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A DIFFERENT WORLD:
ENDURING EFFECTS OF SCHOOL DESEGREGATION ON IDEOLOGY AND ATTITUDES

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ABSTRACT

In 1975, a federal court ordered the desegregation of public schools in Jefferson County, KY. In order to approximately equalize the share of minorities across schools, students were assigned to a busing schedule that depended on the first letter of their last name. We use the resulting quasi-random variation to estimate the long-run impact of attending an inner-city school on political participation and preferences among whites. Drawing on administrative voter registration records and an original survey, we find that being bused to an inner-city school significantly increases support for the Democratic Party and its candidates more than forty years later. Consistent with the idea that exposure to an inner-city environment causes a permanent change in ideological outlook, we also find evidence that bused individuals are much less likely to believe in a “just world” (i.e., that success is earned rather than attributable to luck) and, more tentatively, that they become more supportive of some forms of redistribution. Taken together, our findings imply that witnessing economic deprivation can durably sensitize individuals to issues of inequality and fairness.

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1. Introduction

In 1954, the U.S. Supreme Court ruled racial segregation of children in public schools unconstitutional. Although the Court’s landmark decision in *Brown v. Board of Education* ended the *de jure* segregation of schools—particularly in the South—*de facto* integration did not begin in earnest until the Civil Rights Act of 1964 empowered the U.S. Department of Justice to marshal the resources of the federal government in order to create equal access to education independent of race. Between 1966 and 1975, nearly one in ten public school districts undertook substantial efforts to desegregate schools (U.S. Commission on Civil Rights 1977). One of the most common methods to achieve racial integration was to impose mandatory busing programs. These programs transported black students to better-resourced, predominantly white schools and, less frequently, white students to predominantly black schools in the inner city.

In this paper, we draw on a unique natural experiment to study the long-run impact of being bused to an inner-city school on the social and political preferences of whites.¹ In the summer of 1975, the newly merged Louisville-Jefferson County school district implemented a court-mandated busing plan to racially integrate schools. By design, the court-ordered busing schedule approximately equalized the share of black students across schools. It did so by transporting black students to formerly white schools, whereas white students were to be bused to formerly black schools. As a consequence, all white Louisville-area public school students experienced significant cross-racial contact. The environments in which they encountered their black peers, however, differed dramatically. White children who had not been assigned to be bused saw an influx of black students into their relatively affluent, suburban school environments. Bused students, by contrast, experienced the distinctive social dynamics of an inner-city school.

When and, for the initial cohorts, *if* a student was bused depended on the first letter of their last name. For instance, white students in the graduating class of 1977 were assigned to be bused if they belonged to alphabet group “A, B, F, Q” but not if their last names placed them in, say, group “D, E, N, W, Z.” The design of the court-mandated busing plan thus created plausibly exogenous variation in busing assignments, which we leverage in our empirical work.

The quasi-random nature of treatment assignments allows us to estimate the causal effect of being bused using a simple difference-in-differences strategy. Intuitively, we compare contemporaneous outcomes between individuals who belong to the same cohorts but had different busing assignments because their last names place them in different alphabet groups,

¹This experiment was first exploited by Tuttle (2019), who estimates the long-run impact of busing on income.

with differences between individuals who belong to the same alphabet groups but who graduated prior to the busing plan being fully phased in. A unique feature of this research design is that it isolates the effect of being bused to an inner-city school net of greater cross-racial contact. The identifying assumption is that, absent busing, treated and untreated alphabet groups would have experienced similar changes in outcomes. This parallel trends assumption is especially plausible in the context of our natural experiment.

A priori, it is unclear what, if any, effects busing might have had. On the one hand, busing was extremely contentious. In light of deep-seated racial tensions, many communities reacted to forced school desegregation with fierce opposition and organized—sometimes violent—protests. As the Democratic Party had championed civil rights legislation and since prominent Democrats publicly supported school desegregation, the controversy surrounding busing might have led to political backlash and realignment, especially among students who were actually bused.² On the other hand, being exposed to an inner-city environment might sensitize white students to issues of inequality and economic deprivation, potentially making them more rather than less sympathetic to left-leaning political views.

The first part of our analysis studies the impact of busing by drawing on digitized high school yearbooks from Louisville-area public schools, which we link to present-day administrative voter registration and turnout records. Focusing on cohorts graduating between 1970 and 1980, and relying on yearbooks published *prior* to the announcement of the district’s desegregation plan, we identify individuals who were and were not subject to busing. For reasons of statistical power, we restrict attention to about 32,500 white males, of whom we can match about 57% (84%) to exactly (at least) one voter registration record.³

Our results indicate that being bused does not increase political participation, as measured by voter registration rates and turnout. Although bused individuals do not become more politically active, we do find that having been bused to an inner-city school increases the likelihood of registering as a Democrat more than forty years later by about three percentage points. Rather than provoking conservative backlash, it appears that busing shifted treated students’ attitudes towards the left.

²President Johnson himself predicted significant backlash in response to the passage of the Civil Rights Act (see also Kuziemko and Washington 2018). To illustrate how controversial busing itself was, in his autobiography, then-Senator Joseph Biden described the policy as “a liberal trainwreck” (Biden, 2007). Echoing similar sentiments, President Reagan referred to busing as “a social experiment that nobody wants” (Reagan Presidential Library, n.d.).

³Our focus on males is due to the fact that too many females change their last name upon getting married for us to be able to reliably match them across data sets. We restrict attention to whites because African Americans accounted for only about 20% of students in the merged Louisville-Jefferson County school system (Sedler 2007). Our sample of black students is, therefore, much smaller and does not allow us to reliably detect even moderately large effect sizes. For completeness, we report results for black students in Appendix C.

To corroborate this finding, the second part of our analysis draws on an original survey of more than five hundred former Jefferson County Public Schools (JCPS) students who graduated from high school in the mid to late 1970s. This survey allows us to examine the effect of having been bused on outcomes that we cannot observe in our administrative data, including racial attitudes, social views, trust in government institutions, and support for redistribution. We also ask proxy questions for possible mechanisms through which busing might have impacted partisanship decades later. For instance, having been bused to an inner-city school might have affected who students befriended, how they perceived and interacted with their teachers, or how they experienced the surrounding community.

Our survey results confirm that bused individuals are more supportive of the Democratic Party and its candidates today. Moreover, we find that bused individuals score far lower on the Belief in a Just World Scale (Lipkus 1991). That is, they are less likely to believe that success is earned rather than attributable to luck. The estimated treatment effect equals about 0.4 standard deviations, which corresponds to more than three times the in-sample difference between self-reported Democrats and Republicans on this scale. Consistent with a permanent shift in ideological outlook, we also find that bused individuals become more supportive of unions and, perhaps, other forms of redistribution.⁴

Our survey results additionally show that bused individuals are more likely to recall poverty at their school and that they are more likely to have befriended children who were poor. There is no conclusive evidence, however, that busing increased cross-racial friendships, and our results vis-à-vis racial attitudes are decidedly mixed. On the one hand, we find that, today, bused individuals live in zip-codes with similar incomes but a higher share of African Americans. On the other hand, we do not detect any effect of busing on survey measures of racial prejudice.

The mixed evidence on racial attitudes may or may not be surprising. Since all Louisville-area public schools became racially integrated after the summer of 1975, our difference-in-differences estimates capture the effect of exposure to minority peers only to the extent that the environment in which contact occurs matters. While it might have seemed plausible that exposure to African Americans in an inner-city environment would be especially effective at reducing prejudice, the answer to this question depends, at least in part, on how exactly racial attitudes are measured.

Broadly summarizing, our findings suggest that greater exposure to poverty durably shifted students' views about economic inequality and fairness. As one survey respondent reflected:

“Going to high school in an economically depressed area of the city made a big impression on me.

⁴All of our survey results account for multiple testing by controlling the false discovery rate (Storey, 2003; Anderson, 2008).

[...] I realized that the people in these areas were just like me except they did not have the same resources that I had.”

Such realizations appear to have had lasting effects, continuing to influence partisan loyalties and support for unions more than four decades later.

It bears emphasizing that all of our results should be interpreted as intent-to-treat effects. In the mid-1970s, many cities in the U.S. were afflicted by white flight, and the Louisville metropolitan area was no exception. We provide evidence that busing increased the pace of this phenomenon, with white students who were assigned to be bused being especially likely to leave the Jefferson County public school system. We further find that many of those who left attended a Catholic high school instead, with a smaller number of students moving to public schools in other counties. In light of this fact, our intent-to-treat estimates correspond to averages of the treatment effects for compliers (i.e., students who were assigned to be bused and actually ended up attending an inner-city high school) and non-compliers (i.e., those who instead went to a private school or moved to another school district).

In the appendix, we provide a sufficient condition on potential outcomes under which our intent-to-treat estimates provide a lower bound on the local average treatment effect as well as on the average treatment effect. Our setup expands upon textbook treatments of two-sided non-compliance because busing assignment can induce a third possible response: leaving Jefferson County public schools entirely. Given that students who left the public school system tended to attend other affluent, predominantly white institutions, our condition can be interpreted as an individual-level monotonicity restriction on the effect of greater exposure to socioeconomic diversity.⁵

Related Literature There is a burgeoning literature assessing the impact of affirmative action and racial integration programs in American schools (see, e.g., Guryan 2004; Angrist and Lang 2004; Reber 2005, 2010; Card and Rothstein 2007; Lutz 2011; Baum-Snow and Lutz 2011; Boustan 2012; Billings et al. 2013; Bergman 2018; Johnson 2015, 2019; Gordon and Reber 2018; Tuttle 2019; Bleemer 2022). The closest paper to ours is Billings et al. (2021), which studies the end of race-based busing in Charlotte-Mecklenburg. Consistent with Allport’s (1954) contact hypothesis, the authors find that an increase in the share of minority peers significantly decreases the likelihood of registering as a Republican roughly

⁵Intuitively, we show that if the effect of experiencing socioeconomic diversity on the outcome of interest is positive (negative) and monotonic among all students, then the intent-to-treat effect bounds both the local average treatment effect and the average treatment effect from below (above). An alternative assumption might be that the average effect among treatment-avoiders is weakly smaller than that among students who comply with their busing assignments. If this is the case, say because the former sought out (relatively more conservative) Catholic schools, then our intent-to-treat estimates still provide a lower bound on the local average treatment effect.

fifteen years later.

While our headline results on partisanship mirror those of Billings et al. (2021), there are a number of important differences. First, Billings et al. (2021) examine the *resegregation* of schools, which, at the time, was much less controversial than the introduction of mandatory busing programs in the aftermath of the Civil Rights Act. Second, our survey allows us to study a greater range of socioeconomic outcomes, including racial preferences, views on inequality and the role of luck, as well as attitudes toward redistribution. Third, and perhaps most importantly, there are differences in estimands. Billings et al. (2021) focus on the effect of changes in schools’ racial composition. By contrast, our difference-in-differences design isolates the causal effect of being bused to an inner-city high school, net of greater cross-racial exposure. The estimates in both papers thus answer distinct causal questions.

More broadly, our findings contribute to a large literature on diversity and prejudice in a variety of settings (see Paluck et al. 2019 and Lowe 2024 for recent reviews). Boisjoly et al. (2006), for instance, provide evidence that cross-race interactions increase whites’ support for affirmative action. Carrell et al. (2019) find that white U.S. Air Force cadets who are randomly assigned to squadrons with more black peers are more likely to choose a black roommate, and Rao (2019) shows that greater enrollment of poor students in Delhi private schools causes wealthy students to become more pro-social and less discriminatory. Others, however, find that greater diversity generates backlash (see, e.g., Enos 2014; Halla et al. 2017; Tabellini 2020; Calderon et al. 2023). Lowe (2021) distinguishes between collaborative and adversarial contact to show that the former increases cross-caste friendships among Indian cricket players, while the latter type of contact reduces it.

We make at least three contributions to this literature. First, we analyze the impact of a large-scale social integration program in the U.S., which, given the salience and widespread use of busing at the time, is of independent historical interest. Second, our quasi-experimental research design isolates the effect of experiencing an inner-city environment, holding exposure to minorities constant. Our findings thus complement work on Allport’s (1954) contact hypothesis, which fixes the environment in order to estimate the effect of cross-racial exposure. Our findings support the idea that witnessing economic deprivation can have large effects on individual attitudes and beliefs, especially on issues of inequality and fairness. Third, we present long-run estimates—more than four decades after the intervention. Our work thus also sheds light on the persistence of such effects.⁶

⁶In addition, our findings contribute to a broad literature on how exposure to economic shocks affects the formation of preferences and beliefs (see, e.g., Giuliano and Spilimbergo 2024 for a recent review).

2. Historical Background

The U.S. Supreme Court’s landmark decision in *Brown v. Board of Education* (1954) declared racial segregation of public schools unconstitutional. Although the Court overturned its long-standing “separate but equal” doctrine, the justices did not establish any mechanisms for actively integrating schools. As a consequence, only a relatively small number of school districts desegregated in the direct aftermath of *Brown*. Racial integration did not gain widespread momentum until the passage of the Civil Rights Act of 1964, which, among other things, empowered the U.S. Department of Justice to actively intervene in local school districts in order to ensure equal access to education independent of race. Before 1964, less than 2% of African-American students in the South attended majority-white schools. By 1972, that number had risen to 36% (Orfield and Frankenberg 2014). In addition to desegregation orders from the Department of Justice, many school districts became racially integrated as a result of court orders. *Green v. New Kent* (1968) and *Swann v. Charlotte-Mecklenburg* (1971), in particular, ushered in a wave of court-mandated desegregation.⁷

Like many other metropolitan areas, in the early 1970s, the Louisville metro area was highly segregated by race.⁸ Most of the area’s black residents were concentrated in just a few neighborhoods in the city of Louisville (see Appendix Figure A.1). At the time, the city and the surrounding suburbs in Jefferson County operated separate and highly unequal school districts. Louisville public schools were not only much poorer than their counterparts in Jefferson County but *de facto* segregated by race. About 80% of white children attended schools that were at least 90% white, while 76% of black students were enrolled in schools that were at least 90% black. The makeup of schools in neighboring Jefferson County was nearly all white.

In 1971/72, the Kentucky Civil Liberties Union (KCLU), the local branch of the National Association for the Advancement of Colored People (NAACP), the Kentucky Commission on Human Rights (KCHR), and the Legal Aid Society of Louisville joined forces and filed federal lawsuits against the Jefferson County Board of Education and the Louisville Independent School District. Drawing on the seventeen-year-old precedent set in *Brown*, the lawsuits alleged that the reality of segregation in both districts violated the equal protection clause of the 14th Amendment.

The Louisville and Jefferson County cases were consolidated and tried before U.S. District Judge James F. Gordon of the Western District of Kentucky, who initially ruled that the districts were in constitutional compliance. He further indicated that a federal court could not

⁷The Supreme Court decisions in *Green v. New Kent* and *Swann v. Charlotte-Mecklenburg* mandated active desegregation beyond simply prohibiting future segregation, extending this requirement even to cases where segregation had occurred unintentionally.

⁸In what follows, we borrow heavily from the historical account of Sedler (2007).

order cross-district busing. This ruling seemingly aligned with the Supreme Court’s eventual decision in *Milliken v. Bradley* (July 1974), which held that federal courts could not impose desegregation plans if segregation resulted from residential sorting across school districts. As a consequence, as of the fall of 1974, Louisville-area residents had strong reason to believe their schools would remain racially segregated.

In December of 1974, however, the 6th Circuit Court of Appeals found that a “crucial difference” between this case and *Milliken* was that “school district lines in Kentucky have been ignored in the past for the purpose of aiding and implementing continued segregation” (quoted in Sedler 2007, pp. 18). Thus, in a surprising turn of events, the 6th Circuit Court of Appeals overturned Judge Gordon’s initial ruling.

Faced with a looming integration order, the Kentucky State Board of Education decided to merge the Louisville and Jefferson County school districts. The next few months were spent litigating what kind of desegregation plan the newly merged school district should implement and when it would go into effect. Judge Gordon’s initial position was that it was not feasible to fully desegregate schools until the 1976/77 school year. In July of 1975, however, the 6th Circuit Court of Appeals decided that full desegregation had to be implemented by the beginning of the upcoming school year, which was less than two months away.

To meet this deadline, Judge Gordon ordered the KCHR’s expert on desegregation as well as staff members from the Jefferson County and Louisville school districts to work with him on a comprehensive plan that would (i) bus students of a given race equally—to the degree possible—and (ii) keep the number of students attending a particular school roughly constant over time. Under the desegregation plan that was eventually adopted, every school in the newly merged district would have a significant share of black enrollment. Schools that already fell in the target range of 12.5–35% were exempt from busing.⁹ In a few cases, schools on the outskirts of Louisville could be more efficiently integrated through redistricting instead of busing. These schools were also labeled exempt. In total, the combined school district taught more than 120,000 students per year, of whom about 22,600 would need to be bused at any given point in time. Among the latter, about half were black.

Figure 1 depicts the busing schedule at the heart of Judge Gordon’s desegregation plan. Important for our purposes, if and when a particular student would be bused was determined by their race, grade level, and the first letter of their last name. For instance, whites whose last name started with A, B, F, or Q were bused in grades 11 and 12, whereas whites whose last name put them in “alphabet group” G, H, or L were bused in grades 2 and 7. While white students were assigned busing for either one or two years, black children would be

⁹This was initially the case for 16 elementary schools and 12 secondary schools, all of which were formerly part of the Louisville district (Sedler 2007).

Figure 1: Court-Ordered Busing Plan

How to tell when your child will be bused . . . unless		
If child's last name begins with letters:	White child will be bused in grades:	Black child will be bused in grades:
A, B, F, Q	11, 12	2, 3, 7, 8, 9, 10, 11, 12
G, H, L	2, 7	2, 3, 7, 8, 9, 10, 11, 12
C, P, R, X	3, 8	2, 3, 4, 5, 6, 7, 8, 9
M, O, T, U, V, Y	4, 9	2, 3, 4, 5, 6, 10, 11, 12
D, E, N, W, Z	5, 10	4, 5, 6, 7, 8, 9, 10, 11, 12
I, J, K, S	6	4, 5, 6, 7, 8, 9, 10, 11, 12

Exempted students:

- ✓ Kindergarten students
- ✓ First graders during the fall quarter *
- ✓ Students who will be seniors this year, however, in subsequent years seniors will participate in the plan
- ✓ Students in special schools, primarily for the emotionally or physically handicapped
- ✓ Students attending schools exempted under the plan

* In grade one no child will be bused during the fall quarter; after that, entire classes will be bused with their teachers on a schedule to be determined later.

Source: The Courier-Journal (1975, July 31)

bused for eight or nine. Only kindergartners and first graders were exempt from the plan. In addition, the plan exempted rising seniors in the 1975/76 school year.¹⁰

As in many other American cities, the Louisville desegregation plan was extremely controversial. Surveys conducted at the time showed that 98% of suburban residents disapproved of the court-ordered scheme (Semuels 2015). Though not quite as violent as similar protests in Boston and Detroit, school desegregation in Jefferson County did spark significant backlash. At the beginning of the new school year, the Ku Klux Klan and Concerned Parents Inc. organized a demonstration involving 2,500 whites. At one high school, several thousand students protested by throwing rocks and setting school buses on fire (Chicago Tribune 1975). By Saturday, September 6, over 500 white protesters had been arrested, and the governor of Kentucky called in the National Guard. On the following Monday, armed guards rode buses with African-American students and accompanied them to school. Eventually, however, acceptance started to set in (K'Meyer 2013).

According to data submitted to the Office of Civil Rights, after the implementation of busing, African-American enrollment in JCPS high schools ranged from 13.9% to 33.9%. Although the district did not manage to completely equalize the share of African-American students across all schools, Appendix Figure A.2 shows that the court-ordered busing plan eliminated mean differences between formerly majority-black and majority-white schools.

¹⁰To the best of our knowledge, the historical record contains no evidence to suggest that concerned parents manipulated the busing schedule.

Thus, after 1975 all JCPS students experienced significant levels of cross-racial contact.¹¹

3. Data and Descriptive Statistics

Our analysis relies on three main sources of data: (i) yearbooks and graduation lists from public schools in Jefferson County, KY; (ii) administrative data on the universe of registered voters in the U.S.; and (iii) an original survey of former JCPS students. Below, we describe each data source and how we combine them.

Yearbook and Commencement Data In order to identify individuals who attended a Louisville-area public school during the 1970s (i.e., before and after the court-ordered desegregation plan went into effect), we draw on school yearbooks and graduation lists. To the extent possible, we located these documents for all JCPS middle and high schools either electronically on `classmates.com` or in hard copy in the JCPS Archives and Record Center. From each yearbook and commencement list, undergraduate research assistants manually transcribed every student’s first and last name, as well as their current grade level, based on which we impute an approximate year of birth. Relying on students’ yearbook pictures and the best judgment of our coders, we also record students’ race and gender. In order to validate the collected information, we assigned approximately 5,900 individuals to more than one research assistant. Their coding of student race agreed in about 92% of cases, while their classification of gender was in nearly perfect agreement. In total, our research assistants transcribed approximately 177,000 student-year records from more than 150 yearbooks.¹²

Voter Registration Data To speak to the partisanship and political participation of former JCPS students, we acquired information on the universe of registered voters in the United States. These data are current as of early 2021 and come from L2, Inc., a non-partisan for-profit data vendor that maintains high-quality databases of registered voters, political donors, and consumers.¹³ L2 collects, integrates, and standardizes information from different administrative and commercial sources, such as local election boards and Secretaries of State, the Federal Election Commission (FEC), as well as mortgage and real estate records. It sells these data to political candidates and action committees (PACs), advocacy groups, and interested academics, among others. Crucial for our purposes, the L2 data contain individuals’ exact name, gender, date of birth, address, turnout history, and party affiliation.

¹¹The original busing plan remained in effect with only minor modifications until 1985, when the school district switched to a zoning system for middle and high school students. In 2000, after black families sued the district to allow their children to attend a predominantly black school, a federal court lifted the desegregation order; and the Supreme Court decided in *Meredith v. Jefferson County Board of Education* (2007) that the school district could no longer use race as the sole factor in assigning students to schools.

¹²For a list of all schools and years for which we collected data, see Appendix Table A.2.

¹³The following description of the L2 data borrows from Spenkuch et al. (2023).

In all but sixteen states does the partisanship of individuals in the L2 data coincide with the party affiliation in the respective states’ voter registration lists. The remaining sixteen states do not collect information on voters’ partisan affiliation. For voters in these states, L2 uses predictive modeling to impute a “likely” party affiliation.¹⁴ Per the company, their proprietary machine-learning algorithms use an array of public and private data sources, including participation in partisan primaries, demographics available through states’ voter files, exit polling from presidential elections, commercial lifestyle indicators, census data, self-reported party preferences from private polling, and more. L2 does not guarantee that any single voter will self-identify as being associated with the assigned “likely” party, but it claims an accuracy level of 85% or better.

Record Linkage and Sample Restrictions We match the individuals in our yearbook data to voter registration records based on their name and (approximate) year of birth. In the case of multiple matches across data sets, we attempt to determine the most likely one by utilizing ancillary information from Kentucky birth certificates. Provided that one, and only one, of the matched voter registration records has the same date of birth—or, at least, the same year of birth—as a Kentucky birth record, we retain that record and discard all other potential matches for a particular student. We say that an individual in our yearbook data can be uniquely matched to a voter registration record if there exists exactly one record with approximately the same name and year of birth, or if birth certificates allow us to narrow down the set of potential matches to one.¹⁵

Since white students greatly outnumbered blacks in the JCPS system and given that a large fraction of women change their last name after getting married—particularly among the cohorts that we study—our analysis focuses on white males.¹⁶ In Appendix C, we report separate sets of estimates for black students. Given the much smaller sample sizes, however, these results are unfortunately underpowered.

For our baseline sample of white males, we further restrict attention to students who either were already attending or, based on feeder patterns, were scheduled to attend an integrating JCPS high school *prior* to the fall of 1975—when it seemed unlikely that the area’s schools would be desegregated.¹⁷ We impose this additional restriction to address concerns about sample selection as a result of “white flight,” i.e., parents disenrolling their children from

¹⁴Specifically, L2 models party affiliation in the following states: AL, GA, HI, IL, IN, MI, MN, MO, MT, ND, OH, SC, TX, VA, VT, and WA.

¹⁵For additional detail on our record linkage procedure, see Appendix B.

¹⁶We have experimented with linking female students to marriage certificates from Kentucky, but have not been able to achieve satisfactory match rates to the voter registration data.

¹⁷As explained in Section 2, schools that were already racially integrated or schools that could be integrated by other means, such as shifting school catchment areas, were exempt from busing. Our main analysis excludes these students. In Section 5, we present a robustness check that uses them as an alternative control group.

public school in response to the court-ordered desegregation plan. Since, for some schools, we have been able to collect yearbook data going all the way back to seventh grade, the population for our analysis consists of white males who were slotted to graduate from a JCPS high school between 1970 and 1980.

