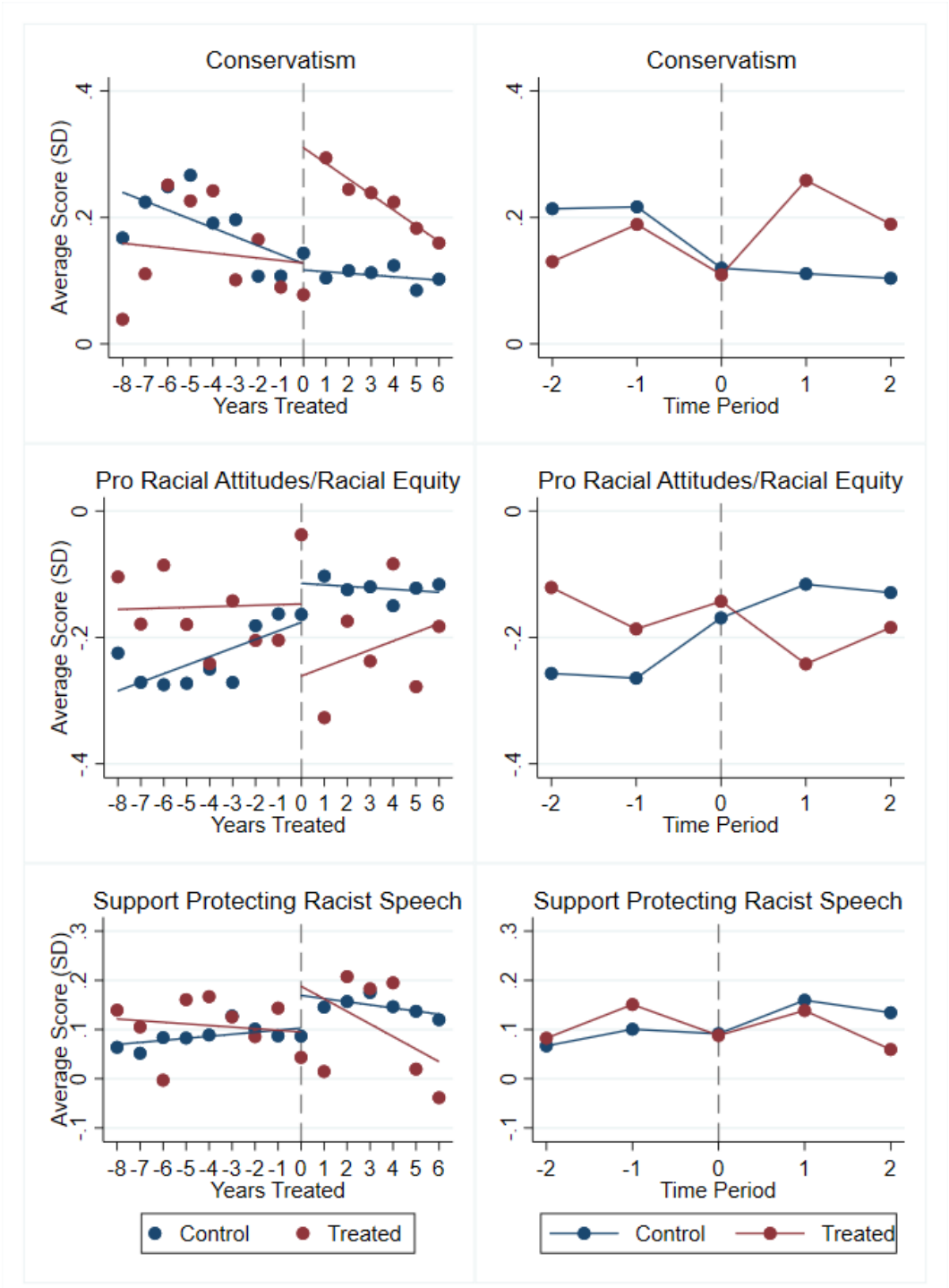

Figure 3. Plotted raw averages of White adults' racial attitudes and politics by years until individuals' counties' earliest court mandate to integrate (or years exposed to racially integrated schools). Time periods captures groups of years (see equation [4]). Treated observations are individuals in counties experiencing school desegregation. Control observations are those in counties who are mandated to desegregate in the future.

