

# Online Appendix

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## Appendix A: Additional Results and Robustness Checks

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

**Appendix Table A.1** details the years of yearbook records and commencement records that we collected by school.

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

**Appendix Table A.3** explores the robustness of our main result with respect to alternative matching restrictions. In addition to including students who we could match to a unique voter registration record (cf. Appendix B), the upper panel of this table also includes individuals from yearbook records that are matched to multiple voter registration records. For the latter set of observations, the outcome is coded as the mean of the respective variables in the matched voter records. The outcome in the lower panel also includes individuals that we were not able to match to any voter registration record. Outcomes for these observations are coded as zero.

**Appendix Table A.4** explores the intensive margin effect of busing assignment for one year versus two years. It shows the effect of busing is similar for both one-year and two-year assignments.

**Appendix Table A.5** probes the robustness of our main results to the exclusion of alphabet-group fixed effects. It also shows robustness to including state-of-residence and school fixed effects.

Given potential concerns about improper aggregation of heterogeneous treatment effects in two-way fixed effects models (see, e.g., Goodman-Bacon 2021), **Appendix Table A.6** shows results for the effect of busing on partisanship based on two alternative difference-in-differences estimators that do not suffer from this problem. The upper panel reports estimated average effects based on the approach of Callaway and Sant’Anna (2021), while the lower panel employs the stacked estimator of Cengiz et al. (2019). Both estimators produce results that are qualitatively equivalent to those in Table 5.

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

**Appendix Tables A.8 and A.9** report difference-in-differences estimates of the effect of busing on answers to the individual questions on our survey. All outcomes are normalized by

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

**Appendix Table A.10** presents numerical results for the effect of busing on each of our survey indices (upper panel) as well as each question pertaining to potential mechanisms. The entries in this table, therefore, complement Figures 4 and 5 in the main text.

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

**Appendix Table A.12** studies compliance with busing assignments based on survey-takers’ answers’ to an open-ended question asking them to list the high schools that they had attended (see the survey instrument in Appendix Figure A.4). The reported coefficients correspond to the estimated effect of being assigned busing on four different outcomes: (i) listing at least one “city high school,” i.e., a high school that was previously part of the Louisville Independent School District; (ii) listing at least one private school; (iii) listing at least one public high school outside of the newly-merged JCPS system; and (iv) listing *only* high schools that were formerly operated by the Jefferson Board of Education.

**Appendix Table A.13** studies compliance with busing assignments based on matches of students between yearbooks from the 1974/75 (pre-busing) and 1975/76 (post-busing) school years. The reported coefficients correspond to the estimated effect of being assigned busing on four different outcomes: (i) whether we can locate the individual in *any* 1975/76 yearbook; (ii) whether the student appears in a yearbook of a city school, i.e., a school that was previously part of the Louisville Independent School District; (iii) whether the student appears in a county-school yearbook, i.e., a yearbook from a school that, prior to the merger, was part of the Jefferson County school system; (iv) whether the student appears in a yearbook for a school that had been exempted from the court-ordered busing plan. The sample in the first four columns consists of all students who appear in a JCPS yearbook from the 1974/75 school year and who, based on their grade level, should also appear in a yearbook for the

following year. The sample in the last three columns is restricted to students who we can match across yearbooks.

**Appendix Table A.14** investigates compliance based on matched commencement records. That is, we attempt to match students from pre-busing yearbooks to post-busing commencement lists. The reported coefficients correspond to the estimated effect of being assigned busing on four different outcomes: (i) whether we can locate the student on *any* post-busing commencement list; (ii) whether the student appears on a commencement list of a city school, i.e., a school that was previously part of the Louisville Independent School District; (iii) whether a student appears on a county-school list, i.e., a commencement list from a school that, prior to the merger, was part of the Jefferson County school system; and (iv) whether a student appears in a commencement list of a school that had been exempted from the court-ordered busing plan. The sample in the first four columns consists of all students who appear in a pre-busing JCPS yearbook and who, if they remain enrolled in a JCPS high school, we should be able to match to an entry in one of the post-busing commencement lists that we collected. The sample in the last three columns is restricted to matched students only.

