

School Desegregation and Political Preferences: Long-Run Evidence from Kentucky*

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February 2021

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Abstract

In 1974, a federal court ordered that public schools in Jefferson County, KY be desegregated. To achieve racial integration, students were assigned to a busing schedule that depended on the first letter of their last name. This led to quasi-random variation in the number of years of busing and, for the initial cohorts, whether individuals were bused at all. We use this 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 turnout. We do, however, find that busing significantly increases Democratic party affiliation more than forty years later. Consistent with the idea that attending a formerly black, city school causes a change in the broad ideological outlook of whites, we also find that bused individuals are less likely to donate to organizations that advocate for conservative causes.

*We have benefited from helpful comments and suggestion by Allan Drazen, Jesse Shapiro, as well as seminar participants at the University of Queensland, the University of New South Wales, the University of Houston, the University of Massachusetts Amherst, Princeton University, and the 7th Columbia University Conference in Political Economy. We are also grateful to Jordan Taylor and Frank Girolami from Aristotle Inc. for help with the voter registration data. Jimmy Grant, Alessio Ruvinov, Drew White, and Ryan Willoughby provided excellent research assistance. Correspondence can be addressed to kaplan@econ.umd.edu [Kaplan], j-spenkuch@kellogg.northwestern.edu [Spenkuch], or ctuttle@princeton.edu [Tuttle].

1. Introduction

From battles over slavery to disputes about labor mobility for African Americans to protests over police brutality and misconduct, racial conflict has been at the center of American politics since the founding of the United States (Foner 2011; Katznelson 2005; Schickler 2016). One domain in which questions about racial (in)equality have been especially salient is public education. In 1954, the 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 about ten years later, when 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. One common method to integrate schools was to bus white students to predominantly black schools and black students to “white schools.”

A large literature studies the economic effects of busing (see, e.g., Angrist and Lang 2004; Baum-Snow and Lutz 2011; Bergman 2018; Billings et al. 2013; Cook 2019; Guryan 2004; Johnson 2015; Lutz 2011; Reber 2005). One recent paper by Tuttle (2019) uses a particularly credible research design based on a court-ordered desegregation plan for public schools in Jefferson County, KY. Relying on confidential data from the U.S. Census, he finds that, in adulthood, white students who were bused live in neighborhoods with the same average income as those who were not, suggesting that busing had little to no negative impact on the earnings of whites. For blacks, however, having been bused to a predominantly white school increases mean adult-neighborhood income by about three percent. In this paper, we build on Tuttle’s approach and estimate the individual-level long-run impact of busing on political participation and preferences.

In December of 1974, the 6th Circuit Court of Appeals mandated 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 racially integrate schools, the newly merged district implemented a busing schedule based on which black students were to attend a better-resourced formerly white school for up to nine years, while white students would be sent to formerly black schools for up to two. Critical for our purposes, when and, for the initial cohorts, if a given student was bused depended on the first letter of her last name. There is thus quasi-random variation in

exposure to busing, which we exploit to estimate its impact on voter turnout and political preferences more than forty years later.

Our analysis relies on present-day administrative voter registration and turnout data, which we match to digitized yearbooks from high schools under the court-ordered desegregation plan. Importantly, we identify individuals who were and were not subject to busing based on yearbooks from the year *prior* to the court order. For reasons of statistical power, we focus on busing’s effect on white males.¹ Taking our estimates at face value, we find that, conditional on being registered to vote, busing has no long-run effect on turnout. We do, however, find that having been bused to a black school increases the likelihood of registering as a Democrat by more than two percentage points, with an offsetting decrease among registered Republicans.

In line with the latter result, we also detect a negative impact of busing on individuals’ support for conservative causes. Today, bused individuals are significantly less likely to donate to political organizations and candidates that either promote the anti-abortion movement or oppose same-sex marriage. Bused individuals may also be less inclined to support opposition to the Affordable Care Act (ACA), though our estimates with respect to this outcome are more fragile. Taken together, our findings suggest that the experience of having been bused altered students’ ideological outlook and long-run attitudes, making them, on average, more liberal.

It bears emphasizing that our results are intent-to-treat estimates. In the mid-1970s, white flight was in full swing across many cities in the U.S., and the Louisville metropolitan area was no exception. It is entirely plausible that *(i)* the enactment of busing increased the pace of this phenomenon, and *(ii)* white students assigned to be bused to a formerly black school may have been especially likely to leave the public school system. If either of these is true, then our findings should be interpreted as the effect of having been assigned to be bused rather than having been bused. Put differently, our estimates are averages of the treatment effect for compliers (i.e., students that actually ended up attending a formerly black high school) and non-compliers (i.e., those who instead went to private school or moved to another school district). If we are willing to assume that attending a different predominantly white high school does not have a larger impact on students’ liberal outlook than attending a formerly black one, then the coefficients below constitute a lower bound on the effect of having been bused. That is, the true average treatment effect is likely larger than our estimates suggest.

The three most closely related papers to ours are Gordon and Reber (2018), Bergman

¹In total, African Americans accounted for only about 20% of students in the merged Louisville-Jefferson County school system (Sedler 2007). We, therefore, lack the power to detect even moderately large effect sizes for blacks. Our focus on males is due to the fact that too many females change their last name upon getting married for us to be able to reliably match them across data sets (see Section 3).

(2018), and Billings et al. (2020). Gordon and Reber (2018) study the impact of school desegregation on mixed-race births. Despite a strong correlation between black exposure to whites in their school districts, Gordon and Reber’s causal research design relying on court-imposed desegregation orders implies only small to moderate effects that are, for the most part, statistically insignificant.

Bergman (2018) and Billings et al. (2020) are, to the best of our knowledge, the only papers that credibly estimate the impact of school desegregation or *re*segregation on a set of political outcomes. Bergman (2018) uses data from California, where minorities could volunteer to be bused to predominantly white school districts. Since the demand for busing exceeded the number of available spots, a lottery determined which minority students would be able to study in a majority-white school. To estimate the long-run effect of busing on a number of detailed educational outcomes Bergman (2018) compares those who were bused to those who were not due to the lottery outcome. He also estimates causal effects for voter registration and turnout. Compared to Bergman (2018), the proposed project differs in a number of ways. First, Bergman’s estimates refer to an 8–18 year time period, while we intend study impacts more than 40 years later. Second, we will not only examine voter registration and turnout but also partisanship and ideology. In fact, our strongest preliminary results are on partisanship and to some degree ideology (see Section 4). Third, Bergman focuses on the impact of busing on minority students, whereas we intend to estimate effects for both whites and blacks.² Fourth, and most importantly, we will also examine the effect of busing on social preferences and racial attitudes.