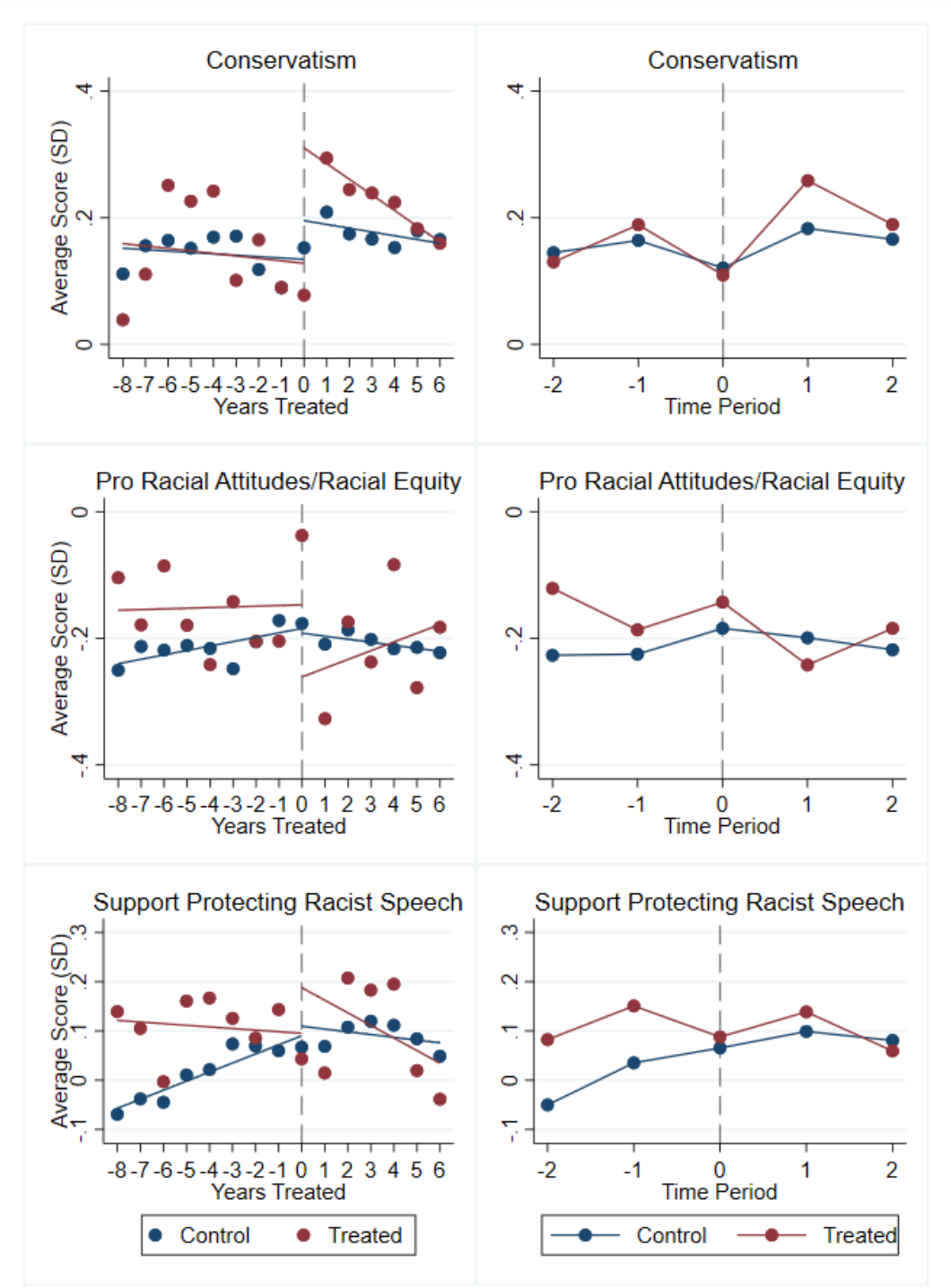

Figure 4. Plotted raw averages of White adults' racial attitudes and politics by years until individuals' counties' earliest court mandate to integrate (or years exposed to racially integrated schools). Time periods captures groups of years (see equation [4]). Treated observations are individuals in counties experiencing school desegregation. Control observations are those in counties that faced court litigation for discriminatory school assignment policies but are never mandated to desegregate.

