Busing's Political Passengers: The Enduring Effects of School Desegregation on Partisanship, Ideology, and Attitudes*

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Abstract

In 1974, a federal court ordered that public schools in Jefferson County, KY be desegregated. In order to achieve racial integration, 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 busing on political participation and preferences. Focusing on white males, we do not detect any effect on voter registration and turnout. We do, however, find that busing significantly increases support for the Democratic party more than forty years later. Consistent with the idea that attending a formerly black, inner-city school causes a permanent change in the ideological outlook of whites, we also find that bused individuals are less likely to believe in a "just world," i.e., that success is earned rather than attributable to luck. At the same time, busing appears to have had no lasting effect on racial attitudes.

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1. Introduction

In 1954, the U.S. Supreme Court ruled that racial segregation of children in public schools is 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 previously all-white schools and, less frequently, white students to predominantly black schools.

In this paper, we study the long-run impact of busing on political participation and preferences. 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 many prominent Democrats supported school desegregation, the controversy surrounding busing might have led to political backlash and realignment. That is, some white voters might adopt more conservative stances in response to the perceived federal overreach.[1] On the other hand, Allport's (1954) contact hypothesis suggests that, under appropriate conditions, greater exposure to African Americans may foster understanding and sensitize white students to civil rights issues, potentially making them more rather than less sympathetic to left-leaning political views.

We estimate the long-run impact of busing on political participation and preferences by drawing on a unique natural experiment.[2] In December of 1974, the Sixth Circuit Court of Appeals ordered that the Louisville and Jefferson County public school systems be merged and desegregated. At the time, the Louisville school district served roughly as many black as white children. Yet, about 80% of white students attended schools that were at least 90% white, and 76% of black students were enrolled in schools that were at least 90% black. By contrast, with a black enrollment level of about 4%, the surrounding Jefferson County public school system was nearly all-white (Sedler 2007). In order to desegregate schools, the newly

[1] To illustrate how controversial busing was, in his autobiography then-Senator Joseph Biden described it as "a liberal trainwreck" (Biden, 2007). Echoing similar sentiments, at a campaign event in 1984, President Reagan referred to busing "a social experiment that nobody wants" (https://www.reaganlibrary.gov/archives/speech/remarks-reagan-bush-rally-charlotte-north-carolina).

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

merged district implemented a busing schedule based on which black students were to attend a better-resourced formerly white school, whereas white students would be sent to formerly black schools.

By design, the district's busing plan approximately equalized the share of black students across schools. As a consequence, all white Louisville-area public school students experienced significant exposure to African Americans. 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, however, experienced the unique social dynamics of attending an inner-city high school.

Important for our purposes, 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 only if they belonged to alphabet group "A, B, F, Q" but not if their last names placed them in one of five other groups. 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 isolate the effect of being bused to an inner-city school, we implement a difference-in-differences design. This design compares differences across students who were and were not assigned to be bused after schools racially integrated with differences across individuals who belong to the same alphabet groups but were entirely unaffected by the court-ordered busing plan because they had graduated just a few years earlier.

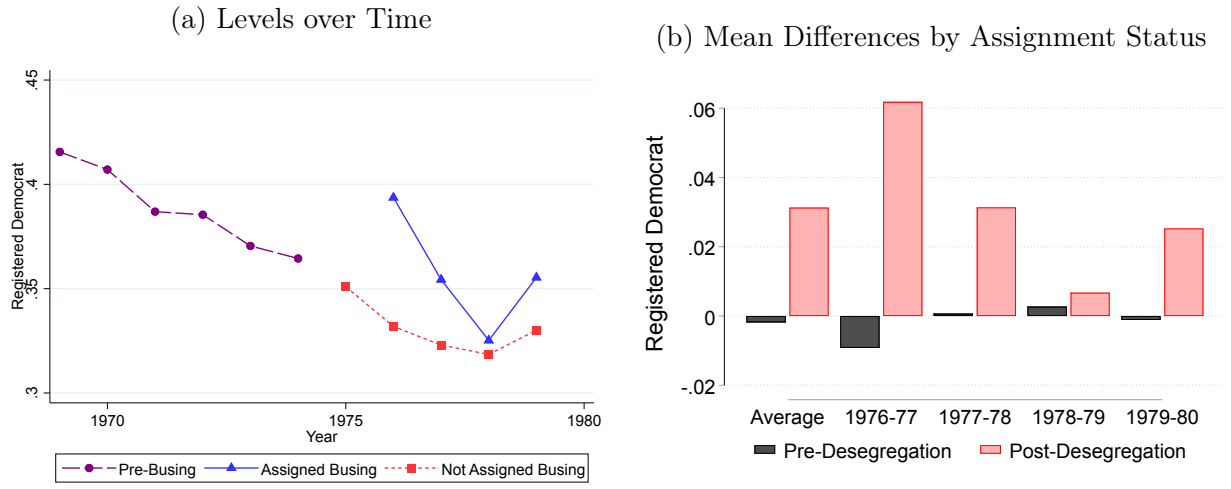
Our analysis relies on digitized high school yearbooks from Louisville-area public schools, which we link to present-day administrative voter registration and turnout records. Focusing on cohorts graduating between 1970 and 1980, and relying on yearbooks published prior to the enactment of the district's desegregation plan, we identify individuals who were and were not subject to busing. For reasons of statistical power, we restrict attention to about 32,000 white males, of whom we can match about 84% (54%) to at least (exactly) one voter registration record.[3]

Our results imply that busing has no long-term impact on registering to vote, and, conditional on being registered, no effect on political participation, i.e., turnout. At the same time, we find that having been bused to a formerly black, inner-city school significantly increases the likelihood of registering as a Democrat more than forty years later.

Figure 1 illustrates this result using raw data. The panel on the left shows the share

[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 4).

Figure 1: Democratic Party Registration, by Cohort and Busing Assignment



Notes:.

of registered Democrats for each cohort in our sample. For cohorts graduating after the desegregation of schools, the figure distinguishes between students who were and were not assigned to be bused. Consistent with white Southerners' exodus from the Democratic Party during this period (Kuziemko and Washington, 2018), we observe a strong secular downward trend in Democratic registration prior to the enactment of busing. After 1975, this trend continues without a noticeable break among students who were not assigned to be bused. Among students who were assigned to be bused, however, school desegregation coincides with a large upward jump in Democratic Party registration.

The right panel of Figure 1 compares the share of registered Democrats across alphabet groups that were and were not assigned to be bused. Light bars correspond the *observed* differences in registration rates among individuals in the respective cohorts. To assess whether these differences might be due to pre-existing heterogeneity across alphabet groups, dark bars show *placebo* differences from applying the busing schedule for a given post-desegregation cohort to students who graduated prior to the enactment of busing. Reassuringly, all placebo estimates are close to and statistically indistinguishable from zero, suggesting that pre-existing differences across alphabet groups are not an important concern.

In the second part of our analysis, we draw on an original survey of more than six hundred former Jefferson County Public School students who graduated from high school in the mid to late 1970s. This survey allows us to examine the effect of having been bused on outcomes that we cannot observe in our administrative data, including racial attitudes, social views, trust in government institutions, and support for redistribution. We also study proxies for possible mechanisms through which busing might have impacted partisanship decades later. 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.

Although our survey results confirm that bused individuals are today more supportive of the Democratic Party and its candidates, we do not find that they become universally more left-leaning. For instance, we do not find meaningful treatment effects on support for progressive abortion and climate policies. Nor do we estimate large effects on support for various forms of redistribution. Perhaps most surprisingly, we detect virtually no effect of having been bused on racial attitudes.

We do, however, find that having been bused increases support for unions, and it makes individuals less likely to believe in a "just world," i.e., that success is earned rather than attributable to luck.[4] We also find that bused individuals are more likely to recall poverty at school and to have made friends who were poor. Broadly summarizing, our survey results are consistent with exposure to poverty shifting students' views about inequality and fairness. These views may influence partisan loyalties but need not be strong enough to measurably affect support for specific progressive policies.

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, with white students who were assigned to be bused being especially likely to leave Jefferson County public schools. In our survey, we find that most of those who left the public school system attended a Catholic high school instead, with a smaller number of students moving to public schools in other counties. in light of this phenomenon, our estimates should be interpreted as averages of the treatment effect for compliers (i.e., students who ended up attending a formerly black, inner-city high school) and non-compliers (i.e., those who instead enrolled in a private school or moved to another school district). If attending a different predominantly white high school does not have a larger impact on students' views and preferences than attending a formerly black one, then our estimates constitute a lower bound on the effect of having been bused. That is, the true average treatment effect is likely larger than implied by the results below.

2. Related Literature

Our work directly contributes to a small, recent literature on the socio-political consequences of school desegregation. Gordon and Reber (2018), for instance, study the impact of school desegregation on mixed-race births. Consistent with our null finding on racial attitudes, their causal research design implies only small to moderate effects of school desegregation, which

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

are for the most part statistically indistinguishable from zero.

The two closest papers to ours are Bergman (2018) and Billings et al. (2020). The former uses data from a randomized, voluntary desegregation program in California to estimate the long-run impact of busing on a number of detailed educational outcomes as well as voter registration and turnout. The latter study the cessation of race-based busing in Charlotte-Mecklenburg schools. Comparing students who had lived on opposite sides of newly drawn school boundaries, Billings et al. find that school resegregation increases Republican voter registration in early adulthood.

While our results with respect to turnout and partisanship mirror those of Bergman (2018) and Billings et al. (2020), there are a number of important differences. First, Bergman focuses on minority students and measures outcomes 8–18 years after desegregation. Billings et al. share our focus on white students but measure partisanship 17 years after treatment. We, by contrast, examine effects more than 40 years later. We also note that Billings et al. study the *re*segregation of schools, which, at the time, was not nearly as controversial as the introduction of mandatory busing programs in the aftermath of the Civil Rights Act. The natural experiment in this paper comes, therefore, with an a priori significant chance of political backlash. Moreover, there are important differences in estimands. Our difference-in-differences design isolates the causal effect of being bused to an inner-city high school, net of changes in cross-racial exposure. Billings et al. however, estimate the effect of changes in schools' racial composition. Both estimands thus complement each other. Perhaps the most important difference between our work and that Bergman (2018) and Billings et al. (2020) is that we study a greater range of socio-political outcomes, including racial preferences, views on inequality and the role of luck, and attitudes toward redistribution. In fact, Billings et al. explicitly suggest looking at racial attitudes and preferences for redistribution to make headway in separating the potential causal channels behind their findings.[5]

More broadly, our findings contribute to a burgeoning literature on the medium- to long-run consequences of different individual experiences, particularly during formative years (see, e.g., Alesina and Fuchs-Schündeln 2007; Chyn and Haggag 2020; Clingingsmith et al. 2009; Gerber et al. 2010; Kaplan and Mukand 2018; Kaplan et al. 2019; Mullainathan and Washington 2009). Our work is also related to numerous studies examining Allport's (1954) contact hypothesis, according to which personal interactions can, under appropriate conditions, reduce prejudice between majority and minority group members (see Paluck et al. 2019 for a recent review). Boisjoly et al. (2006), for instance rely on the random assignment

[5]In unpublished, recent work Mattia (2021) uses court-ordered dismissals from desegregation orders to estimate a positive effect of school resegregation on racial attitudes and support for the Democratic party. Taken at face value, this finding runs counter to both our results and those of Billings et al. (2020).

of roommates at a public university to provide evidence that cross-race interactions increase whites' support for affirmative action. Carrell et al. (2019) find that white U.S. Air Force students who are randomly assigned to squadrons with more black peers are more likely to choose to live with a black roommate in their second year, 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. 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. Summarizing a vast number of findings, Paluck et al. (2019) note that the impact of intergroup contact tends to vary, "with interventions directed at ethnic or racial prejudice generating substantially weaker effects."

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 racial diversity in an inner-city environment rather than a previously all-white suburban high school. Our findings thus inform the debate around Allport's (1954) scope conditions. Third, we present long-run estimates—more than four decades after the intervention. Our work, therefore, also speaks to the persistence of effects.

3. 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. Thus, 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 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 court-mandated desegregation.[6]

Like many other metropolitan areas, in the early 1970s, Jefferson County, KY experienced high levels of residential segregation. As can be seen in Appendix Figure A.1, most of the

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

area's black residents were concentrated in just a few neighborhoods in the city of Louisville. Even though Jefferson County encompasses Louisville and despite the fact that counties are the basic administrative unit of education provision in Kentucky, the city and county operated separate and highly unequal school districts. Louisville public schools were not only poorer than their counterparts in the remainder of the 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. By contrast, the makeup of schools in 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.[7] 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. The judge's reasoning seemingly conformed to the Supreme Court's eventual verdict in *Miliken v. Bradley* which allowed school segregation due to sorting but disallowed intentional school segregation. Despite the fact that the Supreme Court remanded the Louisville case for further consideration in light of the newly established *Miliken v. Bradley* precedent, the 6^{th} Circuit Court of Appeals decided that school-district lines between Louisville and Jefferson County *could* be crossed to achieve desegregation.[8] As a result of this surprising ruling in December of 1974, the courts forced a merger of the two school districts within Jefferson County, Kentucky and furthermore ordered that a desegregation plan be adopted.

According to the retrospective of Sedler (2007), spring and early summer of 1975 were spent litigating what kind of desegregation plan Judge Gordon should impose and when it would go into effect. The judge's 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

[7]The initial Jefferson County suit was filed as *Newburg Area Council, Inc. v. Jefferson Board of Education*, and the lawsuit against the Louisville school district was brought under *Haycraft v. Board of Education of Jefferson County, Kentucky*. The former case also involved the small, rural Anchorage Independent School District, which, at the time, had only one all-white elementary school.