Our usable yearbook data contain 32,568 individuals, of whom 4,213 were assigned to be bused.¹⁸ Table 1 presents sample sizes by year of graduation and treatment assignment. The table also displays the fraction of students who we can successfully match to a voter registration record more than forty years later. Overall, we link about 84% of students to at least one voter registration record, with 57% being uniquely matched. The fact that about 16% of students remain unmatched could either be due to nontrivial transcription errors in the process of our data collection efforts, to early mortality, or to the fact that a significant number of Americans are not registered to vote. According to estimates of the U.S. Census Bureau, in 2020, only about 78.5% of Americans aged 65 or older were registered to vote (Fabina and Scherer 2022).¹⁹

Table 1 further indicates that our record linkage procedures yield a 1.5 percentage point (p.p.) higher probability of finding any match for students that were assigned busing. Considering unique matches, however, treated students are 1.0 p.p. *less* likely to be matched than those in the control group. These mean differences are quantitatively modest but statistically significant. Importantly, however, the results in Table 4 below demonstrate that busing has no effect on match rates once we control for cohort and alphabet-group fixed effects. In total, there are 18,541 uniquely matched individuals, on which we base most of our subsequent analyses.

Survey Since voter registration data do not contain information on social views and attitudes, we contracted with the National Opinion Research Center (NORC) at the University of Chicago to conduct an original survey of individuals in our sample. Specifically, we provided NORC with a list of 2,400 students from the graduating cohorts of 1976–80, of whom half had been assigned busing.²⁰

In July 2022, sampled individuals were sent a NORC-branded letter asking them to participate in the Jefferson County Study, “an independent, scientific study to understand the views of people like you who attended high school in Jefferson County, Kentucky.” The initial letter included a \$5 bill and promised a \$30 gift card for completing our survey. It also

¹⁸Appendix Table A.3 shows how we go from 177,032 student-year observations to a baseline sample of 32,568 white males.

¹⁹To the degree that some of our sample consists of false positive matches, the true treatment effects are likely to exceed the estimates below.

²⁰Due to limits on our budget, we restricted the survey to cohorts that graduated during the phase-in of busing. This choice helps to minimize the potential influence of long-run trends while ensuring within-alphabet-group variation in treatment status.

Table 1: Match Statistics

Cohort	Number of Students		Any Match			Unique Match		
	Not Bused (1)	Assigned Busing (2)	Not Bused (3)	Assigned Busing (4)	p -value (3) = (4)	Not Bused (5)	Assigned Busing (6)	p -value (5) = (6)
1970	1,553	0	0.900	.	.	0.728	.	.
1971	2,328	0	0.869	.	.	0.631	.	.
1972	2,922	0	0.857	.	.	0.629	.	.
1973	3,027	0	0.861	.	.	0.586	.	.
1974	3,242	0	0.828	.	.	0.546	.	.
1975	4,177	0	0.826	.	.	0.549	.	.
1976	4,031	0	0.820	.	.	0.516	.	.
1977	2,991	635	0.810	0.827	0.337	0.536	0.546	0.629
1978	2,260	1,082	0.809	0.837	0.046	0.529	0.543	0.471
1979	1,270	1,256	0.828	0.831	0.807	0.565	0.557	0.684
1980	554	1,240	0.839	0.852	0.476	0.558	0.587	0.245
Total	28,355	4,213	0.837	0.852	0.002	0.571	0.561	0.040

Notes: Entries are match statistics between yearbook records and voter registration data by graduation year and busing assignment. Columns (1) and (2) show the number of students in each graduating cohort by busing assignment. Columns (3) and (4) refer to cases for which yearbook data and L2 voter registration data contain at least one entry with an approximate match based on first name, last name, and year of birth. Columns (5) and (6) indicate how often we can match a particular student in our yearbook data to exactly one registered voter. For a detailed description of our matching process, see Appendix B.

listed a unique URL, as well as a toll-free telephone number to call and complete the survey by phone with a NORC interviewer. Non-responders to the first appeal were mailed a reminder postcard that, again, included the survey URL, inbound phone number, and repeated the offer of \$30 for completing the survey. Non-responders with a known email address were additionally contacted via email. A second physical mailing included a reminder letter with a self-administered paper questionnaire and a postage-paid return envelope. Finally, in September and October 2022, NORC telephone interviewers called all non-responders with a known phone number and asked them to complete the survey. Sampled individuals were thus contacted up to five times.

In addition to eliciting basic demographic information, our survey contained questions related to six broad areas: (i) political participation and preferences, (ii) trust in government, (iii) views on fairness and inequality, (iv) support for redistribution and other progressive policies, (v) racial prejudice, and (vi) students' high school experience. In designing the relevant questions, we borrowed as much as possible from traditional social science surveys such as the American National Election Survey (ANES), Gallup polls, and the General Social Survey (GSS), as well as from extant work in economics and political science (e.g.,

from Alesina et al. 2018; Henry and Sears 2002 and Tesler 2013).²¹

In total, NORC collected 559 in-scope responses on our behalf, for an overall response rate of 23.3%.²² Of the 559 former JCPS students in our survey sample, 270 had been assigned busing. Comparing individuals in the treatment and control groups, we observe similar response rates (22.5% for treated vs. 24.1% for control).

Descriptive Statistics Table 2 presents descriptive statistics for the set of uniquely matched students in our yearbook sample (upper panel), as well as the respondents to our survey (lower panel). Among the former, nearly 53% still live in Kentucky. Another 22% of individuals in our data live in a closed-primary state other than Kentucky. Closed-primary states require voters to register with a particular party to participate in that party’s primaries. Below, we present results for all states and results restricted to closed-primary states only.

The upper panel of Table 2 further shows that slightly more than one-third of the individuals in our data are currently registered as Democrats, while approximately half are registered Republicans. Relative to a nationally representative sample, Republicans are, therefore, over-represented by double-digit percentage points. Relative to male Kentucky residents in the same age group, however, the share of Republicans in our sample is only about 4 p.p. higher. We also see the familiar pattern of lower turnout in midterm than in presidential elections, with overall participation rates that broadly mirror those among all Americans aged 65 or older.²³

The lower panel of Table 2 presents descriptive statistics for our sample of survey respondents. Reassuringly, the shares of Democrats and Republicans in this sample are very close to those in the yearbook data. So is respondents’ average age. We do note, however, that survey respondents are more likely to still reside in Kentucky.

The next set of variables corresponds to summary indices measuring respondents’ views on redistribution and other progressive policy issues (such as abortion, gun control, and climate change), trust in government, attitudes towards African Americans, and their belief in a just world (i.e., the idea that success is earned rather than attributable to luck). Each index averages standardized answers to several survey questions that relate to the same underlying concept.

Not shown in Table 2, our survey also included questions about students’ high school

²¹For a copy of the survey instrument, see Appendix D.

²²NORC collected 629 responses in total. However, we discovered after data collection that 70 respondents had attended schools that were exempt from the alphabet-based busing plan. These schools were not on the initial exemption list published by *The Courier-Journal*, but were later made exempt through additional redistricting.

²³Note, turnout rates in Table 2 are conditional on being registered to vote, which is why they are significantly higher than common estimates of voter turnout.

Table 2: Descriptive Statistics

Variable	N	Mean	SD	Min	Median	Max
<u>A. Matched Yearbook Sample</u>						
<i>Treatment</i>						
Assigned Busing	18,541	0.127	0.333	0	0	1
<i>Demographics</i>						
Age	18,541	65.009	2.861	60	65	70
Lives in Closed Primary State	18,541	0.753	0.431	0	1	1
Lives in Kentucky	18,541	0.529	0.499	0	1	1
Share Black in Zip Code	18,535	0.102	0.123	0	0.071	0.954
Share Other Minority in Zip Code	18,535	0.151	0.105	-0.000	0.127	0.938
<i>Party Registration</i>						
Democrat	18,541	0.365	0.481	0	0	1
Republican	18,541	0.502	0.500	0	1	1
Independent	18,541	0.133	0.340	0	0	1
<i>Political Participation</i>						
Voted in 2008	18,541	0.717	0.451	0	1	1
Voted in 2010	18,541	0.619	0.486	0	1	1
Voted in 2012	18,541	0.734	0.442	0	1	1
Voted in 2014	18,541	0.630	0.483	0	1	1
Voted in 2016	18,541	0.790	0.407	0	1	1
Voted in 2018	18,541	0.741	0.438	0	1	1
Voted in 2020	18,541	0.871	0.336	0	1	1
<u>B. Survey Respondents</u>						
<i>Demographics & Politics</i>						
Age	559	63.136	1.355	61.000	63.000	65.000
Lives in Kentucky	559	0.751	0.433	0.000	1.000	1.000
Registered Democrat (L2)	559	0.377	0.485	0.000	0.000	1.000
Registered Republican (L2)	559	0.509	0.500	0.000	1.000	1.000
Ideology (Likert Scale)	542	3.245	1.641	1.000	3.000	7.000
Verified Turnout, 2020 (L2)	559	0.905	0.294	0.000	1.000	1.000
Self-Reported Turnout, 2020	557	0.905	0.293	0.000	1.000	1.000
<i>Summary Indices</i>						
Belief in a Just World	537	-0.021	0.624	-1.789	-0.051	1.639
Progressive Policy	495	0.017	0.400	-1.206	0.022	1.351
Racial Prejudice	522	0.012	0.602	-1.492	0.049	1.390
Trust in Government	537	-0.020	0.500	-1.288	-0.108	1.253

Notes: Entries are summary statistics for the most important variables in our analyses. Panel A is based on our yearbook and voter registration data, restricting attention to uniquely matched students. Panel B is based on valid responses to our survey. “Belief in a Just World,” “Progressive Policy,” “Racial Prejudice,” and “Trust in Government” are index variables that average the standardized answers to several related questions. “Ideology” is measured on a seven-point Likert scale, ranging from “very conservative” (1) to “very liberal” (7). See Appendix D for additional details.

Table 3: Treatment Assignment, by Alphabet Group and Cohort

Alphabet Group	Graduating Cohort					
	≤ 1974/75	1975/76	1976/77	1977/78	1978/79	1979/80
A, B, F, Q	Not Bused		Bused			
G, H, L						
C, P, R, X						
M, O, T, U, V, Y						
D, E, N, W, Z						
I, J, K, S						

Notes: Entries show whether white children in a particular cohort and alphabet group were assigned to be bused to an inner-city school for at least one year. Black fields indicate assignment to busing, grey fields imply that the respective set of students was not assigned to be bused.

experience, i.e., their friends, classmates, school environment, and teachers, a real-stakes question (which allowed participants to authorize a donation to either the Black Lives Matter movement or the National Police Foundation), a free response question about formative experiences during high school, as well as a question about perceived bias.²⁴

4. Econometric Strategy

Our identification strategy leverages the quasi-random variation in busing assignments in a difference-in-differences approach. This research design compares within-cohort differences in contemporaneous outcomes between different alphabet groups across cohorts in which the respective groups were and were not subject to busing. Table 3 displays the relevant variation in treatment assignment. For a concrete example, consider students in the 1976/77 graduating cohort. Among the students in this cohort, only children in alphabet group “A, B, F, Q” were bused to inner-city schools. Intuitively, we are asking whether, more than forty years later, the students in this alphabet group have different political outcomes compared to those in other groups; and whether this difference emerges only among cohorts that were, in fact, bused.

To answer this question, we estimate the following regression model:

$$(1) \quad Y_{i,a,c} = \beta \text{Assigned Busing}_{a,c} + \mu_a + \chi_c + \epsilon_{i,a,c},$$

where $Y_{i,a,c}$ denotes the outcome of interest for individual i , who is part of graduating cohort c and whose last name puts him in alphabet group a . $\text{Assigned Busing}_{a,c}$ is an indicator variable equal to one if, and only if, alphabet group a of cohort c was bused, while μ_a and

²⁴Conditional on perceiving bias in the survey, respondents were evenly split about its direction, with 93 reporting a liberal bias and 94 reporting conservative bias.

χ_c are alphabet-group and graduation-year cohort fixed effects, respectively. By including these fixed effects, the specification in eq. (1) controls for any systematic differences between cohorts and alphabet groups. As in a typical difference-in-differences design, the identifying assumption is that, in the absence of busing, mean differences in outcomes among alphabet groups would have remained constant across consecutive cohorts.

Our setting differs from a standard difference-in-differences framework in that we observe outcomes only at a single point in time, long after treatment. We can thus only test for parallel trends by comparing outcomes in cohorts that graduated prior to desegregation. As shown in Appendix Figure A.3, among pre-desegregation cohorts, all six alphabet groups do appear to be on similar trajectories. In fact, with p -values ranging from 0.62 to 0.95, we can neither reject the null hypothesis of no mean differences across alphabet groups (in any cohort) nor that of no differences in trends—consistent with quasi-random assignment to treatment. We also note that if treatment was, indeed, as good as random, then the identifying assumption in our difference-in-differences strategy is trivially satisfied.

Estimand The coefficient of interest in eq. (1) is β . It identifies the impact of being assigned to be bused to an inner-city high school. Since all JCPS schools became racially integrated after the summer of 1975, β does not correspond to the effect of greater cross-racial contact. Only to the extent that the *environment* in which exposure occurs matters do our difference-in-differences estimates capture any impact of cross-racial exposure.

Alternative Estimators A recent econometrics literature has shown that two-way fixed effects (TWFE) models like the one above need not properly aggregate heterogeneous treatment effects (Goodman-Bacon 2021). In particular, when different units adopt treatment at different points in time, then the standard difference-in-differences estimator effectively relies on early adopters as controls for later ones, which can result in treatment effects for early units receiving negative weight. Since our setting is one in which this concern is potentially relevant, we have calculated the relevant TWFE weights for each alphabet-group-cohort (see Appendix Figure A.4). All of them are positive, which ensures that the difference-in-differences model in eq. (1) does recover a convex combination of alphabet group and cohort-specific treatment effects.

We also complement our main results with estimates based on the approaches by Callaway and Sant’Anna (2021) and Cengiz et al. (2019). These alternative estimators do not suffer from the same potential problem as TWFE but are statistically less efficient. Reassuringly, all three approaches yield qualitatively equivalent conclusions.

Statistical Inference To assess statistical uncertainty, we rely on conventional sampling-based standard errors, which we cluster at the cohort-by-alphabet-group level (i.e., at the

level of treatment assignment). An alternative way to conduct inference when assignment to treatment is (as good as) random is to calculate design-based standard errors (Fisher 1935; Abadie et al. 2020). This approach has two benefits. First, it relies on the same variation that is used to identify the treatment effect in order to compute variability in the estimate. Second, since the distribution of treatment assignment is known, computation of p -values and construction of hypothesis tests does not rely up asymptotic approximations.

We follow this alternative approach, assuming exchangeability in the assignment of alphabet groups to the grade levels in which they were bused (see Figure 1). To be clear, this robustness check does not assume that when the courts ordered desegregation was random, nor does it assume that the assignment of last names to alphabet groups was random. It assumes that the *timing* of busing for specific alphabet groups is as good as randomly assigned. Since there were six alphabet groups assigned to six different grade levels, there exists a total of 720 ($= 6!$) possible treatment assignments. Below, we conduct design-based inference by estimating the model in eq. (1) for all 719 counterfactual assignments. We then calculate p -values as the fraction of estimates that are at least as extreme as the actual outcome. Again, this approach yields similar conclusions.

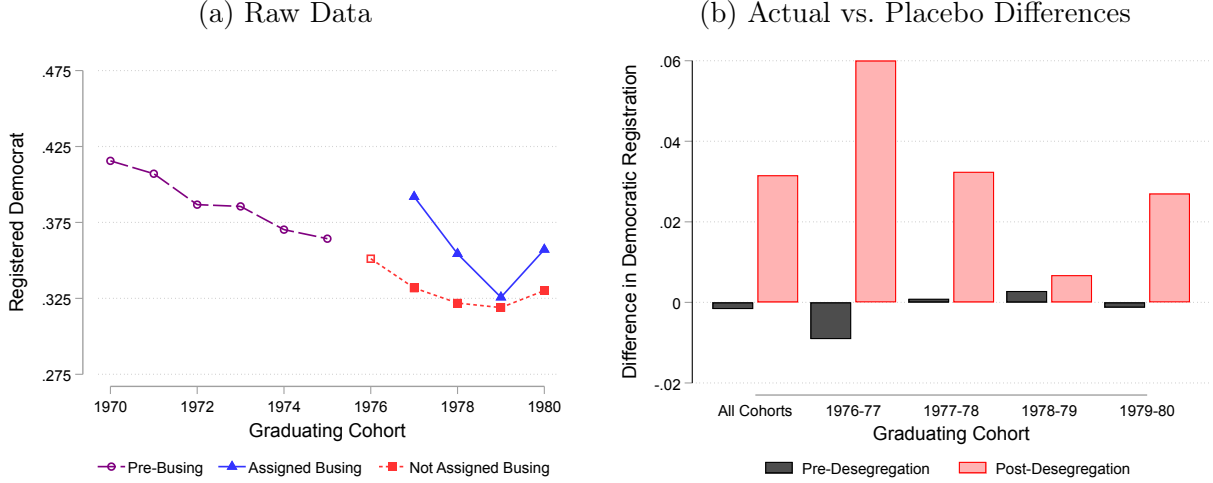
Measurement Error One potential source of bias in our estimates is measurement error due to false positive matches of yearbook data and voter registration records. There is likely a non-negligible number of treated students who are erroneously matched to the voter registration record of someone else who was never bused. There might even be a small number of untreated students that have been erroneously matched to the voter registration record of someone who was actually bused to an inner-city school. Provided that errors in our matching procedure are as good as random, this kind of measurement error biases our estimates toward zero (see, e.g., Meyer and Mittag 2017).

Noncompliance Measurement error notwithstanding, our estimates of β in eq. (1) should be interpreted as intent-to-treat effects (ITT). That is, we estimate the impact of having been *assigned* to be bused to an inner-city school rather than the effect of having actually been bused. In light of anecdotal evidence on “white flight,” according to which some students left the JCPS system either in response to being assigned busing or due to their home schools becoming racially integrated, the ITT need not correspond to the average treatment effect (ATE).²⁵ Both anecdotes as well as the survey results in Section 8 imply that some students moved to local private schools or, occasionally, to other suburban public school districts.

In Appendix E, we provide a sufficient condition on potential outcomes, under which our intent-to-treat estimates bound both the local average treatment effect (LATE) and

²⁵Given the specifics of the court-ordered alphabet plan, there are no “always takers” in our setting. Thus, in the absence of white flight, the ATE corresponds to the effect of treatment on the treated (TOT).

Figure 2: Democratic Party Registration, by Cohort and Busing Assignment



Notes: Panel (a) displays raw means of Democratic party registration by graduating cohort. The dashed purple line shows the share of registered Democrats among all students graduating prior to desegregation. The dashed red line shows the same share for students who were not assigned busing but graduated post-desegregation. The solid blue line shows the share of Democrats among students who were assigned to be bused. In panel (b), red bars display, for each graduating cohort, the raw difference in Democratic party registration between students who were and were not assigned to be bused. Black bars serve as a placebo test by calculating the same difference across alphabet groups but among students who graduated prior to desegregation.

the average treatment effect (ATE). This condition can be interpreted as a monotonicity restriction on the effect of exposure to greater socioeconomic diversity. We show that if the effect of experiencing socioeconomic diversity on the outcome of interest is positive (negative) and monotonic among all students, then the ITT bounds both the LATE and the ATE from below (above). We also outline alternative assumptions under which our intent-to-treat estimates can still be interpreted as bounds on the local average treatment effect (see Appendix E).

5. Effects on Political Participation and Preferences

5.1. Raw Data

Figure 2 provides the first piece of evidence that busing might have affected white students' long-run politico-economic outcomes. The panel on the left of this figure plots the current share of registered Democrats for each cohort in the raw data. For cohorts graduating after the desegregation of schools, the plot distinguishes between students who were and were not assigned to be bused. We observe a strong secular downward trend in Democratic registration among cohorts that graduated prior to the enactment of busing, consistent with white Southerners' exodus from the Democratic Party during this period (see also Kuziemko

and Washington 2018). After 1975, this trend continues without a noticeable break among students who were not assigned to be bused. Taking the raw data at face value and comparing students who were not assigned to be bused with those who graduated prior to 1976, racial integration *per se* appears to have had only minimal effects. Among students who were assigned to be bused, however, school desegregation coincides with an upward jump in Democratic party registration.

The right panel of Figure 2 directly compares the share of registered Democrats across alphabet groups that were and were not assigned to be bused. Light bars correspond to the *observed* differences between treated and untreated alphabet groups in the respective cohorts. Pooling across all post-desegregation cohorts, students that were assigned to busing are, today, about 3.2 percentage points more likely to be registered as Democrats than individuals in the control group. In order to assess whether these differences simply reflect heterogeneity across alphabet groups, we calculate *placebo* differences by applying the busing schedule for a given post-desegregation cohort to students who graduated prior to 1975 (i.e. prior to the enactment of busing). These placebo differences are depicted as dark bars.²⁶ Reassuringly, all placebo estimates are close to zero, suggesting that heterogeneity across alphabet groups cannot explain the observed post-desegregation differences. Nonetheless, in order to more rigorously evaluate the impact of busing on political participation and preferences, we next implement our difference-in-differences approach.

5.2. Voter Registration and Turnout

We first analyze the impact of busing on political participation, as proxied by voter registration and turnout. To measure voter registration, we ask whether a given individual in our yearbook data can be successfully matched either to at least one or to exactly one current voter registration record. The upper panel of Table 4 presents the results. Odd-numbered columns estimate our baseline specification in eq. (1). Even-numbered columns additionally control for alphabet-group-specific linear trends across cohorts. The estimated coefficients range from 0.53 to 0.89 percentage points. With *t*-statistics near or below one, the point estimates in this table are not only quantitatively small but also statistically indistinguishable from zero.

The lower panel of Table 4 investigates whether busing affects turnout conditional on being registered to vote. Irrespective of whether we consider turnout in presidential or midterm elections, or whether we pool across all general elections between 2008–2020, all point estimates are quantitatively small and statistically insignificant. We note, however, that our turnout

²⁶The placebo differences vary across cohorts due to differences in which alphabet groups were assigned to be bused.

Table 4: Effect of Busing on Voter Registration and Turnout

	(1)	(2)	(3)	(4)	(5)	(6)
Voter Registration						
	Any Match		Unique Match			
Assigned Busing	0.85 (0.81)	0.89 (0.96)	0.53 (0.89)	0.83 (1.12)		
Mean of Dep. Var.	83.73	83.73	56.94	56.94		
R-squared	0.005	0.005	0.011	0.011		
Observations	32,568	32,568	32,568	32,568		
Cohort FEs	Yes	Yes	Yes	Yes		
Alphabet-Group FEs	Yes	Yes	Yes	Yes		
Alphabet-Group Linear Trend	No	Yes	No	Yes		
General Election Turnout						
	All Years		Presidential		Midterms	
Assigned Busing	-0.06 (0.91)	-0.43 (1.26)	0.25 (0.81)	-0.24 (1.14)	-0.46 (1.11)	-0.70 (1.51)
Mean of Dep. Var.	72.88	72.88	77.78	77.78	66.34	66.34
R-squared	0.008	0.008	0.006	0.006	0.010	0.010
Observations	129,789	129,789	74,166	74,166	55,623	55,623
Cohort FEs	Yes	Yes	Yes	Yes	Yes	Yes
Alphabet-Group FEs	Yes	Yes	Yes	Yes	Yes	Yes
Alphabet-Group Linear Trend	No	Yes	No	Yes	No	Yes

Notes: Entries are point estimates and standard errors from estimating the difference-in-differences model in eq. (1). The outcomes in the upper panel are indicators for observing at least one matching voter registration record for a particular student (cols. 1 and 2), or exactly one matching record (cols. 3 and 4). The outcome in the lower panel is voter turnout across all general elections from 2008–2020 (cols. 1 and 2), in presidential elections from 2008–2020 (cols. 3 and 4), and midterm elections only from 2010–2018 (cols. 5 and 6). Odd-numbered columns control for cohort and alphabet-group fixed effects, while even-numbered columns add alphabet-group-specific linear trends. All estimates are scaled to correspond to percentage-point changes. In the upper panel, the unit of observation is a student in our yearbook data. In the lower panel, the unit of observation is a student–election year. Since the lower panel’s sample is balanced on election year, the inclusion of election year fixed effects does not change the results. Standard errors are clustered at the cohort-by-alphabet-group level. ***, ** and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

estimates are imprecise, especially in specifications that control for alphabet-group-specific trends. Taken together, the findings in Table 4 suggest that being bused to an inner-city school did not have clear-cut long-run effects on political participation.

5.3. *Party Preferences*

We now return to the impact of busing on partisanship. Since incentives to declare party preferences vary across states and given that L2 imputes party support for voters in states that do not collect information on partisanship at the point of registration, Table 5 presents difference-in-differences estimates for individuals in all states (cols. 1 and 2) and for voters in closed-primary states only (cols. 3 and 4). For the latter, imputation is not a concern. Mirroring the setup of the previous table, odd-numbered columns estimate our baseline specification in eq. (1), while even-numbered ones additionally control for alphabet-group-specific linear time trends.

The range of estimates in Table 5 implies that, among registered voters, busing increases the probability of registering as a Democrat by 3.03 to 3.53 percentage points. This is a large effect. It corresponds roughly to the difference in the shares of registered Republicans and Democrats in Kentucky today. Comparing estimates across panels, the apparent increase in Democratic registration comes largely at the expense of registering as a Republican rather than from a reduction in the share of independents (i.e., the residual category). Out of the eight coefficients in the upper two panels, six are statistically significant at the 5%-level. The two estimates that are not statistically significant are both based on regression models that control for alphabet-group-specific linear trends. Controlling for alphabet-group-specific trends increases standard errors without inducing meaningful changes in the point estimates. In fact, in the top two panels, all coefficients are within 20% of each other. The point estimates are thus remarkably stable.^{27,28}

There are two ways to interpret our findings in Table 5. It is possible that the experience of being bused to an inner-city school induced individuals who would have otherwise registered as Republicans to support the Democratic Party instead. It is also plausible, however, that busing moved all treated students further to the left of the political spectrum. That is, it might have turned counterfactual Republicans into independents and independents into

²⁷The results in Table 5 are based on our sample of uniquely matched yearbook and voter registration records. Appendix Table A.4 shows that these results are qualitatively and quantitatively robust to including individuals that match to multiple or no voter records. For the former set of observations, party affiliation is averaged over all matched voters. For the latter, it is coded as zero.