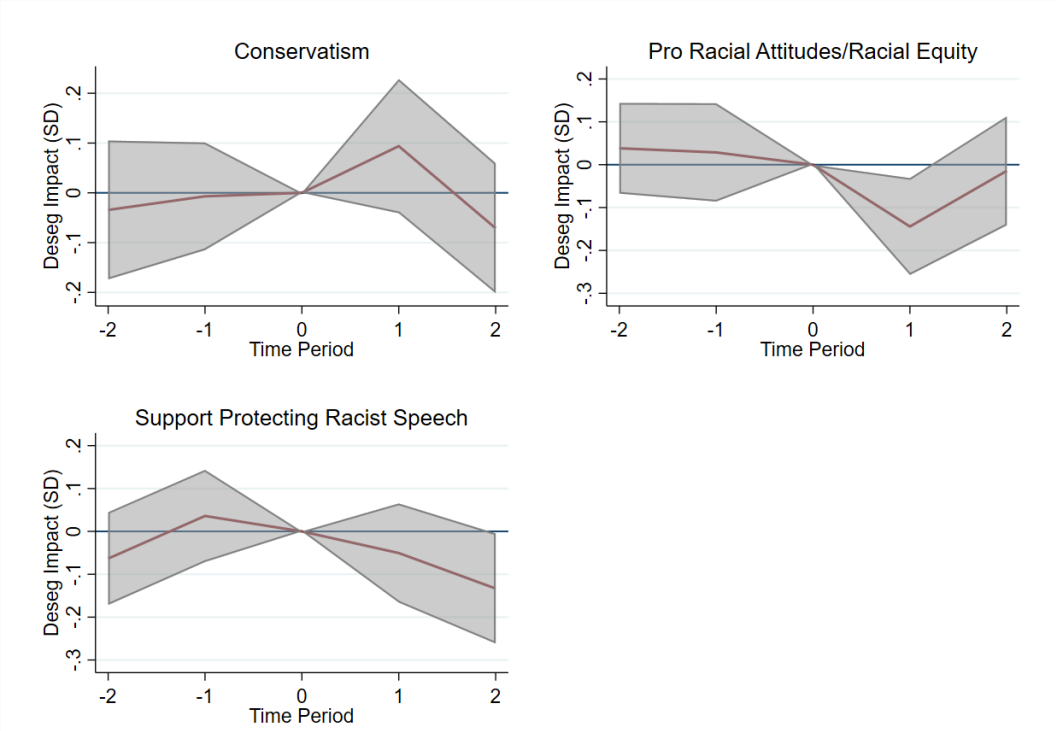

Figure 5. Plotted event study estimates of the impact of desegregation on White adults' racial attitudes and politics from equation (4). Time period reflect years until the treated county's earliest court mandate to integrate (or years of individuals' exposure to racially integrated schools), grouped to improve precision. Control observations are GSS respondents in counties who are mandated to desegregate in the future.