[8]The Circuit Court noted that "school district lines in Kentucky have been ignored in the past," and there was no justification for allowing the two school districts to remain separate if the consequence was that the areas' public schools as a whole would remain segregated.

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, each school in the newly merged district would have a black enrollment share between 12% and 40%. Schools that already met the target range were exempt from busing.[9] In a few other cases, Louisville city schools near White areas were redistricted in order to achieve racial diversity in lieu of busing. These schools were also exempt from busing. 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, a little less than half were black (New York Times 1975).

Table 1 depicts the busing schedule at the heart of Judge Gordon's desegregation plan. Important for our purposes, if and when a particular student would be bused was determined by race, grade level, and the first letter of her 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 rising seniors, kindergartners, and first graders were exempt.

Table 1: Bused by Alphabet Group, By Cohort

	Graduating Cohort					
Alphabet Group	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						

Note: Table 1 shows whether or not white children in a particular cohort and alphabet group were assigned to be bused to a formerly black school for at least one year. Black fields indicate assignment to busing, grey fields imply that the respective set of students was not bused.

As in many other American cities, the Louisville desegregation plan was extremely controversial among whites. Surveys conducted at the time reportedly showed that 98% of

[9]This was the case for 16 elementary schools and 12 secondary schools, all of which were formerly part of the Louisville district.

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, 10,000 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).[10]

4. Data and Empirical Approach

4.1. *Data and Descriptive Statistics*

Our study relies on three main sources of data: (1.) the universe of voter registration data for the United States as of summer, 2020 purchased from the data vendor L2, (2.) yearbook data (1970-1976) and graduation list data (1976-1980) from public schools in Jefferson county, Kentucky over the 1970-1976 time period, and (3.) a summer, 2022 survey of JCPS students designed designed by us and administered by NORC (National Opinion Research Center at the University of Chicago)[11].

Voter Registration Data Our voter registration data come from L2 Inc., a data vendor that purchases and cleans administrative and commercial databases on registered voters and political donors. L2 sources its data from state-level Secretaries of State as well as local election boards, from the Federal Election Commission (FEC), from local real estate records, and from a number of for-profit companies such as Experian. Crucial for our purposes, L2 contains individual data containing exact name, gender, date of birth, state of registration, party registration and turnout history.

In all but fifteen states, the partisanship of individuals in the L2 data coincides with the party affiliation in the respective states' maintained voter registration lists. The remaining fifteen states do not collect information on voters' partisan leanings. For voters in these states, L2 uses predictive modeling to impute a "likely" party affiliation.[12] Per the company,

[10]The original busing plan remained in effect with only minor modifications until 1985, when the 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 cannot use race as the sole factor in assigning students to schools.

[11]The survey was funded by the National Science Foundation, the Russell Sage Foundation, and the Ford Motor Center for Global Leadership at Northwestern University

[12]Specifically, L2 models party affiliation in the following states: HI, IL, WA, MT, ND, MN, WI, MI, VT, SC, MO, AL, TX, VA, and GA.

their proprietary machine-learning algorithms use an array of public and private data sources, including participation in 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. Our paper estimates effects of busing on partisanship on all states but also shows effects restricted to states which maintain partisan registration.

Yearbook and Commencement Data The yearbook data and the graduation lists data were collected by undergraduates and the authors from classmates.com. In some cases, yearbook data was not available online. For most of these school-years, we were able to find the yearbooks in hard copy in the JCPS Archives and Record Center. We use yearbook data from Fall, 1969 through Fall, 1974. Undergraduate research assistants recorded exact name of individual, school name, sex, race, grade level, and year. Race and sex were based upon student judgment. However, we assigned 5,901 individuals to more than one student and 92% of the time, they agreed on coding of race, and 100% of the time they agreed on gender. If we misclassify a White student, since we exclude Blacks from the sample, that will come at the cost of power. If we misclassify a Black student, given that the overwhelming majority of Blacks vote Democrat, then we risk adding a false positive if they are bused and a false negative if they are not. Since, as shown in Table 3 only 13% of our sample is bused, this should on net attenuate our estimates.

For most schools, we have yearbook data going back to seventh grade. This gives us information on individuals in graduating classes from the 1969-1970 school year through until the 1979-1980 school year. We restrict our yearbook data to those in seventh grade no later than Fall, 1974 because in the summer of 1975, the busing plan was announced and White flight ensued, compromising the exogeneity of the treatment group. We list all of the school-years which we collected in Appendix Table A.1. Our sample consists of 19 treated schools and 10 control schools. We also collected graduation lists for select school-years from the JCPS archives. We also list these in Appendix Table A.1.

Since the white students in our yearbook data greatly outnumber blacks, 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 men graduating high school from public schools in Jefferson county, Kentucky in the 1970s. Appendix Table A.6 shows how we take our initial sample of 177,032 student-year records from Jefferson County public school yearbooks in the 1970s to our base sample of 18,528 White, male attendees who we match with voter registration data.

Our baseline dataset contains 32,555 individuals: 28,344 of whom were not bused and 4,211

of whom were. In Table 2, we show our sample sizes by year and treatment status. Table 2 also displays the fraction of individuals in the yearbook data who we were able to match to L2 voter registration data. In approximately 85% of cases, we were able to match to at least one individual. The fact that about 15% of students remain unmatched could either be due to nontrivial transcription errors in the process of our data collection efforts or to the fact that a significant fraction of Americans is not registered to vote. The U.S. Census Bureau, for instance, estimates that, in 2016, only about 70.3% of voting-aged citizens were registered (File 2018) though voter registration rates are substantially higher for older Americans who constitute our sample.

In the presence of multiple matches between L2 and the voter registration data, we attempt to determine the most likely one by utilizing ancillary information from Kentucky birth certificates. Specifically, by matching the students to birth certificates, we can recover their exact dates of birth. If multiple individuals were born in Kentucky with birth dates matching their grade in the yearbook data, then we throw them out of the baseline sample unless one and only one of them was born in Jefferson County, Kentucky. We say that an individual in our yearbook data can be uniquely matched to a voter registration record if L2 locates exactly one record with a similar-enough name and year of birth or if the refinement procedure above allows us to narrow down the set of potential matches to one. We also drop attendees of 10 city schools which were exempt from the busing plan due to a sufficiently high level of pre-existing racial integration. Whereas we use our baseline sample to estimate impacts of busing upon registration, our main estimation sample for partisanship and voter turnout outcomes in the end consist of 18,528 White male graduates of Jefferson County High Schools in the 1970s whose schools were subject to mandatory busing and who we uniquely and successfully matched to L2-provided voter registration data.

Overall, we match approximately 85% of our baseline sample. We do match those who were bused a a 1.5 percentage point higher rate and the difference is significant at the 0.2% level. Of course, this could be due to differences in ability to match across the groups, a difference in the average propensity to register, or a causal effect of busing on registration. Once we restrict to uniquely matched individuals, the overall match rate falls to approximately 57% and the difference between those who were bused and not falls to 1.0%. The p-value on the difference between the bused and the non-bused groups also falls to 1.0%.

Table 3 presents descriptive statistics for the set of uniquely matched students, on which we base our subsequent analysis. Among these, nearly 53% still live in Kentucky. Drawing on data from the American Community Survey, we calculate that the same number among all white Kentucky-born males in the same age group is about 65%. Moreover, further inspection of our data reveals that the share of individuals who still live in Kentucky is almost ten per-

Table 2: Match with Voter Registration Data

	Number of Students		Any Match			Unique Match		
	Not Bused	Assigned Busing	Not Bused	Assigned Busing	p-value	Not Bused	Assigned Busing	p-value
Cohort	(1)	(2)	(3)	(4)	(3) = (4)	(5)	(6)	(5) = (6)
1969	1,553	0	0.900	.	.	0.728	.	.
1970	2,330	0	0.869	.	.	0.631	.	.
1971	2,921	0	0.857	.	.	0.628	.	.
1972	3,026	0	0.861	.	.	0.586	.	.
1973	3,242	0	0.828	.	.	0.545	.	.
1974	4,173	0	0.826	.	.	0.549	.	.
1975	4,033	0	0.820	.	.	0.515	.	.
1976	2,992	635	0.810	0.827	0.339	0.535	0.548	0.553
1977	2,255	1,082	0.808	0.837	0.043	0.528	0.543	0.423
1978	1,264	1,256	0.827	0.831	0.766	0.569	0.556	0.508
1979	555	1,238	0.840	0.852	0.494	0.553	0.586	0.188
Total	28,344	4,211	0.837	0.852	0.002	0.570	0.560	0.039

Note: Table 2 displays the match rate statistics between yearbook data and voter registration data, by busing assignment. Entries are raw counts and means for the match between our yearbook and voter registration data, by graduating cohort. Columns 3 and 4 record whether the vendor database contains at least one registered voter with an approximate match on first name, last name, and year-of-birth with a particular individual in the yearbook data. Columns 5 and 6 indicate whether we can match a particular individual to exactly one registered voter.

centage 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. In Section 6, we return to white flight as a potential mechanism behind our main results.

Table 3 also shows that about one-third of the individuals in our data are currently registered as Republicans, while approximately 30% are registered Democrats. Relative to a nationally representative sample, Republicans are, therefore, somewhat over-represented relative to Democrats. Even in Kentucky, a Republican state, as of summer 2020, when we extracted the voter registration data, the two party registration for Republicans was 47%[13]. Important for our purposes, 75% of our sample is from a closed primary state which maintains administrative data on partisan registration.

We further see the familiar pattern of lower turnout in midterm than in presidential elections, with overall participation rates about 10 percentage points higher national averages. The turnout rates are relatively high because our sample is constituted of older adults who have higher turnout rates. The turnout rates in our sample roughly correspond to national

[13]Aggregate voter registration data is available on the Kentucky State Board of Elections website: https://elect.ky.gov/Resources/Pages/Registration-Statistics.aspx

Table 3: Descriptive Statistics

Variable	N	Mean	SD	Min	Median	Max
Treatment						
Bused	18,528	0.127	0.333	0	0	1
State of Residence						
Closed Primary State	18,528	0.753	0.431	0	1	1
Kentucky	18,528	0.529	0.499	0	1	1
Party Registration						
Democrat	18,528	0.365	0.481	0	0	1
Republican	18,528	0.502	0.500	0	1	1
Independent	18,528	0.133	0.340	0	0	1
Political Participation						
Voted in 2008	18,528	0.717	0.451	0	1	1
Voted in 2010	18,528	0.619	0.486	0	1	1
Voted in 2012	18,528	0.734	0.442	0	1	1
Voted in 2014	18,528	0.630	0.483	0	1	1
Voted in 2016	18,528	0.790	0.407	0	1	1
Voted in 2018	18,528	0.741	0.438	0	1	1
Survey Results						
Belief in a Just World	533	0.017	0.976	-3	-0	3
Friends from Poor Families	545	0.035	0.991	-2	0	2
Registered Democrat	555	0.377	0.485	0	0	1
Registered Republican	555	0.508	0.500	0	1	1

Note: Table 3 shows summary statistics for all variables used in the subsequent analysis. The sample is limited to individuals not in exempt schools who could be uniquely matched to a voter registration record.

turnout rates conditional upon age.

Survey Data

In the summer of 2022, with money from the National Science Foundation, the Russell Sage Foundation, and the Ford Center for Global Citizenship at Northwestern University, we surveyed 1,200 bused individuals from our matched voter registration lists and 1,200 more who were not bused. We contracted with National Opinion Research Center (NORC) at the University of Chicago. We initially sent letter with a $5 pre-incentive and then followed up with a $30 post-incentive upon completion. Each person was contacted via mail, phone and email depending upon what information was in the voter registration files. People were contacted up to five times in total. We ended up with a sample of 629 individuals: a response rate slightly lower than 50%. We broke up our questions into sections and constructed equal-weighted indices by section. The sections included questions on: (1.) Trust in government, (2.) Political Participation, Partisanship and Ideology, (3.) Fairness, Inequality and Redistribution, (4.) Racial Attitudes, (5.) a Real Stakes question which allowed participants to make a donation to Black Lives Matter or the National Police Foundation or to refrain from making a donation, (6.) the High School Experience, (7.) Political Information and (8.) Demographic Information. We also asked an open response question: "Thinking back to your high-school days, which experiences have had the largest impact on who you are today?". Finally, we asked a question about whether respondents thought the survey had a political bias.

Out of the 629 individuals who were sampled, only 555 (88%) were actual attendees of Jefferson County Public Schools. So, our final sample is 555 individuals. Out of the 629 individuals, sampled, 185 (30%) thought that the survey was politically biased; however, of these 93 said that the survey had a liberal bias and 94 said that the survey had a conservative bias. As a result, we are not overall concerned about the impact of perceptions of bias and how that impacts responses.

4.2. *Empirical Strategy*

Our identification strategy follows a standard difference-in-differences design. Intuitively, we compare average political participation and preferences across alphabet groups within cohorts, before and after the enactment of the court-ordered desegregation plan. For example, as shown in Table 1, among students in the 1976 graduating cohort, only children in alphabet group “A, B, F, Q” were bused to formerly black schools. Do the political preferences, more than forty years later, of the cohorts of these individuals who were bused differ from those of those who came earlier? And is any difference across cohorts in political preferences substantially larger for the treated "A, B, F, Q" alphabet group?