Billings et al. (2020) 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 *re*segregation increased Republican voter registration in early adulthood. Our preliminary results below support this result. Yet, our project differs from the work of Billings et al. in several ways. First their estimates refer to a 17 year time period, while we examine impacts more than 40 years later. Second, we study the introduction of busing, which, at the time, was much more controversial than when Charlotte-Mecklenburg public schools abandoned it in the early 2000s. Our natural experiment comes, therefore, with an *a priori* greater chance of backlash. Third and most importantly, we aim to expand on our preliminary analysis of partisanship by directly studying racial preferences and attitudes toward redistribution, which Billings et al. do not.³

To the best of our knowledge, our paper is the first to study the long-run individual-level

²Due to reasons of statistical power our preliminary results below are for whites only.

³In 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 results.

impact of court-ordered school desegregation on political preferences and participation. We, therefore, directly contribute to a burgeoning literature that examines 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; Coppock and Green 2016; Gerber et al. 2010; Kaplan and Mukand 2018; Kaplan, Saltiel and Urzúa 2019; Madestam et al. 2013; Mullainathan and Washington 2009). Our work is also closely related to Allport’s (1954) contact hypothesis, according to which personal interactions can, under appropriate conditions, reduce prejudice between majority and minority group members.⁴ While school desegregation undoubtedly led to greater interpersonal contact between white and black children, given the forced nature of the intervention that we study, it is *a priori* unclear whether Allport’s scope conditions were met. In fact, the Louisville busing plan sparked intense opposition from conservative whites, some of whom were motivated by racial animus (see K’Meyer 2013). It is, therefore, also conceivable that the court-ordered desegregation program might have backfired by hardening rather than changing the pre-existing attitudes of whites.

Three closely related papers are Boisjoly et al. (2006), Carrell et al. (2019), and Rao (2019). Boisjoly et al. rely on the random assignment of roommates at a public university to provide evidence that cross-race interactions increase whites’ support for affirmative action. Carrell et al. find that white US 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. This effect is driven by white students’ exposure to black students from the top two terciles of the high school performance distribution. Rao exploits the imposition of a quota in Delhi private schools to show that having poor classmates makes wealthy students more pro-social and less discriminatory. Although our results are broadly consistent with previous findings on cross-race and cross-class exposure, there are at least two important distinctions relative to the 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, we present long-run estimates—more than four decades after the intervention. Our work is, therefore, able to demonstrate persistence of effects.

The remainder of this paper proceeds as follows. In Section 2, we provide background information on the Louisville–Jefferson County school desegregation plan. Section 3 discusses the data and explains our empirical approach. In Section 4, we present our main results,

⁴For a recent review of the literature on the contact hypothesis, see Paluck et al. (2019). Briefly summarizing, Paluck et al. note that the impact of intergroup contact tends to vary, “with interventions directed at ethnic or racial prejudice generating substantially weaker effects.”

followed by a discussion of plausible channels in Section 5. In the last section, we outline how we intend to expand and improve upon the preliminary results in this draft.

2. Historical Background

The U.S. Supreme Court’s landmark decision in *Brown v. Board of Education* (1954) declared racial segregation of public schools unconstitutional. Although the Court overturned its long-standing “separate but equal” doctrine, the justices did not establish any mechanisms for actively integrating schools. 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.⁵

Like many other metropolitan areas, in the early 1970s, Jefferson County, KY experienced high levels of residential segregation. As can be seen in Figure 1, most of the 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.⁶ Drawing on the seventeen-year-old precedent set in *Brown*, the lawsuits

⁵In what follows, we borrow heavily from the historical account of Sedler (2007).

⁶The 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.

alleged that the reality of segregation in both districts violated the equal protection clause of the 14th Amendment.

The Louisville and Jefferson County cases were consolidated and tried before U.S. District Judge James F. Gordon of the Western District of Kentucky, who initially ruled that the districts were in constitutional compliance. He further indicated that a federal court could not order cross-district busing. Although the judge’s reasoning seemingly conformed to the Supreme Court’s eventual verdict in *Miliken v. Bradley*, and despite the fact that the Supreme Court remanded the Louisville case for further consideration in light of this newly established precedent, the 6th Circuit Court of Appeals decided that school-district lines between Louisville and Jefferson County *could* be crossed to achieve desegregation.⁷ As a result of this surprising ruling in December of 1974, both school districts had to be merged and a desegregation plan needed to 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 order 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 6th Circuit Court of Appeals decided that full desegregation had to be implemented by the beginning of the upcoming school year, which was less than two months away. To meet this deadline, Judge Gordon ordered the KCHR’s expert on desegregation as well as staff members from the Jefferson County and Louisville school districts to work with him on a comprehensive plan that would (i) bus students of a given race equally—to the degree possible—and (ii) keep the number of students attending a particular school roughly constant over time.

Under the desegregation plan that was eventually adopted, each school in the newly merged district would have a black enrollment share between 12% and 40%—relative overall African-American enrollment of approximately 20%. Schools that already met the target were exempt from busing.⁸ 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).

Figure 2 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

⁷The 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.

⁸This was the case for 16 elementary schools and 12 secondary schools, all of which were formerly part of the Louisville district.

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.

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 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 white 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).

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. Today, nearly nine in ten Jefferson County parents say that the district’s guidelines should “ensure that students learn with students from different races and economic backgrounds” (Semuels 2015).

3. Data and Empirical Approach

3.1. *Data and Descriptive Statistics*

Our analysis relies on digitized yearbooks for fifteen out of the nineteen high schools that were subject to the court-mandated desegregation plan (see Appendix Table A.1 for a list).⁹ Since parents might have disenrolled their children from public school in response to the court order, we primarily draw on yearbooks from the 1974/75 academic year, when it was still uncertain if and when the area’s schools would be integrated. From each of these books, undergraduate research assistants manually recorded every student’s first and last name, as well as her current grade level, based on which we impute an approximate year of birth. Relying on students’ yearbook pictures and the best judgment of our coders, we also collect information on race and gender. We then contracted with Aristotle Inc. to match our

⁹Eighteen of these schools had a nontrivial enrollment of whites. Eventually, we hope to expand our sample by collecting yearbook data for *all* affected secondary schools.

yearbook data to the universe of registered voters in the United States.