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

**Appendix Figure A.2** shows the average share of African Americans among students in JCPS high schools. In doing so, it distinguishes between “city” and “county” schools. Prior to desegregation, city schools were part of the Louisville Independent School District, whereas county schools belonged to the Jefferson County school system. The data for this figure come from reports by the Office of Civil Rights at the Department of Education.

**Appendix Figure A.3** present additional randomization inference results based on the procedure described in the main text. The outcome is voter registration as “independent,” i.e., neither Democrat nor Republican.

## Appendix B: Matching Procedure

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

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

these records for mistakes and uncovered issues with transcription and data missingness. We, therefore, conducted a complete audit of the MTurk records and corrected mistakes as needed.

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

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

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

Next, we reduce the combined yearbook file to a file that contains each student only once. First, we drop all records from post-desegregation yearbooks (school years from 1975-76 onward). Then, we construct an approximate year of birth for each record by taking the year of the yearbook, which corresponds to the spring semester of the academic year, and subtracting the student’s grade plus six. We use this year of birth to identify duplicates based on school, gender, race, name, and year of birth. Among these duplicates, we keep the one from the latest pre-desegregation yearbook. Next, we identify duplicates based on first name, last name, name suffix, gender, race, and year of birth. Among these duplicates, we

keep the record with non-missing middle initial if it is a record from 12th grade, since middle initials are more commonly included for seniors. If multiple records include a middle initial, we keep the record that contains a full middle name if the record is from 12th grade. Next, we identify duplicates based on first name, middle name/initial, last name, gender, race and year of birth, and we keep the records with non-missing name suffix. Finally, we identify individuals who have the same full name, gender, race and year of birth but are recorded in different schools. If the individuals are in consecutive yearbooks and only one grade apart, we keep the record from the later grade. We remove all remaining duplicates based on name, race, gender, and year of birth, if they cannot be reduced to a single student record via the process above.

The resulting file includes one record per individual student (uniquely identified based on name, race, gender, and year of birth) based only on pre-desegregation yearbooks. This is the file that we merge to nationwide voter registration records from L2, Inc.

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

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

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

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

birth year variable – 1).

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

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

### B.3. *Filtering via Birth Records*

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

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

1. If only one match from the match set matches to the birth records, we keep that match.
2. If multiple matches from the match set match to the birth records, but only one matches to a Jefferson County birth, we keep that match.
3. If multiple matches from the match set match to the birth records, but only one matches to a birth from a county that borders Jefferson County, we keep that match.
4. If multiple matches from the match set match to the birth records, but only one has

an exact middle initial match with the middle initial from L2, we keep that match.

5. If multiple matches from the match set match to the birth records, but only one has an exact middle initial match with the middle initial from the yearbook, we keep that match.

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

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

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

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

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

#### B.4. *Adjustments Based on Survey*

The linked yearbook-voter records sent to NORC for the purposes of fielding the survey were based on an earlier version of the merge. As a result, a small number of individuals in the NORC survey sample are matched to different L2 records. Since the survey respondents explicitly report attending schools in Jefferson County, we take the match in that sample as given and we use it to update our latest version of the yearbook-L2 merge. To do this, we link our survey records with the latest yearbook-L2 merge and replace the L2 information from the latest merge with the L2 information from the survey-era merge. This adjustment only affects a handful of records.

#### B.5. *Final Analysis File*

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

As reported in Table A2, we end up with at least one match to the L2 voter file for 32,568 white, male yearbook records from non-exempt schools. Of those, 18,541 that have a unique match. Of the 18,541 unique matches from non-exempt schools, over half (56.0%) are unique even prior to our filtering via birth records. Another quarter of the unique matches (24.9%) are designated unique at the first step of the birth record filtering. In other words, one quarter of our unique matches were part of a multi-match set among which only one matched to a Kentucky birth record based on first name, last name, and date of birth. Approximately 10% of our unique matches are designated unique at the second step of the birth record filtering.

These matches are designated unique because they linked to only one Jefferson County birth based on first name, last name, and date of birth. This covers almost 90% of our unique matches. The last 10% are designated unique at other stages in the birth record filtering. Our main results on party affiliation are robust to using only those records that are unique without filtering and/or are designated unique based on an exact date of birth match with Kentucky birth records.