We answer this by running a difference-in-differences regression controlling for cohort and alphabet group fixed effects. We thus estimate the following equation:

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

By $Y_{i,a,c}$, we denote the outcome of interest for individual i, who is part of graduating cohort c and whose last name puts her 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 ever bused, while μ_a and δ_c stand for alphabet-group and cohort fixed effects, respectively. By including these, the specification in Equation (1) controls for any systematic difference that may exist between cohorts and across alphabet groups. As in a standard difference-in-differences design, the key identifying assumption is that mean differences across alphabet groups would have remained constant across consecutive cohorts had it not been for busing.

A recent econometric literature has shown that difference-in-difference models do not properly aggregate heterogeneous treatment effects across cohorts. In particular, differences-in-differences effectively uses early adopting cohorts as controls for later adopting cohorts, negatively weighting early adopting cohort treatment effects. We use precisely the type of staggered treatment that has generated concern. We thus, as a robustness check, also show estimates using two recent estimators (Callaway and Sant'Anna (2021); Sun and Abraham (2021)) which carefully construct clean control groups for each treated cohort which are composed only of the untreated from their cohort.

Randomization Inference The two way fixed effect specification uses conventional sampling-based standard errors. However, the randomness we exploit comes from randomness in treatment assignment. Design-based standard errors (Abadie et al. (2020)) are well-suited to our context and in addition do not rely upon asymptotic approximation. We do not assume that the timing of the busing schedules is random nor do we assume that the assignment of first letters of a last name to alphabet groups is random. We *do assume* that the assignment of alphabet groups to timing of busing *is* random. With 6 groups assigned to six different timings of busing, we have 6! or 720 different ways of assigning alphabet groups to treatment timings that could have been chosen, all of which we assume were equally likely. However, two groups [14] both have the same treatment status for all cohorts given that we are restricted to using variation in busing coming from those who were in seventh grade by the fall of 1974. Thus, any permutations across these two groups will leave treatment status ultimately unchanged. Ultimately, we are able to use variation in treatment coming from 360 permutations of alphabet group assignment to treatment. We thus present randomization inference results

[14]The groups which contain "G,H,L" and "I,J,K,S" differ in timing of busing but not in who gets treated within our initial cohorts.

where we compute 359 counterfactual ways of assigning alphabet groups to timing of busing which yield differences across cohorts in ultimate assignment of busing. For each of these, we estimate Equation 1 with the counterfactual treatments and then we compute the p-value of the actual treatment compared to the distribution of 359 counterfactual treatments.

Measurement Error due to Matching Errors One potential source of bias in our estimation is from imperfections in matching yearbook data to voter registration data. In our case, 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 may even be some untreated individuals, individuals not even from Kentucky, who are matched to someone that was assigned to attend a formerly black school. This measurement error in our data likely biases our estimates toward zero (Freeman (1984)).

Non-Compliance of the Treated Measurement error notwithstanding, $\hat{\beta}$ should be interpreted as an intent-to-treat effect (ITT). That is, we estimate the impact of having been *assigned* to be bused to a formerly black school. In light of anecdotal evidence on "white flight": non-compliers who selected out of the Jefferson County public school system and instead attended a local private school or a public school in another county, the ITT is unlikely to correspond to the average treatment effect (ATE).[15] In Appendix D, we show that under the highly plausible assumption that enrolling in another mostly white high school does not have a larger impact on political participation and preferences than actually being assigned to attend a formerly black one, then our intent-to-treat estimates provide a lower bound on the true average treatment effect, i.e., the effect of having actually been bused.

Non-Compliance of the Control Given that Whites were bused to city schools and Blacks to suburban schools, Whites in the control group may also have left the Jefferson County Public School system. Assuming (1.) that the impact on those who left the control group school is no larger in magnitude than the impact on those who left the treated group and (2.) the control group are weakly more likely to comply with their assignment than the treated group, then non-compliance in the control group will attenuate out estimated effects. We show convincing evidence on the second of these assumptions in Appendix D.

5. Does busing Affect Political Participation and Preferences?

In this section, we present our main results on registration, voter turnout, and partisanship. First, however, we document that schools did racially integrate following the implementation of the busing plan. Appendix Figure B.4 shows the fraction black over time for four groups of schools: (a.) non-exempt county schools (predominantly White schools busing White stu-

[15] Given the specifics of the court-ordered alphabet plan, there are no "always takers" in our setting. Thus, under full compliance the average treatment effect would correspond to the effect of treatment on the treated.

dents to the Louisville), (b.) non-exempt city schools (predominantly Black schools busing Black students to suburban schools), (c.) exempt integrating schools – concentrated near city boundaries – which were forced to meet the integration requirements through redistricting rather than busing due to in order to increase the fraction Black and qualify for exempt status and (d.) exempt already integrated schools which already satisfied the integration requirements before the court desegregation order. For most of our specifications, we drop the exempt schools as students who attended exempt schools were more urban and likely from families who were politically different from those in the nearly all White suburbs. Political trends across cohorts for kids attending exempt schools are less likely to be parallel with trends for kids attending highly racially segregated schools further away from Louisville city boundaries.

Though, some white families did leave the JCPS school district, Whites who were bused attended schools which were substantially more integrated than their pre-busing home schools. However, different from many busing plans, busing in Jefferson was two-way. Thus, Whites who were not put on buses nevertheless saw an influx of black peers into their schools. According to data submitted to the Office of Civil Rights, in 1976, black enrollment in the area's high schools ranged from 13.9% to 33.9%. Although the district did not completely equalize the share of African-American students across all schools, it largely eliminated average differences over formerly majority Black and former majority White schools. Appendix Figure B.5 shows the distribution of the average fraction of the student population which was Black in 1976 and 1978 by school[16]. We break up the distribution of schools by fraction black into city schools and county schools. We drop all schools which were exempt from the busing plan. Two important differences between those who were bused and those who experienced integration solely through increased diversity in their home schools is that formerly black schools were located in very different neighborhoods and continued to have fewer resources available (see Tuttle 2019). Our estimates, therefore, capture the effect of having been assigned to attend one of these lower resourced inner-city schools rather than a similarly diverse and more distinctly suburban one. We will discuss this more in Section 6.

5.1. *Registration Impacts*

Much of our analysis will focus on registered voters matched to yearbook records. Thus, it is important to first check whether those who were assigned busing are also more likely to be matched to voter registration files. We estimate effects of being assigned to busing on

[16]We restrict to these years because they are the only years for which (1.) we have official Department of Education data on race by school in the post-integration period and (2.) we can construct a clean treatment and control group from pre-1975 yearbooks which are not impacted by the busing plan.

being matched with a voter registration record using equation 1. We estimate effects in four different ways with very similar results. In the left panel, the outcome variable is whether we obtained any match from the yearbook data to the voter registration data. In the right panel, the outcome variable is whether we obtained a unique match from the yearbook data to the voter registration data. The odd numbered columns estimate our baseline differences-in-differences specification. In the even columns, we add six alphabet group specific linear time trends across cohorts. Using the matching methods outlined in Section 4, we are able to match 84% of our yearbook sample to at least one voter registration file and 57% to *exactly* one voter registration file. Given the large difference in overall match rates, it is surprisingly that the coefficients are so similar across the left and right panels. The coefficients range from those who are bused being matched at a higher rate by 0.42 percentage points to bused students being matched at a higher rate by 0.94 percentage points. These are small estimates and they are statistically not distinguishable from zero at conventional levels of statistical significance. The t-statistics are all less than one or very close to 1. Results are presented in Table 4.

Table 4: Effect of Busing on Voter Registration

	Any Match		Unique Match	
	(1)	(2)	(3)	(4)
Bused	0.88	0.94	0.42	0.72
	(0.80)	(0.95)	(0.91)	(1.14)
Mean of Dep. Var.	83.73	83.73	56.92	56.92
R-squared	0.005	0.005	0.011	0.011
Observations	32,555	32,555	32,555	32,555
Cohort FEs	Yes	Yes	Yes	Yes
Alphabet-Group FEs	Yes	Yes	Yes	Yes
Alphabet-Group Linear Trend	No	Yes	No	Yes

Note: Table 4 reports the point estimates, standard errors, and summary statistics from estimating the effect of busing on observing a given individual in the voter file, following the specification in equation 1. The first and second columns report the result when the dependent variable is an indicator equal to one if an individual has at least one match, while the third and fourth columns report the result when the dependent variable is an indicator equal to one if an individual has one unique match. Columns (1) and (3) include only 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.

Of course, it is possible that there are two offsetting impacts: different alphabet groups might be harder to match to voter registration records and that could be offset by an effect of busing on voter registration. Why might different alphabet groups be harder to match? Imagine an unusual name with multiple spellings on the one hand or on the other hand, a very common name like John Smith which is likely to yield non-unique matches. It is also possible that there could be differences in mortality across alphabet groups. Of course, to

confound effects of busing on voter registration, there would have to be differential trends in our ability to find matches over our sample. Nonetheless. to rule out that our matching procedure works better for certain groups of last names than others, we focus on the 1974 and 1975 cohorts and regress our match quality indicators on fixed effects for the set of alphabet groups in the court-ordered busing schedule. Regardless of whether we consider both untreated cohorts together or in isolation, we obtain small point estimates and can never reject the null of no differential in match rates. This suggests that the gap that we do observe among the treated cohorts might be due to a positive effect of busing on being registered to vote—though the pooled difference is not large and statistically insignificant ($p = 0.114$).

5.2. *Voter Turnout*

We also estimate the impact of busing on voter turnout and present the results in Table 5. The results are presented in three super columns: one for all elections pooled, one for presidential elections only (2008, 2012, and 2016) and one for midterm elections only (2010, 2014, and 2018). The estimates are very similar across the three specifications. Estimates are uniformly small and statistically insignificant with point estimates ranging from -0.79 to +0.41 and t-stats all well below 0.5. Due to large standard errors, however, we cannot reject estimates as large, in some cases, as 5 percentage points with a 95% level of confidence. Ultimately, given our sample size, we do not see evidence of effects of busing on political participation as measured by engagement with voting. In the next subsection, we turn to our main findings where we show that we *do* find statistically significant impacts of busing upon political orientation.

Table 5: Turnout Effects of Busing

	General Election Turnout					
	All Years		Presidential		Midterms	
	(1)	(2)	(3)	(4)	(5)	(6)
Bused	–0.07	–0.34	0.41	0.11	–0.55	–0.79
	(1.52)	(2.00)	(1.13)	(1.41)	(1.93)	(2.50)
Mean of Dep. Var.	70.51	70.51	74.69	74.69	66.34	66.34
R-squared	0.008	0.008	0.006	0.006	0.010	0.010
Observations	111,168	111,168	55,584	55,584	55,584	55,584
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

Note: Table 5 shows the impact of busing on voter turnout. Results are presented separately for all elections, presidential elections from 2008-2016 and midterm elections from 2010-2018. Odd numbered columns present baseline difference-in-differences estimates. Even numbered columns present baseline estimates with alphabet group specific linear trends.

5.3. *Partisan Identification*

Table 6 displays the impact of busing on party preferences. In the first panel, we show effects for all states. This includes states which maintain partisan registration as well as those which do not. For those which do not, we use the partisanship imputations created by L2 which we discussed in Section 4. In the second panel, we restrict to the 35 states with partisan representation. Furthermore, we follow the setup in our prior tables of showing baseline difference-in-difference estimates in odd numbered columns and difference-in-difference estimates augmented by alphabet-group specific linear trends in even numbered columns.

Our estimates are remarkably stable across specifications. Among registered voters, bused increases the probability of being registered as a Democrat by 3.03 and 3.51 percentage points. All estimates are statistically significant at a 95% level of significance. Moreover, the increase in Democrat registration comes at the expense of registration as a Republican and not from a reduction in registration as an independent. The effects on Republican registration also vary only narrowly. They range from -2.45 to -2.86 and are also all statistically significant at a 95% level of confidence with the exception of the estimates for closed primary states (reduced sample size) and state-specific linear trends. This specification is not statistically significant largely because the standard errors increase by 25% with the addition of alphabet-group specific linear trends. The sizable increase in the standard errors without much change in the main coefficient of interest suggests that the alphabet-group linear trends model is overly taxing and largely increases the variance of the estimates.

We also present our randomization inference results in Figure 2. As discussed in Section 4, we consider all 360 permutations of alphabet groups to timing of treatment and compare the actual to the counterfactual estimated treatment effects with the resulting treatment assignments. We present the results in two panels. In the first, we show the results for Democratic registration as an outcome and in the second, we show the results for Republican registration as an outcome. The actual estimates is the second highest in the distribution of the 359 counterfactuals, yielding a p-value of 0.557. In other words, the actual treatment effect is higher than 99.443% of the assignments that could have happened but didn't for random reasons. For the Republican registration outcome, it is the 10th highest, which is higher than 97.22% of the counterfactual assignments that could have happened but didn't for random reasons. Appendix Figure A.3 show the results for independents where the results are very far from statistical significance. The p-value is 32.222%, again reinforcing our main OLS results.

