A Different World: Enduring Effects of School Desegregation on Ideology and Attitudes*

Ethan Kaplan, *University of Maryland*
Jörg L. Spenkuch, *Northwestern University*
Cody Tuttle, *UT Austin*

January 2025

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 point to a poverty-centered version of the contact hypothesis, whereby witnessing economic deprivation durably sensitizes individuals to issues of inequality and fairness.

*We have benefited from helpful conversations with Stephen Billings, John Bullock, Eric Chyn, Allan Drazen, Arindrajit Dube, Kareem Haggag, Tom Hubbard, Ilyana Kuziemko, Matt Lowe, Suresh Naidu, Jesse Shapiro, as well as seminar participants at Harvard University, Princeton University, University of British Columbia, University of Houston, University of Massachusetts at Amherst, University of New South Wales, University of Queensland, the 7th Columbia University Conference in Political Economy, the 2023 Meeting of the European Economic Association, and the 2025 Meeting of the American Economic Association. Jimmy Grant, Alessio Ruvinov, Drew White, Ryan Willoughby, and especially Daniel Kolliner and John Iselin provided excellent research assistance. Additionally, we are grateful to James Cundy and the staff at the JCPS Archives and Records Center for their support in locating and understanding archival yearbook and commencement records. Funding was generously provided by the National Science Foundation (Awards 2018614 and 2018869), the Russell Sage Foundation (Grant 2008-27278), and the Ford Center for Global Citizenship at Northwestern University. The research in this paper has been reviewed by the Institutional Review Boards at Northwestern University, University of Texas, and NORC at the University of Chicago. All errors are our own. Correspondence can be addressed to `kaplan@econ.umd.edu` [Kaplan], `j-spenkuch@kellogg.northwestern.edu` [Spenkuch], or `cody.tuttle@utexas.edu` [Tuttle].

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, previously all-white schools and, 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 socio-political preferences of whites. A unique feature of our research design is that we can isolate the effect of attending an inner-city school net of greater cross-racial exposure. *A priori*, it is unclear what, if any, effects busing might have had. On the one hand, racial integration of public schools 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. That is, in response to the perceived federal overreach, some of the affected students might have adopted more conservative positions and turned to the Republican Party instead.[1] On the other hand, Allport's (1954) contact hypothesis suggests that 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.

We examine the long-run impact of being bused to an inner-city school by drawing on a unique natural experiment.[2] In the summer of 1975, the newly merged Louisville-Jefferson County school district implemented a court-ordered 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 a formerly white school,

[1]President Johnson himself predicted such backlash in response to 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, at a campaign event in 1984, President Reagan referred to busing as "a social experiment that nobody wants" (Reagan Presidential Library, n.d.).

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

whereas white students were to be bused to a formerly black school. 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 attending 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 quasi-random variation in busing assignments, on which we rely in our empirical analysis.

In order to estimate the effect of being bused to an inner-city school, we implement a difference-in-differences design. This design leverages the variation in busing assignments across alphabet groups and cohorts, allowing us to compare contemporaneous outcomes across groups that graduated both before and after the court's desegregation order. Importantly, our difference-in-differences estimates isolate the impact of being bused to an inner-city school net of greater cross-racial exposure.

The first part of our analysis draws 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 imply that being bused does not increase political participation, as measured by voter registration rates and turnout. At the same time, 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. It, therefore, appears that bused individuals do not become politically more active, but their partisan preferences change.

In the second part of our analysis, we draw 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

[3]In total, African Americans accounted for only about 20% of students in the merged Louisville-Jefferson County school system (Sedler 2007). We, therefore, lack the power to detect even moderately large effect sizes for blacks. Our 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 (see Section 3).

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, 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 the surrounding community.

Our survey results corroborate 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 clear evidence, however, that busing increased cross-racial friendships. Nor do we detect an effect on racial attitudes more generally. The latter finding 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 minorities 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, judging by the evidence from our survey, that does not appear to have been the case.

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. [...] 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 are intent-to-treat estimates. 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,

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

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 estimates should be interpreted as 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. Intuitively, 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, potentially more palatable 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 may be less open-minded than the latter, then our intent-to-treat estimates still provide a lower bound on the local average treatment effect.

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 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 *re*segregation 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 intergroup contact 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 difference-in-differences design isolates the effect of experiencing an inner-city environment, holding exposure to minorities constant. While much of the literature focuses on cross-racial contact, we present evidence that exposure to poverty itself can have very large effects on individual attitudes and beliefs. Our findings, therefore, point to a poverty-centered version of the contact hypothesis, whereby witnessing economic deprivation sensitizes individuals to 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.

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.[5]

Like many other metropolitan areas, in the early 1970s, the Louisville metro area was highly segregated by race.[6] Most of the area's black residents were concentrated in just a few neighborhoods in the city of Louisville (cf. Appendix Figure A.1). At the time, the city of Louisville 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 14^{th} 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 order cross-district busing. Although the judge's reasoning seemingly conformed to the Supreme Court's eventual verdict in *Miliken v. Bradley*, in December of 1974, the 6^{th} Circuit Court of Appeals reversed the lower court's ruling.[7] In April, 1975, under the shadow of a potential integration order, the Kentucky State Board of Education mandated a full merger of the Louisville and Jefferson County school systems.

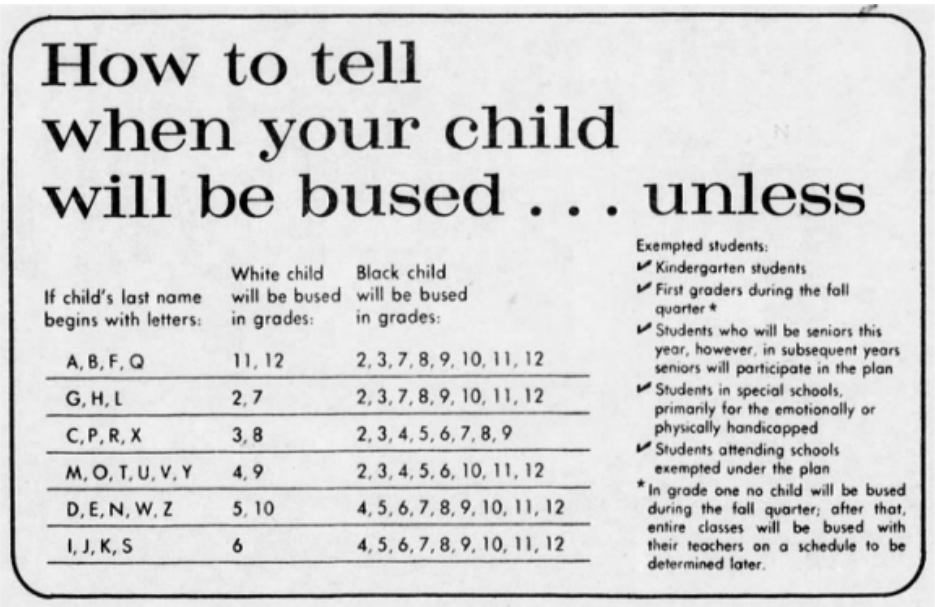
Late spring and early summer of 1975 were spent litigating what kind of desegregation

[5]The 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.

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

[7]*Miliken v. Bradley* allowed school segregation due to residential sorting across school districts. The 6^{th} Circuit Court of Appeal, however, found that there was no justification for permitting city and county to operate separate school districts given 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)

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)

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 6^{th} 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.[8] In a few cases, schools on the outskirts of Louisville could be more efficiently integrated through redistricting rather than 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

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

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 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.[9]

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. Thus, after 1975 all JCPS students experienced significant levels of cross-racial contact.[10]

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

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

[10]The 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.

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.[11]

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 the spring of 2020 and come from L2, Inc., a non-partisan for-profit data vendor that maintains high-quality databases of registered voters, political donors, and consumers.[12] 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, turnout history, and party affiliation.

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.[13] 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

[11]For a list of all schools and years for which we collected data, see Appendix Table A.1.

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

[13]Specifically, 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.

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.[14] 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 the refinement procedure above allows 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] 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 was still uncertain if and how the area's schools would be desegregated.[17] We impose the latter restriction due to concerns about sample selection as a result of "white flight," i.e., parents disenrolling their children from public school in response to the court-ordered desegregation plan. Given that, 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

[14] If a student can be matched to multiple birth certificates and there are multiple corresponding voter registration records, we further pare down the matches by using county of birth and/or middle initial.