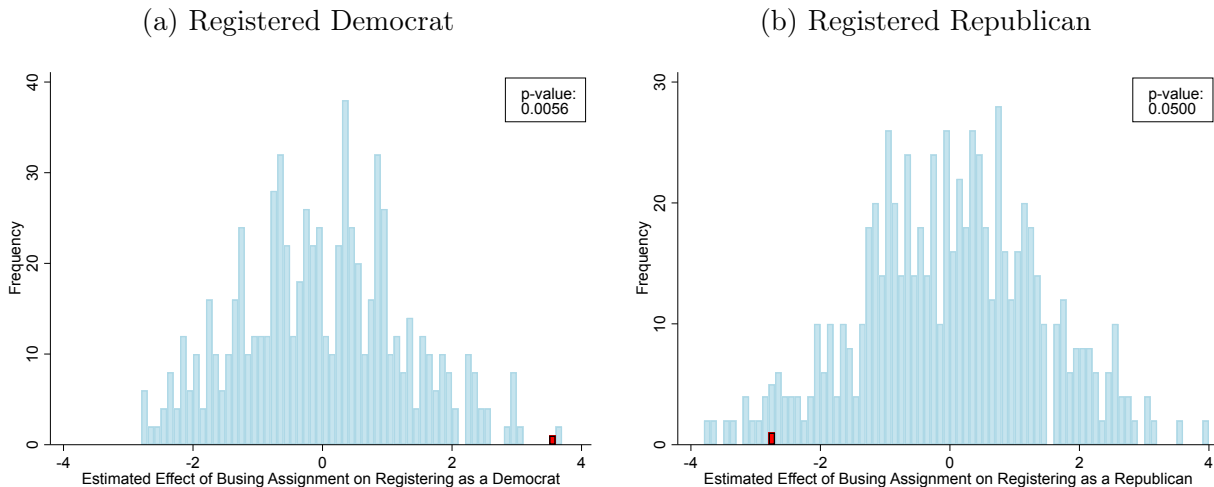
²⁸We have estimated differences-in-differences models that allow for heterogeneity in the treatment effect of busing by destination school. The point estimates range from 2.5 p.p. to 5.6 p.p. but are not statistically distinguishable. While it is certainly true that not all inner-city environments are identical, the destination schools in our setting were all located within 2–5 miles of each other.

Table 5: Effect of Busing on Party Affiliation

	All States		Closed Primary States	
	(1)	(2)	(3)	(4)
Registered Democrat				
Assigned Busing	3.53*** (1.14)	3.43** (1.47)	3.32*** (1.22)	3.03** (1.51)
Mean of Dep. Var.	36.48	36.48	39.02	39.02
R-squared	0.004	0.004	0.004	0.004
Observations	18,541	18,541	13,958	13,958
Registered Republican				
Assigned Busing	-2.77** (1.24)	-2.58* (1.35)	-2.80** (1.22)	-2.33 (1.52)
Mean of Dep. Var.	50.22	50.22	49.15	49.15
R-squared	0.002	0.002	0.003	0.003
Observations	18,541	18,541	13,958	13,958
Independent				
Assigned Busing	-0.76 (1.07)	-0.85 (1.22)	-0.52 (0.99)	-0.70 (1.13)
Mean of Dep. Var.	13.30	13.30	11.82	11.82
R-squared	0.002	0.002	0.001	0.001
Observations	18,541	18,541	13,958	13,958
Cohort FEs	Yes	Yes	Yes	Yes
Alphabet-Group FEs	Yes	Yes	Yes	Yes
Alphabet-Group Linear Trend	No	Yes	No	Yes

Notes: Entries are point estimates and standard errors from estimating the difference-in-differences model in eq. (1). The outcomes in the upper and middle panels are Democratic and Republican party registration, respectively. The outcome in the lower panel is the residual category, i.e., being registered with neither party. Columns (1) and (3) include cohort and alphabet-group fixed effects, while columns (2) and (4) add alphabet-group-specific linear trends. All estimates are scaled to correspond to percentage-point changes. Standard errors are clustered at the cohort-by-alphabet-group level. ***, ** and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

Figure 3: Randomization Inference



Notes: Figure displays results from conducting randomization inference for β in eq. (1) based on all 720 possible treatment assignments from combining different alphabet and grade-level groups. Panel (a) shows the distribution of counterfactual treatment estimates for the probability of registering as a Democrat. Panel (b) does so for the probability of registering as a Republican. The actual estimated treatment effects from Table 5 are highlighted in red. The reported p -values correspond to the share of estimates whose absolute value is weakly greater than that of the true point estimate.

Democrats, leaving the overall share of independents nearly unaffected. Unfortunately, our data do not allow us to distinguish between these two explanations. Either way, the results in Table 5 imply that the experience of being bused permanently changed partisan preferences.²⁹

Randomization Inference As discussed in Section 4, we conduct randomization inference to assess the robustness of our main result. Given that the Louisville desegregation plan implemented busing in a quasi-random fashion, we would expect to see smaller estimated “effects” for alternative, counterfactual treatment assignments. In other words, if the impact of busing on party affiliation is genuine, then the true point estimate should be large relative to estimates from busing assignments that were feasible but not implemented. We evaluate this implication in Figure 3.

There are a total of 720 possible combinations between the six alphabet and grade-level groups based on which white children were assigned busing. Holding fixed the judge’s decision that rising seniors should be exempt and relying on the difference-in-differences model in eq. (1), Figure 3 depicts the distribution of estimated coefficients for each of the feasible assignments. The outcome in the left panel is Democratic party registration. Of all 720 possible coefficients, only four are weakly larger (in absolute value) than the actual estimate

²⁹In Appendix Table A.5, we differentiate between students who were bused for one versus two years. The estimated effects on partisanship are statistically indistinguishable from each other, suggesting that the findings above are driven by the extensive rather than the intensive margin.

in col. (1) of Table 5, yielding a two-sided p -value of 0.006. The right panel of Figure 3 repeats the exercise for Republican registration. Although the estimated effect on Republican registration is less of an outlier than that on Democratic registration, it does exceed 95% of coefficients from counterfactual treatment assignments. Randomization inference, therefore, provides support for our main result.³⁰

Robustness Checks Appendix Table A.6 presents a number of additional robustness checks. The first column of this table shows estimated effects on party preferences without controlling for alphabet-group fixed effects. The point estimate for Democratic party registration decreases by about half a percentage point but remains statistically significant. The coefficients for Republicans and independents are more sensitive. Without accounting for alphabet-group fixed effects, both roughly equalize, with neither being statistically significant.

In addition, Appendix Table A.6 presents results from specifications that add school and state-of-residence fixed effects to the baseline model in eq. (1). School fixed effects are constructed based on the school that a student attended during the 1974/75 school year, i.e., prior to the court-ordered desegregation plan being announced. They account for systematic differences across students due to selection into schools. State-of-residence fixed effects control for local culture, supply of political candidates, and state laws where individuals are registered to vote, all of which could plausibly influence party registration (Cantoni and Pons 2022). We note, however, that busing might have affected whether individuals move out of state, thus rendering state of residence endogenous. In any case, results controlling for school and state fixed effects are qualitatively and quantitatively equivalent to those in Table 5 above.

Since standard difference-in-differences estimators need not properly aggregate treatment effects in the presence of heterogeneity across cohorts, we also conduct robustness checks that instead rely on the estimator of Callaway and Sant’Anna (2021) and the stacked differences-in-differences estimator of Cengiz et al. (2019). Both yield qualitatively similar results, which are presented in Appendix Table A.7.

Placebo Estimates The key identifying assumption in our setting is that, if it had not been for busing, changes in the party preferences of treated alphabet groups would have mirrored changes in the control group. This assumption is fundamentally untestable. We can, however, provide ancillary supporting evidence by exploiting the fact that some Louisville-area public schools had been exempt from busing.

As explained in Section 2, some schools were not subject to court-ordered busing because

³⁰For completeness, Appendix Figure A.5 presents results from conducting randomization inference for independents. Consistent with the evidence in Table 5, randomization inference produces a p -value of 0.625.

Table 6: Exempt School Placebo

	(1)	(2)	(3)	(4)	(5)	(6)
	Party Registration					
	Democrat		Republican		Independent	
Actually Assigned Busing	3.70*** (1.13)	3.07** (1.45)	-3.03*** (1.10)	-2.26** (1.11)	-0.68 (1.04)	-0.81 (1.17)
Placebo Assignment	-0.94 (1.54)	-1.60 (2.00)	0.32 (2.19)	1.24 (2.50)	0.62 (1.52)	0.37 (1.74)
Exempt School	3.32*** (0.83)	3.34*** (0.83)	-2.15*** (0.78)	-2.17*** (0.78)	-1.18** (0.51)	-1.17** (0.52)
Mean of Dep. Var.	37.18	37.18	49.64	49.64	13.18	13.18
R-squared	0.004	0.004	0.002	0.002	0.002	0.002
Observations	23,481	23,481	23,481	23,481	23,481	23,481
Cohort FEs	Yes	Yes	Yes	Yes	Yes	Yes
Alphabet-Group FEs	Yes	Yes	Yes	Yes	Yes	Yes
Alphabet-Group Linear Trend	No	Yes	No	Yes	No	Yes

Notes: Entries are point estimates and standard errors from estimating the difference-in-differences model in eq. (2). The outcomes are Democratic party registration (cols. 1 and 2), Republican party registration (cols. 3 and 4), and the residual category, i.e., being registered with neither party (cols. 5 and 6). Odd-numbered columns include cohort and alphabet-group fixed effects, while even-numbered columns add alphabet-group-specific linear trends. All estimates are scaled to correspond to percentage-point changes. Standard errors are clustered at the cohort-by-alphabet-group level. ***, ** and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

they were deemed to be already racially integrated or because desegregation could be more easily achieved via rezoning. The results above exclude all students who were either already attending or were scheduled to attend an exempt school when busing went into effect. We now incorporate these individuals into a placebo analysis.

Intuitively, our placebo analysis asks whether students who, given their last names, would have been assigned to be bused had they not attended an exempt school experienced similar shifts in party preferences (relative to the control group) as students who were actually assigned to be bused. More formally, we estimate the following econometric model:

$$\begin{aligned}
 (2) \quad Y_{i,a,c} = & \beta \textit{Actually Assigned Busing}_{i,a,c} + \delta \textit{Placebo Assignment}_{i,a,c} \\
 & + \gamma \textit{Exempt School}_i + \mu_a + \chi_c + \epsilon_{i,a,c},
 \end{aligned}$$

where $\textit{Exempt School}_i$ is an indicator for whether individual i attended or was scheduled to attend a school that was not subject to the court-ordered busing plan. $\textit{Placebo Assignment}_{i,a,c}$

denotes an indicator that is equal to one if, and only, if i attended such a school and, given his last name, would have been assigned to be bused otherwise. *Actually Assigned Busing* $_{i,a,c}$ denotes the assignment status of students in non-exempt schools. All other symbols are as defined above.

Results from estimating this specification are presented in Table 6. We first note that those who attended exempt schools are more likely to be registered Democrats and less likely to identify as Republicans or independents. This observation is consistent with cross-racial contact inducing more left-leaning attitudes. It is also consistent with selection into exempt schools. That is, students who attended schools that were already racially integrated prior to the summer of 1975 might have come from more progressive families and thus be more likely to identify as Democrats today. Given that the raw data in Figure 2 do not show a break from trend when non-exempt schools become racially integrated, we interpret $\hat{\gamma}$ as picking up selection effects rather than a genuine impact of exposure to African Americans.

Second, we note that the results in Tables 5 and 6 yield qualitatively equivalent conclusions about the effect of actually being assigned busing. The relevant point estimates are not exactly identical because the inclusion of additional observations impacts the estimated fixed effects and trends.

Third, estimates of the placebo effect of busing ($\hat{\delta}$) range from -1.60 to 1.24 percentage points and are statistically indistinguishable from zero. In fact, all of the placebo estimates are smaller than and differ in sign from the actual effect estimates. Thus, if differential trends across alphabet groups are driving our main result, they are not present in exempt schools.

6. Survey Evidence

We now present evidence on the impact of busing on a broader set of outcomes. As explained above, we contracted with NORC at the University of Chicago to survey a random sample of individuals from the 1976–80 graduating cohorts. We begin by using the survey data to validate the impact of busing on partisan preferences. We supplement these results by estimating long-run effects on more comprehensive measures of ideology and attitudes. Finally, we use the survey to help shed light on the mechanisms that might explain our main findings.

To guard against drawing incorrect conclusions from naïvely testing multiple hypotheses, we report q - in addition to p -values (Storey 2003; Anderson 2008). By controlling the false discovery rate, q -values generalize the notion of a type-I error (false positive) to settings in which multiple hypotheses are tested simultaneously. Intuitively, q -values are constructed such that among related tests with a q -value of 0.05 or less, approximately 5% end up being false positives.³¹

³¹ q -values differ from the family-wise error rate (FWER) in that the latter controls the probability of

6.1. *Political Participation and Partisanship*

To replicate our results on political participation and partisan identification, we estimate our baseline model in eq. (1) on the corresponding variables in the survey data. Appendix Table A.9 shows the results. Mirroring our findings in the matched yearbook data, we continue to find a quantitatively small and statistically insignificant effect on voter turnout. Results on partisanship are also similar to those in Table 5 above. We note, however, that although the survey-based point estimates are, if anything, larger than those based on administrative voter registration data, due to the small size of our survey, they are statistically insignificant. We additionally estimate the impact of busing on self-reported ideology, as measured on a 7-point Likert scale ranging from “very conservative” (1) to “very liberal” (7). Again, treated individuals place themselves about 0.27 standard deviations further to the left of the political spectrum, though this difference is not statistically significant ($p = 0.08$, $q = 0.16$).

In contrast to our survey results on self-reported partisanship and ideology, we do find statistically significant effects on candidate support. When asked about their preference over candidates in the 2020 and 2012 presidential elections, bused individuals are much more likely to report having supported Joe Biden ($p < 0.01$, $q = 0.01$) and Barack Obama ($p = 0.02$, $q = 0.08$). Part of the reason why the estimated effect on candidate support in the 2020 presidential election is statistically distinguishable from zero—even after adjusting for multiple hypothesis testing—is that it is very large. In fact, it exceeds our party registration estimates in the previous section by a factor of about six (cf. Appendix Table A.9 and Table 5). One potential explanation for this apparent discrepancy is survey bias, i.e., people falsely report their political preferences in surveys but not when they register to vote. A second possibility is that administrative voter registration data are stale, which may lead us to underestimate the true long-run effect of busing on party preferences. A third explanation is that our estimates for the 2020 presidential election are subject to a “Trump effect.” We find evidence against all three of these explanations.

Appendix Table A.9 shows that when survey respondents are asked whether they consider themselves Democrats, Republicans, or independents, the estimated effect of busing lines up fairly closely with the coefficients in Table 5. The fact that we obtain quantitatively similar results on this alternative measure of political preferences leads us to discount explanations based on survey bias or stale administrative data. Moreover, we observe similar point estimates for both the 2012 and 2020 presidential elections, which makes it difficult for a “Trump effect” to explain the estimated impact of busing on candidate support. In our view, the most

making one or more type-I errors, whereas the former controls the false discovery rate (FDR), i.e., the proportion of false positives among the significant results. q -values, thus, tend to be more conservative than p -values but less conservative than, say, the Bonferroni correction, which controls the family-wise error rate.

likely reason for the large estimated effect on preferences over candidates is that candidate support is more malleable than party identification.³²

6.2. *Ideology and Attitudes*

We next turn to the effect of being bused to an inner-city school on different measures of ideology and attitudes. To provide a high-level overview of our findings, we combine related survey questions into summary indices for (i) racial prejudice, (ii) trust in government, (iii) support for redistribution and other progressive policies, and (iv) belief in a “just world” (i.e., the idea that success is earned rather than attributable to luck). Each index is constructed by first standardizing and then averaging over the constituent variables.³³

Figure 4 displays the estimated effect of busing on each index. It also shows 90%- and 95%-confidence intervals, as well as q -values. The latter account for multiple hypothesis testing.

Racial Prejudice We do not detect an effect of busing on self-reported attitudes towards African Americans. We arrive at this result irrespective of whether we combine all nine race-related questions on our survey into one composite index or if we focus solely on the four items that make up the Racial Resentment Scale in recent versions of the ANES. Although the respective point estimates are imprecise, they are close to zero. Analyzing the component questions of our racial attitudes index in isolation, we find that only one out of nine shows a statistically significant difference (see Appendix Table A.9). After adjusting for multiple hypothesis testing, none of the observed differences are statistically distinguishable from zero.

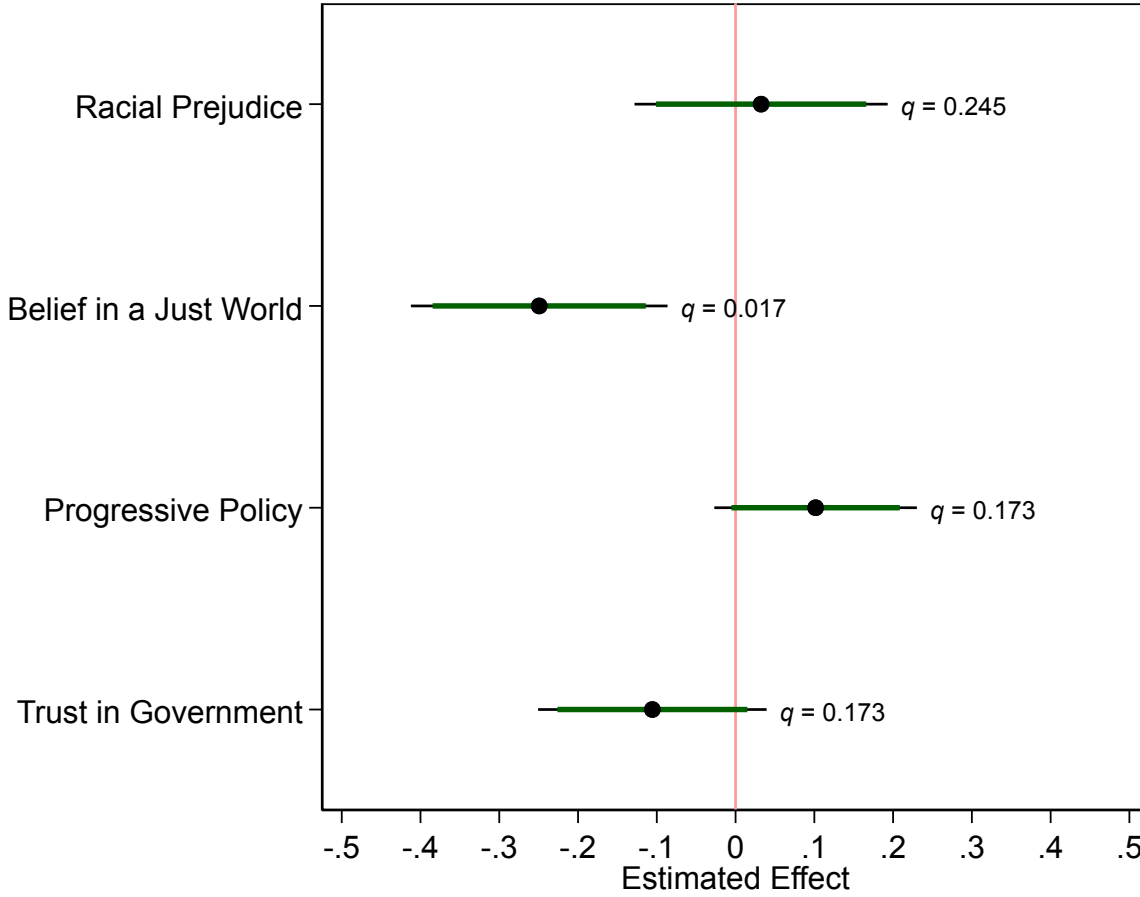
The apparent lack of effect on self-reported attitudes may or may not be surprising. On the one hand, once the court-ordered desegregation plan was implemented, all Louisville-area public school students encountered racially integrated school environments, not only those who were bused.³⁴ On the other hand, the scope conditions in Allport’s (1954) contact hypothesis suggest that exposure to African Americans in an inner-city environment might be especially effective at reducing bias. In Section 7, we return to the issue of racial attitudes by analyzing neighborhood racial diversity as a revealed-preference measure of prejudice. For now, we note that there are no apparent differences in the willingness of treated and untreated individuals to indicate agreement with controversial views about African Americans.

³²In line with this explanation, we observe especially high estimated treatment effects among independents.

³³In Appendix Tables A.9 and A.10, we analyze the answers to each survey question individually.

³⁴Consistent with this view, we do not detect any effect of busing on answers to the real-stakes question on our survey, which allowed the respondent to authorize a donation to either the Black Lives Matter movement or the National Police Foundation.

Figure 4: Impact of Treatment on Summary Indices



Notes: Figure displays results from estimating eq. (1), using the summary indices from our survey as outcomes. Each index is constructed as the average of the standardized values of the component variables. Thick green lines show 90%-confidence intervals based on the respective estimate's asymptotic distribution, accounting for clustering at the cohort-by-alphabet-group level. Thin black lines correspond to 95%-confidence intervals. Alongside each point estimate and confidence interval, we report the respective q -value. The q -values in this figure account for testing a total of four separate null hypotheses, i.e., that the treatment effect on a particular index is zero. Appendix Table A.11 presents the results above in numerical form. For results based on the individual component questions, see Appendix Tables A.9 and A.10.

Belief in a Just World By contrast, we find that having been bused to an inner-city school affects individuals' perceptions of inequality and fairness, as measured by the Belief in a Just World Scale (Lipkus 1991). Individuals who were assigned to be bused score approximately 0.25 units—or about 0.4 standard deviations—lower on this commonly used scale ($p < 0.01$, $q = 0.02$). That is, they are far less likely to say that the world is fair in the sense that “people get what they deserve and deserve what they get” (Lerner and Simmons 1966, p. 66). Inspecting the seven survey items that together make up the index, all but one of them have the same sign, and four of them are statistically significant at the 5%-level—even after accounting for multiple hypothesis testing (see Appendix Table A.9).

To put the estimated effect on subjects’ belief in a just world into better perspective, we note that the mean difference between self-reported Democrats and Republicans in our sample is approximately 0.07 index units, or about 0.12 standard deviations. The point estimate in Figure 4, therefore, corresponds to about 350% of the mean partisan difference—albeit this difference is relatively modest. Even the lower end of the estimated 95%-confidence interval corresponds to 120% of the difference between Democrats and Republicans. For comparison, Kuziemko et al. (2015) find that providing survey respondents with information on the income distribution, the link between top income tax rates and economic growth, and the estate tax bridges about 36% of the partisan gap in concerns about inequality.

Support for Progressive Policies Consistent with the idea that bused individuals are less likely to perceive economic inequality as just, Figure 4 provides some evidence to suggest that they also become more supportive of redistribution and other progressive policies. We emphasize, however, that although the estimated effect on our progressiveness index is nontrivial in size—about 0.1 index units, or about 0.25 standard deviations—it is not statistically significant at conventional levels ($p = 0.117$, $q = 0.173$). Considering each of the twelve component questions individually, Appendix Table A.10 shows treated individuals moving to the left on eight of them. Considering only questions that directly relate to redistribution (rather than other progressive issues, such as abortion, climate change, or gun control), we find that treated individuals are more supportive of redistributive policies and institutions in four out of six cases. However, only in the case of support for labor unions are we able to statistically reject the null hypothesis of no effect ($p < 0.01$, $q = 0.06$).

Trust in Government Lastly, Figure 4 provides some suggestive evidence that busing might have reduced individuals’ trust in the federal government. While the point estimate is large—about 0.2 standard deviations—and consistent with anecdotes according to which the forced desegregation of schools was perceived as federal overreach, the relevant coefficient is, again, not statistically distinguishable from zero ($p = 0.147$, $q = 0.173$).³⁵ However, distrust of government institutions might explain why heightened concerns about fairness and economic inequality translate into greater support for unions but not for redistributive policies like a federal \$15 minimum wage or higher estate taxes (cf. Appendix Table A.10; see also Sapienza and Zingales 2012 and Kuziemko et al. 2015).

³⁵The fact that the q -value for this index matches that of the progressive policy index—despite differences in point estimates and p -values—is explained by the “monotonicity adjustment” in the computation of q -values. An additional test with a slightly higher p -value cannot decrease the false discovery rate; but an additional discovery with a marginally higher p -value cannot increase it either (see Anderson 2008).