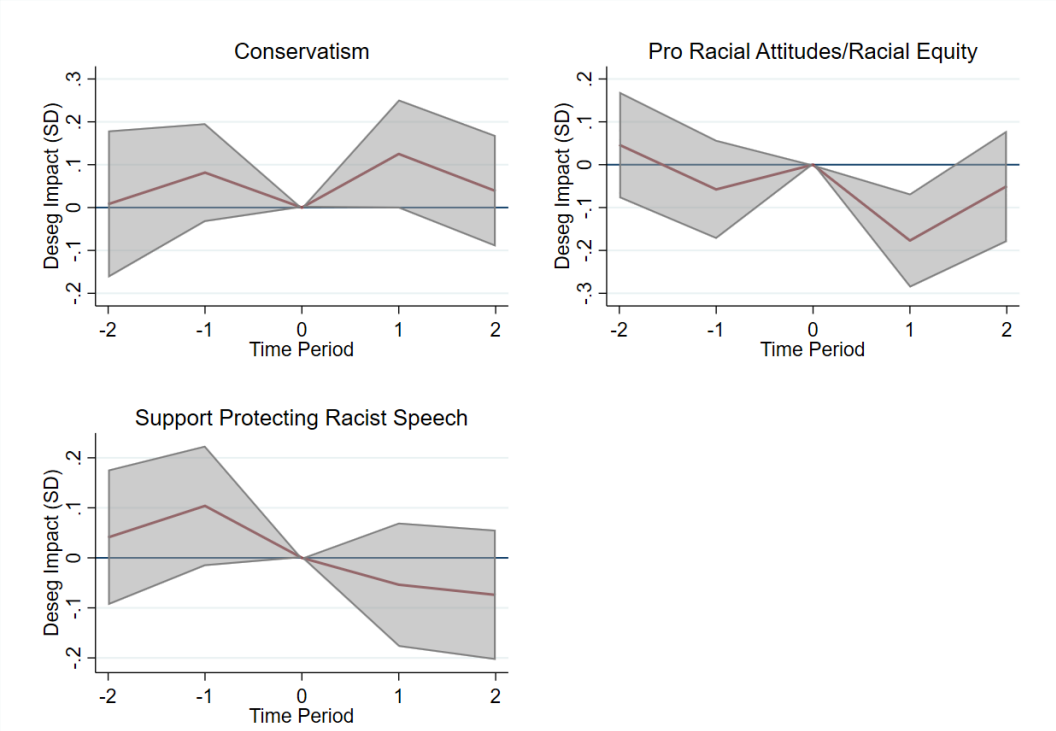

Figure 6. Plotted event study estimates of the impact of desegregation on White adults' racial attitudes and politics from equation (4). Time period reflect years until the treated county's earliest court mandate to integrate (or years of individuals' exposure to racially integrated schools), grouped to improve precision. Control observations are GSS respondents in counties that faced court litigation for discriminatory school assignment policies but are never mandated to desegregate.