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

[16] We 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.

[17] As 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.

[18] Appendix Table A.2 shows how we go from 177,032 student-year observations to a baseline sample of 32,568 white males.

Table 1: Match Statistics

	Number of Students		Any Match			Unique Match		
Cohort	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.

(Fabina and Scherer 2022).

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. Both differences are quantitatively modest but statistically significant. 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. 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 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 attitudes, 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).[19]

In total, NORC collected 559 in-scope responses on our behalf, for an overall response rate of 23.3%.[20] 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. Drawing on data from the American Community Survey, we calculate that the corresponding number among all white Kentucky-born males in the same age group is about 65%. Further inspection of the data reveals that the share of individuals who still live in Kentucky is almost ten percentage points higher among individuals who graduated in 1974—just before the enactment of the desegregation plan—than among subsequent cohorts. This observation is consistent with anecdotal accounts according to which the court-ordered desegregation of schools accelerated white flight.[21] 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

[19]For a copy of the survey instrument, see Appendix C.

[20]NORC 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.

[21]In Section 7, we explore the implications of white flight for the magnitude and interpretation of our results.

Table 2: Descriptive Statistics

Variable	N	Mean	SD	Min	Median	Max
A. Matched Yearbook Sample						
Treatment						
Assigned Busing	18,541	0.127	0.333	0	0	1
Demographics						
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
Party Registration						
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
Political Participation						
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
B. Survey Respondents						
Demographics & Politics						
Age	559	63.136	1.355	61	63	65
Registered Democrat (L2)	559	0.377	0.485	0	0	1
Registered Republican (L2)	559	0.509	0.500	0	1	1
Ideology (Likert Scale)	542	3.245	1.641	1	3	7
Verified Turnout, 2016 (L2)	559	0.844	0.363	0	1	1
Self-Reported Turnout, 2020	557	0.905	0.293	0	1	1
Summary Indices						
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
Conservative Racial Attitudes	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," "Conservative Racial Attitudes," 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 C for additional details.

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.[22]

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 positively selected on political participation, i.e., turnout.

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 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.[23]

4. Econometric Strategy

Our identification strategy follows a standard difference-in-differences approach. That is, we compare average political participation and preferences across alphabet groups within cohorts, before and after the enactment of the court-ordered desegregation plan. 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.

[22]Note, 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.

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

Table 3: Treatment Assignment, by Alphabet Group and Cohort

	Graduating Cohort					
Alphabet Group	$\leq$ 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.

To answer this question, we estimate a standard two-way fixed effects regression model:

$$(1) \qquad Y_{i,a,c} = \beta Assigned\, Busing_{a,c} + \mu_a + \chi_c + \epsilon_{i,a,c},$$

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. $Assigned\, Busing_{a,c}$ is an indicator variable equal to one if, and only if, alphabet group a of cohort c was bused, while μ_a and 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.

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 models like the one above need not properly aggregate heterogeneous treatment effects across cohorts (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 relevant, we 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 problem, and, reassuringly, yield similar 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 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 upon asymptotic approximations.

In a robustness check, we follow this alternative approach, assuming exchangeability in the assignment of alphabet groups to the grade levels in which they were bused (cf. Figure 1).[24] 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.

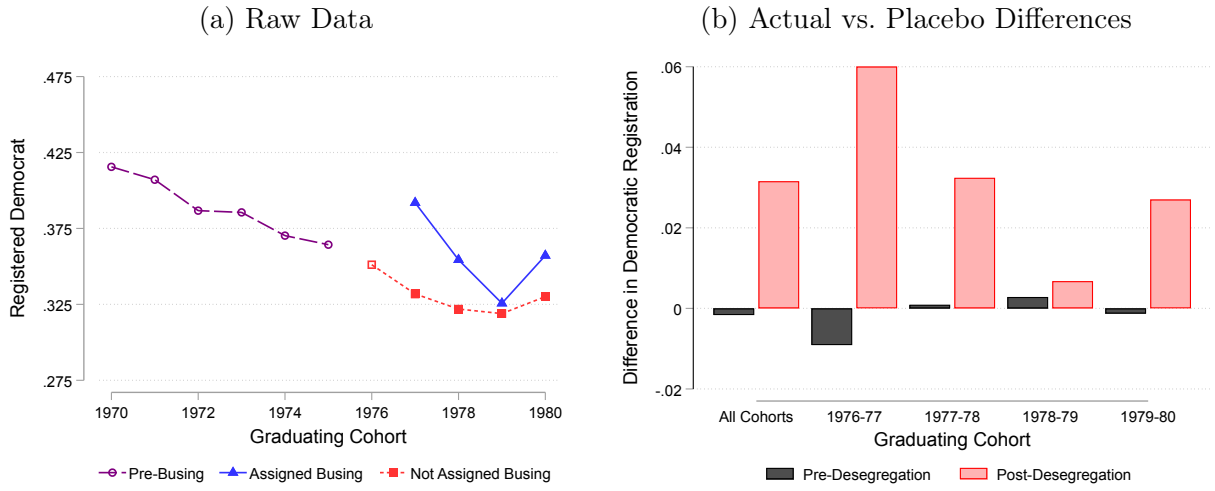
Measurement Error One potential source of bias in our estimates is measurement error due to false positive matches of yearbook data and voter registration records. There is likely a non-negligible number of treated students who are erroneously matched to the voter registration record of someone else 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 7 imply that noncompliers

[24]To be clear, we do not assume that when the courts ordered desegregation was random, nor do we assume that the assignment of last names to alphabet groups was random. We assume that the timing of busing for specific alphabet groups is as good as randomly assigned.

[25]Given the specifics of the court-ordered alphabet plan, there are no "always takers" in our setting. Thus,

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.

tended to either move to other, suburban public school districts or enroll in local private schools instead.

In Appendix D, we provide a sufficient condition on potential outcomes, under which our intent-to-treat estimates bound both the local average treatment effect (LATE) and the average treatment effect (ATE). In our setting, 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, potentially more palatable assumptions under which our intent-to-treat estimates can still be interpreted as bounds on the local average treatment effect (cf. Appendix D).

5. Effects on Political Participation and Preferences

5.1. *Raw Data*

Figure 2 provides the first piece of evidence that busing might have affected treated students' long-run politico-economic outcomes. The panel on the left of this figure plots the current

in the absence of white flight, the ATE corresponds to the effect of treatment on the treated (TOT).

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 (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 statistically indistinguishable

[26] The 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	0.89	0.53	0.83		
	(0.81)	(0.96)	(0.89)	(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	0.00	–0.26	0.47	0.18	–0.46	–0.70
	(0.98)	(1.33)	(0.90)	(1.23)	(1.11)	(1.51)
Mean of Dep. Var.	70.52	70.52	74.69	74.69	66.34	66.34
R-squared	0.008	0.008	0.006	0.006	0.010	0.010
Observations	111,246	111,246	55,623	55,623	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 2008–2018 (cols. 1 and 2), in presidential elections from 2008–2016 (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-by-election year. Standard errors are clustered at the cohort-by-alphabet-group level. ***, ** and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

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 2008–2018, all point es-

timates are quantitatively small and statistically insignificant. We note, however, that our turnout and also our registration estimates are imprecise, especially in specifications that control for alphabet-group-specific trends. Only pooling across all elections can we reject effect sizes larger than two percentage points. Taken together, the findings in Table 4 suggest that busing 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 at the 5% level 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]

There are two ways to interpret our findings in Table 5. It is possible that the experience of being bused 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 independents into Democrats and counterfactual Republicans into independents, leaving the overall share of independents nearly unaffected. Unfortunately,

[27]The results in Table 5 are based on our sample of uniquely matched yearbook and voter registration records. Appendix Table A.3 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.