## Appendix C: Jefferson County Busing Survey

### C.1. Data Collection

We provided NORC with a sample of 2,400 matched yearbook-L2 records, of whom half had been assigned treatment. NORC then mailed potential respondents a recruitment packet that included a letter stating the purpose of the study, a recruitment appeal with a unique study link, as well as a toll-free telephone number to call and complete the survey by phone with a NORC telephone interviewer (see Appendix Figure A.4). The packet included a pre-paid \$5 incentive with the contingent incentive offer of \$30 for completing the survey. Non-responders to the initial appeal were mailed a reminder postcard that again included the survey URL, inbound phone number and the contingent offer of \$30 for the completed survey. Next, non-responders were contacted by email, requesting they complete the survey. A final mailing included a reminder letter with a self-administered paper questionnaire and a postage-paid return envelope. Finally, NORC telephone interviewers outbound dialed all non-responders with a matched phone number to complete the survey. For a copy of our survey instrument, see Appendix Figure A.4.

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

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

### C.2. Data Processing

NORC prepared a fully labeled data file, applying standard cleaning rules to web-mode survey data for quality control. In particular, NORC removed survey responses with response patterns that were indicative of speeding or skipping. Speeding was determined as completing the instrument in less than 1/3rd of the median interview length. In total, 1 survey response

was removed for speeding. Respondents who skipped 50% or more of the survey were removed for skipping. In total, 2 additional survey responses were removed for skipping. In addition, 3 respondents who completed the survey but indicated that they did not, in fact, attend high school in Jefferson County were also removed from the data file. In total, NORC removed 6 survey responses because of these data quality checks.

### C.3. *Weighting*

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

## **Appendix D: Noncompliance and Bounds on Treatment Effects**

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

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

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

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

In addition, let  $R_i(1)$  and  $R_i(0)$  denote the response of  $i$  to his treatment assignment, i.e., the kind of school that  $i$  would attend if assigned to the treatment or control group, respectively.

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

**School if Assigned to Control Group**

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

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

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

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

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

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

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

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

Our fourth assumption is a monotonicity condition on potential outcomes.

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

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

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

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

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

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

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

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

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

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

where the second equality follows from Assumption 2 (which implies that, for any type,  $\bar{Y}_G(z, r) = \bar{Y}_G(1 -$

$z, r) \equiv \bar{Y}_G(r)$ , while the third equality follows from  $R_{NT}(0) = R_{NT}(1)$  and  $R_{AA}(0) = R_{AA}(1)$ . Similarly,

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

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

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

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

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

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

In our setting, Assumption 4 requires that being bused to a formerly black school in the inner-city and leaving the JCPS system have opposite effects on individual attitudes and beliefs, and that the sign of these effects is the same for all students. This is a strong, but, in our view, *prima facie* reasonable assumption. We note, however, that it could be violated if students react to diversity in different ways, or if remaining in their home schools causes especially strong (conservative) backlash relative to moving to another public school system or attending a private school.

We also note that Assumption 4 can easily be weakened by placing restrictions on type frequencies or by introducing comparisons of effect sizes across different types. For instance, the claim that the ITT bounds the magnitude of the LATE continues to go through provided that  $\tilde{\beta}_{TA}$  is not “too large” relative to  $\tilde{\beta}_C$ . More specifically, it would continue to hold if the

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

*average* effect of leaving the JCPS system among treatment-avoiders is either of the opposite sign or, if not, weakly smaller than the *average* impact of attending an inner-city school among compliers, i.e., if either  $\text{sign}(\tilde{\beta}_{TA}) \neq \text{sign}(\tilde{\beta}_C)$  or  $|\tilde{\beta}_{TA}| \leq |\tilde{\beta}_C|$ . For example, this might be the case if conservative families are especially likely to engage in “white flight” and if children from such families are less inclined to change their views of the world, irrespective of the direction in which they update.