There are two ways to interpret our results. It is possible that the experience of being bused took otherwise registered Republicans and made them into Democrats. However, it is also possible that it both turned 3% of Republicans and turned them into independents and

Table 6: Effect of Busing on Party Affiliation

	All States		Closed Primaries	
	(1)	(2)	(3)	(4)
	Registered Democrat			
Bused	3.51***	3.40**	3.34***	3.03**
	(1.16)	(1.49)	(1.24)	(1.52)
Mean of Dep. Var.	36.48	36.48	39.04	39.04
R-squared	0.004	0.004	0.004	0.004
Observations	18,528	18,528	13,948	13,948
	Registered Republican			
Bused	–2.78**	–2.64*	–2.86**	–2.45
	(1.24)	(1.35)	(1.22)	(1.52)
Mean of Dep. Var.	50.21	50.21	49.13	49.13
R-squared	0.002	0.002	0.003	0.003
Observations	18,528	18,528	13,948	13,948
	Registered Independent			
Bused	–0.73	–0.77	–0.48	–0.58
	(1.07)	(1.23)	(1.00)	(1.15)
Mean of Dep. Var.	13.31	13.31	11.83	11.83
R-squared	0.002	0.002	0.001	0.001
Observations	18,528	18,528	13,948	13,948
Cohort FEs	Yes	Yes	Yes	Yes
Alphabet-Group FEs	Yes	Yes	Yes	Yes
Alphabet-Group Linear Trend	No	Yes	No	Yes

Note: Table XX reports the point estimates, standard errors, and summary statistics from estimating the effect of busing on observing a given individual in the voter file, following the specification in equation XX. The first and second columns report the result when the dependent variable is an indicator equal to one if an individual has at least one match, while the third and fourth columns report the result when the dependent variable is an indicator equal to one if an individual has one unique match. Columns (1) and (3) include only 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.

then turned an additional 3% of independents and transformed them into Democrats. We do not have a good way to distinguish between these two possibilities. Either way, however, it is remarkable that we find such large and statistically significant effects a full 46 years after the initiation of busing. The experience of being bused permanently changed politics for a group of voters.

We show a number of robustness checks to our main results. In Appendix Table A.3, we show four specifications across 4 columns in each of three panels (one for each of Democratic registration, Republican registration and registration as an independent). In the first column, we drop alphabet group fixed effects. Our results on Republican registration decrease by approximately 0.5 but remain statistically significant at greater than a 95% level of confidence. However, our estimates on Republican registration and registration as an independent roughly equalize and drop from statistical significance at conventional levels. This suggests that whereas there is not much selection into Democratic registration on alphabet group, there are some systematic differences in registration as a Democrat and independent across alphabet groups.

Beyond the main specification, which we repeat in Column 2 of Appendix Table, A.3, the two additional specifications add fixed effects to our baseline results rather than remove them. First, we put in school fixed effects to account for any selectivity of students into schools. Here we define school as the pre-busing school recorded from the 1974-1975 yearbooks. In Column 4, we also put in 2020 state-of-residence fixed effects. These fixed effects are endogenous as busing could potentially have impacted whether an individual moved out of state and if so the state to which they moved. We include the state-of-residence fixed effects in order to account for local political culture, candidate selection, and state law, all of which could plausibly influence voting. We do this solely as a robustness check. Our estimates are highly robust qualitatively, quantitatively, and in terms of statistical significance.

Since difference-in-difference estimates of treatment effects do not properly aggregate effects in the presence of heterogeneity in effects across cohorts, we also present an estimator due to De Chaisemartin and d'Haultfoeuille (2020). We present our findings in Appendix Table **??**.

The main identification challenge to identification would be if alphabet groups were experiencing political trends. For example, if Italian-Americans were systematically in different alphabet groups from German-Americans and for the cohorts being bused, Italian-Americans were trending differently across cohorts from German-Americans. Luckily our research design presents a natural placebo analysis. We run our baseline estimates including those who attended exempt schools. We then include a dummy variable for exempt schools and an interaction term between exempt and assigned busing. This specification allows for mean

differences in political orientation between those who attended exempt schools and those who attended schools subject to the busing plan. The interaction terms gives the placebo effect of being bused for those who would have been bused in a non-exempt school but weren't bused because they attended an exempt school. The estimation is given below:

$$(2) \quad Y_{i,a,c} = \beta Assigned\,busing_{a,c} + \gamma Exempt_i + \delta Exempt_i X Assigned\,busing_{a,c} + \mu_a + \delta_c + \epsilon_{i,a,c}$$

Our results are presented in Table 7. We first note that those who attended exempt schools are more Democratic and less likely to be Republican or Independent. This is unsurprising and fully consistent with selection. We would expect that White families who chose to send their kids to more integrated schools in the era before mandatory busing would be more progressive families who would align more (in the long run) with the Democratic party and would pass that along to their children. We certainly cannot interpret these results as causal as families self selected into the areas that were declared exempt.

Second, since the *Exempt* variable is not fully interacted with other variables in the model, the *Assigned Busing* coefficient should not be identical to the coefficients in Table 6. However, they should be very close to the baseline treatment coefficients and they are. In particular, the alphabet group and cohort fixed effects remain uninteracted. Nonetheless, these estimates are only 0.1-0.2 away from baseline estimates.

Third, our main coefficient of interest in Table 7 is the placebo effects of busing on students who were in fact not bused because they attended an exempt school. As we can see, look across our six specifications, coefficients are uniformly smaller than 1.31 in magnitude, alternate signs across specifications and are far from statistical significance. Thus if differential political trends across cohorts are responsible for our main effects, they are not present in schools exempt from the busing plan.

Before discussing potential mechanisms, however, we address the robustness of our main finding. First, it is reassuring to note that controlling for alphabet-group fixed effects leaves the point estimates in Table 5 qualitatively unaffected. In columns (1), (4), and (7) of that table, identification comes from comparing the political affiliation of students *within* the same graduating cohort. Unless there are systematic differences in party affiliation between individuals whose last name starts with different letters *and* these differences happen to be correlated with the grouping of letters in the court-ordered busing plan, implementing the "second difference" in our difference-in-difference design should not have an appreciable effect on our results. The pattern of estimates in Table 5 confirms that our findings are not driven by systematic differences in party affiliation between alphabet groups.

Table 7: Exempt School Placebo

	Party Registration					
	Democrat		Republican		Independent	
	(1)	(2)	(3)	(4)	(5)	(6)
Assigned Busing	3.67***	3.06**	–3.02***	–2.28**	–0.66	–0.78
	(1.16)	(1.47)	(1.11)	(1.11)	(1.05)	(1.20)
Placebo Busing x Exempt	–4.34**	–4.37**	3.06	3.21	1.27	1.16
	(2.09)	(2.09)	(2.68)	(2.68)	(1.73)	(1.75)
Exempt School	3.28***	3.29***	–2.11***	–2.13***	–1.17**	–1.17**
	(0.83)	(0.83)	(0.78)	(0.78)	(0.52)	(0.52)
Mean of Dep. Var.	37.18	37.18	49.63	49.63	13.19	13.19
R-squared	0.004	0.004	0.002	0.002	0.002	0.002
Observations	23,472	23,472	23,472	23,472	23,472	23,472
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

Second, we have conducted a placebo check involving the untreated 1974 and 1975 graduating cohorts. If the results in Table 5 are due to a genuine impact of busing, then we would not expect to find a similar effect among students who were, in fact, never assigned to attend a formerly black school. Thus, estimating the empirical model in eq. (1) on the 1974 and 1975 cohorts should produce a null estimate. This is, indeed, what we find. Assuming that school desegregation went into effect three years earlier than it actually did (holding which alphabet groups would be bused fixed), and restricting attention to these two cohorts yields an estimated impact on Republican registration equal to 0.0282. That for Democratic party affiliation is -0.0171. There is, therefore, no evidence of spurious effects on untreated cohorts.

Third, 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, in the spirit of Fisher (1935), 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 never implemented. We evaluate this implication in Figure 2. There are a total of 360 possible combinations between the six alphabet and grade level groups based on which white children were assigned busing (cf. Table 1). Holding fixed the judge's decision that rising seniors should be exempt and relying on the specification in Table 6, Figure 2 depicts the estimated coefficients for each of the feasible treatments. For both Democratic and Republican party affiliation, the treatment assignment

that was, in fact, implemented yields the largest estimated effect. Randomization inference, therefore, provides support for our findings.

6. Impacts on Ideology and Mechanisms

In this section, we present additional evidence on the political impacts of busing using survey evidence. We use the survey for three main distinct yet related purposes. First we use it to validate our matching process. Second, we use it to explore the impact of busing on ideological attitudes: attitudes on race, attitudes towards government, views on whether the world is fair, and support for policies that Democrats and Republicans tend to disagree on. Third, we use the survey to better understand the mechanisms through which busing influenced partisanship. In particular we consider the possibility that friendship formation influenced future partisanship, that the types of teachers or curriculum differed between destination and home schools, and that exposure to poverty and a lower level of resources in the urban schools that Whites were bused to influenced political attitudes.

We randomly sampled 1200 treated individuals and 1200 controls from our subsample of high school students in the 1974-75' yearbooks. These were students whose expected graduation date from Jefferson County Public schools ranged between 1975 and 1979. NORC (National Opinion Research Center) fielded the survey. We obtained 629 completed responses. The survey was fielded in the summer of 2022. Details on the survey, including a copy of the survey instrument, are provided in Appendix C.

6.1. *Treatment Validation and White Flight*

We validate our treatment variable in three ways. We first use the survey. In an open response question, we ask respondents to report the high schools they attended. As can be seen in the survey (Appendix C), we leave room for respondents to report multiple high schools. We classify all reported schools as one of city schools, county school, public schools outside of JCPS, and private schools. Thus an individual who was bused from a county to a city school would have an outcome of 1 for both city school and county school and otherwise 0. We then estimate Equation 1 using these four dependent variables. We find that being assigned to busing increases the probability of reporting a city school by 40%, increases the probability of reporting a private school by 5%, and reduces the probability of listing only a county school by 45%. These estimates are all statistically significant at above a 99% level of significance. We also find a statistically insignificant impact on reporting a non-JCPS public school. Results can be seen in Appendix Table A.8.

Second, we validate compliance by matching students in the Fall 1974 yearbooks to yearbooks from subsequent years. In particular, we estimate the impact of assignment to busing

in 1976 on appearance in subsequent yearbooks. We present our results in Appendix Table A.9. Here, we separate out effects into two different samples: the full sample and the smaller sample of individuals who we were able to match across yearbooks. The effects on the full sample are useful for estimating the impact on White flight out of the school district. The effects on the matched sample allow us to look at compliance for those who remained in the district. We find that overall, those slotted to be bused were 15% less likely to be matched than the control group. We attribute this to white flight. Given that roughly 13% of our sample was bused, we calculate that approximately 2% of students left the school system from the initial cohorts because they were slotted to be bused to another school. We *do* find that those assigned to be bused were 45% more likely to be found in a city school yearbook and overall, 60 percentage points less likely to be in a county school. It is reassuring that our estimates show that the effects on being in the county schools is extremely close to equal to the effect on being matched at all minus the effect of being in the city schools. Out of those we successfully match, we find that 90% of those assigned to busing end up in a city school yearbook and 89% do not end up in a county school yearbook. We find no effect on exempt schools.

Finally, we check collect commencement lists and match them to yearbooks. Here, we restrict our analysis to seniors in high school and ask whether those slotted to be bused end up on city or county commencement lists. Focusing on the full sample, we find that those seniors slotted to be bused are 40% less likely to be on a county school commencement list. They are 8% less likely to be on any JCPS commencement list, 24 percentage points more likely to be on a city school commencement list, and 7% more likely to be on an exempt school's commencement list. This latter finding is interesting. Some families moved into the city from the county when they knew their child would graduate from a city school. Many of the exempt schools were actually less integrated in the end than city schools which were destinations for bused White students. These results are presented in Appendix Table A.10.

Where did students who left the school district go? Some went to more rural and less integrated public schools in other counties or states. The private segregation academies prevalent in the South did not exist in Kentucky; however, many others did go to private Catholic schools. We note that the imperfect compliance with treatment and particularly the asymmetric compliance where the treated are more likely to fail to comply means that there is a gap between the average treatment effect and the intention to treat estimate. We can interpret our estimates as an intention to treat estimate. Alternatively, under the plausible assumption that leaving the school district for a Catholic school or a less integrated public school district does not move students towards the Democratic party, then we can see our estimates as a lower bound of the Average Treatment Effect of busing.

6.2. *Persuasion Rates*

The primary purpose of busing was to increase minority access to higher quality education. It was not to alter political preferences of children being bused decades later. Nonetheless, we can compute a persuasion rate. We can think of two potential persuasion rates. What fraction of non-Republicans were persuaded to become Republicans and what fraction of non-Democrats were persuaded to become Democrats. We compute both of these. In computing persuasion rates, we follow the literature initiated in DellaVigna and Kaplan (2007) and reviewed in Jae and Lee (2023).

We compute the persuasion rate by expressing the fraction of people who were 'persuaded' by being bused out of the fraction of those who who were bused that could have been persuaded. This ends up being the ratio of three numbers:

1. The treatment effects (3.51 for Democrats, -2.78 for Republicans)
2. The fraction of the population differentially bused (40% in the survey)
3. The fraction of the population potentially subject to influence (non-Democrats: 76.89%, non-Republicans: 51.42%)

We first divide the treatment effects, which were estimated by the assignment to treatment (i.e. intention to treat). This is akin to dividing a reduced form by a first stage in an instrumental variables regression. We then divide the effective IV by the fraction of the population potentially subject to influence. The equation is given by

$$f_p = \frac{\hat{\beta}}{e_2 - e_1} \frac{1}{1 - \tilde{y}_p} \tag{3}$$

where $\hat{\beta}$ indicates the relevant point estimate from Table 6, $e_2 - e_1$ is the differential exposure or compliance rate which we take from the survey results in Appendix Table A.8 and $1 - \tilde{y}$ denotes the fraction of potentially persuadable individuals, i.e., students in the control group who do not identify with party p. We get this latter number from the survey. We use the survey results, following Jae and Lee (2023), because we are able to point identify the persuasion rate in the survey data where we measure compliance and outcome at the individual level.

We choose to measure those subject to persuasion from those in alphabet groups that were not bused. If there were large differences across alphabet groups, this could cause some bias to our results. However, as shown in Figure 1, the differences in partisanship across treated and untreated alphabet groups are miniscule.