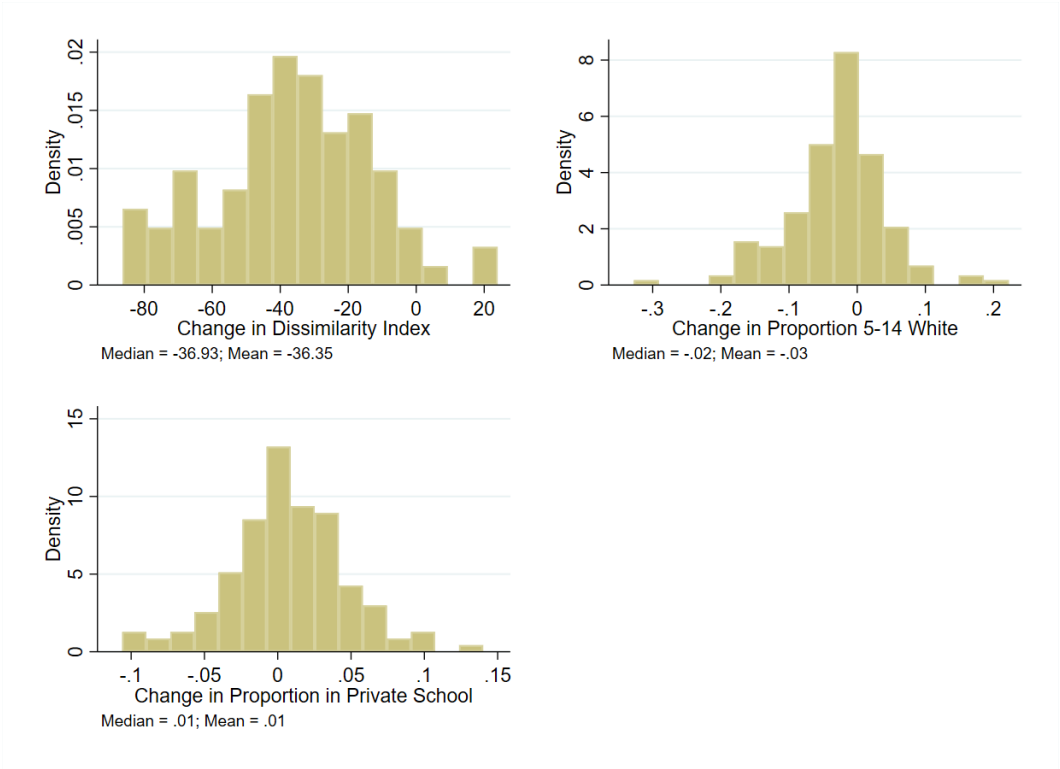

Figure 7. Histograms of changes in county-level characteristics before and after counties' earliest court case mandating school integration.

Table 1. County-level baseline characteristics

	Out of sample	In sample
N counties	435	159
Total population in 1950	36885.12	319417.91
Proportion White in 1950	0.64	0.81
Proportion unemployed in 1950	0.03	0.04
Median income in 1950	1586.47	2740
Proportion urban in 1950	0.52	0.57
Percent population growth 1940 to 1950	2.45	30.13
Proportion voted Eisenhower in 1952	0.62	0.49

Table 2. Main difference-in-difference impacts of desegregation on racial attitudes and politics

	(1)	(2)	(3)	(4)	(5)
Panel A. Conservatism					
Impact of desegregation	0.0831~	0.0761~	0.0852~	0.0769~	0.0615
	(0.0448)	(0.0448)	(0.0500)	(0.0437)	(0.0493)
Panel B. Positive racial attitudes/support racial equity policies					
Impact of desegregation	-0.105**	-0.0934**	-0.107*	-0.103**	-0.0835*
	(0.0368)	(0.0356)	(0.0434)	(0.0385)	(0.0418)
Panel C. Support the protection of racist speech					
Impact of desegregation	-0.0127	-0.0189	0.000363	-0.00849	-0.0255
	(0.0506)	(0.0491)	(0.0495)	(0.0506)	(0.0562)
N individuals	10985	10987	10985	10985	10963
N counties	156	156	156	156	156
County FE		X			
Cohort FE	X	X	X	X	
County-by-GSS-sample FE	X		X	X	X
Age FE	X		X	X	X
GSS survey design weights			X		
County linear trends				X	
Cohort-by-region FE					X

Note: Standard errors clustered at the county level reported in parentheses. *~p<.1, *p<.05, **p<.01, ***p<.001.*

Table 3. Difference-in-difference impacts of desegregation on racial attitudes and politics by baseline county characteristics

	Conservatism	Positive racial attitudes/support racial equity policies	Support the protection of racist speech
Panel A. Heterogeneity by baseline proportion White age 5-14 (in 1950)			
Desegregation & below median	0.103~	-0.146**	0.0224
	(0.0523)	(0.047)	(0.0589)
Desegregation & above median	0.0753	-0.0862*	-0.0285
	(0.0498)	(0.04)	(0.0541)
Panel B. Heterogeneity by baseline private school enrollment (in 1960)			
Desegregation & below median	0.112~	-0.0989*	0.0544
	(0.062)	(0.0477)	(0.0562)
Desegregation & above median	0.0822	-0.852~	0.0279
	(0.05)	(0.0448)	(0.0541)
Panel C. Heterogeneity by baseline school segregation (in 1968-70)			
Desegregation & below median	0.136	-0.108	-0.0213
	(0.0831)	(0.0735)	(0.0882)
Desegregation & above median	0.143~	-0.183*	-0.0287
	(0.0783)	(0.0761)	(0.101)