Aristotle Inc. is a non-partisan, for-profit data vendor that maintains high-quality databases with information on registered voters and political donors. Using proprietary algorithms, the company collects and integrates data from different administrative and commercial sources, such as local election boards and Secretaries of State, the Federal Election Commission (FEC), mortgage and real estate records, magazine subscriptions, and marketing mailing lists. It sells these data to campaigns, political action committees (PACs), and advocacy groups. Important for our purposes, Aristotle’s data contain information on party registration, turnout history, and donations to various political groups.

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 the nearly 8,900 white males who attended one of the fifteen high schools in our sample in the year *prior* to the enactment of the desegregation plan. As shown in Table 1, for almost 70% of these individuals, Aristotle is able to locate at least one voter registration record with a nearly identical name and the same likely year of birth.¹⁰ The fact that about 30% 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).

In the case of multiple matches between an individual in our yearbooks and Aristotle’s voter file, 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. Provided that one, and only one, of Aristotle’s matched records has the same date of birth—or, at least, the same year and month of birth—we retain this record and discard all other potential matches for a particular name. If a student can be matched to multiple birth certificates and there are multiple corresponding voter registration records, then we either retain the one that matches a birth in Jefferson County or none. We say that an individual in our yearbook data can be uniquely matched to a voter registration record if Aristotle 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. Overall, nearly 57% of white males in our data can be uniquely matched in this fashion (cf. Table 1).

Table 1 further shows that match rates differ considerably across cohorts of students. We

¹⁰We allow for minor differences in the spelling of first and last names due to the potential for transcription errors. In the same vein, we allow for a margin of error of ± 1 with respect to year of birth.

can, therefore, not rule out that the sample of individuals for whom we observe outcomes forty years later varies depending on when someone graduated from high school. In the analysis below, we deal with this issue by always controlling for cohort fixed effects.

Interestingly, we also see small differences in match rates by busing assignment. Given that rising seniors were initially exempt and because the 1974 cohort graduated just before the enactment of the court-ordered segregation plan, only students from the 1976 and 1977 cohorts were actually assigned to attend a formerly black school—and among those only the ones whose last names started with certain letters. Among the 1976 and 1977 cohorts, about 54.4% of students who were assigned to be bused can be uniquely matched to a voter registration record, compared to approximately 52.2% for those who were not.

To rule out that our matching procedure works better for certain groups of last names than others, which, in turn, may produce a (spurious) correlation between match rates and students' busing assignment, 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 ($p = 0.114$).

Table 2 presents descriptive statistics for the set of uniquely matched students, on which we base our subsequent analysis. Among these, nearly 49% 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 percentage points higher among individuals who graduated in 1974—just before the enactment of the desegregation plan—than among subsequent cohorts. This observation is consistent with anecdotal accounts according to which the court-ordered desegregation of schools accelerated white flight. In Section 5, we return to white flight as a potential mechanism behind our main results.

Table 2 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. We further see the familiar pattern of lower turnout in midterm than in presidential elections, with overall participation rates that are broadly comparable to national averages.¹¹ The remaining variables at the bottom of Table 2 are based on donation

¹¹Note, the numbers in Table 2 condition on voter registration, which explains why they exceed commonly

records. For instance, roughly one percent of the individuals in our data have donated to either advocacy groups or candidates that actively support the pro-life movement. Given that political donations are relatively rare, we think of these variables as revealed-preference measures of strong ideological views.

3.2. 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 3, among students in the 1976 graduating cohort, only children in alphabet group “A, B, F, Q” were bused to formerly black schools. Are, more than forty years later, the preferences of these individuals different than that of their classmates whose last names started with other letters? And are whichever differences do exist between alphabet groups larger or smaller than in previous cohorts, which were not subject to busing?

To systematically average all relevant comparisons, we estimate the following difference-in-differences model:

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

where $Y_{i,a,c}$ denotes the outcome of interest for individual i , who is part of graduating cohort c and whose last name puts her in alphabet group a . $\text{Assigned Busing}_{a,c}$ is an indicator variable equal to one if, and only if, alphabet group a of cohort c was ever bused, while μ_a and δ_c stand for alphabet-group and cohort fixed effects, respectively. By including these, the specification in eq. (1) controls for any systematic difference that may exist between cohorts and alphabet groups. As in a standard difference-in-differences design, the key identifying assumption is that mean differences between alphabet groups would have remained constant across consecutive cohorts had it not been for busing.

Unlike most difference-in-differences designs, however, even if this assumption holds, the estimates below need not be unbiased. Matching records across data sets often involves errors. 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 untreated individuals who are matched to someone that was, in fact, assigned to attend a formerly black school—though probably not many. As a consequence, our data likely contain measurement error, which makes it more difficult to detect an effect of busing by biasing our estimates toward zero.

cited turnout figures.

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 non-compliers who selected out of the Jefferson County public school system and instead attended a local private school or another public school somewhere else, ITT is unlikely to correspond to the average treatment effect (ATE).¹² If one is willing to assume 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.

Another important interpretative issue is that, in our data, only the graduating class of 1974 was entirely unaffected by the court-ordered desegregation plan. Among later cohorts, even whites who were not put on buses saw an influx of black peers into their schools. According to statistics that the newly merged school district submitted to the Office of Civil Rights, in 1976, black enrollment in the area’s high schools ranged from 13.9% to 28.2%. Thus, although the district did not completely equalize the share of African-American students across schools, it significantly reduced previous disparities. Nonetheless, white children who were bused did not only end up attending schools with an, on average, slightly higher share of black classmates but also more African-American staff. In addition, 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 inner-city schools rather than a slightly less diverse and more distinctly suburban one.

4. Main Results

4.1. Turnout

Recall, our goal is to estimate the long-run impact of busing on political participation and preferences. We begin by studying the former, as proxied by voter turnout.