We present our results in Table 8. We find that being bused 'persuaded' 12.24% of non-Democrats to become Democrats and 14.30% of Republicans to become non-Republicans. These estimates are similar to many estimates of persuasion rates in other contexts as surveyed by DellaVigna and Kaplan (2007). However, our estimates differ from many estimates of persuasion rates in three qualitative respects. First, our estimates are over 45 years of a person's life and are thus essentially permanent. Second, our treatment is a larger intervention than most interventions. It is much longer lasting and had a larger impact upon peoples' lives. Third, our treatment very likely did not have partisan intent. Thus, the political impacts of busing were likely an unintended consequence.

Table 8: Persuasion Rates

	Democrat	Republican
Persuasion Rate	26.93	-1.99
	(24.21)	(27.09)

To place our findings into the proper historical context, it is important to note that until the late 1940s, the Democratic Party was mainly based in the South and associated with white supremacy. Eventually, however, the party's stance on racial equality began to change. So much so that it was Democratic presidents and a Democratically controlled Congress that advanced and signed into law the landmark Civil Rights Act of 1964 and the Voting Rights Act of 1965—even at the expense of hurting the party's electoral fortunes in the South. Kuziemko and Washington (2018) show that defection among racially conservative whites explains nearly all of the Democratic Party's decline between the early 1960s and late 1990s. If school desegregating permanently lessened racial prejudice among bused whites, then we would expect more of them to identify as Democrats today. The results above are consistent with this view.

6.3. *Effect on Support for President*

We now estimate the impact of being bused on support for president. The survey also asked survey respondents who they preferred for president in 2012 and in 2020. The question explicitly asked for preferences independent of whether the respondent turned out to vote. We estimate our baseline model (Equation 1) and show results in Appendix Table A.11. Estimates on support for president are almost an order of magnitude larger than estimates on partisan registration. The estimated effects of being bused on the Obama vote share in 2012 and the Biden vote share in 2020 are both between 20 and 23 percentage points. Both effects are statistically significant at well below a 5% level. We also report q-values which adjust for multiple hypotheses numerically next to the estimate. The q-value, developed

by Storey (2003), generalizes the notion of the probability of a single false detection (type 1 error) of a hypothesis to a false discovery rate across multiple hypotheses. The q-value adjusts for the background rate of false positives given the number of tests and computes the probability of false discovery for an individual test in a group. It differs from the family-wise error rate (FWER) in that FWER tests computes the probability that a given test in a family of hypotheses is false as opposed to the probability that at least one of a family hypotheses is false. The q-value is 4.6% for Biden in 2020 and 14.4% for Obama in 2012.

What accounts for the differences between our estimates of impacts on partisan registration and impacts upon stated support for president? One possibility is that people falsely report their preferences in surveys but not on registration data. Another possibility is that voter registration data is stale and that long-run impacts are larger than shorter run impacts of busing. We find evidence against both of these interpretations. In the same survey, when we estimate impacts upon partisanship ("Do you consider yourself a Democrat, a Republican, an Independent or none of these"), we find estimates very much in line with our voter registration estimates not with our estimates of effect upon political support. Moreover, effects on presidential voting are of a similar size in the presidential race of 2012 when Mitt Romney was the Republican candidate and in the presidential race of 2020 when Donald Trump was the Republican nominee. Thus, we do not attribute the large size of the effects as due to a Trump effect but rather a gap between presidential voting and party identification.

6.4. *Ideology and Attitudes*

In addition to estimate impacts upon voting behavior, our survey allows to ask questions about political ideology and social attitudes. In the survey, we asked six categories of questions for a total of 32 questions. The first set of question was on trust in government. They ask questions about trustworthiness of government officials, questions about corruption, and questions about government waste. We also asked questions about policy support. We break these into two subgroups: (a.) questions about policy inequality and redistribution (support for unions, support for the minimum wage, support for welfare, support for the estate tax and other similar policies) and (b.) questions not directly about redistribution (questions about abortion, gun control, climate change, and gay marriage). We ask two sets of questions to measure racial animus. One set of questions ask respondents whether African-Americans are subject to discrimination and, if so, whether they bear responsibility for racial income inequality. A second set of questions asks whether the civil war was fought over slavery, whether the US government should apologize for slavery, whether police are biased against Blacks, and whether African-Americans are too quick to claim that innocent remarks are racist. Finally, we ask a set of questions on, taken directly from Alesina and Angeletos (2005),

whether respondents believe that the world is just.

We show our results graphically in Figure 3 and in tabular form in Appendix Table A.12. The figure represents the coefficient sizes on the X-axis. 10% confidence intervals for t-tests are thinner and in black whereas 5% confidence intervals are in green. Surprisingly, we find no effect on any of the race-related questions. Not only are the q-values statistically insignificant at conventional levels, the p-values are also statistically insignificant. Though this is somewhat surprising, the degree of racial integration, different from Bergman (2018), Billings et al. (2020), and Mattia (2021), does not vary across our treated and control groups. However, we do find statistically significant p-values and q-values both for 4 of the 6 "belief in a just world" questions. Those who were bused are significantly less likely to believe that the world is just. Again, this is consistent with the variation we are using. Those who were bused to city schools were exposed to greater poverty both in the communities they were bused to and in the schools that they attended.

We do not find any other statistically significant q-values with one exception. We do find that those who were assigned to be bused are more likely to support unions relative to those who were assigned to remain in their home school. Coefficients are sizable and close to significance for support for welfare and support for economic policies benefiting children with those assigned to be bused being more supportive. However, they are not statistically distinguishable from zero individually and the false discovery rates are large. In the next subsection, we discuss and provide evidence for possible mechanisms which explain the joint effects on political support, policy support and ideology.

6.5. *Mechanisms*

We now turn to a discussion of the mechanisms through which busing had a permanent impact on political preferences almost half a century later. Sending students to other schools can introduce them to different peer groups, teachers, curricula, and communities. To look at the channels through which busing altered partisan orientation, we again turn to survey evidence. We use the survey to assess whether those who were bused differed in the types of friends they made, the teachers they had (or the curriculum they were taught), and whether the school differed.

For friendship formation, we ask four questions. We ask them about whether they made most of their friends in school, whether the friends they made helped them learn about American society, whether their friends were poor, and whether many of them differed racially. We asked whether teachers were helpful with academic preparation, whether they were of a different race, whether they were impressive, and whether they helped with exposure to other aspects of American society. We also asked whether students felt safe at school, whether a

substantial fraction of students were poor at school, whether attending school in a different part of town helped with understanding American society, and whether a large fraction of students at school were of a different race. We group questions into three categories: a category focused on friendship formation, a category focused on teachers and teaching, and a final category on the school and its location. We estimate effects using Equation 1. We report the coefficients in Appendix Table A.12. Out of the twelve coefficients, three are individually statistically significant at conventional levels. However, only two have statistically distinguishable q-values. Those who were bused were more likely to report being in a school with students coming from poor families and they were also more likely to report having friends who were poor.

We combine these results on poor friends with three other pieces of evidence. First, the results showing that there was greater exposure to poverty are consistent with the results on policy preferences. The only statistically significant q-value on policy preferences was that those who were bused were substantially more supportive of unions. The other two policies which had p-values that were close to statistically significant at a 95% level were support for programs helping poor children and welfare policies. Since Tuttle (2019) shows no substantial or significant difference in income between Whites who were bused and Whites who were not, we see this shift in policy support as due to ideology rather than material interest. Third, this set of results is also consistent with the effects on belief in a just world.

Overall, though we cannot attribute magnitudes to mechanisms, we surprisingly *do not* see a direct impact of being bused on racial attitudes. Of course, the 'contact hypothesis' mechanisms of exposure to people of a different race is largely cut off due to our setting and research design. We do, however, see that those who were bused were exposed to greater poverty and had more friends who were poor. Almost half a century later, they believe that the world is less fair, have greater support for unions and show weakly greater support for welfare policies. They also are more likely to vote Democrat.

6.6. *Evidence from Open Response Question*

We now supplement our evidence on mechanisms with additional evidence from a free response question on the NORC survey. We use this section to validate our results from the structured response section. The open response question, because of their their more limited structure, are useful for measuring what was salient from the respondents' experience of high school (Haaland et al., 2024). The open response question was "Thinking back to your high-school days, which experiences have had the largest impact on who you are today?". From the answers to this question, we create four binary variables. First, to validate that the experience of busing was particularly salient for those who were bused, we create a dummy

variable which takes on a value of 1 if the person mentioned the stem "bus" and 0 otherwise. Then to validate the results on the three mechanisms we investigated, we coded a variable for teachers and curriculum with the stem "teach", we coded for a variable related to friendships being differentially salient with the stem "friend", and exposure to poverty with the roots "poor" and "poverty".

We then run regressions where the dependent variable is respectively: mention busing, mention teach, mention friend and mention poor. We estimate the baseline difference-in-differences model given in Equation 1 and display our results in Table 6.6. In Column 1, we validate the psychological impact of being bused. Despite the inflow of bused students into county schools for Whites who were note bused out, those who were bused are more likely to mention busing in the open response question. The p-value is 1.5%.

	(1) Mention Busing	(2) Mention Teaching	(3) Mention Friends	(4) Mention Poverty
Bused	0.089*	-0.016	0.072**	0.024*
	(0.034)	(0.051)	(0.022)	(0.009)
Constant	0.044*	0.107**	0.034**	-0.001
	(0.016)	(0.024)	(0.010)	(0.002)
Observations	559	559	559	559
R-squared	0.035	0.039	0.024	0.011

Turning to mechanisms, we now investigate whether those who were bused were more likely to mention teaching, friends and poverty. Mirroring our results on the structured questions, We find a small and statistically insignificant coefficient on teaching. However, we *do* find statistically significant differences on mentions of both friends and poverty. Those who were bused were more than 7% more likely to mention friends and approximately 2.5% more likely to mention poverty. Both of these coefficients are statistically significant at below a 2% level. We thus validate our findings from the structured response questions of the survey that being bused altered perceptions about fairness and friendships. This is consistent with the channels identified by Alesina and Angeletos (2005): busing exposed students to conditions of poverty and bused students were more likely to make friends who were poor. This, in turn, reduced the level of belief that the world is just and shifted (Alesina and Angeletos, 2005) the demands for redistribution, resulting in a shift towards the Democratic party.

7. Conclusion

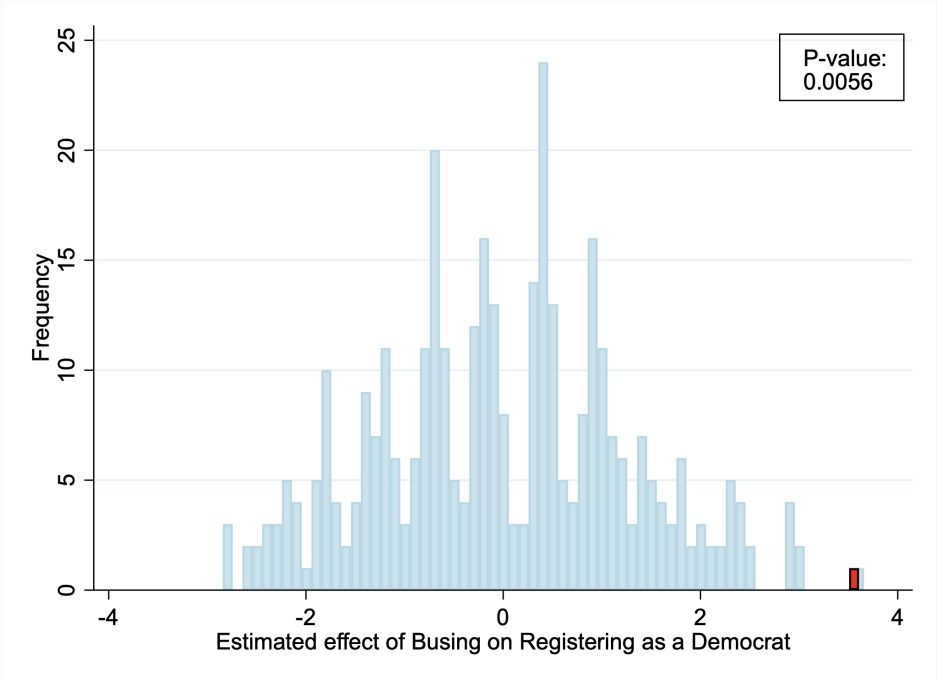
In this paper, we have estimated the impact of being bused during high school upon the partisan political and ideological preferences of Whites almost half a century later. Though we find no effects on voter registration or voter turnout, we do find that those Whites who were bused for random reasons are more than 3 percentage points more likely to be Democrats and close to 3 percentage points less likely to be Republican. Different from prior literature on the impact of racial integration in schools on subsequent political preferences, our setting and research design considers the impact of being bused holding fixed the racial composition of students in the school.

We also estimate the impact of being bused on policy and ideological preferences for a subset of 629 individuals who responded to a survey fielded by NORC on our behalf. Somewhat surprisingly, we *do not* find a large or statistically significant impact on racial attitudes. However, destination schools were as integrated for those being bused as the schools those being bused left behind.