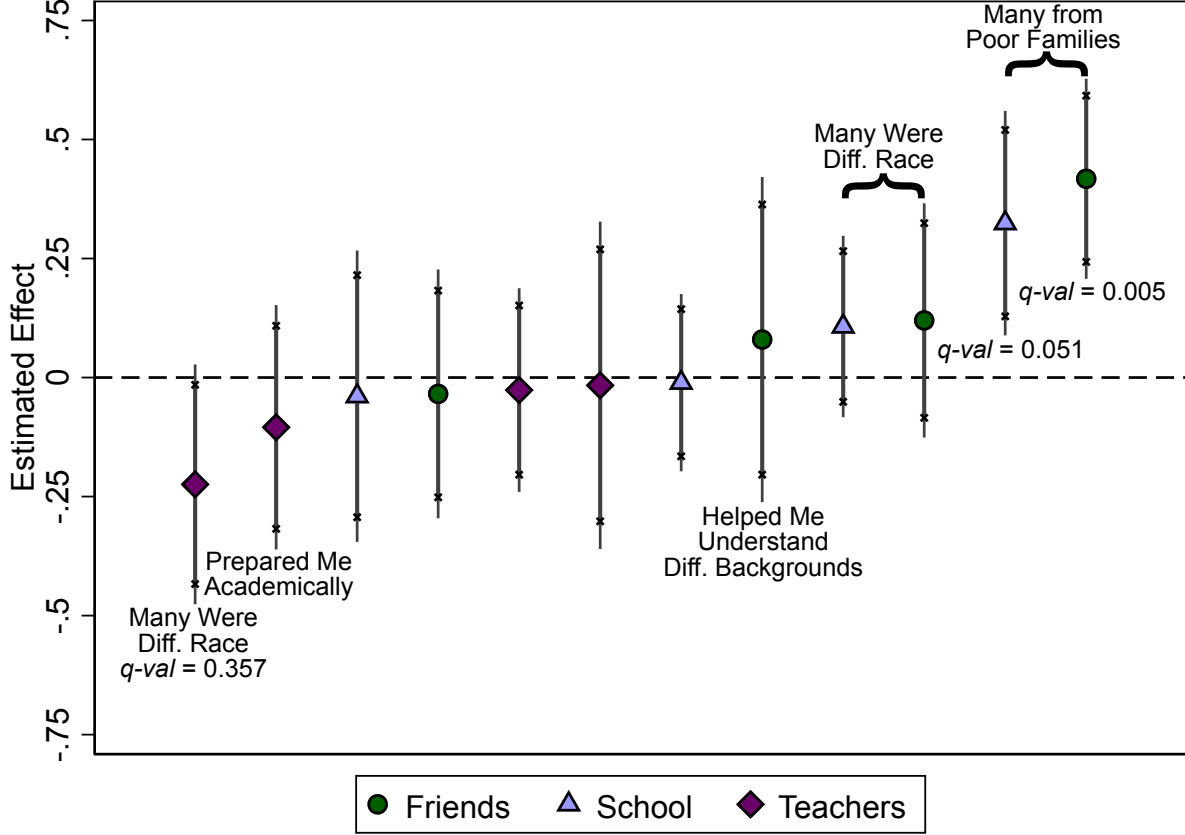
6.3. *Potential Mechanisms*

We next investigate potential mechanisms through which busing might have impacted political preferences and attitudes. Tuttle (2019) studies the economic consequences of the Louisville busing plan. He shows that assignment to busing did not affect the economic well-being of white students. In Appendix Table A.12, we validate this result for our linked yearbook-L2 sample by estimating our workhorse difference-in-differences model in eq. (1) with zip-code-level average incomes as the outcome. Today, students who were assigned to be bused live in areas with similar average incomes as their counterparts who were not assigned busing. We also show that there are no differences in the partisan composition of the neighborhoods in which treated and untreated individuals choose to live. In light of these findings, we discount both present-day peer effects as well as the direct economic impact of being bused to an inner-city school as explanations for the results above.

In order to explore other possible mechanisms, we again draw on our survey. On a conceptual level, being bused to an inner-city school might have affected who students befriend, how they perceive and interact with their teachers, or how they experience their school environment. For each of these channels, we asked four questions. We asked survey-takers whether they made most of their friends at school, whether a substantial fraction of their friends were of a different race, whether a substantial fraction were poor, and whether their friends helped them to better understand people from different parts of society. As for teachers, we asked whether respondents were impressed by their teachers, whether their teachers prepared them academically for life after high school, whether their teachers were of a different race, and whether their teachers helped them better understand people from different parts of society. Finally, to elicit individuals' perceptions of their school environment, we asked whether they felt safe at school, whether a substantial fraction of students were poor, or of a different race, and whether attending high school in a different part of town aided their understanding of others.

Figure 5 presents results from estimating our baseline model with the answers to these questions as outcomes. We arrange the twelve estimates in order of effect size. Overall, out of the twelve point estimates, two are statistically significant after adjusting for multiple hypothesis testing. Individuals who were assigned to be bused are about 0.3 standard deviations more likely to agree with the statement that a substantial fraction of students at their school came from poor families, and they are 0.4 standard deviations more likely to agree that a significant fraction of their friends were poor. If the estimates across all twelve questions were statistically independent, then the probability that the two questions concerning poverty would have the largest values is about 1.5% ($= \frac{10!2!}{12!}$). Based on the evidence in Figure 5, we conclude that busing increased individuals' awareness of socioeconomic differences.

Figure 5: Survey Evidence on Potential Mechanisms



Notes: Figure displays results from estimating eq. (1), using our survey questions on friends, school, and teachers as outcomes. Point estimates are color-coded based on these three groups. All coefficients are scaled based on the standard deviation of the respective answers in the control group. Thick solid lines show 90%-confidence intervals based on the respective estimate's asymptotic distribution, accounting for clustering at the cohort-by-alphabet-group level. Thin solid lines correspond to 95%-confidence intervals. q -values are calculated separately for each of the three categories of outcomes. For a numerical presentation of the results above, see Appendix Table A.11.

To corroborate this finding, we turn to the free-response question on our survey: "Thinking back to your high-school days, which experiences have had the largest impact on who you are today?" Due to their limited structure, open-ended questions like this one are useful for measuring which experiences were especially salient (Haaland et al. 2024). From the answers to this question, we create four indicator variables for whether an individual's response contains the stem (i) "bus," (ii) "teach," (iii) "friend," or (iv) the roots "poor" or "poverty." We then use these indicators as outcomes in our baseline difference-in-differences model. Table 7 presents the results.

Col. (1) of Table 7 verifies that busing itself was salient. Despite the inflow of bused minority students into suburban schools, students who were assigned to be bused were about

Table 7: Evidence from an Open-Ended Question

	(1)	(2)	(3)	(4)
	Mention Busing	Mention Teachers	Mention Friends	Mention Poverty
Assigned Busing	17.36*** (4.16) [0.002]	3.29 (6.38) [0.180]	8.69** (3.45) [0.022]	3.92** (1.61) [0.022]
Mean of Dep. Var.	13.65	14.60	9.68	1.43
R-squared	0.077	0.035	0.038	0.022
Observations	559	559	559	559
Cohort FEs	Yes	Yes	Yes	Yes
Alphabet-Group FEs	Yes	Yes	Yes	Yes

Notes: Entries are point estimates, standard errors, and q -values (in brackets) from estimating the difference-in-differences model in eq. (1). The q -values account for testing the null of no effect for all four outcomes in this table. The outcome variables are based on answers to the free-response question, “Thinking back to your high-school days, which experiences have had the largest impact on who you are today?” Col. (1) indicates use of the stem “bus*”; col. (2), the stem “teach*”; col. (3), the stem “friend*”; and col. (4), the stem “poor*” or “poverty.” All estimates are scaled to correspond to percentage-point changes. Standard errors are clustered at the cohort-by-alphabet-group level. ***, ** and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

17 percentage points more likely to mention busing than those who were not ($p < 0.01$, $q < 0.01$). Given a control-group mean of 14 percent, this is a very large effect, consistent with the idea that being bused to an inner-city school mattered for reasons other than racial mixing.

Mirroring our findings for the structured questions about friends, teachers, and the school environment, cols. (2)–(4) show no impact of busing on mentioning teachers but do show effects on mentioning friends and poverty. The former effect is estimated to be 9 percentage points ($p = 0.02$, $q = 0.02$), while the latter one equals 4 percentage points ($p = 0.02$, $q = 0.02$). Again, relative to the respective means both estimates are quantitatively very large.

We can only speculate as to why exactly poverty became more salient among students who were bused to inner-city schools—after all, following the summer of 1975, all white Louisville-area students experienced greater exposure to poorer minority peers. One possibility is that bused students encountered a larger share of economically disadvantaged non-minority peers. As shown in Section 8, a non-trivial share of students opted for private schools rather than adhere to their busing assignments. If noncompliers were disproportionately affluent, then the classmates of bused students would have been relatively economically disadvantaged. A

second possibility is that bused students made new friends at their destination schools. If friendship networks were less segregated by social class than in students' suburban home schools, then this might explain why bused students are more likely to say that their friends were poor. A third possibility is that the impoverished environment at the destination schools affected students' *perceptions* of economic disparities irrespective of changes in the actual composition of their peer group.

Either way, the overall picture that emerges is that poverty in their school environment and among their group of friends left a lasting impression on bused students. In fact, this mechanism is vividly illustrated in the responses of several survey participants:

“Going to high school in an economically depressed area of the city made a big impression on me. [...] I realized that the people in these areas were just like me except they did not have the same resources that I had.”

“Being bused to an inner city school exposed me to people and situations I wouldn't have otherwise experienced.”

“During busing, I got to meet a lot of kids from other parts of town, some poor and of different races.”

“I was bussed to an inner city school from the outside county [...]. It showed me how disadvantaged the inner schools were as far as [the] level of education [was concerned].”

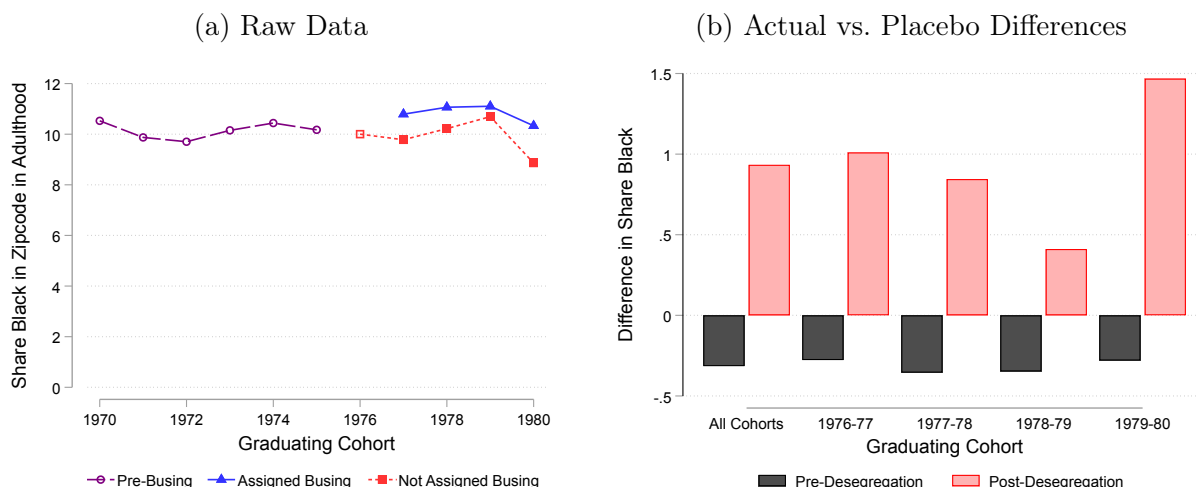
Almost half a century later, bused individuals are more skeptical of the world being just (i.e., that success is earned rather than due to luck), and they are more likely to approve of unions and, perhaps, other forms of pre- and redistribution (Bosio et al. 2024; Kuziemko et al. 2023). In line with this apparent shift in ideological outlook, they are also more likely to support the Democratic Party and its candidates. More broadly, our findings suggest that witnessing economic deprivation at a young age durably shapes political attitudes and beliefs.

7. Effects on Neighborhood Racial Diversity

We now return to the question of how busing affected racial preferences. Given well-known issues with extant survey measures of racial prejudice (see, e.g., Sniderman and Tetlock 1986; Feldman and Huddy 2005; Carney and Enos 2017), we complement the survey results in the previous section with evidence on neighborhood racial diversity as a revealed-preference measure of race-related attitudes.

Although treated and untreated individuals live in areas with comparable incomes (see Tuttle 2019 and Appendix Table A.12), we find clear evidence that the former tend to choose neighborhoods with a higher share of African Americans. Figure 6 illustrates this pattern based on the raw data. The panel on the left of this figure shows similar zip-code-level shares of African Americans among all cohorts that graduated prior to desegregation

Figure 6: Zip-Code-Level Share of African Americans, by Cohort and Busing Assignment



Notes: Panel (a) shows the average zip-code-level share of African Americans based on individuals' residential address in 2021, by graduating cohort and treatment assignment. The dashed purple line refers to students graduating prior to desegregation. The dashed red line refers to students who graduated after 1975 but were not assigned to be bused. The solid blue line refers to students who were assigned to be bused. In panel (b), red bars display, for each graduating cohort, the raw difference in the zip-code-level share of African Americans between students who were and were not assigned to be bused. Black bars serve as a placebo test by calculating the same difference across alphabet groups among cohorts that graduated prior to desegregation.

and among students who graduated after 1975 but were not assigned to be bused. Among bused students, however, the zip-code-level share of African Americans jumps by nearly 1 p.p., or roughly 10% of the mean.³⁶

The right panel of Figure 6 compares the actual differences across alphabet groups that were and were not assigned to be bused (light bars) with placebo differences from applying the busing schedule for a given post-desegregation cohort to students who graduated prior to 1975 (dark bars). The fact that all placebo differences are negative suggests that unobserved heterogeneity across alphabet groups is unlikely to explain why, today, bused students live in areas with a higher proportion of African Americans.

Table 8 presents difference-in-differences estimates of the effect of busing on neighborhood diversity. The estimate in the first column confirms that the patterns in the previous figure are unlikely to be due to chance ($p < 0.01$). The specification in the second column additionally controls for zip-code-level incomes. The evidence therein implies that the higher share of African Americans cannot be explained by treated students choosing to live in poorer neighborhoods. In the third column, we introduce alphabet-group linear trends to control

³⁶As of the 2020 census, 14.2% of Americans reported being African American. Since African Americans live disproportionately in urban areas, the share of African Americans in the average zip-code in our sample is lower than the share of African Americans nationally.

Table 8: Effect of Busing on Residential Diversity

	Share Black			Share Other Minority		
	(1)	(2)	(3)	(4)	(5)	(6)
Assigned Busing	1.18*** (0.31)	1.10*** (0.33)	1.14** (0.45)	−0.17 (0.26)	−0.15 (0.25)	−0.43* (0.23)
Zip Code Income (\$k)		−0.09*** (0.01)	−0.09*** (0.01)		0.03*** (0.00)	0.03*** (0.00)
Mean of Dep. Var.	10.18	10.18	10.18	15.10	15.10	15.10
SD of Dep. Var.	12.33	12.33	12.33	10.46	10.46	10.46
R-squared	0.001	0.047	0.047	0.001	0.007	0.007
Observations	18,535	18,535	18,535	18,535	18,535	18,535
Cohort FEs	Yes	Yes	Yes	Yes	Yes	Yes
Alphabet-Group FEs	Yes	Yes	Yes	Yes	Yes	Yes
Alphabet-Group Linear Trend	No	No	Yes	No	No	Yes

Notes: Entries are point estimates and standard errors from estimating the difference-in-differences model in eq. (1). The outcomes are based on zip-code-level racial shares from the 2020 Decennial Census. Columns (1)–(3) examine the share of African Americans in the zip-code, whereas columns (4)–(6) examine the share of other minorities in the zip-code. Share of other minorities is calculated by subtracting the zip-level share black and share white from one hundred. Columns (1) and (4) include cohort and alphabet-group fixed effects, columns (2) and (5) control for zip-level total income per person, and columns (3) and (6) add alphabet-group-specific linear trends. All estimates are scaled to correspond to percentage-point changes. Standard errors are clustered at the cohort-by-alphabet-group level. ***, ** and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

for time-varying unobserved differences across groups.

The remaining three columns of Table 8 extend our analysis to the share of other minorities in individuals’ zip codes. For this outcome, however, we find no impact of busing. That is, being assigned to be bused appears to have made students more willing to live in areas with a higher share of African Americans, but has had no appreciable effect on the proportion of other minorities.³⁷

One way to reconcile our results on neighborhood choice with the apparent null effect on self-reported racial attitudes is to note that the questions on our survey measure individuals’ willingness to *admit* to holding controversial views about African Americans. If respondents are simply unwilling to make such admissions, then we should not expect to detect any effect—even if busing did, in fact, reduce bias. To assess the plausibility of this explanation, we ask how many respondents report agreement with controversial views related to race. On eight out of nine survey items, more than thirty percent of respondents say that they either

³⁷Appendix Figure A.6 and Appendix Table A.13 confirm that randomization inference and exempt-school placebo estimates yield qualitatively identical conclusions.

“somewhat agree” or “strongly agree” with the relevant statement.

Another way to reconcile the two sets of results is to note that busing did not affect *whether* but *where* students experienced exposure to minority peers. Interacting with African Americans in an inner-city environment rather than a more familiar suburban setting may only be enough to reduce mild but not blatant or overt forms of racism. The fact that we do observe an effect of busing on choosing to live near African Americans (but not other minorities) suggests that exposure to black, inner-city neighborhoods may have had at least some effect on treated students’ racial attitudes in the long run.

A third potential reconciliation is to argue that neighborhood racial diversity reflects preferences and attitudes that are related to but ultimately distinct from racial prejudice and resentment. Consistent with this view, busing did differentially expose students to racially diverse *areas*. It did not differentially expose students to more racially diverse *peers*. More than forty years later, those who were exposed to an inner-city, predominantly African-American environment choose to live in more diverse areas than those who were not—even if both groups’ stated views on race do not differ.

Finally, we note that the confidence interval for the estimated effect on self-reported prejudice is very wide. Although the point estimate in Figure 4 is close to zero, the survey does not afford us enough statistical power to rule out meaningfully large effects in either direction.

8. Compliance, White Flight, and Persuasion Rates

8.1. *Compliance and White Flight*

As noted earlier, our estimates should be interpreted as intent-to-treat effects. There are two kinds of non-compliance in our setting. Some treated students might have refused their busing assignment and simply stayed in their home schools. In addition, both treated- and control-group students might have left the JCPS system. We next use our survey, as well as high school yearbook entries and graduation lists to bound the degree of compliance in the treatment group. We then use these bounds to compute bounds on persuasion rates.

We first draw on a survey question asking respondents to list the high schools they attended.³⁸ Relying on online searches for school names, we classify all reported schools as either “city schools” (i.e., schools that were formerly part of the Louisville Independent School Districts), “county schools” (which, prior to the merger, were part of the Jefferson County school system), public schools outside of JCPS, or private schools. We then create indicator variables for (i) reporting only county schools, (ii) at least one city school, (iii) at least one private school, and (iv) at least one public school in another county. Estimating our

³⁸As can be seen in Appendix D, the survey instrument leaves room to report multiple high schools, and about 29% of respondents list more than one.

difference-in-differences model in eq. (1) with these variables as outcomes, we find that being assigned busing decreases the probability of reporting only county schools by 39 percentage points. Correspondingly, being assigned to be bused increases the probability of reporting a city school by 30 percentage points, the probability of reporting a private school by 4 percentage points, and that of reporting a public school in another county by 5 percentage points (see Appendix Table A.15).

Although all but one of these point estimates are statistically significant at either the 5%- or 1%-levels, we note that this approach will tend to understate the true rate of compliance if survey-takers fail to accurately report all the high schools that they attended. This problem is especially severe if respondents in the treatment group are more likely to omit city than county schools, i.e., their “home schools.” In order to sidestep this issue, we draw on a second, complementary approach to measuring compliance. Specifically, we attempt to match students who appear in a county-school yearbook from the 1974/75 school year—just prior to the implementation of busing—to yearbooks from the following year. We then estimate the effect of being assigned to be bused on appearing in (i) a city-school yearbook, (ii) a county-school yearbook, (iii) a yearbook from a school that was already racially integrated and, therefore, exempt from the busing plan, as well as (iv) *any* JCPS yearbook.

As shown in Appendix Table A.16, being assigned busing reduces the probability of us being able to find the respective student in any yearbook from the next school year by 15 percentage points. We interpret this effect as evidence of white flight. We further observe that treatment assignment reduces the probability of being matched to a county-school yearbook by 60 percentage points, while that of a match to a city-school yearbook increases by 46 percentage points. There does not appear to be an effect on matches to yearbooks from exempt schools.

The advantage of this second approach to measuring compliance is that it is not subject to recollection and reporting biases. Its disadvantages are twofold. It does not allow us to measure where students who leave the JCPS system go instead, and the accuracy of our estimates depends on our ability to accurately match students across time. While some failures to find a match can be attributed to white flight, others are due to transcription errors and different spellings of names (e.g., nicknames). To calculate a likely upper bound on compliance in the treatment group, we restrict attention to students who we can successfully match across the 1974/75 and 1975/76 yearbooks. Among this selected set of students, we estimate that being assigned to be bused increases the probability of attending a city school by 90% (cf. Appendix Table A.16). Alternatively, we can address attenuation bias due to imperfect matching by scaling our yearbook-based compliance estimate by the match rate

in the control group. Doing so produces a compliance rate of 66% ($= 0.46/0.70$).³⁹

In light of the evidence above, we conclude that compliance in the treatment group was probably greater than thirty but smaller than ninety percent. Interestingly, being assigned to be bused appears to have accelerated white flight, with about nine to fifteen percent of treated students leaving the Jefferson County public school system altogether.

Where did students who left JCPS go? Judging by the answers on our survey, some went to more rural and less integrated public schools in other counties or states. Given that private segregation academies, which, at the time, were prevalent in the South, did not exist in Kentucky, many others who left attended a private religious school instead. Among respondents who indicated that they attended a private school, nearly 58% list a Catholic one, 38% list a Baptist school or another religious institution, while only 4% list a non-religious high school.

8.2. Persuasion Rates

The primary purpose of busing was to eliminate racial differences in access to high-quality education. Even though any long-run impact on political views and preferences was likely incidental, we can compare our results with others in the political economy literature by calculating persuasion rates (DellaVigna and Kaplan 2007; DellaVigna and Gentzkow 2010). Intuitively, persuasion rates help to make results from different interventions comparable because they normalize estimated effects by the size of the population that is, in principle, “persuadable” and by exposure to treatment.

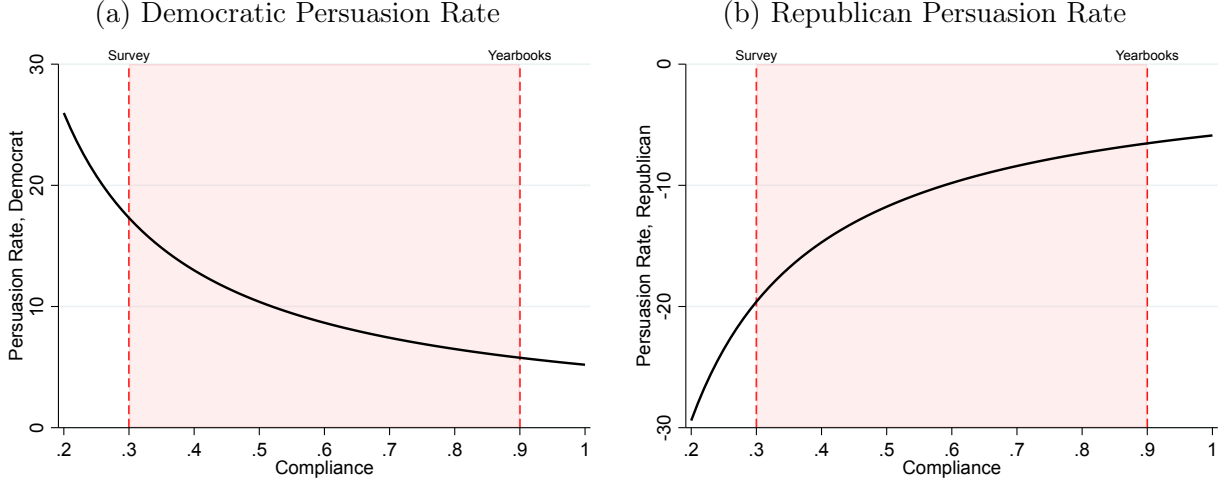
In our setting, there are two relevant persuasion rates. We ask what fraction of bused Republicans and independents became Democrats; and, conversely, what fraction of bused Democrats and independents turned Republican? Formally, the persuasion rate for supporting party p is defined as:

$$(3) \quad f_p = 100 \frac{y_{p,T} - y_{p,C}}{(e_T - e_C)(1 - \tilde{y}_p)}.$$

Here, $y_{p,T} - y_{p,C}$ denotes the difference in party support between the treatment and control group, i.e., the estimated ITT effect. $e_T - e_C$ is the difference in exposure to treatment,

³⁹We have also collected commencement lists and matched them to yearbook records. Here, we must confine ourselves to asking whether high school seniors who were slotted to be bused appear on the commencement lists of city or county schools. We find that seniors who were assigned to be bused are 40 percentage points less likely to appear on a county-school commencement list. They are 8 percentage points less likely to be on *any* JCPS commencement list, 24 percentage points more likely to be on a city-school list, and 7 percentage points more likely to appear on the commencement list of an exempt school. These estimates, however, should be taken with a grain of salt. Anecdotally, seniors who were bused may have nonetheless participated in commencement at their home school.