Note: Standard errors clustered at the county level reported in parentheses. *~p<.1, *p<.05, **p<.01, ***p<.001.*

Table 4. Difference-in-difference impacts of desegregation on racial attitudes and politics by geography and time

	Conservatism	Positive racial attitudes/support racial equity policies	Support the protection of racist speech
Panel A. Heterogeneity by geography			
Desegregation & South	0.0959~	-0.112*	0.0145
	(0.0535)	(0.0449)	(0.0572)
Desegregation & not South	0.0734	-0.0987*	-0.0333
	(0.0503)	(0.0407)	(0.0538)
Panel B. Heterogeneity by timing of initial court case			
Deseg. & pre-Civil Rights Act	0.0266	-0.110*	0.00683
	(0.0605)	(0.0534)	(0.0667)
Deseg. & post-Civil Rights Act	0.101*	-0.103**	-0.0189
	(0.0486)	(0.0392)	(0.0541)
Panel C. Heterogeneity by geography and timing			
Deseg. & Pre-CRA X South	0.0265	-0.0770	0.0581
	(0.0698)	(0.0590)	(0.0826)
Deseg. & Post-CRA & South	0.133*	-0.132*	-0.00898
	(0.0650)	(0.0524)	(0.0570)
Deseg. & not South	0.0764	-0.100*	-0.0351
	(0.0509)	(0.0407)	(0.0537)

Note: Standard errors clustered at the county level reported in parentheses. *~p<.1, *p<.05, **p<.01, ***p<.001.*

Table 5. Difference-in-difference impacts of desegregation on racial attitudes and politics by 1960 to 1968 change in proportion voting for the Democratic presidential candidate

	Conservatism	Positive racial attitudes/support racial equity policies	Support the protection of racist speech
Desegregation & Group 1	0.173~	-0.207*	0.0499
	(0.101)	(0.0912)	(0.0822)
Desegregation & Group 2	0.121	-0.134~	0.00540
	(0.0741)	(0.0746)	(0.0838)

Note: Group 1 captures counties where the drop in the Democratic share of the vote was above the median, i.e., there was a larger drop in the Democratic share of the vote. Group 2 captures counties where the drop in the Democratic share of the vote was below the median, i.e., there was a smaller drop (or even an increase) in the Democratic share of the vote. Standard errors clustered at the county level reported in parentheses. *~p<.1, *p<.05, **p<.01, ***p<.001.*

Table 6. Difference-in-difference impacts of desegregation on other potential mechanisms

	Educational attainment (1)	Real income (log) (2)	Perceptions of class (3)	Same city at age 16 (4)	Conservatism (5)	Positive racial attitudes/support racial equity policies (6)	Support the protection of racist speech (7)
Impact of desegregation	-0.000590 (0.0512)	0.0198 (0.0423)	0.00487 (0.0433)	-0.0365 (0.0229)			
Desegregation & same city at age 16					0.149** (0.0477)	-0.102* (0.0423)	0.0219 (0.0671)
Desegregation & different city at age 16					0.0610 (0.0514)	-0.127** (0.0427)	-0.0369 (0.0523)

Note: Standard errors clustered at the county level reported in parentheses. *~p<.1, *p<.05, **p<.01, ***p<.001.*