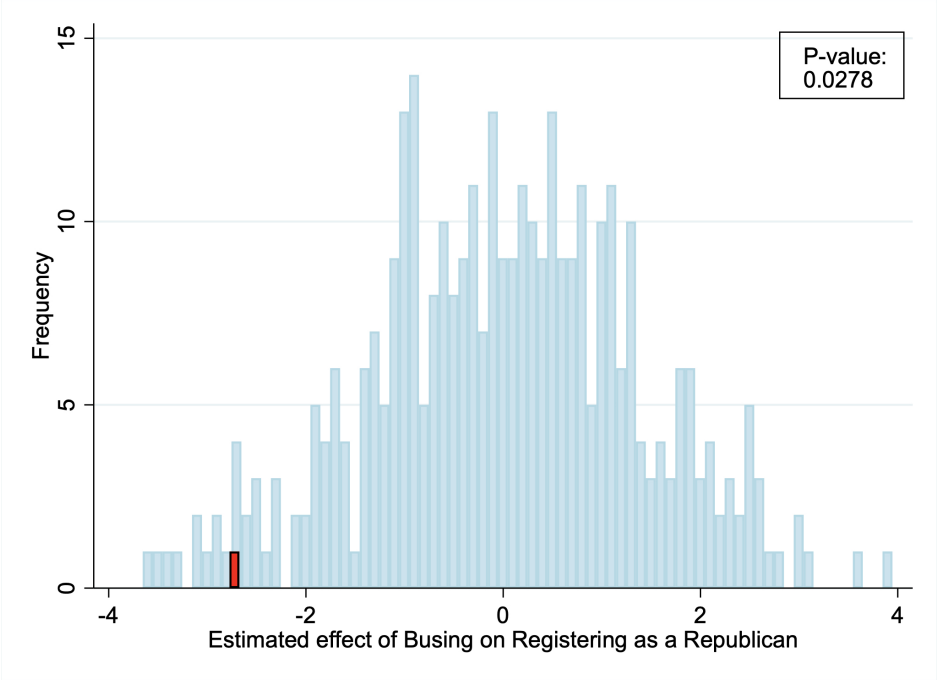
Nonetheless, those who were bused were more likely to report having friends who are poor, are less likely to believe that the world is just, and are more likely to support labor unions. Our findings are consistent with early life exposure to poverty affecting social beliefs and these, in turn, impacting political partisanship permanently.

Figure 2: Party Affiliation, Randomization Inference

(a) Registered Democrat

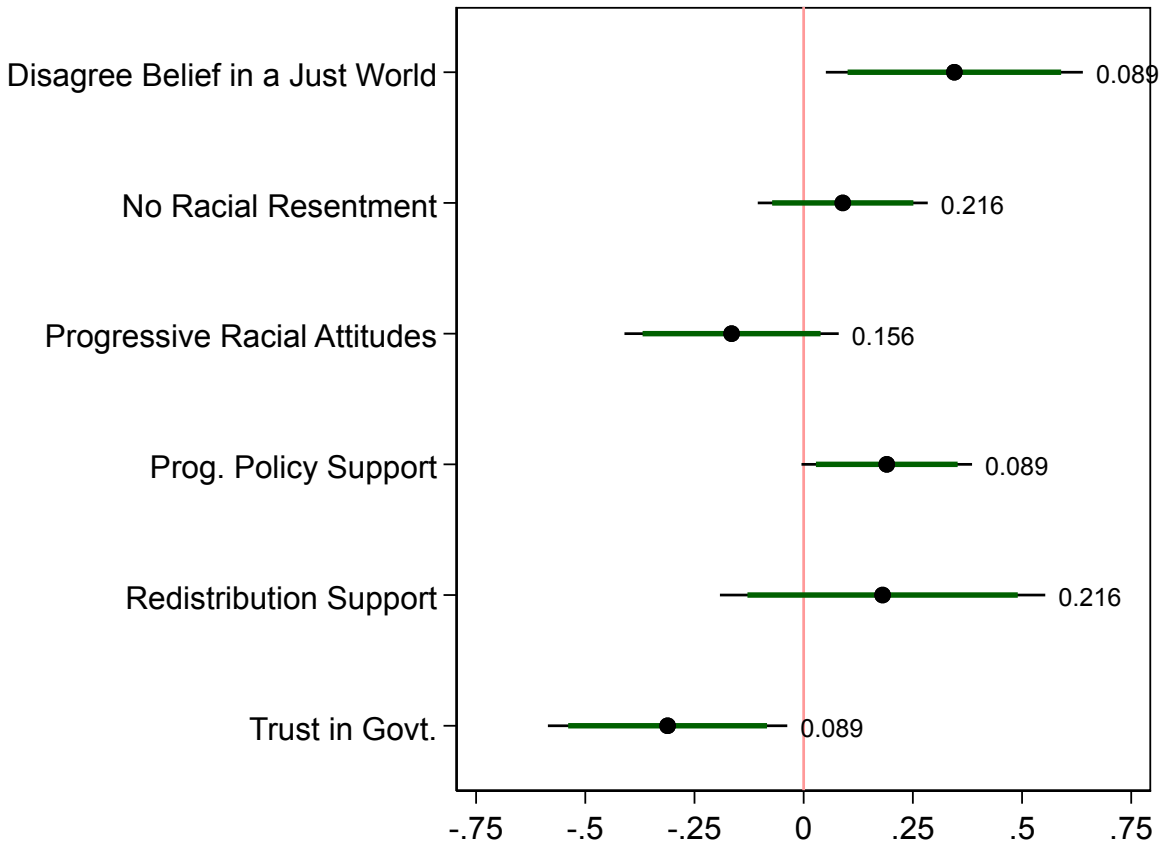


(b) Registered Republican



Notes: This figure is broken into two panels. Panel A plots a histogram of factual and counterfactual estimates of treatment on the probability of being a Democrat. Panel B plots a histogram of factual and counterfactual estimates of treatment on the probability of being a Republican. Counterfactual treatment effects are based upon reassignment of alphabet groups to timing of busing. There are 359 counterfactual ways of assigning timing of busing to alphabet groups. The counterfactual assignment treatment effects are shown by a histogram in blue and the actual treatment effect is shown in red.

Figure 3: Impact of Treatment on Indices



Note: This figure shows coefficients estimates of the impact of busing on six indices of attitudes. The indices are constructed as the average of the standardized values of the variables within the index. Estimates are two way fixed effect estimates, controlling for alphabet group and cohort effects.

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A. Appendix Figures and Tables

Table A.1: Commencement and Yearbook Records: Years Collected

School Name	Commencement Records	Yearbook Records
Ahrens	1972-1975, 1978-1980	1972-1976
Atherton	1970-1975, 1977, 1983	1970-1972, 1974-1976
Ballard	1972-1983	1974, 1976
Barrett Meddle		1974, 1976
Bruce Meddle		1975, 1976
Browns	1974-1977	1975, 1976
Butler		1974-1976
Carrithers Middle		1974
Central	1970-1978	1970-1976
Chenoweth Elementary		1975-1976
Conway Middle		1970-1975
Doss	1971-1980, 1983	1970-1971, 1973, 1975, 1977
Durrett	1971-1980	1971, 1973-1976
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	1970-1972, 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
Manual	1970-1980, 1983	1970-1973
Middletown Elementary		1975-1976
Mill Creek Elementary		1975
Moore	1976, 1978-1980, 1983	1972-1976
Myers Middle		1975-1976
PRP		1970, 1972, 1975-1976
Seneca	1970-1980, 1983	1970-1977
Shawnee	1970-1980, 1983	1971-1972, 1974-1977
Southern		1970-1977
Southern Middle		1975-1977
Stuart		1970, 1972, 1975-1976
Summer	1972-1974, 1975-1976	
Thomas Jefferson	1970-1980	1970-1973, 1975
Valley	1976-1980, 1983	1970-1976
Waggener	1979, 1984	1970-1976
Western	1974, 1976, 1979-1980, 1983	1970-1971, 1975
Westport	1976, 1978, 1980	1970-1977
Williams Middle		1976

Note: Appendix Table A.1 shows how we restrict our sample from 170,715 student-years to our main working sample of 18,528 students.

Table A.2: Sample Construction

Sample Description	Sample Size
Beginning sample	
Full Sample	177,032
Sample Restrictions	
Drop same student in multiple yearbooks across years	102,076
Drop women from yearbooks	48,191
Drop people with missing value for race	47,864
Drop people with same name and implied birth year	47,640
Drop people born after 1963	47,640
Drop City high schools	45,658
Keep graduation years 1969-1979	45,081
Keep white students	43,282
Drop non-matches to voter registration data	36,035
Drop non-matches (+ 3 records that have missing party ID info)	24,631
Keep non-exempt	18,528

Note: Appendix Table A.2 shows how we restrict our sample from 177,032 student-years to our main working sample of 18,528 students.

Table A.3: Effect of Busing on Turnout, Inclusion of Various Fixed Effects

	(1)	(2)	(3)	(4)
		Registered Democrat		
Bused	3.03**	3.51***	3.25***	3.61***
	(1.18)	(1.16)	(1.21)	(1.06)
Mean of Dep. Var.	36.48	36.48	36.48	36.48
R-squared	0.003	0.004	0.007	0.038
Observations	18,528	18,528	18,528	18,528
		Registered Republican		
Bused	–1.61	–2.78**	–2.66**	–2.76**
	(1.19)	(1.24)	(1.23)	(1.17)
Mean of Dep. Var.	36.48	36.48	36.48	36.48
R-squared	0.001	0.002	0.005	0.028
Observations	18,528	18,528	18,528	18,528
		Registered Independent		
Bused	–1.42	–0.73	–0.59	–0.85
	(0.96)	(1.07)	(1.10)	(0.98)
Mean of Dep. Var.	36.48	36.48	36.48	36.48
R-squared	0.001	0.002	0.002	0.058
Observations	18,528	18,528	18,528	18,528
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

Note: Table XX reports the point estimates, standard errors, and summary statistics from estimating the effect of busing on observing a given individual in the voter file, following the specification in equation XX.

Table A.4: Effect of Busing on Party Affiliation, Robustness to Difference-in-Differences Method

	Registered Democrat (1)	Registered Republican (2)	Registered Independent (3)
	Callaway-Sant'anna		
Bused: Cohort Average	2.73*	–2.27**	–0.46
	(1.47)	(0.98)	(0.90)
Mean of Dep. Var.	36.48	50.21	13.31
Observations	18,528	18,528	18,528
	Stacked Difference-in-Differences		
Bused	2.50***	–3.56***	1.07
	(0.86)	(1.11)	(0.91)
Mean of Dep. Var.	36.48	50.21	13.31
R-squared	0.004	0.003	0.002
Observations	37,239	37,239	37,239
Year x Stack FEs	Yes	Yes	Yes
Alphabet-Group x Stack FEs	Yes	Yes	Yes

Table A.5: Effect of Busing on Turnout, Robustness to Difference-in-Differences Method

	All Elections (1)	Presidential Elections (2)	Midterm Elections (3)
	Callaway-Sant'anna		
Bused: Cohort Average	1.35	0.98	1.71
	(1.11)	(1.15)	(1.16)
Mean of Dep. Var.	70.51	74.69	66.34
Observations	18,528	18,528	18,528
	Stacked Difference-in-Differences		
Bused	0.25	0.89	–0.40
	(1.05)	(0.92)	(1.24)
Mean of Dep. Var.	70.51	74.69	66.34
R-squared	0.014	0.010	0.016
Observations	37,239	37,239	37,239
Year x Stack FEs	Yes	Yes	Yes
Alphabet-Group x Stack FEs	Yes	Yes	Yes

Table A.6: Effect of Busing on Party Affiliation, Alternative Matching Restrictions

	Registered Democrat (1)	Registered Republican (2)	Registered Independent (3)
	Including Non-Unique Matches		
Bused	3.86***	–2.65***	–1.22
	(0.75)	(0.99)	(0.75)
Mean of Dep. Var.	36.21	49.09	14.69
R-squared	0.004	0.001	0.002
Observations	27,254	27,254	27,254
	Including Non-Registrants		
Bused	3.49***	–1.78*	–0.85
	(0.70)	(1.00)	(0.59)
Mean of Dep. Var.	30.32	41.10	12.30
R-squared	0.005	0.001	0.001
Observations	32,555	32,555	32,555
Cohort FEs	Yes	Yes	Yes
Alphabet-Group FEs	Yes	Yes	Yes

Note: Table XX reports the point estimates, standard errors, and summary statistics from estimating the effect of busing on observing a given individual in the voter file, following the specification in equation XX.

Table A.7: Effect of Busing on Party Affiliation and Turnout, Black Students

	(1)	(2)	(3)	(4)	(5)	(6)
Assigned Busing	0.02	–0.12	0.04	–0.01	–0.06*	0.12*
	(0.06)	(0.08)	(0.04)	(0.05)	(0.03)	(0.07)
Non-Exempt School		–0.01		0.01		0.00
		(0.03)		(0.03)		(0.02)
Assigned Busing x Non-Exempt		0.15*		0.02		–0.17**
		(0.07)		(0.06)		(0.07)
Mean of Dep. Var.	0.75	0.74	0.18	0.18	0.07	0.08
Adj. R2	–0.003	0.001	–0.004	–0.002	x-0.006	x0.015
Observations	842	1,308	842	1,308	842	1,308
Assigned Busing	9.78**	–2.96	10.00***	–6.38	9.55	0.45
	(4.66)	(5.50)	(3.38)	(5.82)	(6.12)	(7.22)
Non-Exempt School		–0.82		–1.01		–0.64
		(1.32)		(1.87)		(1.87)
Assigned Busing x Non-Exempt		13.09***		16.10**		10.08
		(4.81)		(6.45)		(7.04)
Mean of Dep. Var.	67.72	67.18	72.72	72.12	62.71	62.23
Adj. R2	0.015	0.015	0.015	0.015	x0.015	x0.015
Observations	5,052	7,848	2,526	3,924	2,526	3,924
Cohort FEs	Yes	Yes	Yes	Yes	Yes	Yes
Alphabet-Group FEs	Yes	Yes	Yes	Yes	Yes	Yes

Note: Table XX reports the point estimates, standard errors, and summary statistics from estimating the effect of busing on observing a given individual in the voter file, following the specification in equation XX.