Table 5: Effect of Busing on Party Affiliation

	All States		Closed Primary States	
	(1)	(2)	(3)	(4)
	Registered Democrat			
Assigned Busing	3.53***	3.43**	3.32***	3.03**
	(1.14)	(1.47)	(1.22)	(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**	–2.58*	–2.80**	–2.33
	(1.24)	(1.35)	(1.22)	(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	–0.85	–0.52	–0.70
	(1.07)	(1.22)	(0.99)	(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

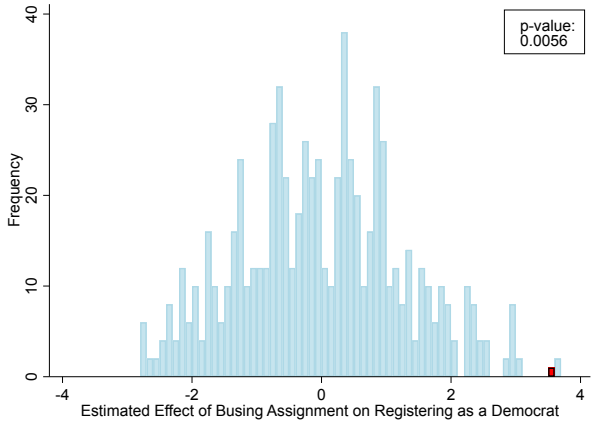
Notes: Entries are point estimates and standard errors from estimating the difference-in-differences model in eq. (1). The outcomes in the upper and middle panels are Democratic and Republican party registration, respectively. The outcome in the lower panel is the residual category, i.e., being registered 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.

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.[28]

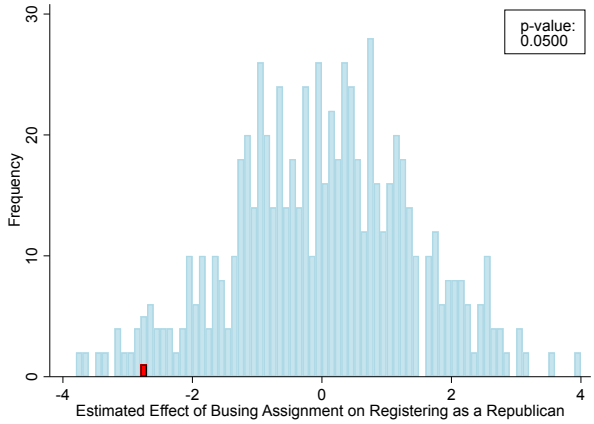
[28]In Appendix Table A.4, we differentiate between students who were bused for one versus two years.

Figure 3: Randomization Inference

(a) Registered Democrat



(b) Registered Republican



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.

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 treatments. 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 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

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.

support for our main result.[29]

Robustness Checks Appendix Table A.5 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.5 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.6.

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 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.

[29]For completeness, Appendix Figure A.3 presents results from conducting randomization inference for

Table 6: Exempt School Placebo

	(1)	(2)	(3)	(4)	(5)	(6)
	Party Registration					
	Democrat		Republican		Independent	
Actually Assigned Busing	3.70***	3.07**	–3.03***	–2.26**	–0.68	–0.81
	(1.13)	(1.45)	(1.10)	(1.11)	(1.04)	(1.17)
Placebo Assignment	–0.94	–1.60	0.32	1.24	0.62	0.37
	(1.54)	(2.00)	(2.19)	(2.50)	(1.52)	(1.74)
Exempt School	3.32***	3.34***	–2.15***	–2.17***	–1.18**	–1.17**
	(0.83)	(0.83)	(0.78)	(0.78)	(0.51)	(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.

Intuitively, our placebo analysis asks whether students who, given their last names, would have been assigned to be bused if they had not attended an exempt school experienced similar changes 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:

$$Y_{i,a,c} = \beta \mathit{Actually\ Assigned\ Busing}_{i,a,c} + \delta \mathit{Placebo\ Assignment}_{i,a,c} + \gamma \mathit{Exempt\ School}_i + \mu_a + \chi_c + \epsilon_{i,a,c}, \tag{2}$$

where $\mathit{Exempt\ School}_i$ 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. $\mathit{Placebo\ Assignment}_{i,a,c}$ 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. $\mathit{Actually\ Assigned\ Busing}_{i,a,c}$ denotes the assignment status of students in non-exempt schools. All other symbols are as

independents. Consistent with the evidence in Table 5, randomization inference produces a p-value of 0.625.

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, however, 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 affects 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 statically 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

In this section, we 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.[30]

[30] q-values differ from the family-wise error rate (FWER) in that the latter controls the probability of 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.

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.8 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 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.8 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.8 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 likely reason for the large estimated effect on preferences over candidates is that candidate support is more malleable than party identification.[31]

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

6.2. *Ideology and Attitudes*

We next turn to the effect of busing 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) subjects' racial attitudes, (ii) trust in government, (iii) support for redistribution and other progressive policies, and (iv) their 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.[32]

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 Attitudes We do not detect any effect of busing on attitudes towards African Americans. We arrive at this conclusion 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 and, if anything, imply racial views that are slightly more conservative. Analyzing the component questions of our racial attitudes index in isolation, we find that only one out of nine shows a statistically significant difference (cf. Appendix Table A.8). After adjusting for multiple hypothesis testing, none of the observed differences are statistically distinguishable from zero. In sum, we find no evidence to conclude that busing affects self-reported racial attitudes.

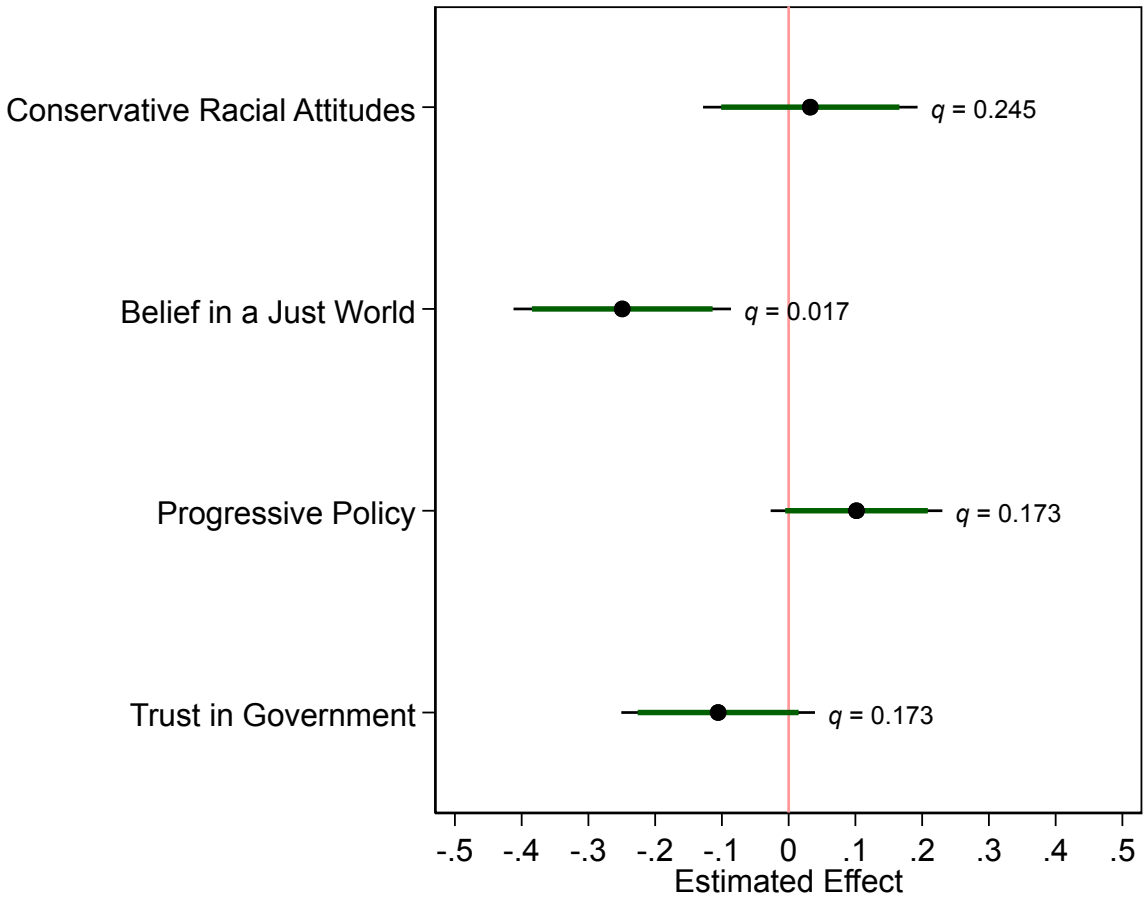
The apparent lack of effect may or may not be surprising. After all, once the court-ordered desegregation plan was implemented, all Louisville-area public school students encountered racially integrated school environments, not only those who were bused. Nonetheless, our null result on racial attitudes is noteworthy because it informs the debate around the scope conditions in Allport's (1954) contact hypothesis. *A priori* it might have seemed plausible that exposure to African Americans in an inner-city environment would be especially effective at reducing racial prejudice. Judging by our results, however, that does not appear to be the case.[33]

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

[32]In Appendix Tables A.8 and A.9, we analyze the answers to each survey question individually.