Appendix Table 1. GSS item summary statistics and text

GSS item	Mean	SD	Range	Total prop. missing	Total prop. MCAR	Item text
Panel A. Conservatism composite						
partyid	3.04	2	[0,6]	2.7	2.15	Generally speaking, do you usually think of yourself as a Republican, Democrat, Independent, or what?
polviews	3.17		[0,6]	10.7	7.57	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]	5.95	5.77	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?

eqwlth	2.97	1.98	[0,6]	40.42	39.72	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	4.7	1.35	[0,10]	49.47	47.81	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. Racial Attitudes and Support Racial Equity Policies Composite

natrace	1.12	0.72	[0,2]	56.93	52.06	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?
discaff	1.12	0.71	[0,2]	51.98	49.82	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?
racliv	0.77		[0,1]	15.12	10.81	Are there any Blacks living in this neighborhood now?
affrm	0.61	0.88	[0,3]	47.95	45.16	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?

wrkwayup	1.02	1.19	[0,4]	45.99	45.16	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	6.53	2.27	[0,16]	57.71	56.68	In general, how close Do you feel to Blacks (relative to Whites)?
workblk-workwht	5.19	1.48	[0,12]	48.72	46.79	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?

intlblk-intlwht	5.57	1.18	[0,12]	53.92	52.02	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?
helpblk	1.3	1.18	[0,4]	41.56	39.72	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?
racdif1	0.33		[0,1]	44.15	41.45	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?

racdif4	0.5		[0,1]	44.63	41.45	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?
Panel C. Support Protection of Racist Speech Composite						
spkrac	0.66		[0,1]	41.41	40.42	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	0.51		[0,1]	42.42	40.42	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	0.7		[0,1]	41.98	40.42	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	2.56	0.68	[1,4]	3.85	3.23	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?

satfin	1.92	0.75	[1,3]	11.08	10.8	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	3	0.91	[1,5]	11.71	10.8	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.)

Appendix Table 2. Event study estimates of the impact of desegregation on racial attitudes and politics

	Conservatism			Positive racial attitudes/support racial equity policies			Support the protection of racist speech		
Years exposed	(1)	(2)	(3)	(1)	(2)	(3)	(1)	(2)	(3)
$y \leq -12$	-0.0396			-0.0153			0.0954		
	(0.0879)			(0.0801)			(0.0805)		
$-11 \leq y \leq -9$	0.0496			-0.0456			0.0359		
	(0.0768)			(0.0832)			(0.0718)		
$-8 \leq y \leq -6$	-0.0248	-0.0344	0.00794	0.0687	0.0384	0.0465	0.0171	-0.0634	0.0409
	(0.0812)	(0.0712)	(0.0876)	(0.0739)	(0.0539)	(0.0633)	(0.0645)	(0.0552)	(0.0691)
$-5 \leq y \leq -3$	0.0648	-0.00695	0.0816	-0.0395	0.0287	-0.0577	0.0578	0.0360	0.104~
	(0.0592)	(0.0551)	(0.0587)	(0.0585)	(0.0585)	(0.0589)	(0.0538)	(0.0548)	(0.0614)
$1 \leq y \leq 3$	0.161*	0.0938	0.125~	-0.148**	-0.144*	-0.177**	0.0216	-0.0505	-0.0537
	(0.0636)	(0.0688)	(0.0648)	(0.0523)	(0.0577)	(0.0560)	(0.0584)	(0.0590)	(0.0633)
$4 \leq y \leq 6$	0.0348	-0.0711	0.0386	-0.0626	-0.0142	-0.0500	-0.0369	-0.133*	-0.0740
	(0.0667)	(0.0667)	(0.0663)	(0.0605)	(0.0649)	(0.0663)	(0.0682)	(0.0655)	(0.0664)
$7 \leq y \leq 9$	0.121			-0.128~			0.0534		
	(0.0729)			(0.0650)			(0.0640)		
$10 \leq y \leq 12$	0.0549			-0.104~			-0.0409		
	(0.0732)			(0.0623)			(0.0736)		
$y \geq 13$	0.0686			-0.138~			-0.0309		
	(0.0831)			(0.0702)			(0.0802)		