Table 4 presents our findings. The results therein correspond to $\hat{\beta}$ in eq. (1), estimated on a panel with one observation per person and election year. Columns (1)–(2) consider turnout in all general elections since 2008, while columns (3)–(4) and (5)–(6) focus on presidential and midterm elections, respectively.

All in all, we find no evidence to conclude that busing affects voter turnout more than forty years later. Although our estimates are too imprecise to confidently rule out a meaningfully large impact, we note that three out of six coefficients are, in fact, negative, while the

¹²Given 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.

positive ones are not particularly large. Even when we analyze turnout in each election year separately, we can never reject the null hypothesis of no effect.

4.2. *Partisan Identification*

We now turn to the impact of busing on party registration. Partisanship is a useful measure of political preferences for at least two reasons. First, information on party affiliation from voter registration lists tend to be high quality, provided that a state chooses to collect it. In many states, parties can and do prohibit out-partisans from participating in their primaries. Some states even impose such restrictions themselves. As a result, Kentucky and twenty-nine other states elicit information on party affiliation directly at the point of registration.¹³ Second, in the contemporary U.S., party preferences are highly correlated with individuals’ views on many economic, social, and racial issues (see, e.g., Levendusky 2009). We, therefore, think of partisanship as a convenient summary measure of students’ ideology forty years later.

Table 5 explores the impact of busing on party preferences. Columns (1)–(3) consider Republican affiliation as outcome, while columns (4)–(6) use an indicator for whether an individual registered as Democrat instead. The remaining three columns pertain to being currently registered as neither. Since state of residence is potentially endogenous, we present results in two ways. Within each set of regressions, the first two columns include observations from all states, regardless of whether the state asks voters to declare a party affiliation on its voter registration form. In these specifications, voters who live in states that do not are coded as “neither.” The last column for each outcome restricts attention to states that do collect information on partisanship. These results are easier to interpret but come from a self-selected sample.

Irrespective of which sample we use, our findings suggest that busing exerts a negative effect on Republican registration. With point estimates ranging from -3.4 to -5.9 percentage points, the estimated impact is very large—more than 10% of the mean. Conversely, we find a positive and about equally large effect on registering as a Democrat. Taken together, the evidence suggests that busing had a significant and persistent impact on the party preferences of whites, making affected students, on average, more likely to identify as Democrats.

Since it has become common in the political economy literature to compare effect sizes according to the implied persuasion rate, we follow DellaVigna and Kaplan (2007) and calculate

$$(2) \quad f_p = \frac{\hat{\beta}}{1 - \tilde{y}_p},$$

¹³The following states do not: AL, AR, GA, HI, ID, IL, IN, MI, MN, MO, MS, MT, ND, SC, TN, TX, VA, VT, WA, and WI.

where $\hat{\beta}$ indicates the relevant point estimate from Table 5, and $1 - \tilde{y}$ denotes the fraction of potentially persuadable individuals, i.e., students who do not already identify with party p . Given that different studies use different dependent and independent variables, and in light of the fact that the share of individuals who are susceptible to changing their behavior as a result of being treated varies from one setting to the next, it is useful to scale effect sizes in this fashion to make them more comparable.

In our setting, we calculate f_p by relying on the point estimates in columns (3) and (6) of Table 5, and we take $1 - \tilde{y}$ to be the share of out-partisans and independents among the untreated alphabet groups in the 1976 and 1977 graduating cohorts. Doing so yields a long-run persuasion rate of $f_D = 0.101$ for the impact of busing on identifying as a Democrat and $f_R = -0.083$ for that of identifying as Republican. Remarkably, the long-run persuasion rates in our setting are about equally large as the short-run ones in the survey of DellaVigna and Gentzkow (2010).

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.

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.

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 3. There are a total of 30 possible combinations between the six alphabet and grade level groups based on which white children were assigned busing (cf. Figure 2). Holding fixed the judge’s decision that rising seniors should be exempt and relying on the specification in columns (3) and (6) of Table 5, Figure 3 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.

4.3. *Intensity of Treatment*

Not all white children in our sample were bused for two years. According to Judge Gordon’s alphabet plan, no one was bused among the 1974 and 1975 graduating cohorts, and everyone in the class of 1976 with last names starting with A, B, F, or Q was assigned busing for two years. In the 1977 cohort, however, children in the “A, B, F, Q” alphabet group received one year of busing, while their peers in the “D, E, N, W, Z” group received two. In Table 6, we explore whether the effect of busing increases with the treatment intensity, i.e., the length of exposure.

Taking the evidence at face value, the answer appears to be “yes.” While there is still no consistent evidence of an impact on voter turnout, we generally see larger effects on party affiliation for students who were bused for two rather than one year; and in all four specifications can we statistically reject the null hypothesis of equal effect sizes

5. Potential Mechanisms

We now discuss likely mechanisms behind the impact of busing on political preferences. In light of the results in Tuttle (2019), which suggest that the Louisville busing plan had, at most, very small effects on the economic outcomes of whites, we focus on white flight and changes in attitudes as two potential explanations.

5.1. *Changes in Values and Attitudes*

In our view, the most likely explanation for the results above is that attending a formerly black, inner-city school changed white students' attitudes and ideological outlook. Consistent with the contact hypothesis, one of K'Meyer's (2013, pp. 94) white interview subjects recalls that there was "truly a sense of community" at her new, formerly black school. "They took care of another, and when I joined their school, even though I looked different, I became a member of their community. They just took me in with open arms, and not just me, but I mean all of us." Another white subject explains, "there were other friends of mine from that team who were black kids, and by just meeting them and learning a little bit about their life, you learned about a part of Louisville that you'd never experience before" (p. 93).

It is important to note, however, that the contact hypothesis makes no direct predictions about individuals' political preferences and that we currently lack suitable empirical measures of racial attitudes.¹⁴ Nonetheless, given that individuals' social and political views tend to be closely related, it is plausible that being bused caused the affected individuals to become more liberal in general. The contact hypothesis is also consistent with our finding of larger effects for white students who were bused for two years rather than one.

In support of the argument that busing affected an array of social attitudes, Table 7 presents evidence of effects on different revealed-preference measures of students' ideological leanings more than forty years later. Specifically, we explore whether busing affects the probability that someone donated to politicians or advocacy groups that either explicitly support the anti-abortion movement, oppose same-sex marriage, oppose the Affordable Care Act, or actively support gun control measures. Since only about one percent of Americans are political donors, we think of these outcomes as proxies for particularly strong views on the respective issue.