## Appendix Tables

Appendix Table A.1: Commencement and Yearbook Records

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

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

Appendix Table A.2: Sample Construction

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

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

Appendix Table A.3: Effect of Busing on Party Affiliation, Alternative Matching Restrictions

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

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

Appendix Table A.4: Intensive Margin Effect of Busing on Party Affiliation

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

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

Appendix Table A.5: Effect of Busing on Party Registration, Inclusion of Various Fixed Effects

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

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

Appendix Table A.6: Effect of Busing on Party Affiliation, Robustness to D-i-D Estimator

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

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

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

	(1)	(2)	(3)
	All Elections	Presidential Elections	Midterm Elections
<b>Callaway-Sant'anna</b>			
Assigned Busing ( <i>Cohort Average</i> )	1.44 (1.08)	1.04 (1.12)	1.83 (1.13)
Mean of Dep. Var.	70.52	74.69	66.34
Observations	18,541	18,541	18,541
<b>Stacked Difference-in-Differences</b>			
Assigned Busing	0.30 (1.04)	0.94 (0.91)	-0.34 (1.23)
Mean of Dep. Var.	70.52	74.69	66.34
R-squared	0.014	0.010	0.016
Observations	37,255	37,255	37,255
Year x Stack FEs	Yes	Yes	Yes
Alphabet-Group x Stack FEs	Yes	Yes	Yes

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

Appendix Table A.8: Survey Results: Index Components and Other Outcomes

Outcome	Coef.	SE	<i>p</i> -value	<i>q</i> -value	Mean	SD
Panel A. Voting and Partisanship						
Wanted Biden Win, 2020	0.229	0.066	0.002	0.013	0.360	0.480
Wanted Obama Win, 2012	0.202	0.085	0.024	0.080	0.502	0.500
Democrat	0.058	0.078	0.458	0.585	0.244	0.430
Independent	-0.030	0.051	0.554	0.585	0.290	0.454
Republican	-0.028	0.063	0.659	0.605	0.466	0.499
Ideology (1=Very Cons., 7=Very Lib.)	0.266	0.148	0.082	0.160	0.044	1.004
Voted in 2020	-0.019	0.031	0.546	0.585	0.905	0.293
Panel B. Belief in a Just World Index						
People Get What Entitled To	-0.464	0.144	0.003	0.010	-0.023	0.991
Efforts are Rewarded	-0.323	0.130	0.019	0.022	-0.003	0.986
Earn Rewards and Punishments	-0.150	0.131	0.263	0.152	-0.026	1.016
Misfortune is Brought On Self	0.092	0.107	0.396	0.205	0.019	1.024
People Get What They Deserve	-0.356	0.127	0.009	0.016	-0.037	1.003
Rewards and Punishments are Fair	-0.185	0.125	0.149	0.098	-0.009	1.007
World is a Fair Place	-0.384	0.104	0.001	0.007	-0.045	0.996
Panel C. Conservative Racial Attitudes Index						
Blacks Should Work Way Up Without Favors	0.215	0.100	0.040	0.565	0.015	0.996
Black Issues due to Slavery and Discrimination	0.047	0.099	0.636	1.000	-0.004	0.994
Blacks Gotten Less Than They Deserve	-0.076	0.114	0.511	1.000	0.006	0.997
Racial Disparities due to Individual Effort	-0.130	0.142	0.367	1.000	-0.008	1.027
Civil War Over States' Rights	-0.042	0.148	0.778	1.000	-0.029	0.995
Police Racial Bias	0.054	0.148	0.716	1.000	0.036	1.030
US Should Not Apologize for Slavery	0.027	0.110	0.809	1.000	0.009	0.987
African Americans Too Quick to Claim Racism	0.190	0.137	0.176	1.000	0.020	1.004
Discourage Marrying an African American Partner	0.042	0.119	0.729	1.000	-0.000	0.996

*(continued on next page)*

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

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

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

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

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

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

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

Appendix Table A.11: Effect of Busing on Zip-Code Income

	Total Income Per Return		Total Income Per Person	
	(1)	(2)	(3)	(4)
Assigned Busing	-1,555 (1,467)	-1,621 (1,693)	-932 (865)	-1,001 (1,012)
Mean of Dep. Var.	78,182	78,182	42,201	42,201
SD of Dep. Var.	49,175	49,175	27,679	27,679
R-squared	0.004	0.005	0.005	0.005
Observations	18,538	18,538	18,538	18,538
Cohort FEs	Yes	Yes	Yes	Yes
Alphabet-Group FEs	Yes	Yes	Yes	Yes
Alphabet-Group Linear Trend	No	Yes	No	Yes