Figure 7: Persuasion Rates



Notes: Panel (a) plots the implied persuasion rate with respect to Democratic party registration (f_D) for different rates of compliance ($e_T - e_C$). Panel (b) plots the persuasion rate with respect to Republican party registration. All calculations are based on eq. (3), with $\hat{\beta}_D = 0.035$, $\tilde{y}_D = 0.326$, $\hat{\beta}_R = -0.028$, and $\tilde{y}_R = 0.524$

which, in our setting, corresponds to the rate with which students complied with their busing assignments; and $1 - \tilde{y}_p$ denotes the fraction of potentially persuadable individuals. We approximate the latter based on the share of individuals in alphabet-group cohorts that were not assigned to be bused and who do not identify with party p in our matched voter registration data.

Using $\{\hat{\beta}_D = 0.035, \tilde{y}_D = 0.326\}$ and $\{\hat{\beta}_R = -0.028, \tilde{y}_R = 0.524\}$, Figure 7 plots implied persuasion rates for different values of $e_T - e_C$. Assuming perfect compliance (i.e., $e_T - e_C = 1$) yields persuasion rates of $f_D \approx 5.2$ and $f_R \approx 5.9$. Compliance, however, was likely far from perfect, and lower compliance implies higher persuasion rates. Above, we estimate that compliance might have been as low as 30%, which would yield persuasion rates of $f_D \approx 17.3$ and $f_R \approx -19.6$.

For realistic levels of compliance—say, between 30% and 90%—the persuasion rates in Figure 7 are of a similar magnitude as many of the rates in the literature (see, e.g., the review by DellaVigna and Gentzkow 2010). Yet it is noteworthy that the persuasion rates here apply more than forty years after treatment. Treatments as intense as the one in our setting can thus have large persuasive effects that are essentially permanent—even if such effects are merely unintended consequences.

9. Conclusion

One of the most common methods to desegregate public schools in the aftermath of the Civil Rights Act of 1964 was to impose mandatory busing programs. These programs transported black students to better-resourced, previously all-white schools and, somewhat less frequently, white students to predominantly black, inner-city schools. In this paper, we study the long-run impact of being bused to an inner-city school on the ideology and attitudes of white males.

Our identification strategy draws on a natural experiment in Louisville, KY, which introduced quasi-random variation in exposure to busing based on the first letter of individuals' last names. Using a difference-in-differences design, we find that being assigned to be bused to an inner-city school had little to no long-run impact on political participation. It did, however, increase support for the Democratic Party and its candidates more than forty years later. It also made individuals more supportive of unions—and maybe other forms of redistribution—and less likely to believe in a “just world,” i.e., that success is earned rather than attributable to luck.

Perhaps surprisingly, we find only mixed evidence on the impact of busing on racial attitudes. Today, bused individuals tend to live in neighborhoods with a higher share of African Americans; but there does not appear to be an effect of busing on survey measures of racial prejudice. Given that all Louisville-area public schools became racially integrated, we interpret the lack of clear-cut effects as speaking to the scope conditions in Allport's (1954) contact hypothesis. That is, exposure to African Americans in an inner-city environment rather than a previously all-white suburban setting may only be enough to reduce mild but not blatant or overt forms of bias.

Nonetheless, we do find that bused students are more likely to recall poverty among their classmates and more likely to report having befriended others who were poor. Taken together, our findings suggest that early-life exposure to a poor, inner-city environment shapes individual beliefs and long-run politico-economic attitudes. Whether these results travel to other settings remains an important question for future research.

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Appendix A: Additional Results and Robustness Checks

In this appendix, we provide additional information on our data collection and construction efforts; and we report several additional results and robustness checks.

Appendix Table A.1 presents estimates of the impact of busing on black students. For a description of the underlying sample and a brief explanation of the challenges in conducting inference on this sample, see Appendix C.

Appendix Table A.2 details the years of yearbook records and commencement records that we collected by school.

Appendix Table A.3 details the construction of our main working sample from the data we collected.

Appendix Table A.4 explores the robustness of our main result with respect to alternative matching restrictions. In addition to including students who we could match to a unique voter registration record (cf. Appendix B), the upper panel of this table also includes individuals from yearbook records that are matched to multiple voter registration records. For the latter set of observations, the outcome is coded as the mean of the respective variables in the matched voter records. The outcome in the lower panel also includes individuals that we were not able to match to any voter registration record. Outcomes for these observations are coded as zero.

Appendix Table A.5 explores the intensive margin effect of busing assignment for one year versus two years. It shows the effect of busing is similar for both one-year and two-year assignments.

Appendix Table A.6 probes the robustness of our main results to the exclusion of alphabet-group fixed effects. It also shows robustness to including state-of-residence and school fixed effects.

Given potential concerns about improper aggregation of heterogeneous treatment effects in two-way fixed effects models (see, e.g., Goodman-Bacon 2021), **Appendix Table A.7** shows results for the effect of busing on partisanship based on two alternative difference-in-differences estimators that do not suffer from this problem. The upper panel reports estimated average effects based on the approach of Callaway and Sant’Anna (2021), while the lower panel employs the stacked estimator of Cengiz et al. (2019). Both estimators produce results that are qualitatively equivalent to those in Table 5.

Appendix Table A.8 repeats the exercise in Appendix Table A.7 with turnout as the outcome. Again, the results are qualitatively similar to those reported in the main text.

Appendix Tables A.9 and A.10 report difference-in-differences estimates of the effect of busing on answers to the individual questions on our survey. All outcomes are normalized by the mean and standard deviation for the respective question in the control group. Results are grouped by topic. Panel A shows questions on voting and partisanship. Panel B contains all questions from the “Belief in a Just World” module, while Panel C shows questions on racial attitudes. Panels D and E present results for questions on progressive policies and trust in government, respectively. Panel F shows two sub-indices on racial attitudes, while Panel G presents answers to a real-stakes question that allowed respondents to authorize a donation to the Black Lives Matter movement, the National Police Foundation, or neither.

Appendix Table A.11 presents numerical results for the effect of busing on each of our survey indices (upper panel) as well as each question pertaining to potential mechanisms. The entries in this table, therefore, complement Figures 4 and 5 in the main text.

Appendix Table A.12 displays estimates for the effect of busing assignment on zip-code-level shares of registered Democrats and Republicans as well as average incomes for white males. The latter set of results confirm the findings of Tuttle (2019) in our linked yearbook-L2 sample. That is, white males assigned busing live in similar-income areas today as their counterparts not assigned busing.

Appendix Table A.13 repeats the “exempt-school placebo” in Table 6 in the main text but with measures of neighborhood racial diversity as outcomes. Again, the results provide evidence that being assigned to be bused caused treated students to choose neighborhoods with a higher share of African Americans (but not other minorities).

Appendix Table A.14 repeats the exercise in Appendix Table A.7 with neighborhood racial diversity as the outcome. Again, the results are qualitatively similar to those reported in the main text.

Appendix Table A.15 studies compliance with busing assignments based on survey-takers’ answers to an open-ended question asking them to list the high schools that they had attended (see the survey instrument in Appendix Figure A.7). The reported coefficients correspond to the estimated effect of being assigned busing on four different outcomes: (i) listing at least one “city high school,” i.e., a high school that was previously part of the Louisville Independent School District; (ii) listing at least one private school; (iii) listing at least one

public high school outside of the newly-merged JCPS system; and (iv) listing *only* high schools that were formerly operated by the Jefferson Board of Education.

Appendix Table A.16 studies compliance with busing assignments based on matches of students between yearbooks from the 1974/75 (pre-busing) and 1975/76 (post-busing) school years. The reported coefficients correspond to the estimated effect of being assigned busing on four different outcomes: (i) whether we can locate the individual in *any* 1975/76 yearbook; (ii) whether the student appears in a yearbook of a city school, i.e., a school that was previously part of the Louisville Independent School District; (iii) whether the student appears in a county-school yearbook, i.e., a yearbook from a school that, prior to the merger, was part of the Jefferson County school system; (iv) whether the student appears in a yearbook for a school that had been exempted from the court-ordered busing plan. The sample in the first four columns consists of all students who appear in a JCPS yearbook from the 1974/75 school year and who, based on their grade level, should also appear in a yearbook for the following year. The sample in the last three columns is restricted to students who we can match across yearbooks.

Appendix Table A.17 investigates compliance based on matched commencement records. That is, we attempt to match students from pre-busing yearbooks to post-busing commencement lists. The reported coefficients correspond to the estimated effect of being assigned busing on four different outcomes: (i) whether we can locate the student on *any* post-busing commencement list; (ii) whether the student appears on a commencement list of a city school, i.e., a school that was previously part of the Louisville Independent School District; (iii) whether a student appears on a county-school list, i.e., a commencement list from a school that, prior to the merger, was part of the Jefferson County school system; and (iv) whether a student appears in a commencement list of a school that had been exempted from the court-ordered busing plan. The sample in the first four columns consists of all students who appear in a pre-busing JCPS yearbook and who, if they remain enrolled in a JCPS high school, we should be able to match to an entry in one of the post-busing commencement lists that we collected. The sample in the last three columns is restricted to matched students only.

Appendix Figure A.1 displays the share of African Americans residing in each census tract in Jefferson County, KY as of the 1970 Decennial Census.

Appendix Figure A.2 shows the average share of African Americans among students in JCPS high schools. In doing so, it distinguishes between “city” and “county” schools. Prior to desegregation, city schools were part of the Louisville Independent School District, whereas

county schools belonged to the Jefferson County school system. The data for this figure come from reports by the Office of Civil Rights at the Department of Education.

Appendix Figure A.3 shows “pretrends” in the share of registered Democrats for all six alphabet groups. It also reports p -values from formal tests of the null hypothesis of no mean differences and no differences in trends across groups. Broadly summarizing, the evidence in this figure is consistent with assignment to treatment being as good as random.

Appendix Figure A.4 depicts the weights that the TWFE estimator uses to aggregate the treatment effects for all alphabet-group-cohorts. Reassuringly, all weights are positive, which implies that our difference-in-differences estimator recovers a convex combination of treatment effects.

Appendix Figure A.5 presents additional randomization inference results based on the procedure described in the main text. The outcome is voter registration as “independent,” i.e., neither Democrat nor Republican.

Appendix Figure A.6 presents randomization inference results based on the difference-in-differences model in eq. (1) with the zip-code-level share of African Americans as outcome.

Appendix B: Matching Procedure

B.1. *Importing and Cleaning of Yearbook Records*

The yearbook records were collected in four phases. In three of these phases, the records were transcribed by undergraduate research assistants. We inspected these records for mistakes and made a few minor corrections, as needed. In the fourth phase, the records were collected by individuals working on the Amazon Mechanical Turk (MTurk) platform. We also inspected these records for mistakes and uncovered issues with transcription and data missingness. We, therefore, conducted a complete audit of the MTurk records and corrected mistakes as needed.

We begin by importing the yearbook records from each phase and with cleaning and standardizing the fields. For the records collected by undergraduate research assistants, we implement the minor fixes. For example, we fix the coding of race or gender when the two have been accidentally swapped, e.g., race is coded as “F” (female) and gender is coded as “B” (black). We also correct a small number of duplicate entries. For the MTurk records, we import the original records and then join a series of spreadsheets that contain the corrected records from our audit. When a corrected record exists, we drop the original record. We further correct a small number of records in which race or gender are recorded in a nonstandard

fashion (e.g., “FEM” instead of “F”), or in which the race or gender fields are inadvertently filled with name, race, and gender.

Next, we combine the yearbook records across all four phases into a single file. In this combined file, we standardize school names, and we reduce duplicates that match on name, gender, race, school, grade, and year to a single record. A subset of records was double-coded across different phases of data collection. We assess disagreement on gender and race in this subset. If two records have the same name, school, grade, and year but disagree on gender because one entry has recorded gender as missing, we keep the entry with non-missing gender. Similarly, if two records have the same name, school, grade, and year but disagree on race because race is missing in one entry but not in the other, we keep the entry with non-missing race. If, however, two records disagree on gender or race and both have non-missing values recorded, then we re-code race and gender as missing. These records are ultimately removed from the sample when we limit our focus to white men.

The combined file contains all collected yearbook records, and thus many students are included multiple times. For example, we collect data from the Ballard High School yearbook for the 1973-74 school year and for the 1974-75 school year. Many 9th grade Ballard students in 1973-74 will show up as 10th grade Ballard students in the 1974-75 school year.

Next, we reduce the combined yearbook file to a file that contains each student only once. First, we drop all records from post-desegregation yearbooks (school years from 1975-76 onward). Then, we construct an approximate year of birth for each record by taking the year of the yearbook, which corresponds to the spring semester of the academic year, and subtracting the student’s grade plus six. We use this year of birth to identify duplicates based on school, gender, race, name, and year of birth. Among these duplicates, we keep the one from the latest pre-desegregation yearbook. Next, we identify duplicates based on first name, last name, name suffix, gender, race, and year of birth. Among these duplicates, we keep the record with non-missing middle initial if it is a record from 12th grade, since middle initials are more commonly included for seniors. If multiple records include a middle initial, we keep the record that contains a full middle name if the record is from 12th grade. Next, we identify duplicates based on first name, middle name/initial, last name, gender, race and year of birth, and we keep the records with non-missing name suffix. Finally, we identify individuals who have the same full name, gender, race and year of birth but are recorded in different schools. If the individuals are in consecutive yearbooks and only one grade apart, we keep the record from the later grade. We remove all remaining duplicates based on name, race, gender, and year of birth, if they cannot be reduced to a single student record via the process above.

The resulting file includes one record per individual student (uniquely identified based on

name, race, gender, and year of birth) based only on pre-desegregation yearbooks. This is the file that we merge to nationwide voter registration records from L2, Inc.

B.2. *Initial Merge of Yearbook Records to L2 Voter File*

Before matching, we limit the yearbook records to male students with non-missing race. Since our matching procedure is based on name and year of birth, we further drop individuals who have the same name and year of birth but a different race. Finally, we define an alternative year of birth variable that is one year earlier than our primary year of birth variable. We do this because some individuals graduating in 1976, for example, will be born in 1958 whereas others will be born in 1957.

Next, we import the voter registration records from L2, limiting to male voters born in the relevant range of years for our sample. For both the L2 and yearbook records, we standardize common nicknames (e.g., converting “BEN” to “BENJAMIN”). Then, we perform a full outer join based on standardized first name and last name, keeping only those records that are an exact match based on those variables. Many yearbook records will merge to multiple L2 voter records. We define a hierarchy below that dictates our preference over matches within a given set.

1. Matches that match on middle initial, when middle initial is non-missing, as well as on our primary birth year variable or on our alternative birth year variable.
2. Matches that match on our primary birth year variable or on our alternative birth year variable.
3. Matches that match on middle initial, when middle initial is non-missing, as well as on our (primary birth year variable + 1) or on our (alternative birth year variable - 1).
4. Matches that match on our (primary birth year variable + 1) or on our (alternative birth year variable - 1).

In short, we only keep matches that have the same standardized first and last name and have a birth year from L2 that is two years below or one year above our primary birth year variable. Among those match sets, we keep the highest match in our hierarchy and remove those below it. For example, if a yearbook record matches to two L2 voter records on first name, last name, and the birth year range, but only one of those matches on middle initial and primary birth year, then we keep the record that matches on middle initial and primary birth year.

In some cases, after eliminating matches according to the hierarchy above, we are left with one record from the yearbooks linked to only one record from L2. We call that a unique

match. However, in many cases, even after we eliminate matches based on the hierarchy above, we have one record from the yearbooks linked with many potential records from L2. We implement two main procedures to pare those matches down even further. First, we check whether any matches in the match set match exactly on the raw first name variable, rather than the standardized first name variable. If only one of the potential matches is also a match in terms of raw first name, we keep that match and designate it as unique. Second, we take the remaining non-unique matches and link them with Kentucky birth records.

B.3. *Filtering via Birth Records*

At this point, we have a file that contains our valid and preferred matches from the yearbook data to L2. Some of those matches are unique, meaning a single yearbook record is linked to a single L2 record. Other matches are non-unique, meaning a single yearbook record is linked to many L2 records. For the non-unique matches, we next link them to Kentucky birth records. The birth records were acquired from the Kentucky Department of Libraries and Archives and cover all births in the state from 1911-1999.

First, we merge the non-unique matches to birth records based on standardized first name, last name, and exact date of birth (taken from L2). We use this merge to filter the non-unique matches in the following order:

1. If only one match from the match set matches to the birth records, we keep that match.
2. If multiple matches from the match set match to the birth records, but only one matches to a Jefferson County birth, we keep that match.
3. If multiple matches from the match set match to the birth records, but only one matches to a birth from a county that borders Jefferson County, we keep that match.
4. If multiple matches from the match set match to the birth records, but only one has an exact middle initial match with the middle initial from L2, we keep that match.
5. If multiple matches from the match set match to the birth records, but only one has an exact middle initial match with the middle initial from the yearbook, we keep that match.

The above process leaves us with a new set of unique matches and a new, smaller set of non-unique matches. We take the new set of non-unique matches and merge them to birth records based on standardized first name, last name, and year of birth. We then use that merge to filter the non-unique matches as follows:

1. If only one match from the match set matches to the birth records, we keep that match.

2. If multiple matches from the match set match to the birth records, but only one matches to a Jefferson County birth, we keep that match.
3. If multiple matches from the match set match to the birth records, but only one matches to a birth from a county that borders Jefferson County, we keep that match.
4. If multiple matches from the match set match to the birth records, but only one has an exact middle initial match with the middle initial from L2, we keep that match.
5. If multiple matches from the match set match to the birth records, but only one has an exact middle initial match with the middle initial from L2 and is born in Jefferson County, we keep that match.
6. If multiple matches from the match set match to the birth records, but only one has an exact middle initial match with the middle initial from L2 and is born in a county that borders Jefferson County, we keep that match.

Again, the above process leaves us with a new set of unique matches and a new, even smaller set of non-unique matches. We take the new set of non-unique matches and merge them to birth records based on raw first name, last name, year of birth, and month of birth. We then use that merge to filter the non-unique matches as follows:

1. If only one match from the match set matches on middle initial from L2 and exact date of birth, we keep that match.
2. If none of the matches match on middle initial, but only one match from the match set matches on exact date of birth, we keep that match.
3. If none of the matches match on middle initial or exact date of birth, but only one match from the match set matches on name, month of birth, and year of birth, we keep that match.

Ultimately, this filtering process leaves us with a final set of unique matches and non-unique matches. We primarily focus on the unique matches in our analyses, but we check for difference in “any match” rates by treatment status and we incorporate party affiliation status from the non-unique matches in robustness checks.

B.4. *Adjustments Based on Survey*

The linked yearbook-voter records sent to NORC for the purposes of fielding the survey were based on an earlier version of the merge. As a result, a small number of individuals in the NORC survey sample are matched to different L2 records. Since the survey respondents explicitly report attending schools in Jefferson County, we take the match in that sample as

given and we use it to update our latest version of the yearbook-L2 merge. To do this, we link our survey records with the latest yearbook-L2 merge and replace the L2 information from the latest merge with the L2 information from the survey-era merge. This adjustment only affects a handful of records.

B.5. *Final Analysis File*

We take a few additional steps to arrive at our final analysis file. First, we focus only on graduation years 1970–80, on white students, and on individuals attending County schools prior to desegregation. Second, in a small number of cases, a single L2 record matches to multiple yearbook records. In most cases, this is because the separate yearbook records correspond to the same student but in different years. We catch most of these cases when we import and clean the yearbook records but some remain due to name differences across years that were later corrected when standardizing nicknames. In these cases, we keep the latest yearbook record. In other cases, a single L2 record matches to multiple yearbook records within the same year. In this case, if one record has a non-missing middle initial and the other has a missing middle initial, we keep the one with the non-missing middle initial. If duplicates still remain, we remove them from the sample. This leaves us with our final analysis sample.

As reported in Table A2, we end up with at least one match to the L2 voter file for 32,568 white, male yearbook records from non-exempt schools. Of those, 18,541 that have a unique match. Of the 18,541 unique matches from non-exempt schools, over half (56.0%) are unique even prior to our filtering via birth records. Another quarter of the unique matches (24.9%) are designated unique at the first step of the birth record filtering. In other words, one quarter of our unique matches were part of a multi-match set among which only one matched to a Kentucky birth record based on first name, last name, and date of birth. Approximately 10% of our unique matches are designated unique at the second step of the birth record filtering. These matches are designated unique because they linked to only one Jefferson County birth based on first name, last name, and date of birth. This covers almost 90% of our unique matches. The last 10% are designated unique at other stages in the birth record filtering. Our main results on party affiliation are robust to using only those records that are unique without filtering and/or are designated unique based on an exact date of birth match with Kentucky birth records.

Appendix C: Impact of Busing on Black Students

The analysis in the main text focuses on the impact of being bused to an inner-city school on the political preferences of white males. We restrict attention to white students because

our sample of usable observations for blacks is *much* smaller.

The smaller number of observations for black students is due to three factors. First, blacks accounted for only about 20% of the students in the unified JCPS school district. Second, for one of the three formerly black schools, the 1974/75 yearbook only lists individuals in their senior year. In the other two yearbooks, only individuals in 10th through 12th grade are listed. As a consequence, we cannot match many of the individuals who attended these schools just prior to school desegregation to present-day voter registration records. Third, we lose additional observations because the match rate of black students to the L2 data is lower than for whites. Ultimately, this means that we can only match 890 black students from pre-desegregation yearbooks.

Such a small number of observations results in greatly reduced statistical power, which leads to noisier estimates. In addition, there is another, more subtle issue with estimating the impact of busing on black students. Under the court-ordered desegregation plan, black students were bused for eight or nine years in order to equalize the flow of blacks into formerly white schools and that of whites into formerly black schools. Given that black students were bused for so many years, only one of the six alphabet groups was not assigned treatment during the period for which we were able to collect data. As a result, the asymptotic validity of conventional standard errors based on the normal approximation may be compromised; and given that there are only six possible treatment configurations for blacks, randomization inference does not constitute a practical alternative. Due to these issues, our analysis in the main text focuses on white students only.

Nonetheless, in an attempt to analyze the impact of busing on black students' political preferences, we address the small number of usable observations by augmenting our sample with individuals whom we can locate in post-desegregation yearbooks. For whites, such an approach would not be advisable because of differential attrition (i.e., white flight). For blacks, however, differential attrition appears to be less of an issue. In cases for which we have both pre- and post-desegregation yearbooks, we find that match rates for blacks are nearly identical and certainly not statistically different between students that were and were not assigned to be bused. More specifically, we estimate that for blacks differential attrition is less than one quarter of one percentage point.

Using this larger, augmented sample, Appendix Table A.1 presents difference-in-difference estimates of the impact of being bused to an affluent suburban school on political participation and preferences among blacks. Similar to our registration estimates for whites, we see no evidence of an effect of busing on being registered to vote. The relevant point estimates are economically small and far from statistically significant. Turning to partisanship, the pattern is very similar. All point estimates are much smaller than our estimates on Demo-

cratic and Republican registration for whites (cf. Table 5). Moreover, none of the estimated effects on party preferences are statistically distinguishable from zero. Only when it comes to voter turnout do we observe large and, in some cases, statistically significant point estimates. With 3–7 percentage points, the estimated effects are similarly large for turnout in presidential and midterm elections. We note, however, that four out of the six turnout estimates are so imprecise that the respective 95%-confidence intervals include zero. In light of this observation, and given the statistical caveats described above, we caution against taking the estimates in this table at face value.

Appendix D: Jefferson County Busing Survey

D.1. Data Collection

We provided NORC with a sample of 2,400 matched yearbook-L2 records, of whom half had been assigned treatment. NORC then mailed potential respondents a recruitment packet that included a letter stating the purpose of the study, a recruitment appeal with a unique study link, as well as a toll-free telephone number to call and complete the survey by phone with a NORC telephone interviewer (see Appendix Figure A.7). The packet included a pre-paid \$5 incentive with the contingent incentive offer of \$30 for completing the survey. Non-responders to the initial appeal were mailed a reminder postcard that again included the survey URL, inbound phone number and the contingent offer of \$30 for the completed survey. Next, non-responders were contacted by email, requesting they complete the survey. A final mailing included a reminder letter with a self-administered paper questionnaire and a postage-paid return envelope. Finally, NORC telephone interviewers outbound dialed all non-responders with a matched phone number to complete the survey. For a copy of our survey instrument, see Appendix Figure A.7.

In total, NORC collected 629 interviews, 480 by web mode, 72 by phone (including inbound and outbound), and 77 by paper mode. Out of those 629 respondents, we discovered that 70 had attended schools that were exempt from the alphabet-based busing plan. These schools were not on the initial exemption list published by *The Courier-Journal*, but were later made exempt through additional redistricting.

Including all respondents, NORC completed 304 interviews with individuals in the treatment group (yield rate: 25.3%), and 325 interviews with individuals in the control group (yield rate: 27.1%).

D.2. Data Processing

NORC prepared a fully labeled data file, applying standard cleaning rules to web-mode survey data for quality control. In particular, NORC removed survey responses with response

patterns that were indicative of speeding or skipping. Speeding was determined as completing the instrument in less than 1/3rd of the median interview length. In total, 1 survey response was removed for speeding. Respondents who skipped 50% or more of the survey were removed for skipping. In total, 2 additional survey responses were removed for skipping. In addition, 3 respondents who completed the survey but indicated that they did not, in fact, attend high school in Jefferson County were also removed from the data file. In total, NORC removed 6 survey responses because of these data quality checks.

D.3. *Weighting*

For the final, cleaned sample, NORC calculated survey weights to adjust for the probability of selection from the initial subsampling process, and the probability that an individual responded to the survey. We use these weights throughout our analysis of the survey data.

Appendix E: Noncompliance and Bounds on Treatment Effects

In this appendix, we present sufficient conditions for our intent-to-treat estimates to be interpretable as a lower bound on the local average treatment effect as well as on the average treatment effect.