[33]Consistent with this conclusion, 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.10 presents the results above in numerical form. For results based on the individual component questions, see Appendix Tables A.8 and A.9.

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 (cf. Appendix Table A.8).

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 suggestive evidence 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.9 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 ($p = 0.147, q = 0.173$).[34] 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.9; see also Sapienza and Zingales 2012 and Kuziemko et al. 2015).

6.3. *Potential Mechanisms*

We now 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.11, we validate this result for our linked

[34]The 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).

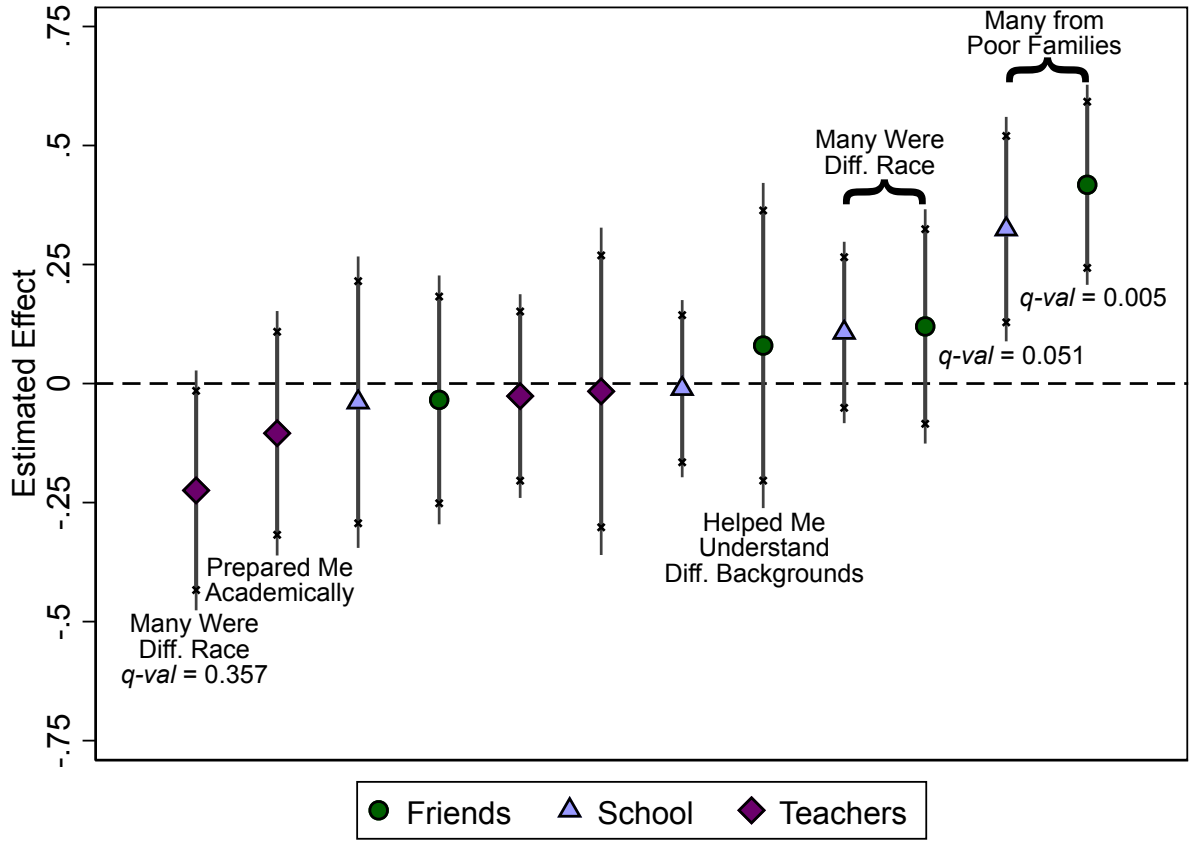
yearbook-L2 sample by estimating our workhorse difference-in-differences model in eq. (1) with zip-code-level average incomes as the outcome. Today, students who were assigned to be bused live in similar-income areas as their counterparts who were not assigned busing. In light of this finding, we discount the direct economic impact of being bused to an inner-city school and explore other explanations for the results above.

In order to examine other potential mechanisms, we 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, it appears that busing increased individuals' awareness of socioeconomic differences.

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.

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.10.

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 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

Table 7: Evidence from an Open-Ended Question

	(1)	(2)	(3)	(4)
	Mention Busing	Mention Teachers	Mention Friends	Mention Poverty
Assigned Busing	17.36***	3.29	8.69**	3.92**
	(4.16)	(6.38)	(3.45)	(1.61)
	[0.002]	[0.180]	[0.022]	[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 equ. (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.

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.

Although we cannot attribute magnitudes to mechanisms, the overall picture that emerges is that greater exposure to 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 are consistent with a poverty-centered version of Allport's (1954) contact hypothesis, whereby witnessing economic deprivation at a young age durably shapes political attitudes and beliefs.

7. Compliance, White Flight, and Persuasion Rates

7.1. *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.[35] 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 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 (cf. Appendix Table A.12).

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 schools 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

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

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.13, 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.13). 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% ($= 0.46/0.70$).[36]

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

[36]We 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.

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.

7.2. *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:

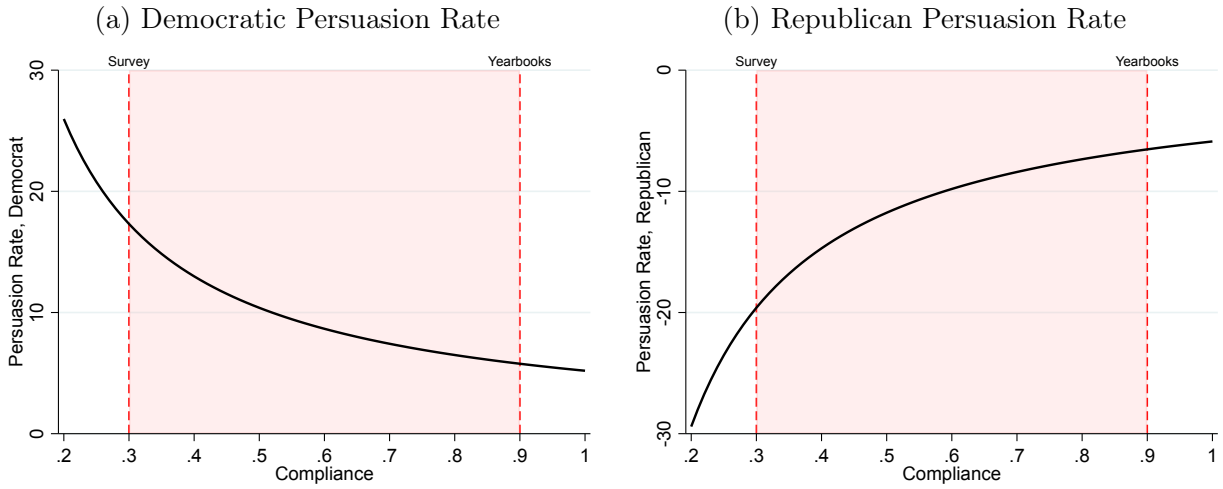
$$f_p = 100 \frac{y_{p,T} - y_{p,C}}{(e_T - e_C)\,(1 - \tilde{y}_p)}. \tag{3}$$

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, 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 6 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 6 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

Figure 6: 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 $\tilde{y}_R = 0.524$

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.

8. 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 attending 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 do not detect any effect of busing on racial attitudes. Given that

all Louisville-area public schools became racially integrated, we interpret the apparent lack of effect as speaking to the scope conditions in Allport's (1954) contact hypothesis. That is, exposure to African Americans in an inner-city environment does not appear to be more effective at reducing prejudice than exposure in a previously all-white suburban setting.

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 imply that early-life exposure to poverty shapes individual beliefs and long-run politico-economic attitudes. We view these results as pointing to a poverty-centered contact hypothesis, whereby witnessing economic deprivation sensitizes individuals to issues of inequality and fairness—even decades later.