Table A.8: Compliance: NORC Survey

	City	Private	Other Public	Only County
	(1)	(2)	(3)	(4)
Bused	0.40***	0.05***	0.03	–0.45***
	(0.04)	(0.02)	(0.02)	(0.05)
Mean of Dep. Var.	0.19	0.05	0.05	0.68
R-squared	0.243	0.018	0.005	0.242
Observations	519	519	519	519
Cohort FEs	Yes	Yes	Yes	Yes

Table A.9: Compliance: Yearbook to Yearbook Matching

	Any Match	City	County	Exempt	City	County	Exempt
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Bused in 1976	–0.15***	0.46***	–0.60***	–0.00	0.90***	–0.89***	0.00
	(0.02)	(0.02)	(0.02)	(0.01)	(0.02)	(0.02)	(0.01)
Mean of Dep. Var.	0.69	0.05	0.63	0.01	0.08	0.92	0.01
R-squared	0.028	0.410	0.191	0.004	0.850	0.862	0.007
Observations	6,843	6,843	6,843	6,843	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	All	All	All	Matches	Matches	Matches

Table A.10: Compliance: Yearbook to Commencement Matching

	Any Match	City	County	Exempt	City	County	Exempt
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Bused in Grade 12	–0.08***	0.24***	–0.40***	0.07**	0.54***	–0.71***	0.16***
	(0.02)	(0.04)	(0.02)	(0.03)	(0.07)	(0.02)	(0.06)
Mean of Dep. Var.	0.53	0.05	0.42	0.06	0.09	0.80	0.10
R-squared	0.015	0.169	0.100	0.017	0.017	0.017	0.017
Observations	11,620	11,620	11,620	11,620	6,133	6,133	6,133
Cohort FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Alphabet-Group FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Sample	All	All	All	All	Matches	Matches	Matches

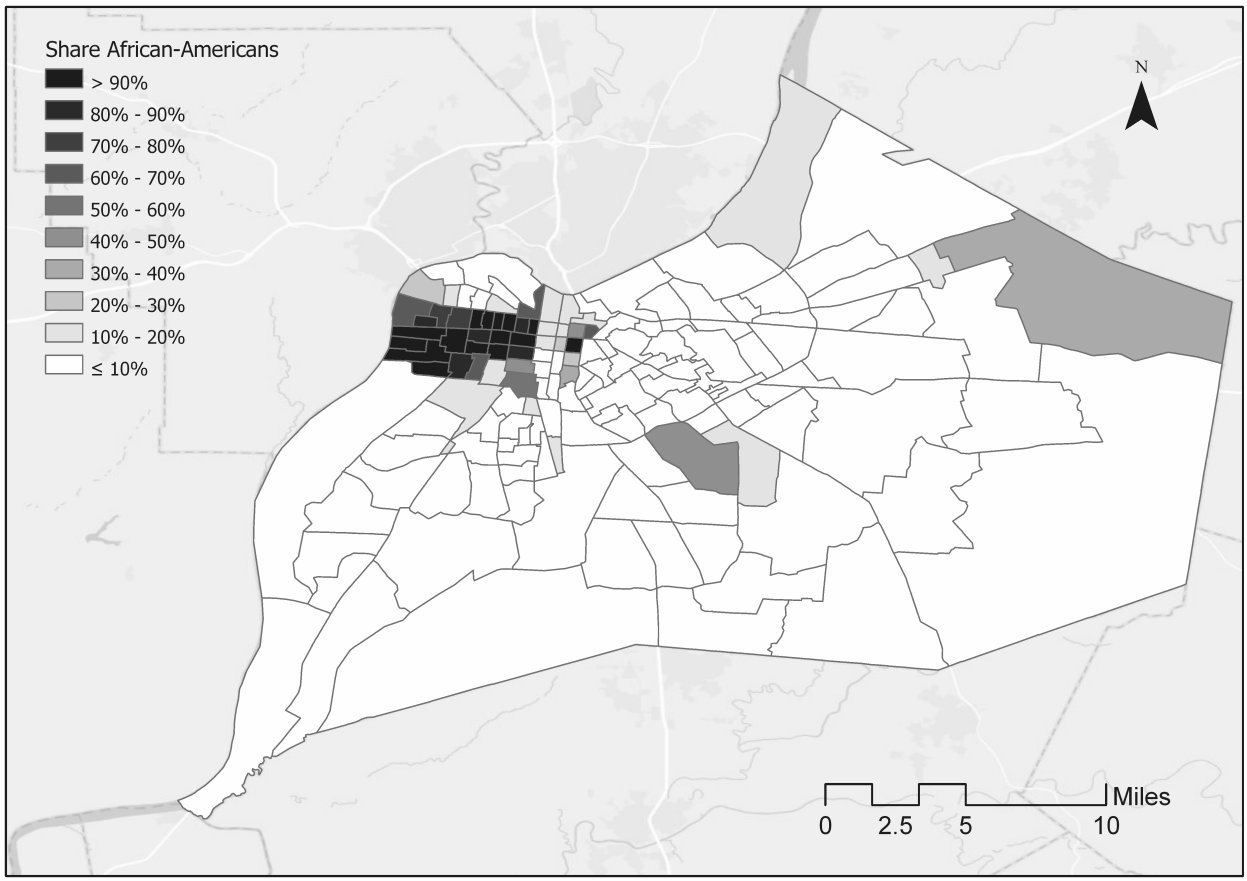
Table A.11: Survey Results: Index Components and Other Outcomes

Outcome	Coefficient	Std. Err.	P-value	Q-value
Wanted Biden Win, 2020	0.229	0.066	0.002	0.036
Wanted Obama Win, 2012	0.202	0.085	0.024	0.140
Democrat	0.058	0.078	0.458	0.913
Independent	–0.030	0.051	0.554	0.913
Republican	–0.028	0.063	0.659	1.000
Ideology (1=Very Cons., 7=Very Lib.)	0.265	0.147	0.082	0.415
Voted	–0.019	0.031	0.546	0.913
Real Stakes (1=BLM, 0=Neither, –1=NPI)	–0.083	0.117	0.486	0.913
People Get What Entitled To	–0.459	0.142	0.003	0.042
Efforts are Rewarded	–0.323	0.130	0.019	0.128
Earn Rewards and Punishments	–0.145	0.127	0.263	0.712
Misfortune is Brough On Self	0.089	0.103	0.396	0.879
People Get What They Deserve	–0.349	0.124	0.009	0.069
Rewards and Punishments are Fair	–0.182	0.123	0.149	0.515
World is a Fair Place	–0.383	0.103	0.001	0.036
Blacks Should Work Way Up Without Favors	0.214	0.100	0.040	0.199
Black Issues due to Slavery and Discrimination	0.047	0.099	0.636	1.000
Blacks Gotten Less Than They Deserve	–0.076	0.114	0.511	0.913
Racial Disparities due to Individual Effort	–0.125	0.137	0.367	0.833
Civil War Over States' Rights	–0.042	0.148	0.778	1.000
Police Racial Bias	0.052	0.143	0.716	1.000
US Should Apologize for Slavery	0.027	0.111	0.809	1.000
African Americans Too Quick to Claim Racism	0.188	0.135	0.176	0.578
Discourage Marrying an African American Partner	0.042	0.119	0.729	1.000
Abortion Policy	–0.001	0.114	0.992	1.000
Climate Policy	–0.047	0.111	0.674	1.000
Gay Marriage Policy	0.156	0.163	0.346	0.816
Gun Control Policy	0.115	0.098	0.252	0.712
Affirmative Action	0.133	0.121	0.280	0.712
Estate Tax	–0.031	0.140	0.827	1.000
Spend More Money on Housing	0.014	0.161	0.933	1.000
Redistribution to Reduce Inequality	0.104	0.148	0.488	0.913
$15 Minimum Wage	–0.148	0.114	0.204	0.652
Spend More Money on Poor Children	0.217	0.136	0.123	0.499
Labor Unions	0.378	0.124	0.005	0.048
Spend More Money on Welfare	0.174	0.115	0.142	0.515
Gov't is Not Crooked	–0.189	0.111	0.100	0.471
Gov't is for All People	–0.217	0.137	0.125	0.499
Trust in Gov't	0.102	0.164	0.539	0.913
Not Much Gov't Waste	–0.020	0.168	0.907	1.000

Table A.12: Survey Results: Index Outcomes and Mechanisms

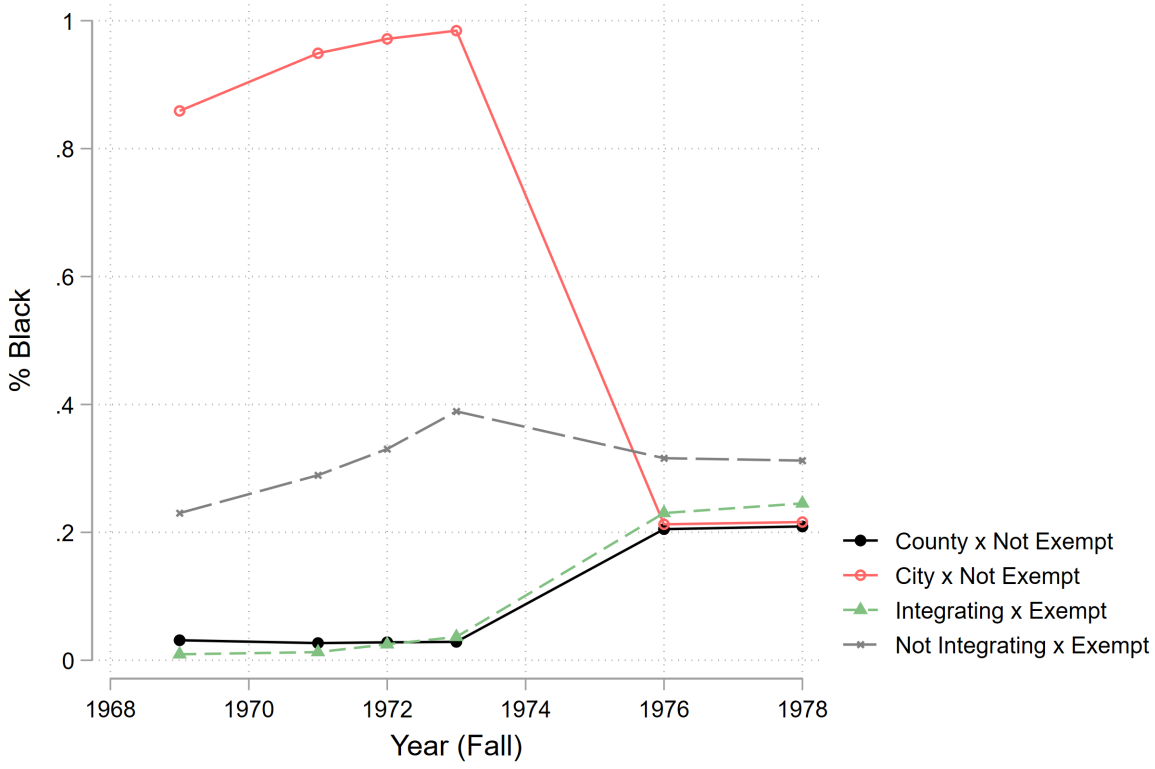
Outcome	Coefficient	Std. Err.	P-value	Q-value
Just World Index	–0.391	0.124	0.004	0.024
Modern Racism Index	0.015	0.111	0.892	0.562
Other Racism Index	0.103	0.138	0.461	0.562
Progressive Policy Index	0.109	0.091	0.239	0.428
Redistribution Index	0.198	0.161	0.227	0.428
Trust in Gov't Index	–0.179	0.149	0.240	0.428
Met Friends at School	–0.034	0.128	0.790	1.000
Friends were Other Race	0.120	0.120	0.327	1.000
Friends were Poor	0.418	0.103	0.000	0.005
Friends Helped Understand Different People	0.078	0.164	0.636	1.000
Fellow Students were Other Race	0.105	0.091	0.259	1.000
Fellow Students were Poor	0.322	0.115	0.009	0.051
Felt Safe at School	–0.040	0.152	0.796	1.000
School Helped Understand Different People	–0.010	0.084	0.907	1.000
Academically Prepared by Teachers	–0.102	0.123	0.412	1.000
Impressed by Teachers	–0.016	0.168	0.923	1.000
Teachers were Other Race	–0.228	0.125	0.079	0.357
Teachers Helped Understand Different People	–0.026	0.103	0.803	1.000