While we find no evidence that busing affected whites' support for gun control, the remainder of the evidence in Table 7 suggests that it did reduce support for conservative causes, especially the pro-life and anti-same-sex marriage movement. Although our point estimates decrease after controlling for alphabet-group fixed effects, the coefficients in columns (2) and

¹⁴In future versions of this paper, we hope to remedy this shortcoming.

(4) remain economically very large and statistically significant, or at least marginally so. Broadly summarizing, the findings in Table 7 provide suggestive evidence that being bused to a formerly black, inner-city school affected white’s views on several high-profile social controversies.

5.2. *White Flight*

As explained above, all of our estimates correspond to ITT effects, and there is abundant evidence of opposition to school integration among white Jefferson County residents. Consistent with claims according to which school desegregation increased white flight, the evidence in Figure 4 suggests that the decrease in white enrollment in the newly merged school district might have accelerated after introduction of the busing plan.¹⁵

For white flight to explain our results, assignment to busing must make white students especially likely to disenroll from the public school system. It further needs to be the case that attending either private school or another (presumably mostly white) public school somewhere else causes students to become *less* conservative. While this is certainly possible, we think it unlikely.

Parents who responded to desegregation by disenrolling their child from Jefferson County public schools were probably more conservative than those who did not. Judging by the first-person accounts in K’Meyer (2013), the former were often motivated by a mixture of racial prejudice and disapproval of federal meddling in local affairs. Presumably, they sought new environments for their children that more-closely resonated with their own values. We would, therefore, expect white flight to make children weakly more conservative and, as a result, more rather than less Republican. If correct, then the ITT estimates in Tables 5–7 are lower bounds on the true size of the average treatment effect of being bused. Put differently, white flight should work against our finding that busing made students more liberal.

6. Future Directions

In this paper, we estimate the long-run impact of busing on political preferences and participation among white males. Our identification strategy exploits a court-ordered desegregation plan that introduced quasi-random variation in exposure to busing based on the first letter of individuals’ last names. Using a difference-in-differences design, we find that being assigned to be bused had little to no impact on political participation forty years later. It did, however, make the affected students more liberal-leaning.

¹⁵Note, given the end of the baby boom during the early 1960s, the raw change in white enrollment likely overstates the impact of school desegregation on public school attendance. However, even when measured as a fraction of the relevant Jefferson County birth cohort, white enrollment falls by more than ten percentage points after the 1974/75 school year.

Interestingly, they are not only more (less) likely to register as Democrats (Republicans) today, but they are also less likely to financially support different conservative causes, such as the pro-life and the anti-same-sex marriage movement. Overall, our findings are consistent with a general version of the contact hypothesis. Being exposed to a more diverse and less privileged environment appears to have had long-run effects on students' liberal attitudes and preferences.

Naturally, at this stage, our findings are still preliminary. Among many other things, we intend to collect yearbook data for additional cohorts—both before and after the introduction of Judge Gordon's busing plan. Our hope is that collecting more data will increase the precision of our estimates. In addition, collecting yearbook data on previous cohorts might provide us with enough statistical power to draw a further distinction between students who were entirely unaffected by the court-mandated desegregation plan as well as those who were passively affected, i.e., students who were not bused but still saw an influx of black peers into their mostly white suburban school. As explained above, our current estimates only distinguish between students who were assigned to be bused and everybody else. It is conceivable that being passively affected by school desegregation had long-run effects as well; and it seems important to explore this possibility.

Perhaps most importantly, we intend to survey at least a few hundred of the individuals in our data. Given that our voter registration records provide us with an address for most of them—sometimes even a phone number and email address—we would like to elicit their views on different social and economic issues. In order to be able to directly speak to the contact hypothesis, we intend to ask about racial attitudes as well.

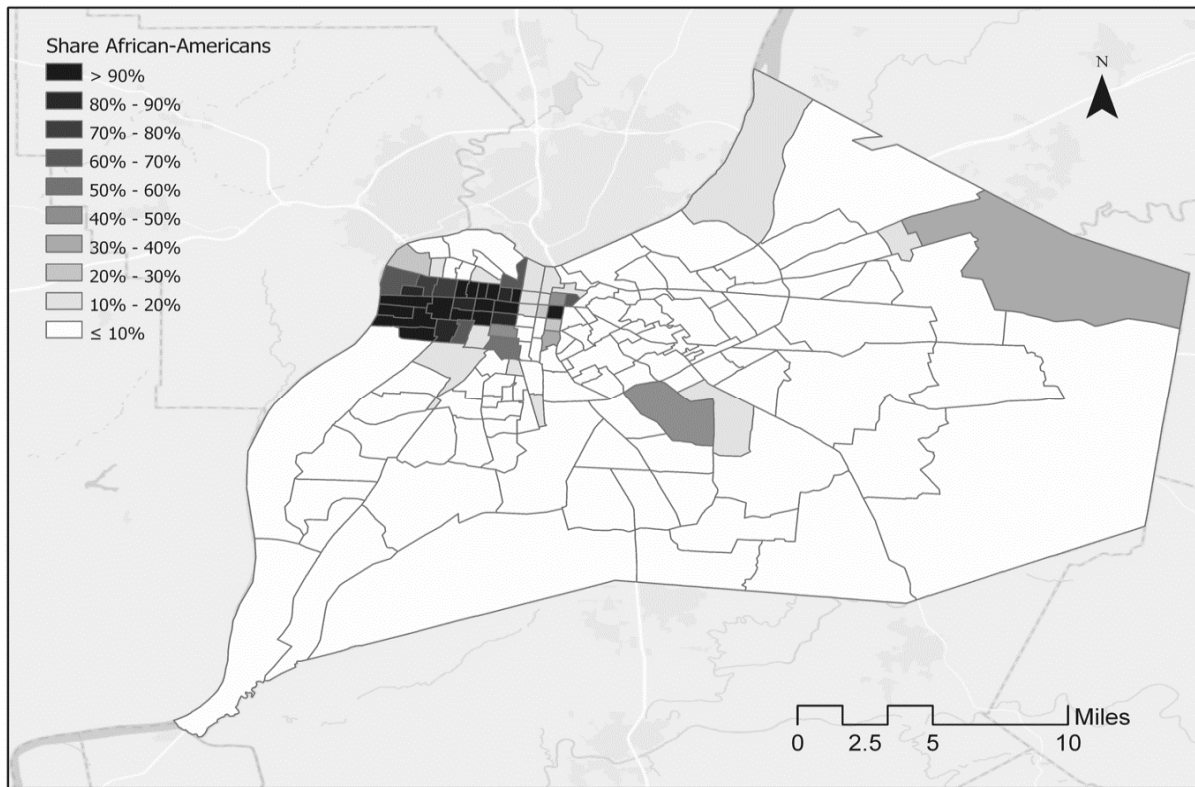
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Figure 1: Residential Segregation in Jefferson County, 1970