References

Abadie, A., S. Athey, G. Imbens, and J. Wooldridge. 2020. "Sampling-Based versus Design-Based Uncertainty in Regression Analysis." *Econometrica*, 88(1): 265–296.

Alesina, A., S. Stantcheva, and E. Teso. 2018. "Intergenerational Mobility and Preferences for Redistribution." *American Economic Review*, 108(2): 521–554.

Allport, G. 1954. *The Nature of Prejudice.* Cambridge, MA: Perseus Books

Anderson, M. 2008. "Multiple Inference and Gender Differences in the Effects of Early Intervention: A Reevaluation of the Abecedarian, Perry Preschool, and Early Training Projects." *Journal of the American statistical Association*, 103(484): 1481–1495.

Angrist, J. and K. Lang. 2004. "Does School Integration Generate Peer Effects? Evidence from Boston's Metco Program." *American Economic Review*, 94(5): 1613–1634.

Baum-Snow, N. and Lutz, B. 2011. "School Desegregation, School Choice, and Changes in Residential Location Patterns by Race." *American Economic Review*, 101(7): 3019–3046.

Bergman, P. 2018. "The Risks and Benefits of School Integration for Participating Students: Evidence from a Randomized Desegregation Program." IZA Discussion Paper No. 11602.

Biden, J. 2007. *Promises to Keep: On Life and Politics.* New York: Random House.

Billings, S., D. Deming, and J. Rockoff. 2013. "School Segregation, Educational Attainment and Crime: Evidence from the End of Busing in Charlotte-Mecklenburg." *Quarterly Journal of Economics*, 129(1): 435–76.

Billings, S., E. Chyn, and K. Haggag. 2021. "The Long-Run Effects of School Racial Diversity on Political Identity." *American Economic Review: Insights*, 3(3): 267–284.

Bleemer, Z. 2022. "Affirmative Action, Mismatch, and Economic Mobility after California's Proposition 209." *Quarterly Journal of Economics*, 137(1): 115–160.

Boisjoly, J., G. Duncan, M. Kremer, D. Levy and J. Eccles. 2006. "Empathy or Antipathy? The Impact of Diversity." *American Economic Review* 96(5): 1890–1905.

Boustan, L. 2012. "School Desegregation and Urban Change: Evidence from City Boundaries."

American Economic Journal: Applied Economics 4(1): 85–108.

Bozio, A., B. Garbinti, J. Goupille-Lebret, M. Guillot, and T. Piketty. 2024. "Predistribution versus Redistribution: Evidence from France and the United States." *American Economic Journal: Applied Economics* 16(2): 31–65.

Calderon, A., V. Fouka, and M. Tabellini. 2023. "Racial Diversity and Racial Policy Preferences: The Great Migration and Civil Rights." *Review of Economic Studies*, 90(1): 165–200.

Callaway, B., and P. Sant'Anna. 2021. "Difference-in-Differences with Multiple Time Periods." *Journal of Econometrics* 225(2): 200–230.

Cantoni, E. and V. Pons. 2022. "Does context outweigh individual characteristics in driving voting behavior? Evidence from relocations within the United States." *American Economic Review* 112(4): 1226–1272.

Card, D. and J. Rothstein. 2007. "Racial Segregation and the Black–White Test Score Gap." *Journal of Public Economics* 91(11-12): 2158–2184.

Carrell, S., M. Hoekstra, and J. West. 2019. "The Impact of College Diversity on Behavior Toward Minorities." *American Economic Journal: Economic Policy*, 11(4): 159–182.

Cengiz, D., A. Dube, A. Lindner, and B. Zipperer. 2019. "The Effect of Minimum Wages on Low-Wage Jobs." *Quarterly Journal of Economics*, 134(3): 1405–1454.

Chicago Tribune. 1975. "10,000 Rampage in Louisville Busing Fight" September 6, p. S4.

DellaVigna, S. and E. Kaplan. 2007. "The Fox News Effect: Media Bias and Voting." *Quarterly Journal of Economics*, 122(3): 1187–1234.

DellaVigna, S. and M. Gentzkow. 2010. "Persuasion: Empirical Evidence." *Annual Review of Economics*, 2: 643–669.

Enos, R. 2014. "Causal Effect of Intergroup Contact on Exclusionary Attitudes." *Proceedings of the National Academy of Sciences*, 111(10): 3699–3704.

Fabina, J., and Z. Scherer. 2020. "Characteristics of Voters in the Presidential Election of 2020." U.S. Census Bureau Report No. P20-585.

Fisher, R. 1935. *The Design of Experiments*. Oxford: Oliver & Boyd.

Goodman-Bacon, A. 2021. "Difference-in-Differences with Variation in Treatment Timing." *Journal of Econometrics*, 225(2): 254–277.

Gordon, N. and S. Reber. 2018. "The Effects of School Desegregation on Mixed-Race Births." *Journal of Population Economics*, 31: 561–596.

Guryan, J.. 2004. "Desegregation and Black Dropout Rates." *American Economic Review* 94(4): 919–943.

Haaland, I., C. Roth, S. Stantcheva, and J. Wohlfart. "Measuring What is Top of Mind." NBER Working Paper No. 32421.

Halla, M, A. Wagner, and J. Zweimüller. 2017. "Immigration and Voting for the Far Right." *Journal of the European Economic Association*, 15(6): 1341–1385.

Henry, P.J. and D. Sears. 2002. "The Symbolic Racism 2000 Scale." *Political Psychology*, 23(2):

253–283.

Imbens, G. and D. Rubin. 2015. *Causal Inference for Statistics, Social, and Biomedical Sciences*. New York: Cambridge University Press.

Johnson, R. 2015. "Long-run Impacts of School Desegregation and School Quality on Adult Attainments." NBER Working Paper No. 16664.

Johnson, R. 201. *Children of the Dream: Why School Integration Works*. New York: Basic Books.

K'Meyer, T. 2013. *From Brown to Meredith: The Long Struggle for School Desegregation in Louisville, Kentucky, 1954–2007*. Chapell Hill, NC: University of North Carolina Press.

Kuziemko, I., M. Norton, E. Saez, and S. Stantcheva. 2015. 'How Elastic Are Preferences for Redistribution? Evidence from Randomized Survey Experiments." *American Economic Review*, 105(4): 1478–1508.

Kuziemko, I. and E. Washington. 2018. "Why Did the Democrats Lose the South? Bringing New Data to an Old Debate." *American Economic Review*, 108(10): 2830–2867.

Kuziemko, I, N. Longuet-Marx, and S. Naidu. 2023. "Compensate the Losers? Economic Policy and Partisan Realignment in the US." NBER Working Paper No. 31794.

Lerner, M., and C. Simmons. 1966. "Observer's Reaction to the "Innocent Victim": Compassion or Rejection?" *Journal of Personality and Social Psychology*, 4(2): 203–210.

Lipkus, I. 1991. "The Construction and Preliminary Validation of a Global Belief in a Just World Scale and the Exploratory Analysis of the Multidimensional Belief in a Just World Scale." *Personality and Individual Differences*, 12(11): 1171–1178.

Lowe, M. 2021. "Types of Contact: A Field Experiment on Collaborative and Adversarial Caste Integration." *American Economic Review*, 111(6): 1807–1844.

Lowe, M. 2024. "Has Intergroup Contact Delivered?" *Annual Review of Economics*, forthcoming.

Lutz, B. 2011. "The End of Court-Ordered Desegregation." *American Economic Journal: Economic Policy*, 3(2): 130–68.

Meyer, B. and N. Mittag. 2017. "Misclassification in Binary Choice Models." *Journal of Econometrics*, 200(2): 295–311.

Orfieled, G. and E. Frankenberg. 2014. "Brown at 60: Great Progress, a Long Retreat and an Uncertain Future." UCLA Civil Rights Project.

Paluck, E., S. Green, and D. Green. 2019. "The Contact Hypothesis Re-Evaluated." *Behavioural Public Policy*, 3(2): 129–158.

Rao, G. 2019. "Familiarity Does Not Breed Contempt: Generosity, Discrimination and Diversity in Delhi Schools." *American Economic Review*, 109(3): 774–809.

Reagan Presidential Library. n.d. "Remarks at a Reagan-Bush Rally in Charlotte, North Carolina." `https://www.reaganlibrary.gov/archives/speech/remarks-reagan-bush-rally-charlotte-north-carolina`

Reber, S. 2005. "Court-Ordered Desegregation Successes and Failures Integrating American Schools Since Brown versus Board of Education." *Journal of Human Resources*, 40(3): 559–590.