Setup and Notation Following the textbook treatment of Imbens and Rubin (2015), we distinguish between different types of individuals, depending on their behavior when assigned to either the treatment or the control group. Our setting differs from the standard analysis of two-sided noncompliance described by Imbens and Rubin (2015) because any individual can react to their treatment assignment in three rather than two ways: (i) comply with the assignment; (ii) refuse assignment but stay within the JCPS school system; or (iii) avoid busing and school desegregation altogether by leaving the JCPS school system. Thus, in contrast to textbook treatments, our setting features nine rather than four possible treatment-response types.

More formally, we distinguish between different types of individuals, using the following notation. Let Z_i denote the assignment status of individual i .

$$Z_i = \begin{cases} 0 & \text{if assigned to control group} \\ 1 & \text{if assigned to treatment group} \end{cases}$$

In addition, let $R_i(1)$ and $R_i(0)$ denote the response of i to his treatment assignment, i.e., the

kind of school that i would attend if assigned to the treatment or control group, respectively.

$$R_i(Z) = \begin{cases} 1 & \text{if attend inner-city school} \\ 0 & \text{if remain in home school} \\ -1 & \text{if leave JCPS system} \end{cases}.$$

With this notation in hand, the type of any individual is given by the tuple $(R_i(0), R_i(1))$. We further use $Y_i(z, r)$ to denote the potential outcomes for individual i , given assignment status $z = Z_i$ and response $r = R_i(Z_i)$.

In light of the historical context, we assume that no student is bused unless he is actually assigned to the treatment group, and that all students who would leave the JCPS system if assigned to the control group would also leave if they were assigned to the treatment group. The first of these assumptions is based on the observation that busing was extremely unpopular and that the court-ordered busing plan included no provisions to accommodate volunteers. The second assumption can be justified by appealing to the motivations behind “white flight.” If someone is not willing to continue to attend their home school after it becomes racially integrated, then it stands to reason that they would also not be willing to be bused to a racially integrated school in the inner-city, where such concerns are likely to weigh even more heavily.

The matrix above displays all treatment-response types that are plausibly present in our setting, together with the labels and abbreviations that we use to refer to them below. Conceptually, the key difference between our analysis and the textbook treatment of Imbens and Rubin (2015) is that non-compliance in our setting may involve substitution towards a second, entirely different “treatment.” In this sense, our setup generalizes the typical textbook treatment of two-sided non-compliance. To see this, note that the top-left field in the matrix above corresponds to, in the language of Imbens and Rubin (2015), “always-takers”, whereas the field just below it maps to “defiers” (i.e., individuals who would attend an inner-city school if, and only if, they were assigned to the control group). The matrix above thus nests the usual four quadrants (see, e.g., Imbens and Rubin (2015)).

Analysis We next state four assumptions that, together, imply that our intent-to-treat estimates provide a lower bound on the size of the local average treatment effect (LATE) as well as on that of the average treatment effect (ATE). The first two of these assumptions are standard in the treatment effects literature (cf. Imbens and Rubin 2015). The third one restricts the type composition of the population, as in the matrix above.

ASSUMPTION 1: *Assignment status (Z) is as good as random.*

		School if Assigned to Control Group		
		Inner-City	Home	Leave JCPS
School if Assigned to Treatment Group	Inner-City		<u>Compliers</u> (C)	
	Home		<u>Never-Takers</u> (NT)	
	Leave JCPS		<u>Treatment-Avoiders</u> (TA)	<u>Always-Avoiders</u> (AA)

ASSUMPTION 2: *Assignment status (Z) affects outcomes (Y) only through individuals' responses ($R(Z)$).*

ASSUMPTION 3: *The population consists only of compliers (C), never-takers (NT), treatment-avoiders (TA), and always-avoiders (AA).*

Our fourth assumption is a monotonicity condition on potential outcomes.

ASSUMPTION 4: *Potential outcomes are ordered as follows:*

Case (i): $Y_i(z, 1) \geq Y_i(z, 0) \geq Y_i(z, -1)$ for all i and z ; or

Case (ii): $Y_i(z, 1) \leq Y_i(z, 0) \leq Y_i(z, -1)$ for all i and z .

Together with Assumptions 1–3, Assumption 4 is sufficient to bound both the local average treatment effect and the average treatment effect by the intent-to-treat effect. That is:

CLAIM 1: *Under Assumptions 1-3 and 4(i), $ITT \leq LATE$.*

Under Assumptions 1-3 and 4(ii), $LATE \leq ITT$.

CLAIM 2: *Under Assumptions 1-3 and 4(i), $ITT \leq ATE$.*

Under Assumptions 1-3 and 4(ii), $ATE \leq ITT$.

PROOF: For any group $G \in \{C, TA, AA, NT\}$, define $\bar{Y}_G(z, r) = E_{i \in G} [Y_i(z, r)]$, with the expectation taken over individuals of that type. Given Assumption 1–3, we can express the intent-to-treat and average treatment

effects in terms of type shares, $\{p_G\}$, and expectations over potential outcomes.

$$\begin{aligned}
ITT &= p_C [\bar{Y}_C(1, R_C(1)) - \bar{Y}_C(0, R_C(0))] + p_{NT} [\bar{Y}_{NT}(1, R_{NT}(1)) - \bar{Y}_{NT}(0, R_{NT}(0))] \\
&\quad + p_{TA} [\bar{Y}_{TA}(1, R_{TA}(1)) - \bar{Y}_{TA}(0, R_{TA}(0))] + p_{AA} [\bar{Y}_{AA}(1, R_{AA}(1)) - \bar{Y}_{AA}(0, R_{AA}(0))] \\
(A.1) \quad &= p_C [\bar{Y}_C(R_C(1)) - \bar{Y}_C(R_C(0))] + p_{NT} [\bar{Y}_{NT}(R_{NT}(1)) - \bar{Y}_{NT}(R_{NT}(0))] \\
&\quad + p_{TA} [\bar{Y}_{TA}(R_{TA}(1)) - \bar{Y}_{TA}(R_{TA}(0))] + p_{AA} [\bar{Y}_{AA}(R_{AA}(1)) - \bar{Y}_{AA}(R_{AA}(0))] \\
&= p_C [\bar{Y}_C(R_C(1)) - \bar{Y}_C(R_C(0))] + p_{TA} [\bar{Y}_{TA}(R_{TA}(1)) - \bar{Y}_{TA}(R_{TA}(0))],
\end{aligned}$$

where the second equality follows from Assumption 2 (which implies that, for any type, $\bar{Y}_G(z, r) = \bar{Y}_G(1 - z, r) \equiv \bar{Y}_G(r)$), while the third equality follows from $R_{NT}(0) = R_{NT}(1)$ and $R_{AA}(0) = R_{AA}(1)$. Similarly,

$$\begin{aligned}
ATE &= p_C [\bar{Y}_C(1, 1) - \bar{Y}_C(0, 0)] + p_{NT} [\bar{Y}_{NT}(1, 1) - \bar{Y}_{NT}(0, 0)] \\
&\quad + p_{TA} [\bar{Y}_{TA}(1, 1) - \bar{Y}_{TA}(0, 0)] + p_{AA} [\bar{Y}_{AA}(1, 1) - \bar{Y}_{AA}(0, 0)] \\
(A.2) \quad &= p_C [\bar{Y}_C(1) - \bar{Y}_C(0)] + p_{NT} [\bar{Y}_{NT}(1) - \bar{Y}_{NT}(0)] + p_{TA} [\bar{Y}_{TA}(1) - \bar{Y}_{TA}(0)] \\
&\quad + p_{AA} [\bar{Y}_{AA}(1) - \bar{Y}_{AA}(0)].
\end{aligned}$$

Using $\tilde{\beta}_G \equiv \bar{Y}_G(R_G(1)) - \bar{Y}_G(R_G(0))$ to denote the effect of being *assigned* treatment on the outcome of type G , and $\beta_G \equiv \bar{Y}_G(1) - \bar{Y}_G(0)$ to denote that of *actually* receiving the *intended* treatment, eqs. (A.1) and (A.2) can be more compactly written as:

$$(A.3) \quad ITT = p_C \tilde{\beta}_C + p_{TA} \tilde{\beta}_{TA}$$

$$(A.4) \quad ATE = p_C \beta_C + p_{TA} \beta_{TA} + p_{AA} \beta_{AA} + p_{NT} \beta_{NT}$$

Now, consider the case in which Assumption 4(i) holds. If so, then $LATE = \beta_C = \tilde{\beta}_C \geq 0$ and $\tilde{\beta}_{TA} \leq 0$. Hence, by eq. (A.3), $ITT \leq \beta_C = LATE$, as required for the respective part of Claim 1. Next, observe that by Assumptions 2 and 4(i), $\beta_{TA}, \beta_{AA}, \beta_{NT} \geq 0$. As consequence $\beta_{TA} - \tilde{\beta}_{TA} \geq 0$, and $ATE - ITT \geq 0$, as required for the first part of Claim 2. The proof for the case in which Assumption 4(ii) holds is analogous. *Q.E.D.*

Discussion The key assumption above is Assumption 4. In light of our finding that most students who left JCPS public schools attended a different affluent, predominantly white institution, Assumption 4 can be understood as an individual-level monotonicity condition on the impact of greater exposure to socioeconomic diversity. Claims 1 and 2 then imply that if the effect of experiencing socioeconomic diversity on the outcome of interest is positive (negative) and monotonic among all students, then the ITT bounds both the LATE and the ATE from below (above).¹

In our setting, Assumption 4 requires that being bused to a formerly black school in the inner-city and leaving the JCPS system have opposite effects on individual attitudes and beliefs, and that the sign of these effects is the same for all students. This is a strong, but,

¹It also turns out the ITT scaled by the rate of compliance (i.e., p_C) bounds the TOT from below (above)

in our view, *prima facie* reasonable assumption. We note, however, that it could be violated if students react to diversity in different ways, or if remaining in their home schools causes especially strong (conservative) backlash relative to moving to another public school system or attending a private school.

We also note that Assumption 4 can easily be weakened by placing restrictions on type frequencies or by introducing comparisons of effect sizes across different types. For instance, the claim that the ITT bounds the magnitude of the LATE continues to go through provided that $\tilde{\beta}_{TA}$ is not “too large” relative to $\tilde{\beta}_C$. More specifically, it would continue to hold if the *average* effect of leaving the JCPS system among treatment-avoiders is either of the opposite sign or, if not, weakly smaller than the *average* impact of attending an inner-city school among compliers, i.e., if either $\text{sign}(\tilde{\beta}_{TA}) \neq \text{sign}(\tilde{\beta}_C)$ or $|\tilde{\beta}_{TA}| \leq |\tilde{\beta}_C|$. For example, this might be the case if conservative families are especially likely to engage in “white flight” and if children from such families are less inclined to change their views of the world, irrespective of the direction in which they update.

Appendix Tables

Appendix Table A.1: Impact of Busing on Black Students

	(1)	(2)	(3)	(4)	(5)	(6)
	Voter Registration					
	Any Match		Unique Match			
Assigned Busing	-1.07 (2.71)	-1.88 (2.78)	-0.67 (2.31)	0.96 (2.29)		
Mean of Dep. Var.	79.49	79.49	51.61	51.61		
R-squared	0.013	0.016	0.013	0.016		
Observations	4,212	4,212	4,212	4,212		
	General Election Turnout					
	All Years		Presidential		Midterms	
Assigned Busing	4.27 (2.66)	5.79** (2.79)	2.88 (2.17)	4.60* (2.33)	6.11 (4.51)	7.38 (4.62)
Mean of Dep. Var.	68.18	68.18	73.99	73.99	60.44	60.44
R-squared	0.009	0.010	0.009	0.009	0.011	0.012
Observations	15,218	15,218	8,696	8,696	6,522	6,522
	Party Affiliation					
	Democrat		Republican		Independent	
Assigned Busing	-0.90 (4.23)	1.26 (4.78)	0.89 (2.98)	-1.95 (2.58)	0.01 (2.78)	0.69 (3.35)
Mean of Dep. Var.	71.02	71.02	19.60	19.60	9.38	9.38
R-squared	0.005	0.007	0.004	0.007	0.006	0.007
Observations	2,174	2,174	2,174	2,174	2,174	2,174
Cohort FEs	Yes	Yes	Yes	Yes	Yes	Yes
Alphabet-Group FEs	Yes	Yes	Yes	Yes	Yes	Yes
Alphabet-Group Linear Trend	No	Yes	No	Yes	No	Yes

Notes: Entries are point estimates and standard errors from estimating the difference-in-differences model in eq. (1) on our sample of black students (see Appendix C). The outcomes in the upper panel are indicators for observing at least one matching voter registration record for a particular student (cols. 1 and 2), or exactly one matching record (cols. 3 and 4). The outcome in the middle panel is voter turnout across all general elections from 2008–2020 (cols. 1 and 2), in presidential elections from 2008–2020 (cols. 3 and 4), and midterm elections only from 2010–2018 (cols. 5 and 6). The outcomes in the lower panel are individuals’ party affiliations. Odd-numbered columns control for cohort and alphabet-group fixed effects, while even-numbered columns add alphabet-group-specific linear trends. All estimates are scaled to correspond to percentage point changes. In the top panel, the unit of observation is a student in our yearbook data. In the middle panel, the unit of observation is a student-by-election year. In the lower panel, the unit of observation is a student in our matched yearbook-voter registration data, as described in Appendix C. All estimates are scaled to correspond to percentage-point changes. Standard errors are clustered at the cohort-by-alphabet-group level. ***, ** and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

Appendix Table A.2: Commencement and Yearbook Records

School Name	Commencement Records	Yearbook Records
<i>Not Exempt from Alphabet Plan:</i>		
Atherton	1970-1975, 1977, 1983	1970-1972, 1974-1976
Ballard	1972-1983	1974, 1976
Barrett Middle		1974, 1976
Carrithers Middle		1974
Central	1970-1978	1970-1976
Chenoweth Elementary		1975-1976
Doss	1971-1980, 1983	1970-1971, 1973, 1975, 1977
Eastern	1970-1980, 1983	1970-1973, 1975-1976
Fairdale	1976-1981, 1983	1972, 1975-1976
Fern Creek	1970-1980, 1983	1971-1976
Highland Middle		1974, 1976
Iroquois Middle		1974
Jeffersontown	1970-1975, 1977-1980, 1983	1971-1976
Kammerer Middle		1975-1976
Lyndon Vocational	1979	
Male	1970-1980, 1983	1975-1976
Middletown Elementary		1975-1976
Moore	1976, 1978-1980, 1983	1972-1976
PRP		1970, 1972, 1975-1976
Seneca	1970-1980, 1983	1970-1977
Shawnee	1970-1980, 1983	1971-1972, 1974-1977
Southern		1970-1977
Stuart		1970, 1972, 1975-1976
Valley	1976-1980, 1983	1970-1976
Waggener	1979, 1984	1970-1976
Westport	1976, 1978, 1980	1970-1977
Williams Middle		1976
<i>Exempt from Alphabet Plan:</i>		
Ahrens	1972-1975, 1978-1980	1972-1976
Brown	1974-1977	1975, 1976
Bruce Middle		1975, 1976
Butler		1974-1976
Conway Middle		1970-1975
Durrett	1971-1980	1971, 1973-1976
Iroquois	1970-1972, 1974-1976	
Manual	1970-1980, 1983	1970-1973
Mill Creek Elementary		1975
Myers Middle		1975-1976
Southern Middle		1975-1977
Summer School	1972-1974, 1975-1976	
Thomas Jefferson	1970-1980	1970-1973, 1975
Western	1974, 1976, 1979-1980, 1983	1970-1971, 1975

Notes: Table lists, for each school, the commencement programs and yearbooks that we collected.

Appendix Table A.3: Sample Construction

Sample Description	Sample Size
<i>Beginning sample:</i>	
Full Student-Year Sample	177,032
<i>Sample Restrictions:</i>	
Drop same student in multiple yearbooks across years	102,075
Drop women from yearbooks	48,192
Drop people with missing value for race	47,865
Drop people with same name and implied birth year	47,641
Drop people born after 1963	47,336
Correct sample based on survey responses	47,331
Drop City high schools	45,654
Keep graduation years 1970-1980	45,077
Keep white students	43,279
Keep students attending non-exempt schools	32,568
Drop non-matches to voter registration data	27,270
Drop non-unique matches	18,543
Drop records that have missing party ID info	18,541

Notes: Table shows how we restrict 177,032 student-year records to our main working sample of 18,541 students.

Appendix Table A.4: Effect of Busing on Party Affiliation, Alternative Matching Restrictions

	(1)	(2)	(3)
	Registered Democrat	Registered Republican	Independent
Including Non-Unique Matches			
Assigned Busing	3.86*** (0.74)	-2.67** (1.01)	-1.20 (0.75)
Mean of Dep. Var.	36.21	49.10	14.69
R-squared	0.004	0.001	0.002
Observations	27,267	27,267	27,267
Including Non-Registrants			
Assigned Busing	3.48*** (0.69)	-1.82* (1.02)	-0.84 (0.59)
Mean of Dep. Var.	30.31	41.11	12.30
R-squared	0.005	0.001	0.001
Observations	32,568	32,568	32,568
Cohort FEs	Yes	Yes	Yes
Alphabet-Group FEs	Yes	Yes	Yes

Notes: Entries are point estimates and standard errors from estimating the difference-in-differences model in eq. (1). The outcome in col. (1) is Democratic party registration. That in col. (2) is Republican party registration, while the outcome in col. (3) corresponds to the residual category. The upper panel includes individuals from yearbook records that are matched to multiple voter registration records. For these observations, the outcome variable is the mean of the respective voter records. The bottom panel also includes non-registrants in the sample. Outcome variables for non-registrants are coded as zero. All estimates are scaled to correspond to percentage-point changes. Standard errors are clustered at the cohort-by-alphabet-group level. ***, ** and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

Appendix Table A.5: Intensive Margin Effect of Busing on Party Affiliation

	All States		Closed Primary States	
	(1)	(2)	(3)	(4)
Registered Democrat				
Assigned Busing, 1 Year	4.09*** (1.14)	4.16*** (1.48)	2.84** (1.23)	3.24* (1.64)
Assigned Busing, 2 Years	2.81 (1.95)	1.78 (2.68)	3.95* (2.18)	2.55 (2.65)
<i>p</i> -value: 1 Year = 2 Years	0.536	0.356	0.653	0.811
Mean of Dep. Var.	36.48	36.48	39.02	39.02
R-squared	0.004	0.004	0.004	0.004
Observations	18,541	18,541	13,958	13,958
Registered Republican				
Assigned Busing, 1 Year	-2.84* (1.55)	-3.19* (1.67)	-2.14 (1.55)	-2.21 (1.85)
Assigned Busing, 2 Years	-2.67 (2.05)	-1.20 (2.11)	-3.67* (1.88)	-2.63 (2.62)
<i>p</i> -value: 1 Year = 2 Years	0.946	0.445	0.524	0.896
Mean of Dep. Var.	50.22	50.22	49.15	49.15
R-squared	0.002	0.002	0.003	0.003
Observations	18,541	18,541	13,958	13,958
Independent				
Assigned Busing, 1 Year	-1.25 (1.60)	-0.97 (1.51)	-0.70 (1.28)	-1.03 (1.29)
Assigned Busing, 2 Years	-0.15 (1.21)	-0.57 (1.63)	-0.28 (1.19)	0.07 (2.06)
<i>p</i> -value: 1 Year = 2 Years	0.562	0.846	0.779	0.637
Mean of Dep. Var.	13.30	13.30	11.82	11.82
R-squared	0.002	0.002	0.001	0.001
Observations	18,541	18,541	13,958	13,958
Cohort FEs	Yes	Yes	Yes	Yes
Alphabet-Group FEs	Yes	Yes	Yes	Yes
Alphabet-Group Linear Trend	No	Yes	No	Yes

Notes: Entries are point estimates and standard errors from estimating the difference-in-differences model in eq. (1), differentiating between students who had been bused one and two years. The outcomes in the upper and middle panels are Democratic and Republican party registration, respectively. The outcome in the lower panel is the residual category, i.e., being registered with neither party. Columns (1) and (3) include cohort and alphabet-group fixed effects, while columns (2) and (4) add alphabet-group-specific linear trends. All estimates are scaled to correspond to percentage-point changes. Standard errors are clustered at the cohort-by-alphabet-group level. ***, ** and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

Appendix Table A.6: Effect of Busing on Party Registration, Inclusion of Various Fixed Effects

	(1)	(2)	(3)	(4)
Registered Democrat				
Assigned Busing	3.05** (1.15)	3.53*** (1.14)	3.28*** (1.18)	3.64*** (1.03)
Mean of Dep. Var.	36.48	36.48	36.48	36.48
R-squared	0.003	0.004	0.007	0.038
Observations	18,541	18,541	18,541	18,541
Registered Republican				
Assigned Busing	-1.62 (1.18)	-2.77** (1.24)	-2.65** (1.23)	-2.76** (1.16)
Mean of Dep. Var.	36.48	36.48	36.48	36.48
R-squared	0.001	0.002	0.005	0.028
Observations	18,541	18,541	18,541	18,541
Independent				
Assigned Busing	-1.43 (0.95)	-0.76 (1.07)	-0.63 (1.09)	-0.88 (0.97)
Mean of Dep. Var.	36.48	36.48	36.48	36.48
R-squared	0.001	0.002	0.002	0.058
Observations	18,541	18,541	18,541	18,541
Cohort FEs	Yes	Yes	Yes	Yes
Alphabet-Group FEs	No	Yes	Yes	Yes
School FEs	No	No	Yes	Yes
State FEs	No	No	No	Yes

Notes: Entries are point estimates and standard errors from estimating fixed effects models. Estimates reflect the impact of busing on partisanship. Outcomes are Democratic party registration in the upper panel, Republican party registration in the middle panel, and the residual category in the bottom panel. The set of included fixed effects varies across columns. Column (1) includes only cohort fixed effects. Column (2) additionally adds in alphabet-group fixed effects. Estimates for this column correspond to β in the baseline differences-in-differences specification in eq. (1). Column (3) adds fixed effects for individuals' schools in 1974/75, and Column (4) adds current-state-of-residence fixed effects. All estimates are scaled to correspond to percentage-point changes. Standard errors are clustered at the cohort-by-alphabet-group level. ***, ** and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

Appendix Table A.7: Effect of Busing on Party Affiliation, Robustness to D-i-D Estimator

	(1)	(2)	(3)
	Registered Democrat	Registered Republican	Independent
Callaway-Sant'Anna			
Assigned Busing (<i>Cohort Average</i>)	2.79* (1.48)	-2.25** (0.99)	-0.54 (0.91)
Mean of Dep. Var.	36.48	50.22	13.30
Observations	18,541	18,541	18,541
Stacked Difference-in-Differences			
Assigned Busing	2.57*** (0.85)	-3.62*** (1.11)	1.05 (0.90)
Mean of Dep. Var.	36.48	50.22	13.30
R-squared	0.004	0.003	0.002
Observations	37,255	37,255	37,255
Year x Stack FEs	Yes	Yes	Yes
Alphabet-Group x Stack FEs	Yes	Yes	Yes

Notes: Entries are point estimates and standard errors from alternative differences-in-differences estimators. Estimates correspond to the average effect of busing on party registration. The upper panel relies on the estimator of Callaway and Sant'Anna (2021). The bottom panel uses the stacked differences-in-differences estimator from Cengiz et al. (2019). Outcomes are Democratic party registration in col. (1), Republican party registration in col. (2), and the residual category in col. (3). All estimates are scaled to correspond to percentage-point changes. Standard errors are clustered at the cohort-by-alphabet-group level. ***, ** and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

Appendix Table A.8: Effect of Busing on Turnout, Robustness to D-i-D Estimator

	(1)	(2)	(3)
	All Elections	Presidential Elections	Midterm Elections
Callaway-Sant'Anna			
Assigned Busing (<i>Cohort Average</i>)	1.38 (1.05)	1.04 (1.05)	1.83 (1.13)
Mean of Dep. Var.	72.88	77.78	66.34
Observations	18,541	18,541	18,541
Stacked Difference-in-Differences			
Assigned Busing	0.34 (0.99)	0.85 (0.86)	-0.34 (1.23)
Mean of Dep. Var.	72.88	77.78	66.34
R-squared	0.015	0.011	0.016
Observations	37,255	37,255	37,255
Year x Stack FEs	Yes	Yes	Yes
Alphabet-Group x Stack FEs	Yes	Yes	Yes

Notes: Entries are point estimates and standard errors from alternative differences-in-differences estimators. Estimates correspond to the average effect of busing on voter turnout. The upper panel relies on the estimator of Callaway and Sant'Anna (2021). The bottom panel uses the stacked differences-in-differences estimator from Cengiz et al. (2019). Outcomes are turnout in all general elections in col. (1), presidential elections in col. (2), and midterm elections in col. (3). All estimates are scaled to correspond to percentage-point changes. Standard errors are clustered at the cohort-by-alphabet-group level. ***, ** and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

Appendix Table A.9: Survey Results: Index Components and Other Outcomes

Outcome	Coef.	SE	<i>p</i> -value	<i>q</i> -value	Mean	SD
Panel A. Voting and Partisanship						
Wanted Biden Win, 2020	0.229	0.066	0.002	0.013	0.360	0.480
Wanted Obama Win, 2012	0.202	0.085	0.024	0.080	0.502	0.500
Democrat	0.058	0.078	0.458	0.585	0.244	0.430
Independent	−0.030	0.051	0.554	0.585	0.290	0.454
Republican	−0.028	0.063	0.659	0.605	0.466	0.499
Ideology (1=Very Cons., 7=Very Lib.)	0.266	0.148	0.082	0.160	0.044	1.004
Voted in 2020	−0.019	0.031	0.546	0.585	0.905	0.293
Panel B. Belief in a Just World Index						
People Get What Entitled To	−0.464	0.144	0.003	0.010	−0.023	0.991
Efforts are Rewarded	−0.323	0.130	0.019	0.022	−0.003	0.986
Earn Rewards and Punishments	−0.150	0.131	0.263	0.152	−0.026	1.016
Misfortune is Brought On Self	0.092	0.107	0.396	0.205	0.019	1.024
People Get What They Deserve	−0.356	0.127	0.009	0.016	−0.037	1.003
Rewards and Punishments are Fair	−0.185	0.125	0.149	0.098	−0.009	1.007
World is a Fair Place	−0.384	0.104	0.001	0.007	−0.045	0.996
Panel C. Racial Prejudice Index						
Blacks Should Work Way Up Without Favors	0.215	0.100	0.040	0.565	0.015	0.996
Black Issues due to Slavery and Discrimination	0.047	0.099	0.636	1.000	−0.004	0.994
Blacks Gotten Less Than They Deserve	−0.076	0.114	0.511	1.000	0.006	0.997
Racial Disparities due to Individual Effort	−0.130	0.142	0.367	1.000	−0.008	1.027
Civil War Over States' Rights	−0.042	0.148	0.778	1.000	−0.029	0.995
Police Racial Bias	0.054	0.148	0.716	1.000	0.036	1.030
US Should Apologize for Slavery	0.027	0.110	0.809	1.000	0.009	0.987
African Americans Too Quick to Claim Racism	0.190	0.137	0.176	1.000	0.020	1.004
Discourage Marrying an African American Partner	0.042	0.119	0.729	1.000	−0.000	0.996

(continued on next page)

Notes: Entries in col. (1) correspond to the estimated impact of busing on survey outcomes, i.e., β in eq. (1). Outcomes are answers to individual survey questions, normalized by the mean and standard deviation in the control group. Standard errors are clustered at the cohort-by-alphabet-group level and are reported in col. (2), together with the corresponding *p*-values in col. (3). *q*-values are presented in col. (4). The *q*-values above adjust for multiple hypotheses testing within each panel. Cols. (5) and (6) present means and standard deviations of the responses to the respective survey questions. Panel A contains questions on voting and partisanship. Panel B contains all questions from the “Belief in a Just World” module, while Panel C shows questions on racial attitudes. In Panel B, all variables are coded such that a more positive response reflects a greater belief that the world is just. In Panel C, all variables are coded such that a more positive response reflects a view consistent with more racial prejudice. Panels D–G are shown on the next page.