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

Figure 2: Busing Plan

How to tell when your child will be bused . . . unless

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

Exempted students:

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

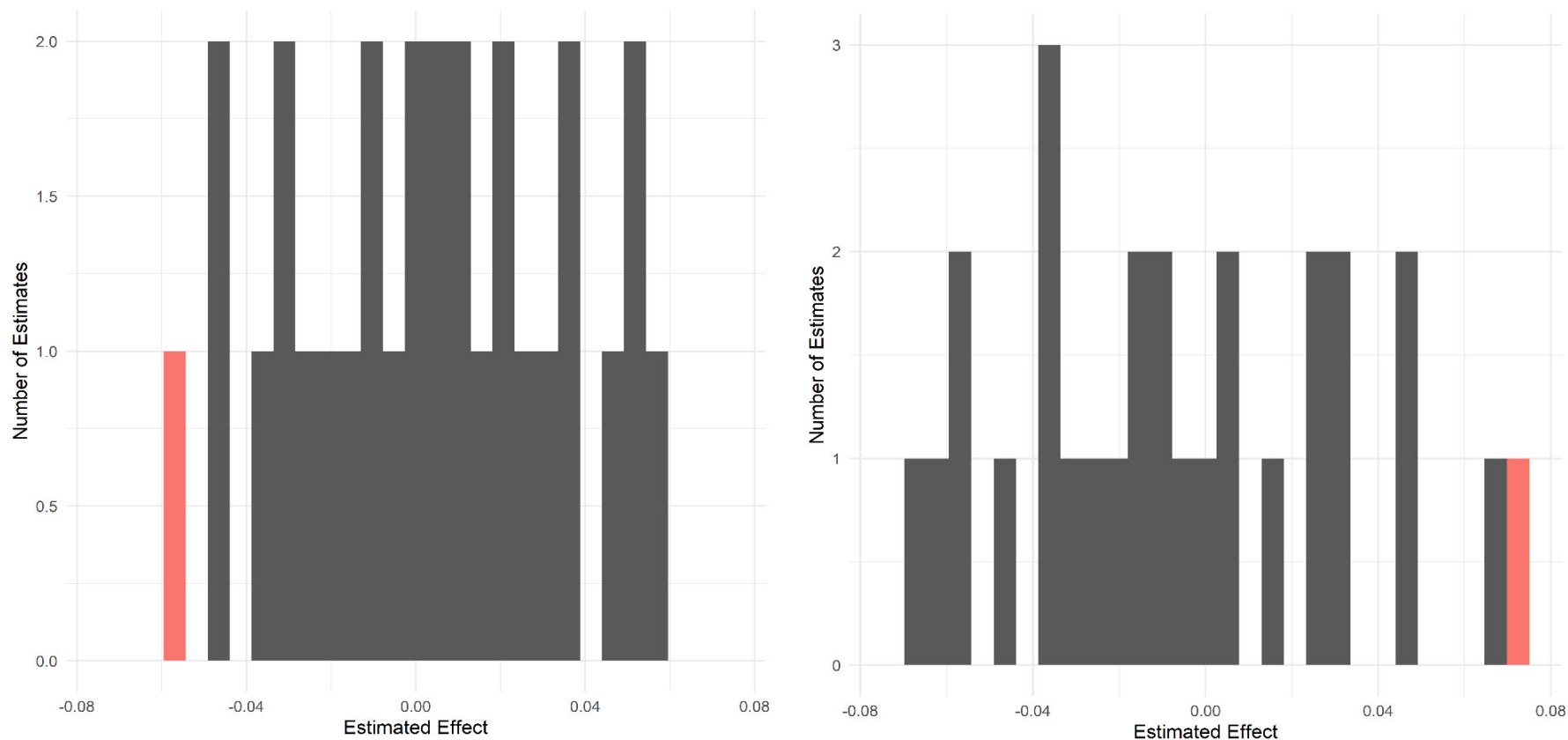
* In grade one no child will be bused during the fall quarter; after that, entire classes will be bused with their teachers on a schedule to be determined later.