*Notes:* Entries are point estimates and standard errors from estimating the difference-in-differences model in eq. (1). The outcomes are based on zip-code level total income derived from 2019 tax returns, as published by the Internal Revenue Service. Columns (1) and (2) scale the outcome per return in the zip-code while columns (3) and (4) scale the outcome per person in the zip-code. Columns (1) and (3) include cohort and alphabet-group fixed effects, while columns (2) and (4) add alphabet-group-specific linear trends. Standard errors are clustered at the cohort-by-alphabet-group level. \*\*\*, \*\* and \* denote statistical significance at the 1%, 5%, and 10% levels, respectively.

Appendix Table A.12: Compliance: NORC Survey

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

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

Appendix Table A.13: Compliance: Yearbook-to-Yearbook Matching

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

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

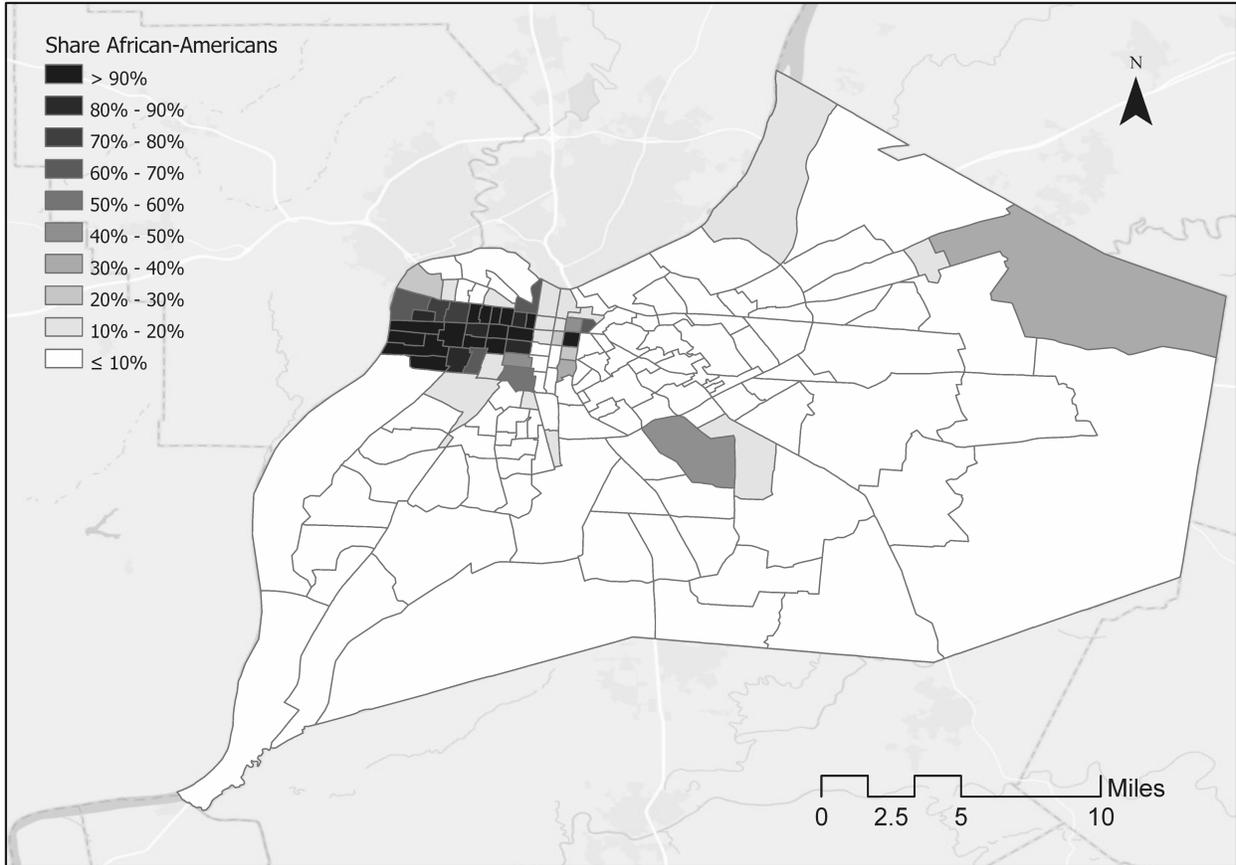
Appendix Table A.14: Compliance: Yearbook-to-Commencement-List Matching