Appendix Table A.10: Survey Results: Index Components and Other Outcomes (Continued)

Outcome	Coef.	SE	<i>p</i> -value	<i>q</i> -value	Mean	SD
<i>(continued from previous page)</i>						
Panel D. Progressive Policy Index						
Abortion Policy	−0.001	0.115	0.992	1.000	−0.010	1.007
Climate Policy	−0.047	0.111	0.674	1.000	−0.034	1.004
Gay Marriage Policy	0.155	0.161	0.346	0.762	0.029	0.989
Gun Control Policy	0.117	0.100	0.252	0.762	0.013	1.011
Affirmative Action	0.192	0.128	0.144	0.657	0.052	1.027
Estate Tax	−0.031	0.139	0.827	1.000	−0.003	0.989
Spend More Money on Housing	0.014	0.160	0.933	1.000	−0.014	0.996
Redistribution to Reduce Inequality	0.102	0.145	0.488	0.953	0.032	0.985
\$15 Minimum Wage	−0.149	0.114	0.204	0.762	−0.014	1.002
Spend More Money on Poor Children	0.217	0.137	0.123	0.657	0.007	1.010
Labor Unions	0.378	0.124	0.005	0.064	0.033	1.001
Spend More Money on Welfare	0.172	0.114	0.142	0.657	−0.000	0.982
Panel E. Trust in Government Index						
Government is Not Crooked	−0.187	0.110	0.100	0.333	−0.051	0.990
Government is for All People	−0.216	0.137	0.125	0.333	−0.019	0.999
Trust Government to Do What is Right	0.101	0.163	0.539	0.562	0.003	1.005
Not Much Government Waste	−0.020	0.166	0.907	0.831	−0.016	0.993
Panel F. Sub-Indices of Racial Prejudice Index						
Racial Resentment Index	0.009	0.098	0.930	0.593	0.002	0.861
Other Racial Attitudes Index	0.055	0.068	0.422	0.326	0.012	0.505
Panel G. Real Stakes Question						
Donation to: 1=BLM, 0=Neither, −1=NPI	−0.082	0.116	0.486	—	−0.053	1.008

Notes: Table continued from the previous page. Entries in col. (1) correspond to the estimated impact of busing on survey outcomes, i.e., β in eq. (1). Outcomes are answers to individual survey questions, normalized by the mean and standard deviation in the control group. Standard errors are clustered at the cohort-by-alphabet-group level and are reported in col. (2), together with the corresponding *p*-values in col. (3). *q*-values are presented in col. (4). The *q*-values above adjust for multiple hypotheses testing within each panel. Cols. (5) and (6) present means and standard deviations of the responses to the respective survey questions. Panel D contains all questions on progressive policies, and the variables are coded such that a more positive response reflects a more progressive policy view. Panel E presents questions from the trust in government module, and the variables are coded such that a more positive response reflects greater trust in government. Panel F shows two sub-indices on racial attitudes, while Panel G presents answers to a real-stakes question.

Appendix Table A.11: Survey Results: Indices and Mechanisms

Outcome	Coef.	SE	<i>p</i> -value	<i>q</i> -value	Mean	SD
Panel A. Summary Indices						
Progressive Policy Index	0.102	0.063	0.117	0.173	0.017	0.400
Racial Prejudice Index	0.032	0.079	0.683	0.245	0.012	0.602
Just World Index	−0.249	0.080	0.004	0.017	−0.021	0.624
Trust in Government Index	−0.106	0.071	0.147	0.173	−0.020	0.500
Panel B. Potential Mechanisms						
<i>Friends:</i>						
Met Most Friends at School	−0.034	0.128	0.790	1.000	−0.014	0.993
Many Friends were Other Race	0.120	0.120	0.327	1.000	−0.035	0.993
Many Friends were Poor	0.418	0.103	0.000	0.005	0.040	0.991
Helped Understand Different People	0.080	0.167	0.636	1.000	−0.008	1.014
<i>School Environment:</i>						
Felt Safe at School	−0.039	0.150	0.796	1.000	−0.000	0.985
Many Fellow Students were Other Race	0.107	0.093	0.259	1.000	−0.012	1.010
Many Fellow Students were Poor	0.324	0.115	0.009	0.051	0.037	1.004
Helped Understand Different People	−0.011	0.091	0.907	1.000	0.018	1.076
<i>Teachers:</i>						
Academically Prepared by Teachers	−0.104	0.126	0.412	1.000	−0.010	1.018
Many Teachers were Other Race	−0.224	0.123	0.079	0.357	−0.031	0.985
Impressed by Teachers	−0.016	0.168	0.923	1.000	−0.015	0.994
Helped Understand Different People	−0.026	0.105	0.803	1.000	−0.009	1.004

Notes: Entries are point estimates and standard errors from estimating fixed effects models. Estimates in col. (1) correspond to the impact of busing on survey outcomes, i.e., β in eq. (1). Outcomes are indices for related survey questions in Panel A, and are answers to individual questions in Panel B. The latter have been normalized by the mean and standard deviation in the control group. Standard errors are clustered at the cohort-by-alphabet-group level and are reported in col. (2), together with the corresponding *p*-values in col. (3). *q*-values are presented in col. (4). The *q*-values above adjust for multiple hypotheses testing within each panel. Cols. (5) and (6) present means and standard deviations of the responses to the respective survey questions.

Appendix Table A.12: Effect of Busing on Zip-Code Income and Partisan Composition

	Total Income Per Return		Total Income Per Person	
	(1)	(2)	(3)	(4)
Assigned Busing	-1,555 (1,467)	-1,621 (1,693)	-932 (865)	-1,001 (1,012)
Mean of Dep. Var.	78,182	78,182	42,201	42,201
SD of Dep. Var.	49,175	49,175	27,679	27,679
R-squared	0.004	0.005	0.005	0.005
Observations	18,538	18,538	18,538	18,538
	Share Democrat (%)		Share Republican (%)	
	(1)	(2)	(3)	(4)
Assigned Busing	0.18 (0.48)	0.31 (0.53)	-0.43 (0.46)	-0.35 (0.41)
Mean of Dep. Var.	42.21	42.21	38.45	38.45
SD of Dep. Var.	15.62	15.62	13.68	13.68
R-squared	0.001	0.001	0.001	0.002
Observations	18,541	18,541	18,541	18,541
Cohort FEs	Yes	Yes	Yes	Yes
Alphabet-Group FEs	Yes	Yes	Yes	Yes
Alphabet-Group Linear Trend	No	Yes	No	Yes

Notes: Entries are point estimates and standard errors from estimating the difference-in-differences model in eq. (1). The outcomes in the upper panel are based on zip-code level total income derived from 2019 tax returns, as published by the Internal Revenue Service. Columns (1) and (2) scale the outcome per return in the zip-code, while columns (3) and (4) scale the outcome per person in the zip-code. The outcomes in the lower panel are zip-level shares of registered Democrats and Republicans, derived from aggregating the individual-level L2 data. In both panels, odd-numbered columns include cohort and alphabet-group fixed effects, while even-numbered columns add alphabet-group-specific linear trends. Standard errors are clustered at the cohort-by-alphabet-group level. ***, ** and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

Appendix Table A.13: Exempt School Placebo for Neighborhood Racial Diversity

	(1)	(2)	(3)	(4)
Residential Racial Shares				
	Share Black		Share Other Minority	
Actually Assigned Busing	0.98*** (0.31)	0.89** (0.44)	-0.16 (0.29)	-0.33 (0.27)
Placebo Assignment	-0.53 (0.82)	-0.62 (0.90)	-0.69 (0.52)	-0.88* (0.52)
Exempt School	2.10*** (0.24)	2.10*** (0.24)	-0.25 (0.17)	-0.24 (0.17)
Mean of Dep. Var.	10.62	10.62	15.02	15.02
R-squared	0.005	0.005	0.001	0.001
Observations	23,474	23,474	23,474	23,474
Cohort FEs	Yes	Yes	Yes	Yes
Alphabet-Group FEs	Yes	Yes	Yes	Yes
Alphabet-Group Linear Trend	No	Yes	No	Yes

Notes: Entries are point estimates and standard errors from estimating the difference-in-differences model in eq. (2). The outcomes are the zip-code-level share of African Americans (cols. 1 and 2) and other minorities (cols. 3 and 4). Share other minority is calculated by subtracting the zip-level share black and share white from one hundred. Odd-numbered columns include cohort and alphabet-group fixed effects, while even-numbered columns add alphabet-group-specific linear trends. All estimates are scaled to correspond to percentage-point changes. Standard errors are clustered at the cohort-by-alphabet-group level. ***, ** and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

Appendix Table A.14: Effect of Busing on Residential Diversity, Robustness to D-i-D Estimator

	(1)	(2)
	Share Black	Share Other Minority
Callaway-Sant'Anna		
Assigned Busing (<i>Cohort Average</i>)	0.85** (0.40)	0.04 (0.20)
Mean of Dep. Var.	10.18	15.10
Observations	18,535	18,535
Stacked Difference-in-Differences		
Assigned Busing	1.32*** (0.25)	-0.15 (0.30)
Mean of Dep. Var.	10.18	15.10
R-squared	0.002	0.002
Observations	37,243	37,243
Year x Stack FEs	Yes	Yes
Alphabet-Group x Stack FEs	Yes	Yes

Notes: Entries are point estimates and standard errors from alternative differences-in-differences estimators. The upper panel relies on the estimator of Callaway and Sant'Anna (2021). The bottom panel uses the stacked differences-in-differences estimator from Cengiz et al. (2019). Outcomes are share black in col. (1) and share other minority in col. (2). All estimates are scaled to correspond to percentage-point changes. Standard errors are clustered at the cohort-by-alphabet-group level. ***, ** and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

Appendix Table A.15: Compliance: NORC Survey

	(1)	(2)	(3)	(4)
Self-Reported Attendance				
	City School	Private School	Other Public School	Only County Schools
Assigned Busing	30.00*** (5.53)	3.83** (1.81)	4.97* (2.51)	-39.24*** (5.76)
Mean of Dep. Var.	18.69	5.01	5.20	68.40
R-squared	0.266	0.033	0.011	0.263
Observations	519	519	519	519
Cohort FEs	Yes	Yes	Yes	Yes
Alphabet Group FEs	Yes	Yes	Yes	Yes

Notes: Entries are point estimates and standard errors from estimating the difference-in-differences model in eq. (1) on the NORC survey sample. Outcomes are indicator variables based on answers to an open-response question asking respondents to list all high schools they attended. The outcome in col. (1) corresponds to a respondent listing at least one “city high school,” i.e., a high school that was previously part of the Louisville Independent School District. The outcome in col. (2) corresponds to a respondent listing at least one private school. The outcome in col. (3) corresponds to a respondent listing at least one public high school outside of the newly-merged JCPS system; whereas the outcome in col. (4) corresponds to a respondent listing *only* high schools that were formerly operated by the Jefferson Board of Education. All estimates are scaled to correspond to percentage-point changes. Standard errors are clustered at the cohort-by-alphabet-group level. ***, ** and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

Appendix Table A.16: Compliance: Yearbook-to-Yearbook Matching

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Entry in 1975/76 Yearbook						
	Any School	City School	County School	Exempt School	City School	County School	Exempt School
Assigned Busing in 1975/76	-14.74*** (2.27)	45.80*** (1.81)	-59.76*** (1.60)	0.05 (0.50)	90.02*** (1.52)	-88.49*** (1.62)	0.55 (0.87)
Mean of Dep. Var.	68.58	5.40	63.39	0.99	7.87	92.43	1.45
R-squared	0.028	0.411	0.191	0.005	0.852	0.856	0.007
Observations	6,839	6,839	6,839	6,839	4,690	4,690	4,690
Cohort FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Alphabet-Group FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Sample	All Students	All Students	All Students	All Students	Matched Students	Matched Students	Matched Students

Notes: Entries are point estimates and standard errors from estimating the difference-in-differences model in eq. (1). The sample in cols. (1)–(4) consists of all students who appear in a JCPS yearbook from the 1974/75 school year (i.e., pre-busing) and who, based on their grade level, should also appear in a yearbook for the following year (i.e., post-busing). Cols. (5)–(7) restrict attention to students that we can match across yearbooks. The outcome in col. (1) corresponds to an indicator for whether we can locate the individual in *any* 1975/76 yearbook. The outcome in cols. (2) and (5) corresponds to an indicator for whether the student appears in a yearbook of a city school, i.e., a school that was previously part of the Louisville Independent School District. The outcome in cols. (3) and (6) corresponds to an indicator for whether a student appears in a county-school yearbook, i.e., a yearbook from a school that, prior to the merger, was part of the Jefferson County school system; and the outcome in cols. (4) and (7) corresponds to an indicator for whether the student appears in a yearbook for a school that had been exempted from the court-ordered busing plan. All estimates are scaled to correspond to percentage-point changes. Standard errors are clustered at the cohort-by-alphabet-group level. ***, ** and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

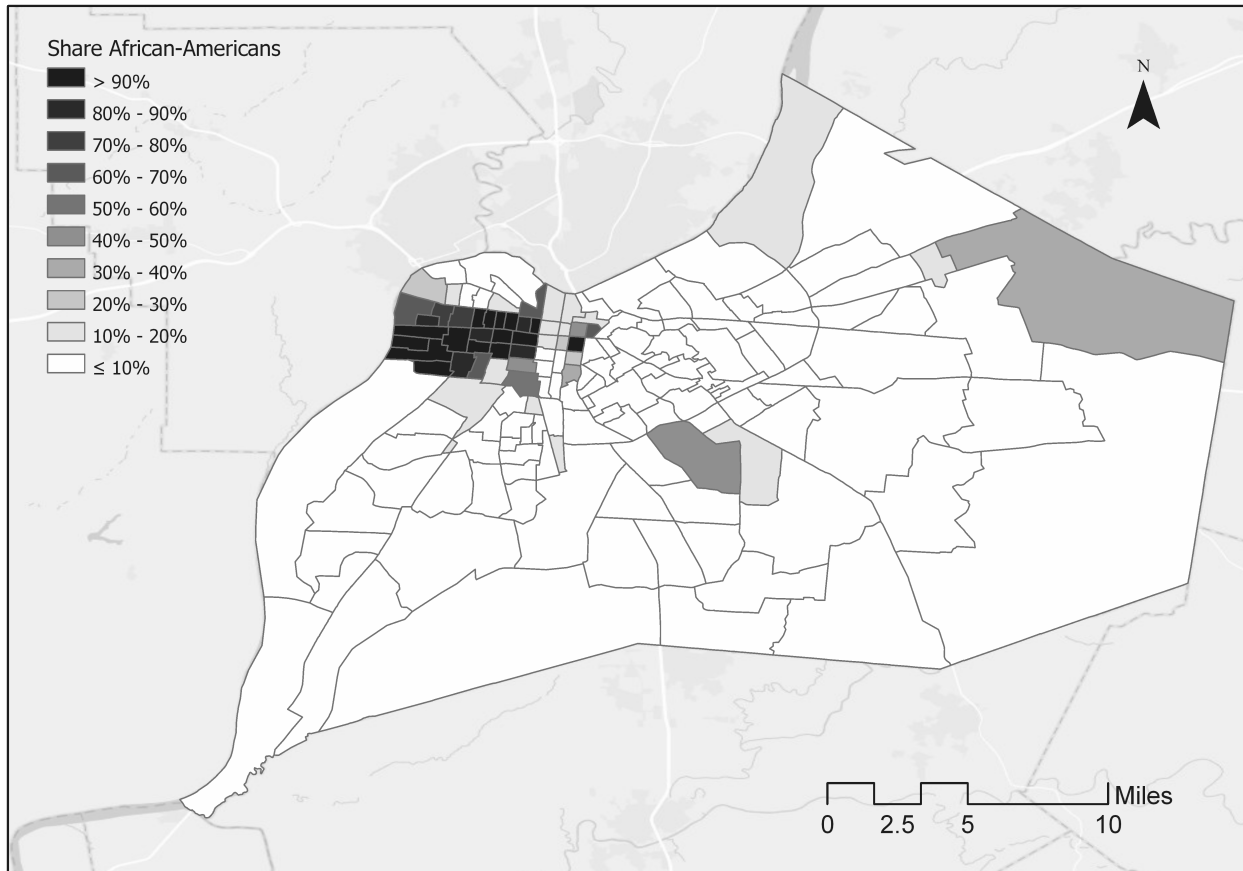
Appendix Table A.17: Compliance: Yearbook-to-Commencement-List Matching

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Commencement List Entry						
	Any School	City School	County School	Exempt School	City School	County School	Exempt School
Assigned Busing in Grade 12	-8.07*** (1.62)	24.31*** (3.77)	-39.69*** (2.46)	6.96** (2.55)	53.46*** (7.29)	-70.87*** (2.01)	16.77*** (5.74)
Mean of Dep. Var.	52.84	4.98	42.56	5.50	9.42	80.54	10.41
R-squared	0.015	0.169	0.100	0.017	0.017	0.017	0.017
Observations	11,614	11,614	11,614	11,614	6,137	6,137	6,137
Cohort FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Alphabet-Group FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Sample	All Students	All Students	All Students	All Students	Matched Students	Matched Students	Matched Students

Notes: Entries are point estimates and standard errors from estimating the difference-in-differences model in eq. (1). The sample in cols. (1)–(4) consists of all students who appear in a pre-busing JCPS yearbook and who, if they remain enrolled in a JCPS high school, we should be able to match to an entry in one of the post-busing commencement lists that we collected. Cols. (5)–(7) restrict attention to matched students only. The outcome in col. (1) corresponds to an indicator for whether we can locate the individual on *any* post-busing commencement list. The outcome in cols. (2) and (5) corresponds to an indicator for whether the student appears on a commencement list of a city school, i.e., a school that was previously part of the Louisville Independent School District. The outcome in cols. (3) and (6) corresponds to an indicator for whether a student appears on a county-school list, i.e., a commencement list from a school that, prior to the merger, was part of the Jefferson County school system; and the outcome in cols. (4) and (7) corresponds to an indicator for whether a student appears in a commencement list of a school that had been exempted from the court-ordered busing plan. All estimates are scaled to correspond to percentage-point changes. Standard errors are clustered at the cohort-by-alphabet-group level. ***, ** and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

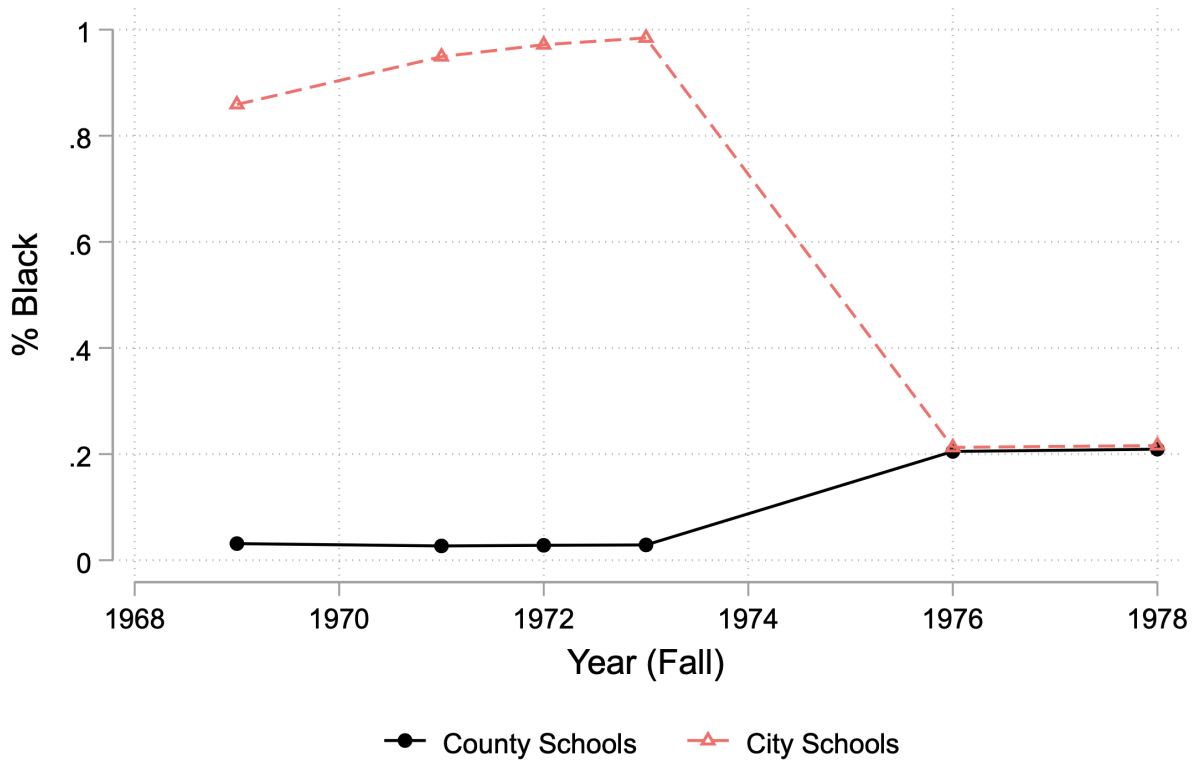
Appendix Figures

Appendix Figure A.1: Residential Segregation in Jefferson County, KY



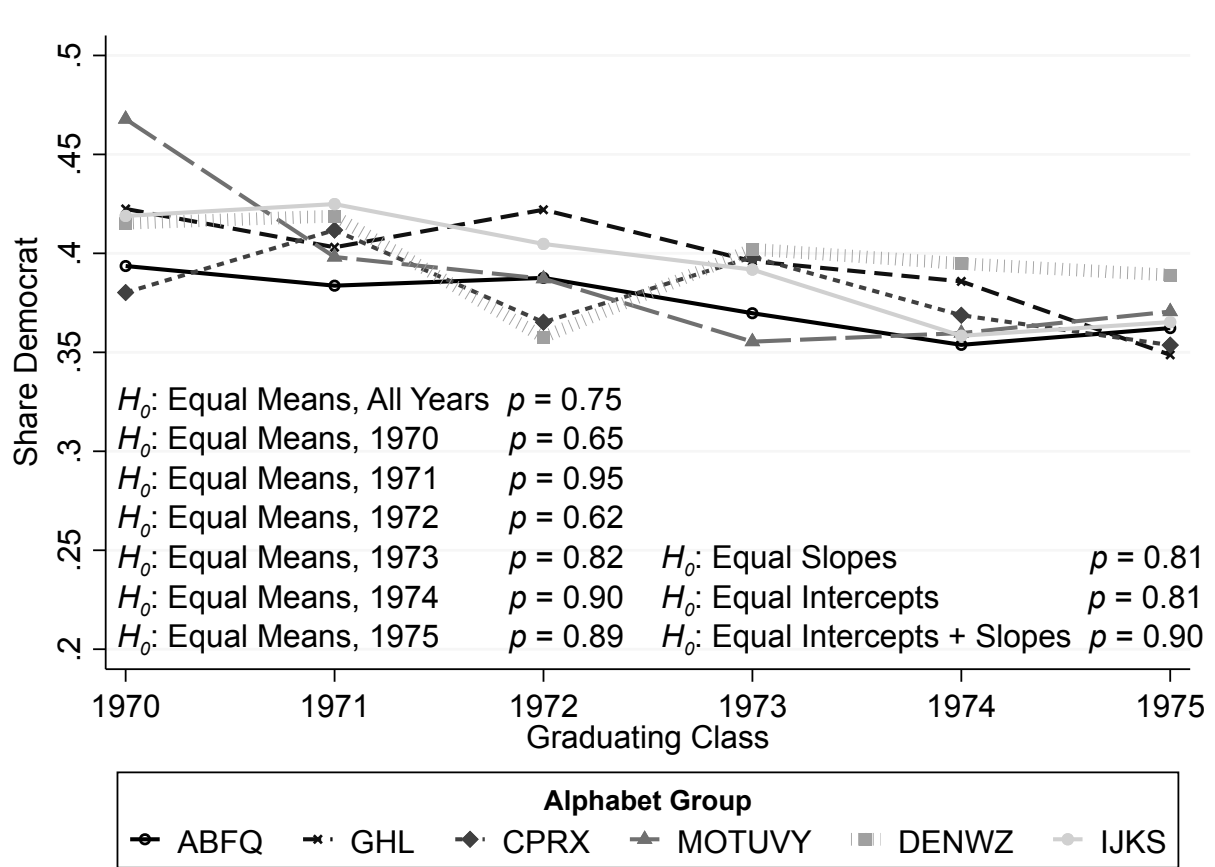
Notes: Figure shows the share of African Americans as of the 1970 Decennial Census for each census tract in Jefferson County, KY

Appendix Figure A.2: Racial Integration by Year and Type of School



Notes: Figure shows the average share of African Americans among students in JCPS high schools, separately for “city” and “county” schools. Prior to desegregation, city schools were part of the Louisville Independent School District, whereas county schools belonged to the Jefferson County school system. The data come from reports by the Office of Civil Rights at the Department of Education.