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

Figure 3: Randomization Inference for the Effect of Busing on Party Affiliation

A. Republican Registration

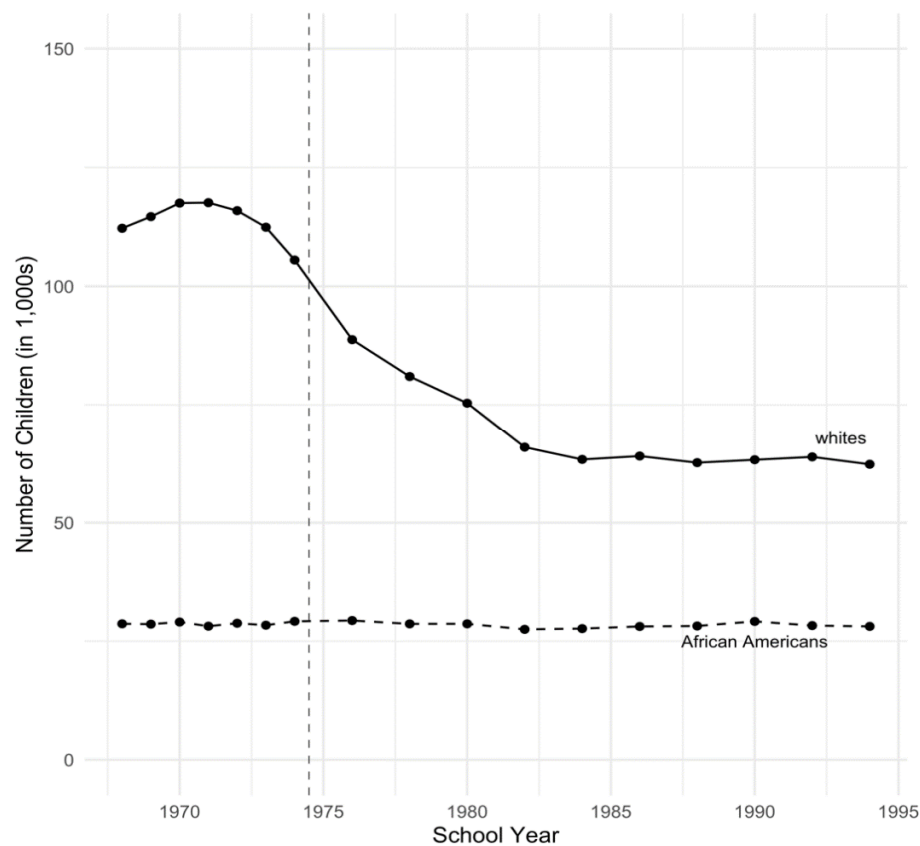
B. Democratic Registration



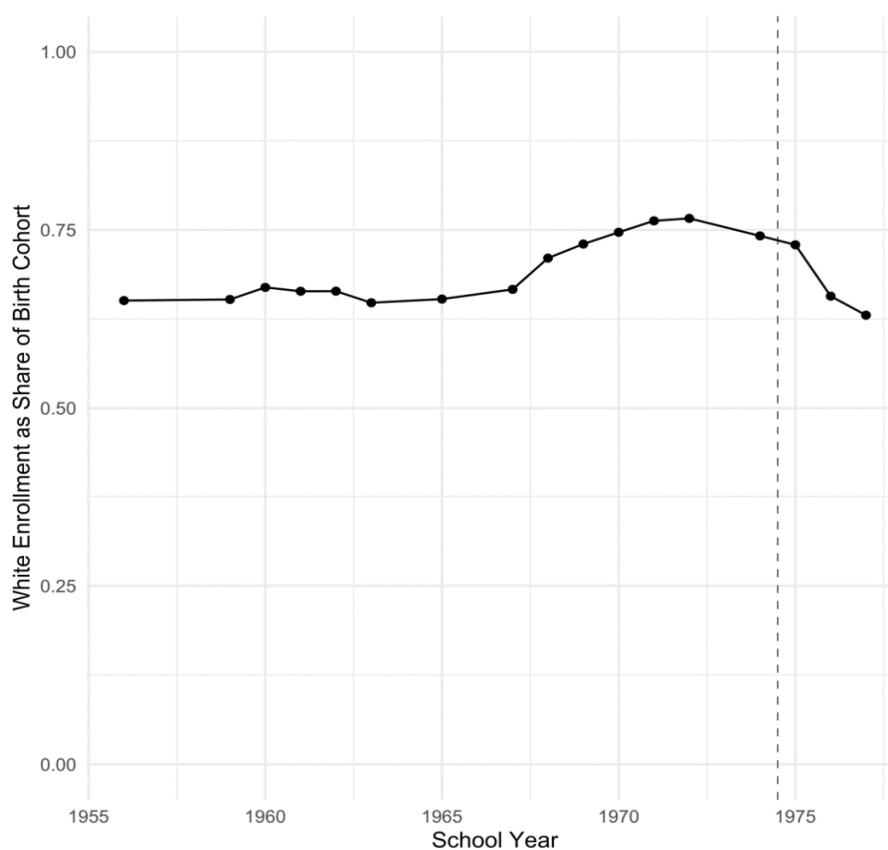
Notes: Figure shows the distribution of placebo estimates for all 30 combinations of alphabet groups and bused grade levels that induce variation in treatment among the 1976-77 and 1977-78 graduating cohorts, as explained in the main text. The red bar indicates the true estimated effect. The outcome in the panel on the left is Republican party affiliation, whereas that in the right panel is Democratic registration.

Figure 4: Enrollment in Louisville and Jefferson County Public Schools

A. Raw Enrollment



B. White Enrollment as Percentage of Birth Cohort



Notes: The left panel shows the raw number of black and white students that are enrolled in Louisville and Jefferson County public schools, based on statistics reported to the Office of Civil Rights. The right panel shows the enrollment of white students in public schools as a fraction of the white school-aged birth cohort in Jefferson County. The numbers therein are based on Cunningham et al. (1978) and Tuttle (2019).

Table 1: Match with Voter Registration Data

Cohort	Number of Students		Any Match			Unique Match		
	Not Bused (1)	Assigned Busing (2)	Not Bused (3)	Assigned Busing (4)	<i>p</i> -value (3) = (4)	Not Bused (5)	Assigned Busing (6)	<i>p</i> -value (5) = (6)
1974	1,943	0	0.831	--	--	0.715	--	--
1975	2,097	0	0.656	--	--	0.511	--	--
1976	1,909	434	0.658	0.682	0.349	0.523	0.571	0.067
1977	1,716	789	0.658	0.684	0.192	0.520	0.529	0.706
All Cohorts	7,665	1,223	0.701	0.684	0.208	0.568	0.544	0.114
	8,888		0.699			0.565		

Notes: Entries are raw numbers and means for the match between our yearbook and voter registration data, by graduating cohort. "Any match" indicates whether the vendor database contains at least one registered voter with a similar-enough first and last name and the same approximate year of birth as a particular individual in the yearbook data. "Unique Match" indicates whether we can match a particular individual to exactly one registered voter, as explained in the main text.