Reber, S. 2010 "School Desegregation and Educational Attainment for Blacks." *Journal of Human Resources*, 45(5): 839–914.

Sapienza, P., and L. Zingales. 2013. "Economic Experts versus Average Americans." *American Economic Review*, 103(3): 636–642.

Sedler, R. 2007. "The Louisville-Jefferson County School Desegregation Case: A Lawyer's Retrospective." *Register of the Kentucky Historical Society*, 105(1): 3–32.

Semuels, A. 2015. "The City That Believed in Desegregation." *The Atlantic*, March 2015. available at `http://bit.ly/2VLCklz`.

Spenkuch, J., E. Teso and G. Xu. 2023. "Ideology and Performance in Public Organizations." *Econometrica*, 91(4): 1171–1203.

Storey, J. 2003. "The Positive False Discovery Rate: A Bayesian Interpretation and the Q-value" *Annals of Statistics*, 31(6): 2013–2035.

Tabellini, M. 2020. "Gifts of the Immigrants, Woes of the Natives: Lessons from the Age of Mass Migration." *Review of Economic Studies* 87(1): 454–486.

Tesler, M. 2013. "The Return of Old-Fashioned Racism to White Americans' Partisan Preferences in the Early Obama Era." *Journal of Politics* 75(1): 110–123.

Tuttle, Cody. 2019. "The Long-Run Economic Effects of School Desegregation." mimeographed, University of Maryland.

U.S. Commission on Civil Rights. 1977. *Reviewing a Decade of School Desegregation, 1966–75: Report of a National Survey of School Superintendents.* Washington, D.C.: U.S. Government Printing Office.

Online Appendix

Contents

A Additional Results and Robustness Checks **3**

B Matching Procedure **5**

B.1 Importing and Cleaning of Yearbook Records 5

B.2 Initial Merge of Yearbook Records to L2 Voter File 7

B.3 Filtering via Birth Records 8

B.4 Adjustments Based on Survey 10

B.5 Final Analysis File 10

C Jefferson County Busing Survey **11**

C.1 Data Collection 11

C.2 Data Processing 11

C.3 Weighting 12

D Noncompliance and Bounds on Treatment Effects **12**

Appendix Tables **17**

Appendix Figures **31**

List of Tables

A.1 Commencement and Yearbook Records 17

A.2 Sample Construction 18

A.3 Effect of Busing on Party Affiliation, Alternative Matching Restrictions 19

A.4 Intensive Margin Effect of Busing on Party Affiliation 20

A.5 Effect of Busing on Party Registration, Inclusion of Various Fixed Effects . . . 21

A.6 Effect of Busing on Party Affiliation, Robustness to D-i-D Esimator 22

A.7 Effect of Busing on Turnout, Robustness to D-i-D Estimator 23

A.8 Survey Results: Index Components and Other Outcomes 24

A.9 Survey Results: Index Components and Other Outcomes (Continued) 25

A.10 Survey Results: Indices and Mechanisms 26

A.11 Effect of Busing on Zip-Code Income 27

A.12 Compliance: NORC Survey 28
A.13 Compliance: Yearbook-to-Yearbook Matching 29
A.14 Compliance: Yearbook-to-Commencement-List Matching 30

List of Figures

A.1 Residential Segregation in Jefferson County, KY 31
A.2 Racial Integration by Year and Type of School 32
A.3 Randomization Inference Results: No Party Affiliation 33
A.4 Survey Materials 34

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 details the years of yearbook records and commencement records that we collected by school.

Appendix Table A.2 details the construction of our main working sample from the data we collected.

Appendix Table A.3 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.4 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.5 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.6** 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.7 repeats the exercise in Appendix Table A.6 with turnout as the outcome. Again, the results are qualitatively similar to those reported in the main text.

Appendix Tables A.8 and A.9 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.10 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.11 displays estimates for the effect of busing assignment on zip-code-level average incomes for white males. The results confirm the findings of Tuttle (2019) in our linked yearbook-L2 sample. The table shows that white males assigned busing live in similar-income areas today as their counterparts not assigned busing.

Appendix Table A.12 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.4). 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.13 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.14 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 present 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 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: Jefferson County Busing Survey

C.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.4). 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.4.

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 responsdents, 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%).

C.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.

C.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 D: 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.

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

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

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.*

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 $\bar{Y}_G(z,r) = E_{i \in G}\left[Y_i(z,r)\right]$, 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.

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

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,

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

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:

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

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

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, 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

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

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: Commencement and Yearbook Records

School Name	Commencement Records	Yearbook Records
Not Exempt from Alphabet Plan:		
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
Exempt from Alphabet Plan:		
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.2: Sample Construction

Sample Description	Sample Size
Beginning sample:	
Full Student-Year Sample	177,032
Sample Restrictions:	
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.3: 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***	–2.67**	–1.20
	(0.74)	(1.01)	(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***	–1.82*	–0.84
	(0.69)	(1.02)	(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 equ. (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.4: 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***	4.16***	2.84**	3.24*
	(1.14)	(1.48)	(1.23)	(1.64)
Assigned Busing, 2 Years	2.81	1.78	3.95*	2.55
	(1.95)	(2.68)	(2.18)	(2.65)
p-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*	–3.19*	–2.14	–2.21
	(1.55)	(1.67)	(1.55)	(1.85)
Assigned Busing, 2 Years	–2.67	–1.20	–3.67*	–2.63
	(2.05)	(2.11)	(1.88)	(2.62)
p-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	–0.97	–0.70	–1.03
	(1.60)	(1.51)	(1.28)	(1.29)
Assigned Busing, 2 Years	–0.15	–0.57	–0.28	0.07
	(1.21)	(1.63)	(1.19)	(2.06)
p-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. 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: Effect of Busing on Party Registration, Inclusion of Various Fixed Effects

	(1)	(2)	(3)	(4)
		Registered Democrat		
Assigned Busing	3.05**	3.53***	3.28***	3.64***
	(1.15)	(1.14)	(1.18)	(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	–2.77**	–2.65**	–2.76**
	(1.18)	(1.24)	(1.23)	(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	–0.76	–0.63	–0.88
	(0.95)	(1.07)	(1.09)	(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.6: Effect of Busing on Party Affiliation, Robustness to D-i-D Esimator

	(1)	(2)	(3)
	Registered Democrat	Registered Republican	Independent
	Callaway-Sant'anna		
Assigned Busing *(Cohort Average)*	2.79*	–2.25**	–0.54
	(1.48)	(0.99)	(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***	–3.62***	1.05
	(0.85)	(1.11)	(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.7: 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 *(Cohort Average)*	1.44	1.04	1.83
	(1.08)	(1.12)	(1.13)
Mean of Dep. Var.	70.52	74.69	66.34
Observations	18,541	18,541	18,541
	Stacked Difference-in-Differences		
Assigned Busing	0.30	0.94	–0.34
	(1.04)	(0.91)	(1.23)
Mean of Dep. Var.	70.52	74.69	66.34
R-squared	0.014	0.010	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.8: Survey Results: Index Components and Other Outcomes

Outcome	Coef.	SE	p-value	q-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 Brough 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. Conservative Racial Attitudes 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 Not 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 Column 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. Panels D–G are shown on the next page.

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

Outcome	Coef.	SE	p-value	q-value	Mean	SD
					(continued from previous page)	
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 Attitudes 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 Column 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. Panel E presents questions from the trust in government module. Panel F shows two sub-indices on racial attitudes, while Panel G presents answers to a real-stakes question.

Appendix Table A.10: Survey Results: Indices and Mechanisms

Outcome	Coef.	SE	p-value	q-value	Mean	SD
Panel A. Summary Indices						
Progressive Policy Index	0.102	0.063	0.117	0.173	0.017	0.400
Conservative Racial Attitudes 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						
Friends:						
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
School Environment:						
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
Teachers:						
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 Column 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.11: Effect of Busing on Zip-Code Income

	Total Income Per Return		Total Income Per Person	
	(1)	(2)	(3)	(4)
Assigned Busing	–1,555	–1,621	–932	–1,001
	(1,467)	(1,693)	(865)	(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
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 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. Columns (1) and (3) include cohort and alphabet-group fixed effects, while columns (2) and (4) 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.12: Compliance: NORC Survey

	(1)	(2)	(3)	(4)
	Self-Reported Attendance			
	City School	Private School	Other Public School	Only County Schools
Assigned Busing	30.00***	3.83**	4.97*	–39.24***
	(5.53)	(1.81)	(2.51)	(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.13: 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***	45.80***	–59.76***	0.05	90.02***	–88.49***	0.55
	(2.27)	(1.81)	(1.60)	(0.50)	(1.52)	(1.62)	(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.