Figure A.1: Residential Segregation in Jefferson County, Kentucky: 1970



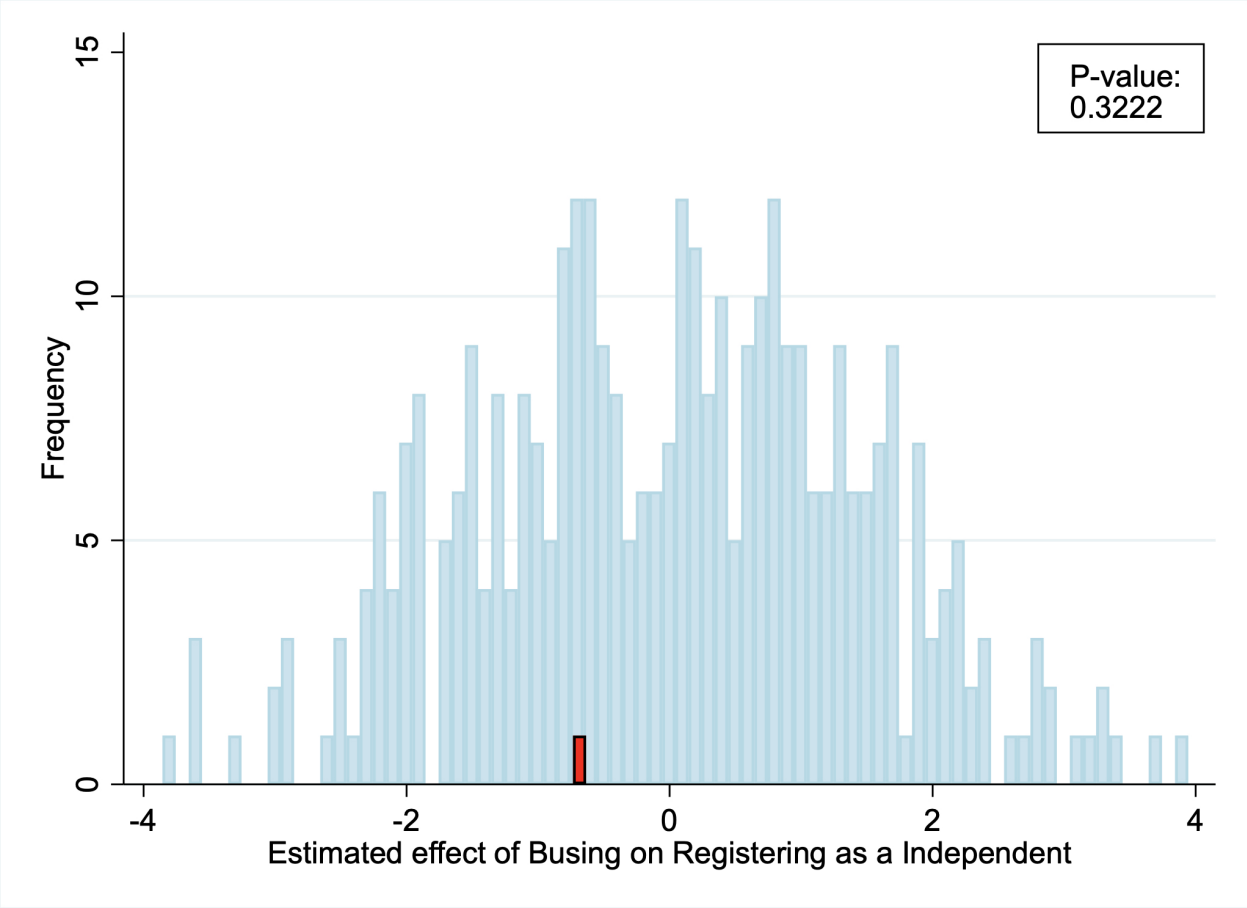
Note: This figure shows the fraction black by neighborhood within Jefferson County, Kentucky as measures in the 1970 Population Census. Darker shading reflects a higher fraction African-American in the neighborhood. The solid black areas in the map are in Louisville.

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



Note: This figure displays the fraction Black in school attendance by type of school. The data comes from reports made by the Office of Civil Rights at the Department of Education. Schools are broken down into those which were exempt from the busing plan and those which were not exempt. Non-exempt schools are further broken down into those located in the city of Louisville and those in the rest of Jefferson County. Exempt schools are broken into integrating and non-integrating. Non-integrating exempt schools had already reached minimal racial integration thresholds set by the courts. Integrating exempt schools were exempt from busing because they achieved minimal integration thresholds via redistricting.

Figure A.3: Randomization Inference Results: Effect on Registration as an Independent



Note: This figure plots a histogram of factual and counterfactual estimates of treatment on the probability of being registration as an Independent. Counterfactual treatment effects are based upon reassignment of alphabet groups to timing of busing. There are 359 counterfactual ways of assigning timing of busing to alphabet groups. The counterfactual assignment treatment effects are shown by a histogram in blue and the actual treatment effect is shown in red.

B. Matching

C. Jefferson County busing Survey

The survey, collected by the National Opinion Research Center (NORC) at the University of Chicago used the following procedure for the implementation of the survey:

1. FedEx/certified mail recruitment packet mailed to all sample members.

a. The letter would describe the study and make the recruitment appeal. We would provide a unique study link to complete the study online, as well as a toll-free telephone number to call and complete the survey by phone with an NORC telephone interviewer. b.

This would include a pre-paid incentive of $5 with a contingent incentive offer of $30 for completing the study. 2. All non-responders to the initial appeal would then be mailed a reminder postcard. This would remind them of the survey completion incentive and would again include the unique URL and inbound phone number to complete the study.

3. Non-responders with an email addresses will be contacted up to two times by email to complete online or via inbound phone.

4. All non-responders will be mailed a reminder letter with a self-administered paper questionnaire and postage paid return envelope.

5. Finally, we will outbound dial (with up to 5 attempts) all non-responders with a matched phone number to complete the survey with the phone interviewer.

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

NORC

55 East Monroe Street
Chicago, IL 60603

Have your voice heard and earn $30

If you have questions, please email us at JeffersonCounty@norc.org.

To participate, follow these three easy steps:

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

Scratch off to show PIN 8698339

**********AUTO**MIXED AADC 601
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Have your voice heard and earn $30 by completing a 10-minute survey.

JEFFERSON COUNTY STUDY

Dear Bernard Manning II,

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

You will receive $30 for completing the 10-minute survey. To participate, please visit us at **JeffersonCo.norc.org** and enter your PIN (see other side) or call us toll-free at **866-795-6586**.

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

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

Sincerely yours,

Jennifer K. Benz
Vice President
NORC at the University of Chicago

9417-55501129

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

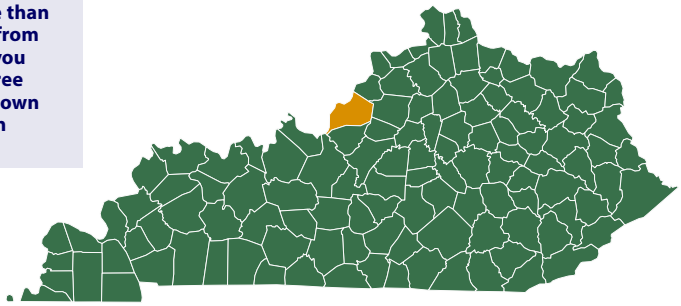


21. Do you agree, disagree, or neither agree nor disagree with each of the following statements?

	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.

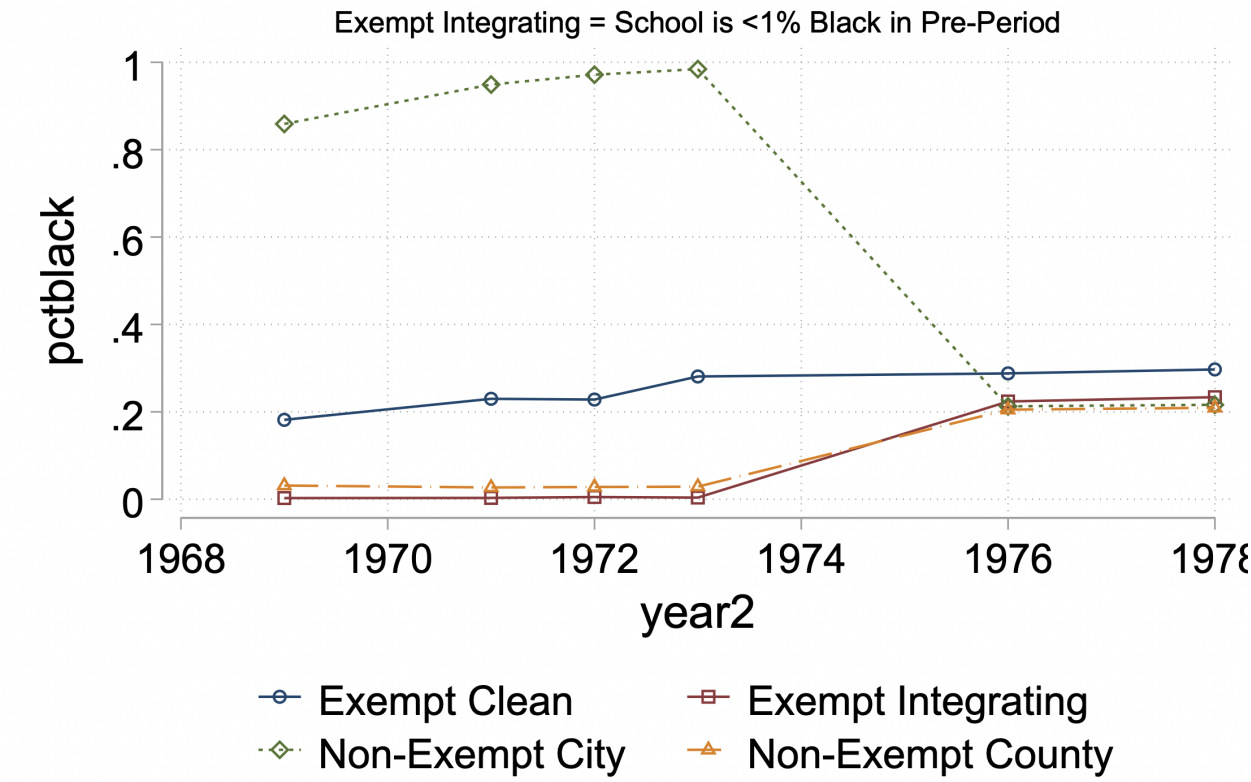
Please return the completed questionnaire in the enclosed postage-paid envelope. If you have lost the envelope, please email us at JeffersonCounty@norc.org.

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

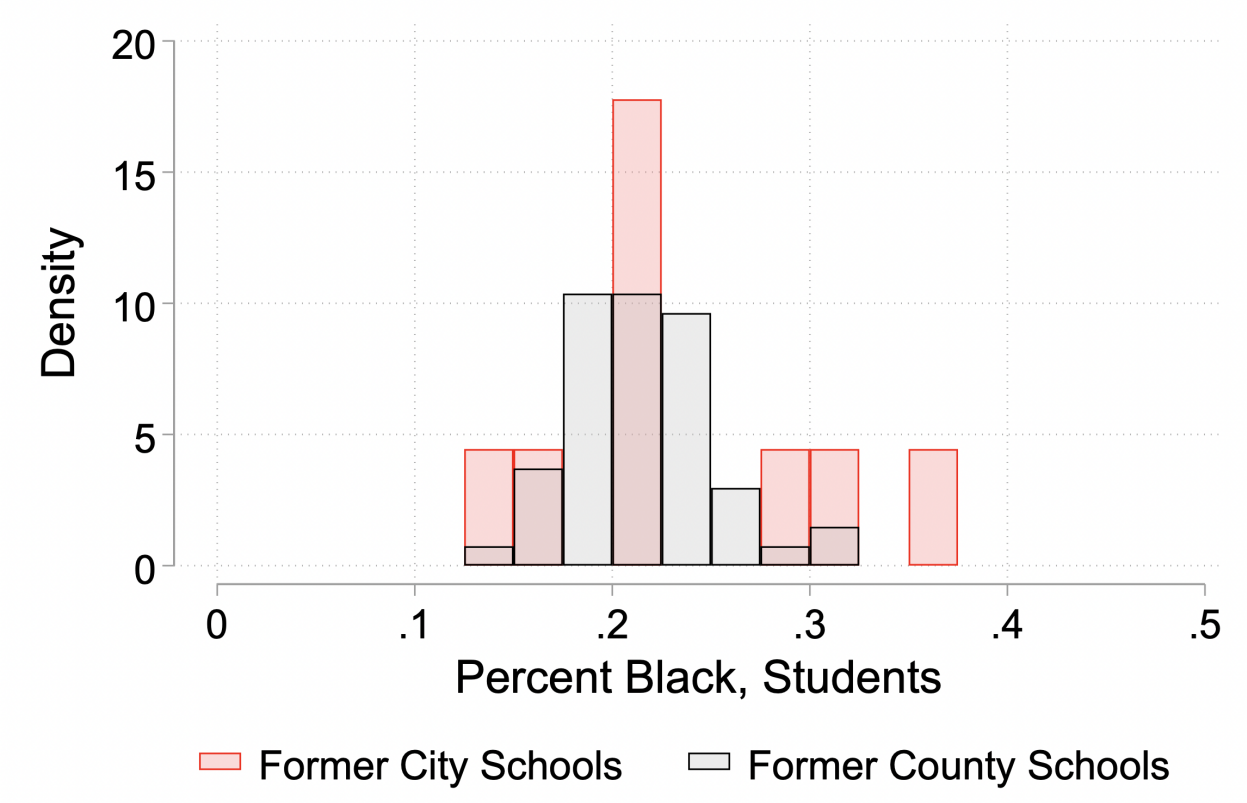
D. Busing Compliance

Figure B.4: Change in Percentage Black by School Type



Note: This figure shows the distribution of the percentage of the student body that was Black in the 1976-1977 school year after the initiation of busing. The data come from Department of Education Office Of Civil Rights reports, which are average across the years in which they are available: 1976, 1978 and 1980. The histogram for former city schools are in pink and the histogram for former county schools are in gray.

Figure B.5: Percentage Black by School



Note: This figure shows the distribution of the percentage of the student body that was Black in the 1976-1977 school year after the initiation of busing. The data come from Department of Education Office Of Civil Rights reports, which are average across the years in which they are available: 1976, 1978 and 1980. The histogram for former city schools are in pink and the histogram for former county schools are in gray.