Table 2: Descriptive Statistics

Variable	N	Mean	SD	Min	Median	Max
<i>Treatment:</i>						
Bused	5,018	0.133	0.339	0	0	1
<i>State of Residence:</i>						
Kentucky	5,018	0.491	0.500	0	0	1
Closed Primary State	5,018	0.731	0.443	0	1	1
<i>Party Registration:</i>						
Republican	5,018	0.337	0.473	0	0	1
Democrat	5,018	0.297	0.457	0	0	1
Independent or Other	5,018	0.366	0.482	0	0	1
<i>Political Participation:</i>						
Voted in 2018	5,018	0.718	0.450	0	1	1
Voted in 2016	5,018	0.789	0.408	0	1	1
Voted in 2014	5,018	0.620	0.486	0	1	1
Voted in 2012	5,018	0.744	0.436	0	1	1
Voted in 2010	5,018	0.624	0.485	0	1	1
Voted in 2008	5,018	0.727	0.445	0	1	1
<i>Donations to Advocacy Groups:</i>						
Pro-Live	5,018	0.013	0.111	0	0	1
Anti Same-Sex Marriage	5,018	0.011	0.106	0	0	1
Anti ACA	5,018	0.011	0.103	0	0	1
Pro Gun Control	5,018	0.012	0.110	0	0	1

Notes: Table shows summary statistics for all variables used throughout our analysis. The sample is limited to individuals who could be uniquely matched to a voter registration record.

Table 3: Bused Alphabet Groups, by Cohort

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

Notes: Table 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, gray ones imply that the respective set of students was not bused.

Table 4: Turnout

	Vote in General Election		Vote in Presidential Election		Vote in Midterm Election	
	(1)	(2)	(3)	(4)	(5)	(6)
Bused	0.0144 (0.0211)	-0.0001 (0.0221)	0.0177 (0.0172)	0.0066 (0.0185)	0.0111 (0.0256)	-0.0068 (0.0271)
Fixed Effects:						
Cohort	Yes	Yes	Yes	Yes	Yes	Yes
Alphabet Group	No	Yes	No	Yes	No	Yes
Mean of Dep. Var.	0.704	0.704	0.753	0.753	0.654	0.654
Elections in Sample	'18, '16, '14, '12, '10, '08	'18, '16, '14, '12, '10, '08	'16, '12, '08	'16, '12, '08	'18, '14, '10	'18, '14, '10
R-Squared	0.007	0.007	0.006	0.006	0.007	0.008
Number of Observations	30,108	30,108	15,054	15,054	15,054	15,054

Notes: Entries are point estimates and standard errors from estimating specifications akin to that in eq. (1) by OLS. The respective outcome is shown at the top of each column, with the sample changing accordingly. Odd-numbered columns control for cohort fixed effects, while even-numbered ones also include alphabet-group fixed effects. Standard errors are clustered on the cohort-by-alphabet-group level. *, **, *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

Table 5: Party Affiliation

	Registered Republican			Registered Democrat			Neither		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Bused	-0.0337*** (0.0082)	-0.0589*** (0.0191)	-0.0548** (0.0236)	0.0191** (0.0094)	0.0368** (0.0164)	0.0726*** (0.0245)	0.0147** (0.0075)	0.0221 (0.0136)	-0.0178 (0.0180)
Fixed Effects:									
Cohort	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Alphabet Group	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
Mean of Dep. Var.	0.337	0.337	0.461	0.297	0.297	0.406	0.366	0.366	0.134
Sample	All States	All States	Affiliation States	All States	All States	Affiliation States	All States	All States	Affiliation States
R-Squared	0.001	0.002	0.022	0.001	0.002	0.042	0.002	0.002	0.100
Number of Observations	5,018	5,018	3,670	5,018	5,018	3,670	5,018	5,018	3,670

Notes: Entries are point estimates and standard errors from estimating specifications akin to that in eq. (1) by OLS. The respective outcome is shown at the top of each column. Results in the first two columns within each set of regressions are based on observations from all states, whereas the last column restricts attention to individuals living in states that collect information on party affiliation on their voter registration forms. Standard errors are clustered on the cohort-by-alphabet-group level. *, **, *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

Table 6: Does Length of Exposure Matter?

	Vote in		Registered Republican		Registered Democrat	
	General Election					
	(1)	(2)	(3)	(4)	(5)	(6)
Bused for 1 Year (β_1)	0.0203 (0.0182)	0.0325 (0.0207)	-0.0269 (0.0232)	-0.0173 (0.0271)	0.0031 (0.0168)	0.0207 (0.0266)
Bused for 2 Years (β_2)	-0.0149 (0.0318)	-0.0248 (0.0282)	-0.0821*** (0.0185)	-0.0821*** (0.0176)	0.0613*** (0.0104)	0.1102*** (0.0166)
Hypothesis Tests [p -value]:						
$\beta_1 = \beta_2 = 0$	0.465	0.147	0.000	0.000	0.000	0.000
$\beta_1 = \beta_2$	0.324	0.073	0.062	0.036	0.000	0.001
Fixed Effects:						
Cohort	Yes	Yes	Yes	Yes	Yes	Yes
Alphabet Group	Yes	Yes	Yes	Yes	Yes	Yes
Mean of Dep. Var.	0.704	0.717	0.337	0.461	0.297	0.406
Sample	All States	Affiliation States	All States	Affiliation States	All States	Affiliation States
R-Squared	0.007	0.009	0.003	0.002	0.002	0.004
Number of Observations	30,108	22,020	5,018	3,670	5,018	3,670

Notes: Entries are point estimates and standard errors from estimating difference-in-differences specifications that differentiate between being bused for only one year or two. The respective outcome is shown at the top of each column. Standard errors are clustered on the cohort-by-alphabet-group level. *, **, *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

Table 7: Revealed-Preference Measures of Ideology

	Support Pro-Life Movement		Oppose Same-Sex Marriage		Oppose ACA		Support Gun Control	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Years Bused	-0.0075*** (0.0012)	-0.0030 (0.0018)	-0.0054*** (0.0015)	-0.0038** (0.0019)	-0.0057*** (0.0010)	-0.0003 (0.0016)	0.0008 (0.0011)	-0.0006 (0.0043)
Fixed Effects:								
Cohort	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Alphabet Group	No	Yes	No	Yes	No	Yes	No	Yes
Mean of Dep. Var.	0.013	0.013	0.011	0.011	0.011	0.011	0.012	0.012
R-Squared	0.002	0.002	0.001	0.001	0.001	0.002	0.000	0.001
Number of Observations	5,018	5,018	5,018	5,018	5,018	5,018	5,018	5,018

Notes: Entries are point estimates and standard errors from estimating specifications akin to that in eq. (1) by OLS. The respective outcome is shown at the top of each column. Odd-numbered columns control for cohort fixed effects, while even-numbered ones also include alphabet-group fixed effects. Standard errors are clustered on the cohort-by-alphabet-group level. *, **, *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.