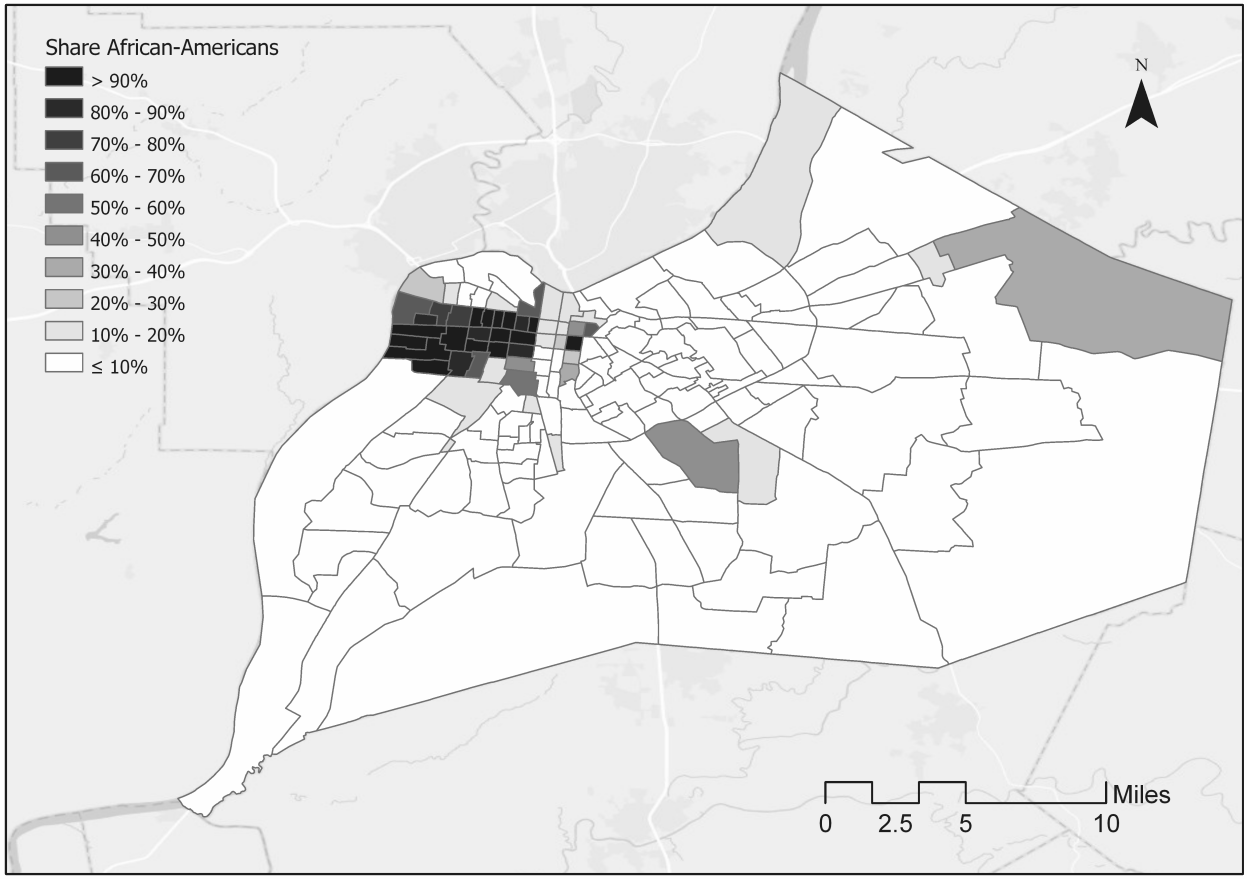
Appendix Table A.14: Compliance: Yearbook-to-Commencement-List Matching

	(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***	24.31***	–39.69***	6.96**	53.46***	–70.87***	16.77***
	(1.62)	(3.77)	(2.46)	(2.55)	(7.29)	(2.01)	(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.