Appendix Figure A.3: Pretrends, by Alphabet Group



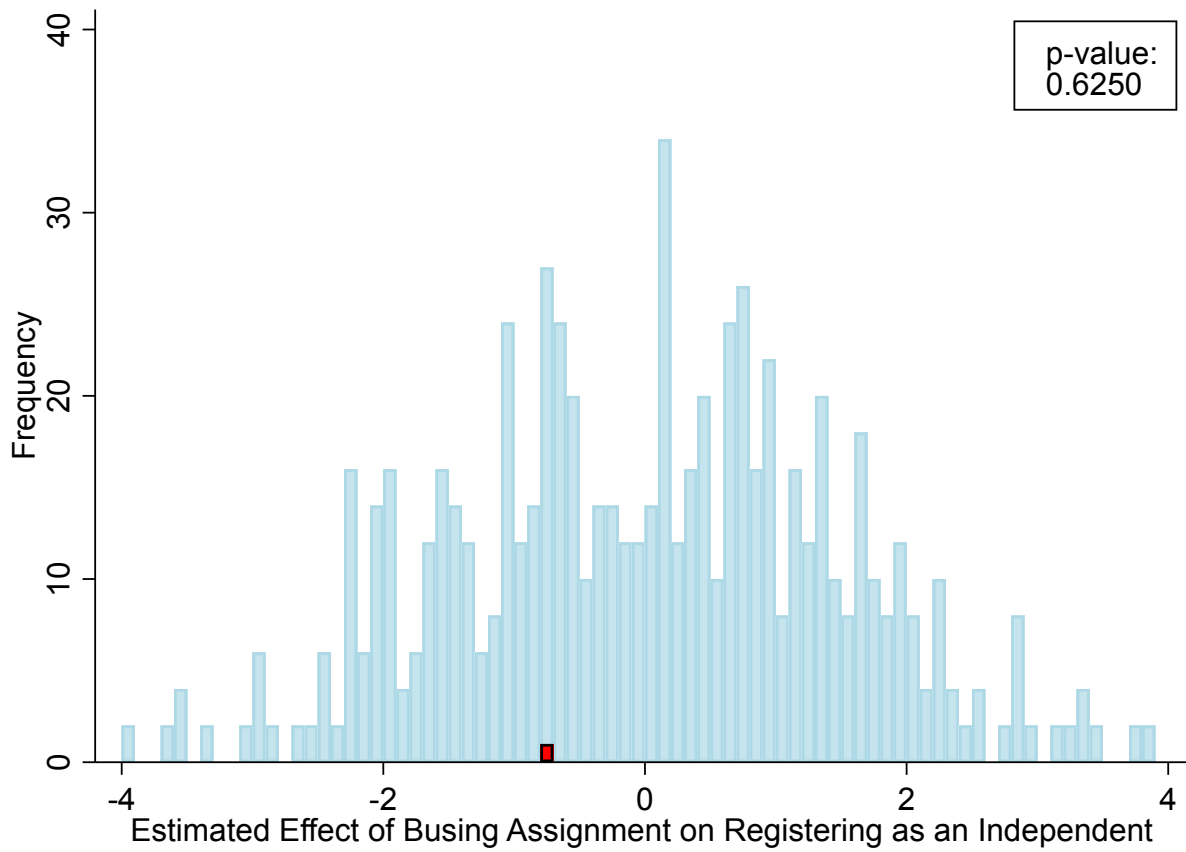
Notes: Figure shows the share of registered Democrats in each alphabet group among individuals in every cohort graduating prior to the court-ordered desegregation plan going into effect. Reported p -values refer to tests of the null hypothesis of no differences across all six alphabet groups. The hypothesis tests in the right column are based on a linear regression model with a separate intercept and slope for each alphabet group.

Appendix Figure A.4: TWFE Weights



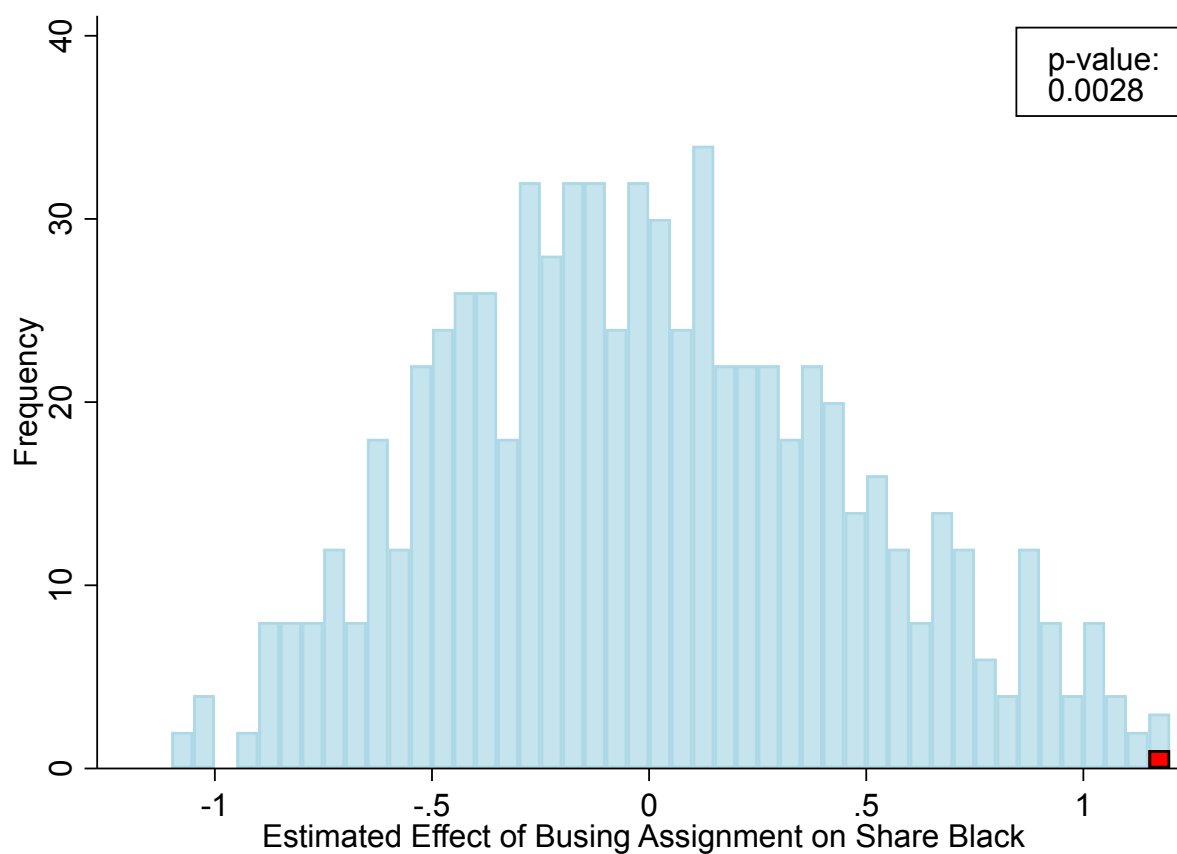
Notes: Figure depicts the weights that the TWFE estimator uses to aggregate treatment effects across all alphabet-group-cohorts. Weights are calculated as in De Chaisemartin and d'Haultfoeuille (2020).

Appendix Figure A.5: Randomization Inference Results: No Party Affiliation



Notes: Figure displays results from conducting randomization inference for β in eq. (1) based on all 720 possible treatment assignments from combining different alphabet and grade-level groups. The outcome is being registered as neither Democrat nor Republican. The actual estimated treatment effect from Table 5 is highlighted in red. The reported p -values correspond to the share of estimates whose absolute value is weakly greater than that of the true point estimate.

Appendix Figure A.6: Randomization Inference Results: Share African Americans in Neighborhood



Notes: Figure displays results from conducting randomization inference for β in eq. (1) based on all 720 possible treatment assignments from combining different alphabet and grade-level groups. The outcome is the zip-code level share of African Americans in individuals' residential neighborhoods. The actual estimated treatment effect from Table 8 is highlighted in red. The reported p -values correspond to the share of estimates whose absolute value is weakly greater than that of the true point estimate.

Appendix Figure A.7: Survey Materials

You've been selected to participate in the Jefferson County Study conducted by NORC at the University of Chicago, a nonpartisan research institution.



55 East Monroe Street
Chicago, IL 60603

Have your voice heard and earn \$30
If you have questions, please email us at JeffersonCounty@norc.org.

To participate, follow these three easy steps:

- 1 Visit **JeffersonCo.norc.org** or or call XXXXXXXXX.
- 2 Complete this 10-minute survey using your unique **PIN**.
Scratch off for PIN.
- 3 Get **\$30** and help us understand the views of people like you who attended high school in Jefferson County.

Scratch off to show PIN

8698339

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Have your voice heard and earn \$30 by completing a 10-minute survey.



Dear XXXXX,

This is an important, nonpartisan study to understand the views and experiences of people who attended high school in Jefferson County, Kentucky. You have been randomly selected to represent hundreds of Jefferson County former students like you.

You will receive \$30 for completing the 10-minute survey. To participate, please visit us at JeffersonCo.norc.org and enter your PIN (see other side) or call us toll-free at XXXXXXXXXX.

This study is being conducted by NORC at the University of Chicago. NORC is an independent, nonprofit research institution that has been delivering reliable data and analysis for nearly 80 years. For more information about NORC, please visit <https://www.norc.org>.

Thank you for your help. We really appreciate your time and effort.

Sincerely yours,



XXXXXXXXXXXXXXXXXX
Vice President
NORC at the University of Chicago



JEFFERSON COUNTY STUDY



This survey has been designed especially for [RESPONDENTSNAME].
Only [RESPONDENTSNAME] should fill out this survey.

Welcome to the Jefferson County Study. Your opinions matter to us at NORC at the University of Chicago. We are conducting an independent, scientific study to understand the views of people like you who attended high school in Jefferson County, Kentucky. We are especially interested in your experiences in school and your views on a range of issues in the news. You have been randomly selected to represent hundreds of Jefferson County former students like you.

You will receive a \$30 gift card of your choice for completing the survey. The survey is easy to do, and all of your responses are anonymous. We will protect your confidentiality and not sell your data.

This study is being conducted by NORC at the University of Chicago. NORC is an independent, nonprofit research institution that has been delivering reliable data and analysis for nearly 80 years. For more information about NORC, please visit <https://www.norc.org>.





INSTRUCTIONS

Thank you for agreeing to participate in our survey!
We are conducting a very important study to understand views of people who attended high school in Jefferson County, Kentucky.

- Please use a blue or black pen to complete this form.
- Mark ☒ to indicate your answer.
- If you want to change your answer, darken the box ☒ on the wrong answer and mark your new answer.
- Please mark only one response for each question.

Before continuing, please know that your participation is voluntary. You may choose to skip any question or end the survey at any point. We will take all possible steps to protect your privacy and we can use your answers only for statistical research. This means that no individual will be identified in any of the analyses or reports from this study. We anticipate this survey will take about 10 minutes to complete.

First, we will ask you some questions about the government.

1. How much of the time do you think you can trust the government in Washington to do what is right?

- ☐ None of the time
☐ Some of the time
☐ Most of the time
☐ Just about always

2. Would you say the government is pretty much run by a few big interests looking out for themselves or that it is run for the benefit of all the people?

- ☐ The government is run by a few big interests looking out for themselves
☐ The government is run for the benefit of all people

3. Do you think that people in government waste a lot of the money we pay in taxes, waste some of it, or don't waste very much of it?

- ☐ Waste a lot of it
☐ Waste some of it
☐ Do not waste very much of it

4. When it comes to the people running the government, do you think that quite a few are crooked, not very many are, or do you think hardly any of them are crooked?

- ☐ Quite a few
☐ Not many
☐ Hardly any

5. In talking to people about elections, we often find that a lot of people were not able to vote because they weren't registered, they were sick, or they just didn't have time. Which of the following statements best describes you in the elections in November 2020?

- ☐ I did not vote
☐ I thought about voting this time, but didn't
☐ I usually vote, but didn't this time
☐ I am sure I voted

6. Regardless of whether or not you actually voted, would you have rather seen Donald Trump or Joe Biden become president of the United States?

- ☐ Donald Trump
☐ Joe Biden

7. What about the 2012 election? Would you have rather seen Mitt Romney or Barack Obama become president?

- ☐ Mitt Romney
☐ Barack Obama

8. As you may know, there are some issues on which Americans tend to disagree. We are interested in your opinion. Do you think the U.S. government is doing too little, too much, or about the right amount in order to reduce the effects of climate change?

- ☐ Too little
☐ Too much
☐ About the right amount

9. Do you think marriages between same-sex couples should be recognized by the law as valid, with the same rights as traditional marriages?

- ☐ Yes
☐ Maybe
☐ No

10. In general, do you feel that the laws covering the sale of firearms should be made more strict, less strict, or kept as they are now?

- ☐ More strict
☐ Less strict
☐ Kept as they are now

11. With respect to the abortion issue, would you consider yourself to be pro-choice or pro-life?

- ☐ Pro-choice
☐ Pro-life
☐ Neither

12. How strongly do you agree or disagree with the following statements?

	Strongly agree	Somewhat agree	Neither agree nor disagree	Somewhat disagree	Strongly disagree
a. I feel that people get what they are entitled to have.	☐	☐	☐	☐	☐
b. I feel that a person's efforts are noticed and rewarded.	☐	☐	☐	☐	☐
c. I feel that people earn the rewards and the punishments they get.	☐	☐	☐	☐	☐
d. I feel that people who meet with misfortune have brought it on themselves.	☐	☐	☐	☐	☐
e. I feel that people get what they deserve.	☐	☐	☐	☐	☐
f. I feel that rewards and punishments are fairly given.	☐	☐	☐	☐	☐
g. I basically feel that the world is a fair place.	☐	☐	☐	☐	☐

13. Some people think that the government in Washington ought to reduce 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 differences in income between the rich and the poor.

On a scale from 1 to 5, what score comes closest to the way you feel? 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 5 meaning that the government should not concern itself with reducing income differences.

- ☐ 1 - the government ought to reduce the income differences between rich and poor
- ☐ 2
- ☐ 3
- ☐ 4
- ☐ 5 - the government should not concern itself with reducing income differences

14. Do you favor, oppose, or neither favor nor oppose more policies to improve the economic opportunities for children born in poor families, such as universal pre-school – even if it would have to be the case that either other policies are scaled down or taxes are raised?

- ☐ Strongly favor
- ☐ Somewhat favor
- ☐ Neither favor nor oppose
- ☐ Somewhat oppose
- ☐ Strongly oppose

15. Do you favor, oppose, or neither favor nor oppose spending more money to provide decent housing for those who cannot afford it – even if it would have to be the case that either other policies are scaled down or taxes are raised?

- ☐ Strongly favor
- ☐ Somewhat favor
- ☐ Neither favor nor oppose
- ☐ Somewhat oppose
- ☐ Strongly oppose

16. Do you think the government currently spends too little, too much, or about the right amount on welfare?

- ☐ Too little
- ☐ Too much
- ☐ About the right amount

17. The estate tax is a tax on the transfer of wealth from a deceased person to her heirs. This tax applies only to individuals with wealth above a certain threshold, and supporters of the estate tax argue that the government should use revenues from the estate to combat inequality. Opponents of the estate tax argue that the estate tax is unfair because it amounts to double taxation.

Do you favor, oppose, or neither favor nor oppose the estate tax?

- ☐ Strongly favor
- ☐ Somewhat favor
- ☐ Neither favor nor oppose
- ☐ Somewhat oppose
- ☐ Strongly oppose

18. Do you favor, oppose, or neither favor nor oppose raising the federal minimum wage to \$15.00 an hour?

- ☐ Strongly favor
- ☐ Somewhat favor
- ☐ Neither favor nor oppose
- ☐ Somewhat oppose
- ☐ Strongly oppose

19. Do you approve or disapprove of labor unions?

- ☐ Approve
- ☐ Disapprove

20. Some people think that African-Americans and other minorities 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 particular racial groups.

On a scale from 1 to 5, where would you place yourself on this spectrum?

- ☐ 1 - Government should help
- ☐ 2
- ☐ 3 - Agree with both statements
- ☐ 4
- ☐ 5 - No special treatment for minorities



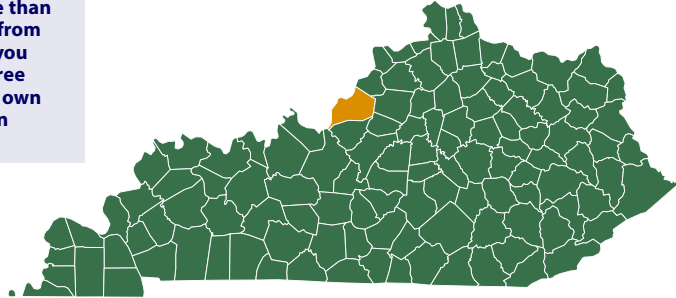
21. Do you agree, disagree, or neither agree nor disagree with each of the following statements?

	Strongly agree	Somewhat agree	Neither agree nor disagree	Somewhat disagree	Strongly disagree
a. Irish, Italians, Jewish and many other minorities overcame prejudice and worked their way up. Blacks should do the same without any special favors.	☐	☐	☐	☐	☐
b. Generations of slavery and discrimination have created conditions that make it difficult for blacks to work their way out of the lower class.	☐	☐	☐	☐	☐
c. Over the past few years, blacks have gotten less than they deserve.	☐	☐	☐	☐	☐
d. It's really a matter of some people not trying hard enough; if blacks would only try harder they could be just as well off as whites.	☐	☐	☐	☐	☐



22. Some people claim it's more common sense than prejudice to discourage your own children from marrying an African American partner. Do you agree, disagree, or neither agree nor disagree that it is common sense to discourage your own children from marrying an African American partner?

- ☐ Strongly agree
☐ Somewhat agree
☐ Neither agree nor disagree
☐ Somewhat disagree
☐ Strongly disagree



23. Do you agree, disagree, or neither agree nor disagree with the following statements?

	Strongly agree	Somewhat agree	Neither agree nor disagree	Somewhat disagree	Strongly disagree
a. The Civil War was mostly fought over states' rights rather than slavery.	☐	☐	☐	☐	☐
b. The U.S. government should officially apologize for slavery.	☐	☐	☐	☐	☐
c. African Americans are too quick to claim that innocent remarks or behavior are signs of racism.	☐	☐	☐	☐	☐

24. These days, there is a lot talk about racial bias in policing. We are interested in your opinion. What do you think? Compared to how the police treat white people, do they treat otherwise similar African Americans...

- ☐ A lot better
☐ A little better
☐ The same
☐ A little worse
☐ A lot worse

25. We will now give you the opportunity to authorize a \$1 donation to either the Black Lives Matter movement or the National Police Foundation.

In case you haven't heard of these, Black Lives Matter is a social movement that advocates against police brutality and racially motivated violence against African Americans. The National Policing Institute is an organization that works with police officers and police agencies across the country to help police be more effective in doing their job. It believes that police are a crucial link in the nation's system for crime control.

If you authorize us, then we – the researchers – will make an anonymous donation on your behalf. Your payment for participating in this survey will not be affected by your choice.

- ☐ Please donate \$1 to Black Lives Matter
☐ Please donate \$1 to the National Policing Institute
☐ Do not donate on my behalf

Now we will ask you some questions about your experience in high school.

26. Please list the names of the high school(s) that you attended, if any.



27. Thinking back to your high-school days, how strongly do you agree or disagree with each of the following statements about your teachers?

	Strongly agree	Somewhat agree	Neither agree nor disagree	Somewhat disagree	Strongly disagree
a. My teachers prepared me academically for life after high school.	☐	☐	☐	☐	☐
b. My teachers helped me better understand other people from different parts of American society.	☐	☐	☐	☐	☐
c. I was impressed with my teachers.	☐	☐	☐	☐	☐
d. A substantial fraction of my teachers were of a different race than I.	☐	☐	☐	☐	☐

28. Thinking back to your high-school days, how strongly do you agree or disagree with each of the following statements about your friends?

	Strongly agree	Somewhat agree	Neither agree nor disagree	Somewhat disagree	Strongly disagree
a. I met most of my friends at school.	☐	☐	☐	☐	☐
b. My friends helped me better understand other people from different parts of American society.	☐	☐	☐	☐	☐
c. A substantial fraction of my friends came from poor families.	☐	☐	☐	☐	☐
d. A substantial fraction of my friends were of a different race than I.	☐	☐	☐	☐	☐

29. Thinking back to your high-school days, how strongly do you agree or disagree with each of the following statements about your school?

	Strongly agree	Somewhat agree	Neither agree nor disagree	Somewhat disagree	Strongly disagree
a. I usually felt safe or very safe at school.	☐	☐	☐	☐	☐
b. Attending high school in a different part of town helped me better understand other people from different parts of American society.	☐	☐	☐	☐	☐
c. A substantial fraction of students came from poor families.	☐	☐	☐	☐	☐
d. A substantial fraction of students were of a different race than I.	☐	☐	☐	☐	☐

[illegible]

- ☐ Democrat (GO to Q31a)
- ☐ Republican (SKIP to Q31b)
- ☐ Independent (SKIP to Q31i)
- ☐ None of these (SKIP to Q31i)

- ☐ Strong Democrat (SKIP to Q32)
- ☐ Not so strong Democrat (SKIP to Q32)

☐ Strong Republican (SKIP to Q32)

☐ Not so strong Republican (SKIP to Q32)

- ☐ Lean Democrat
- ☐ Lean Republican
- ☐ Don't lean

- ☐ Very liberal
- ☐ Liberal
- ☐ Slightly liberal
- ☒ Moderate or middle of the road
- ☐ Slightly conservative
- ☐ Conservative
- ☐ Very conservative

☐ Male
☐ Female
☐ Other

Year

☐ Married

☐ Widowed

☐ Divorced or separated

☐ Never married

- ☐ White
- ☐ Black
- ☐ Hispanic
- ☐ Asian American
- ☐ Other

- ☐ Some high school or less
- ☐ High school or GED
- ☐ Some college or associate degree
- ☐ Bachelor's degree
- ☐ Master's degree or higher

- ☐ Yes, liberal bias
- ☐ Yes, conservative bias
- ☐ No, it did not appear politically biased

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THAT'S IT!

That's the end of the survey. In appreciation of your participation, we will send you a Mastercard Reward Card.* It will take about 3-5 weeks to process and mail the reward once we receive your questionnaire. The card will be sent via USPS mail in a windowed envelope from Reward Center. We will need your full name and the mailing address where we can send you the gift card.

*The MasterCard Reward Card can be used to buy what you want, when you want it. Because it is so flexible and convenient, the MasterCard Reward Card makes it easy to treat yourself to something special or to help cover your everyday expenses. The decision is yours. This card is issued by Sutton Bank, member FDIC, pursuant to license by MasterCard International. Card powered by Marqeta.

First name:

Last name:

Street address:

Apartment:

City:

State:

Zip:

THANK YOU!

Those are all the questions we have for you today. Thank you very much for participating. We really appreciate that you shared your valuable time and opinions.

Please return the completed questionnaire in the enclosed postage-paid envelope. If you have lost the envelope, please email us at JeffersonCounty@norc.org.

If you have any questions about your rights as a study participant, you may call the NORC Institutional Review Board, toll free, at 866-309-0542. Any other questions can be directed to the study's toll free number: 800-795-6586.

**JEFFERSON
COUNTY
STUDY**