Note: Column (1) captures estimates from estimation of the model represented by equation (3). Columns (2) and (3) capture treated group estimates from estimation of the model represented by equation (4). For column (2), the control observations are those in counties who are mandated to desegregate in the future. For column (3), the control observations are GSS respondents in counties that

faced court litigation for discriminatory school assignment policies but are never mandated to desegregate. Standard errors clustered at the county level reported in parentheses. *~p<.1, *p<.05, **p<.01, ***p<.001.*

Appendix Table 3. Difference-in-difference impacts of desegregation on racial attitudes and politics by subgroups of counties and GSS respondents

	Conservatism	Positive racial attitudes/support racial equity policies	Support the protection of racist speech
Panel A. White adults with socioeconomic/education outcomes			
Impact of desegregation	0.0628	-0.120**	-0.0504
	(0.0494)	(0.0418)	(0.0526)
Panel B. White adults with mobility since age 16 data			
Impact of desegregation	0.0910*	-0.115**	-0.0143
	(0.0438)	(0.0380)	(0.0526)
Panel C. Counties with pre/post private enrollment data			
Impact of desegregation	0.0923~	-0.0898*	0.0367
	(0.0481)	(0.0413)	(0.0500)
Panel D. Counties with pre/post school segregation data			
Impact of desegregation	0.139~	-0.142~	-0.0246
	(0.0731)	(0.0720)	(0.0897)
Panel E. Counties with pre/post age 5-14 proportion White data			
Impact of desegregation	0.0839~	-0.105**	-0.0126
	(0.0449)	(0.0369)	(0.0508)
Panel F. Counties desegregating after 1968 with presidential voting in 1960 and 1968			
Impact of desegregation	0.135~	-0.154*	0.0172
	(0.0749)	(0.0749)	(0.0803)

Note: Standard errors clustered at the county level reported in parentheses. *~p<.1, *p<.05, **p<.01, ***p<.001.*

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 restrict my sample of GSS respondents to the White adults in my main analyses (i.e., those in counties that were under court mandate to integrate).

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 support for 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 16.1 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 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.

Appendix A References

Graham, J. W. (2009). Missing data analysis: Making it work in the real world. *Annual Review of Psychology*, *60*, 549-576.

Hu, L. T., & Bentler, P. M. (1999). Cutoff criteria for fit indexes in covariance structure analysis: Conventional criteria versus new alternatives. *Structural Equation Modeling: A Multidisciplinary Journal*, *6*(1), 1-55.

Kaiser, H. F. (1960). The application of electronic computers to factor analysis. *Educational and Psychological Measurement*, *20*(1), 141-151.

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

	Conservatism	Positive racial attitudes/support racial equity policies	Support the protection of racist speech
partyid	-.7653115	.0543514	-.0362552
polviews	-.5954601	-.0892038	-.1123872
discaff	.0969061	.3373433	.0374329
pres?? If??who	.6846305	-.0321766	.0420251
natrace	.216683	.4244992	-.0194086
eqwlth	.4908727	.0161895	-.1074252
spkrac	.0129749	.0285011	.7092785
colrac	.0634976	-.0055378	.6467183
librac	.0426303	.0338448	.5770899
racliv	-.0198583	.1704679	.007307
affrm	.2540119	.3409154	-.1121374
wrkwayup	.2409562	.5540746	.0043107
closeblk-closewht	-.0772652	.3670407	-.0393178
wlthblk-wlthwht	-.2556865	.1964001	-.0198708
workblk-workwht	-.2024336	.6468378	.0471089
intlblk-intlwht	-.1927998	.4620728	.055367
helpblk	.3415883	.4273178	-.086575
racdif1	.2789878	.3095618	-.032123
racdif4	.0368648	.5152835	.0775744

Note: Promax rotation.

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

Fit statistic	Value
RMSEA	0.040
CFI	0.89
TLI	0.88