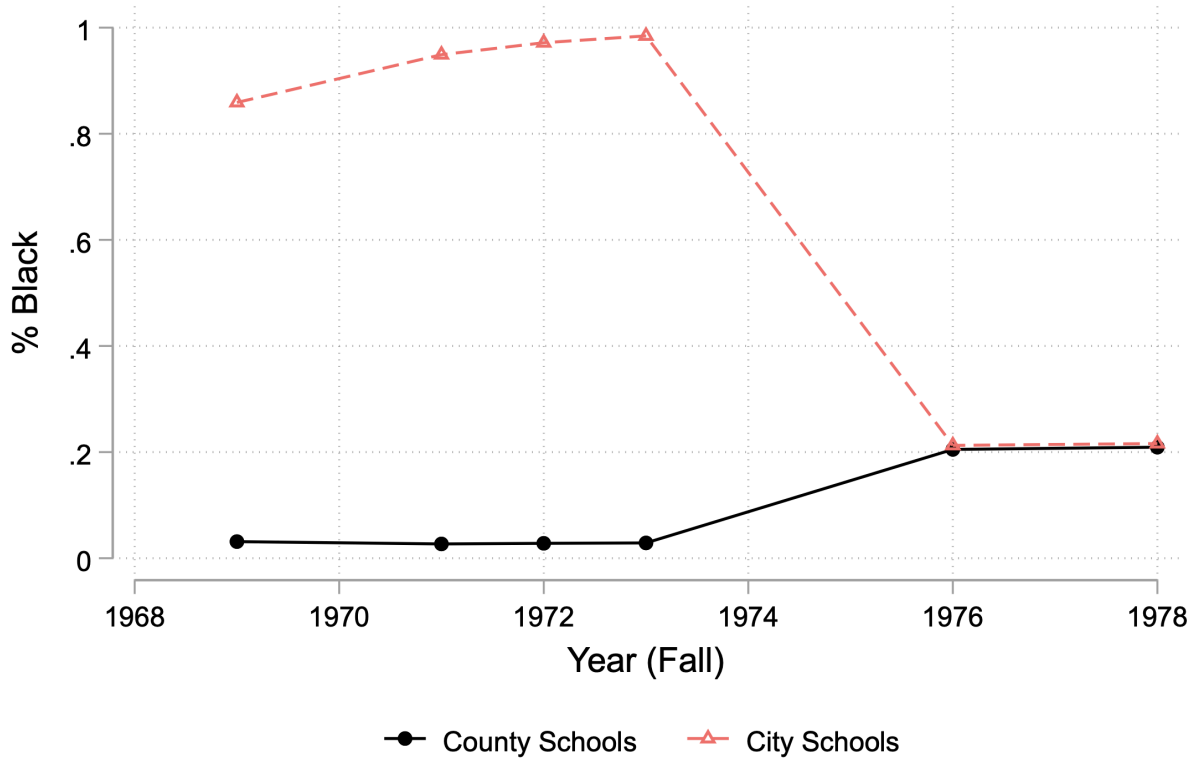
Appendix Figures

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



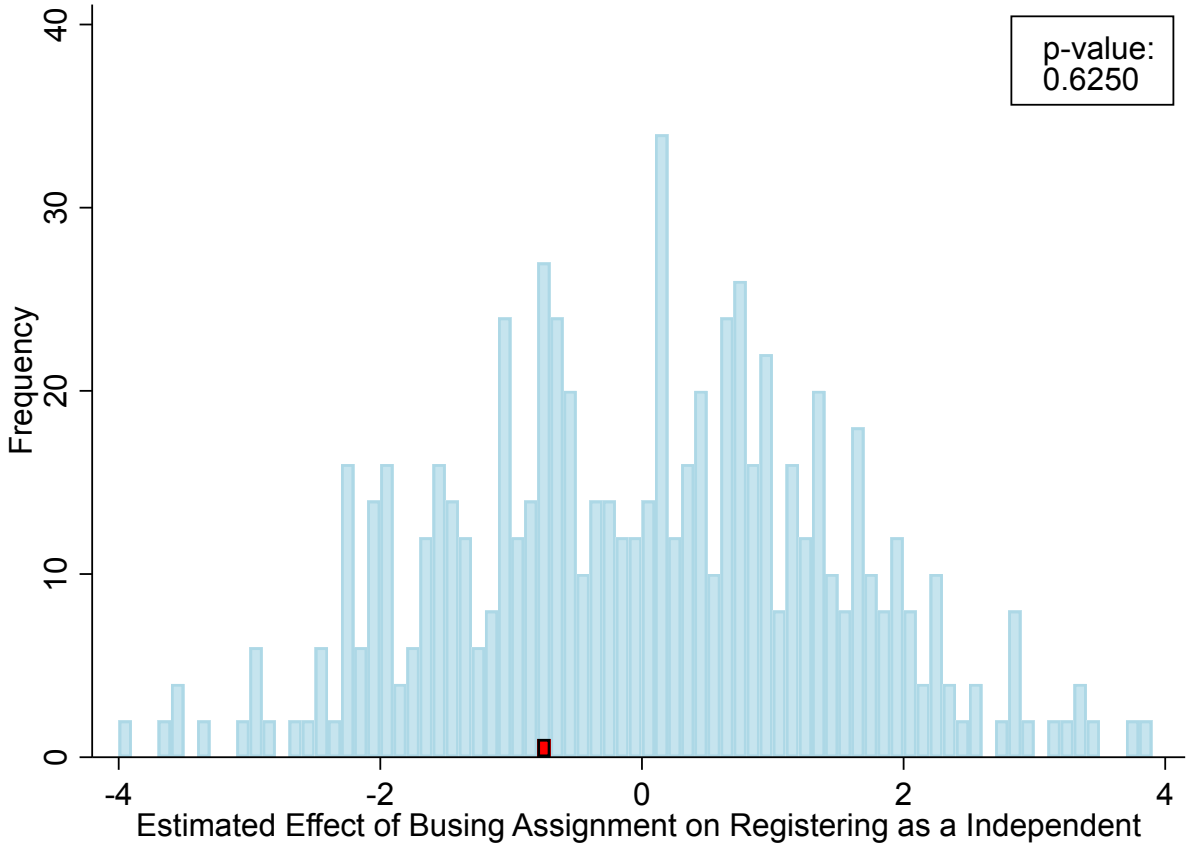
Notes: Figure shows the share of African-Americans as of the 1970 Decennial Census for each census tract in Jefferson County, KY

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



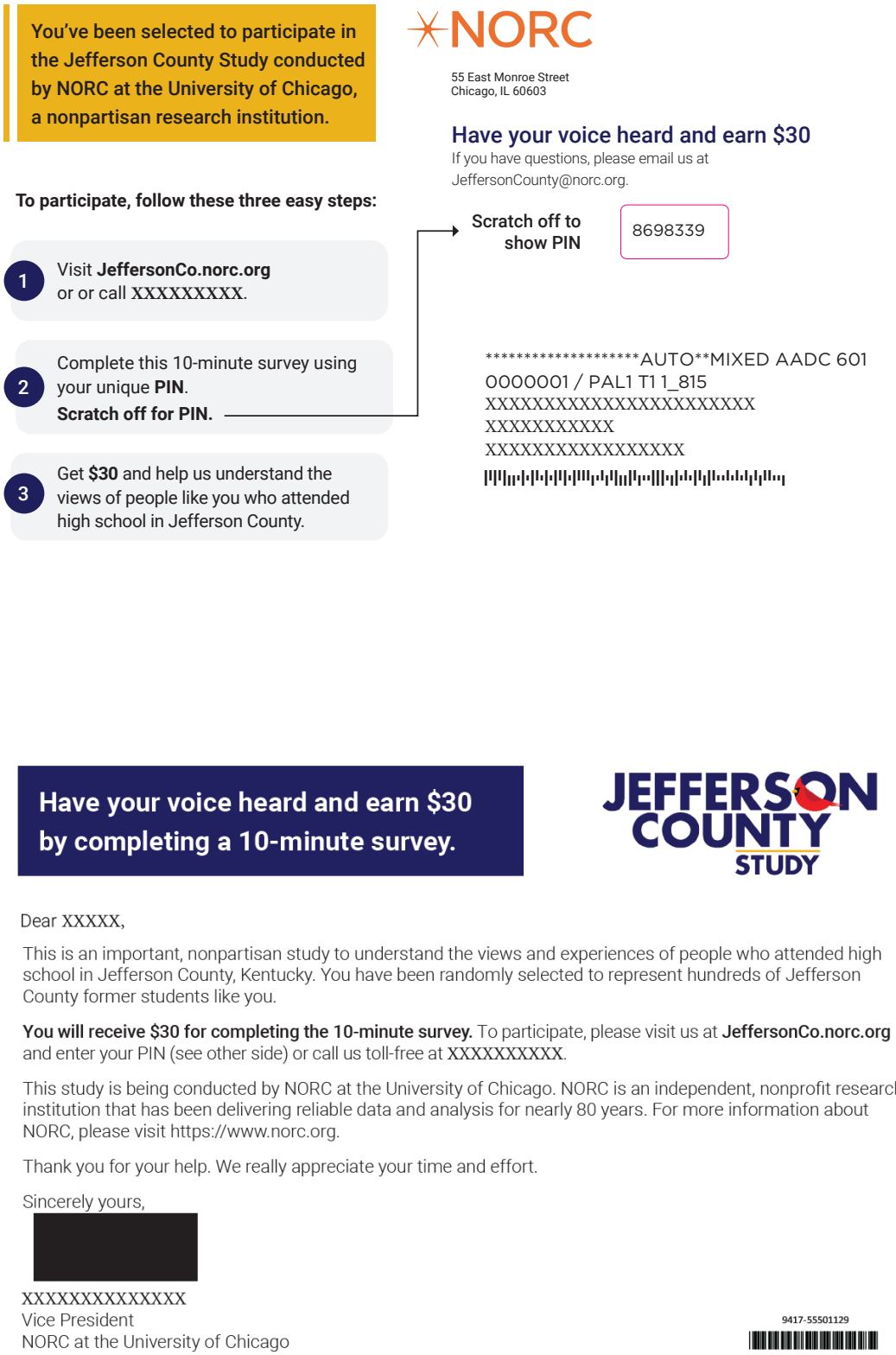
Notes: Figure shows the average share of African Americans among students in JCPS high schools, separately for "city" and "county" schools. Prior to desegregation, city schools were part of the Louisville Independent School District, whereas county schools belonged to the Jefferson County school system.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

Appendix Figure A.3: 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.4: Survey Materials



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.

NORC at the University of Chicago

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 [X] to indicate your answer.
- If you want to change your answer, darken the box [X] 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

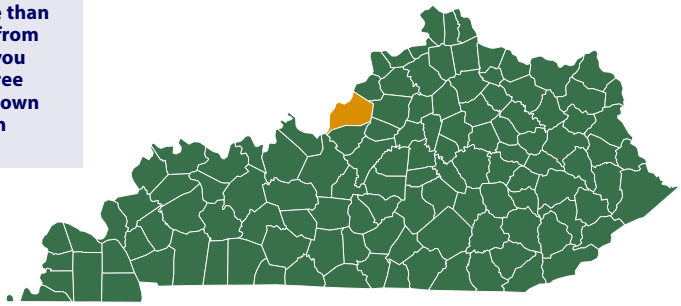


21. Do you agree, disagree, or neither agree nor disagree with each of the following statements?

	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.	☐	☐	☐	☐	☐

30. Thinking back to your high-school days, which experiences have had the largest impact on who you are today?

31. Do you consider yourself a Democrat, a Republican, an Independent or none of these?

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

31a. Do you consider yourself a strong or not so strong Democrat?

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

31b. Do you consider yourself a strong or not so strong Republican?

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

31i. Do you lean more toward the Democrats or the Republicans?

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

32. Different people hold different political views. Here is a scale on which these views are arranged from very liberal to very conservative. Where would you place yourself on this scale, or haven't you thought much about this?

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

Just a few questions about you.

33. What is your gender?

- ☐ Male
- ☐ Female
- ☐ Other

34. In what year were you born?

☐☐☐☐ Year

35. What is your marital status?

- ☐ Married
- ☐ Widowed
- ☐ Divorced or separated
- ☐ Never married

36. What is your race or ethnic background?

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

37. What is the highest level of education that you obtained?

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

38. Thank you very much for answering our questions. Did you, at any point, feel that this survey was biased?

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

Please turn to the back cover.



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