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

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

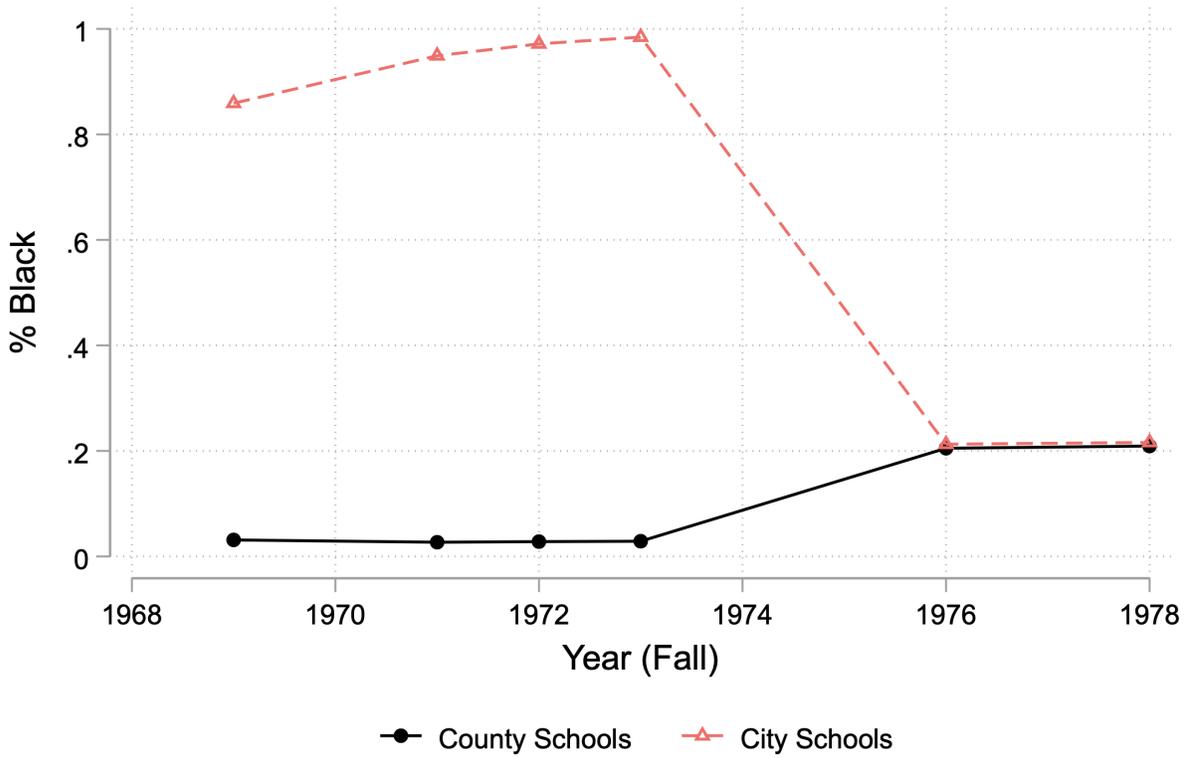
## Appendix Figures

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



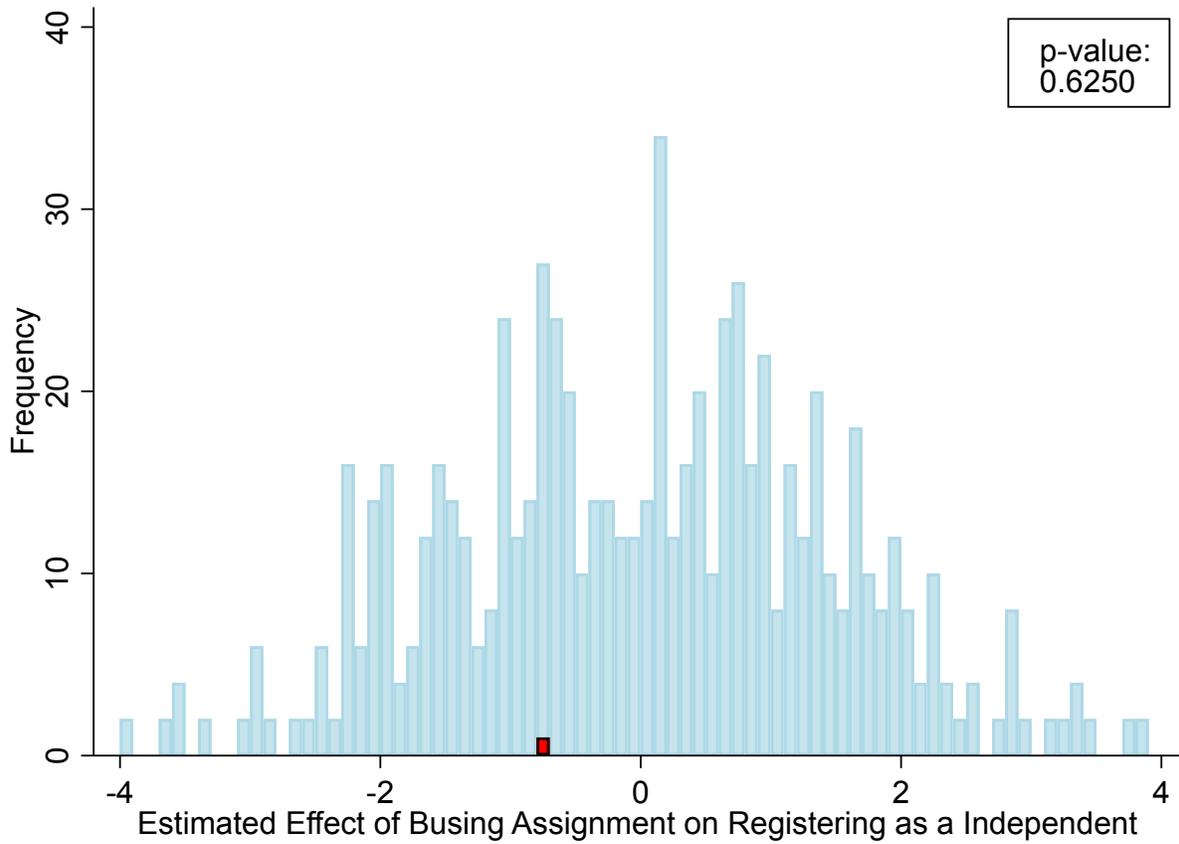
*Notes:* Figure shows the share of African-Americans as of the 1970 Decennial Census for each census tract in Jefferson County, KY

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



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

Appendix Figure A.3: Randomization Inference Results: No Party Affiliation



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



# JEFFERSON COUNTY STUDY



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

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

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

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





## INSTRUCTIONS

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

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

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

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

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

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

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

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

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

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

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

- Quite a few
- Not many
- Hardly any

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

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

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

- Donald Trump
- Joe Biden

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

- Mitt Romney
- Barack Obama

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

- Too little
- Too much
- About the right amount

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

- Yes
- Maybe
- No

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

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

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

- Pro-choice
- Pro-life
- Neither

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

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

**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



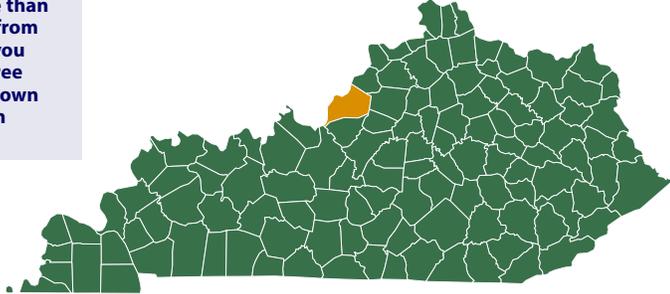
**21. Do you agree, disagree, or neither agree nor disagree with each of the following statements?**

	Strongly agree	Somewhat agree	Neither agree nor disagree	Somewhat disagree	Strongly disagree
<b>a.</b> Irish, Italians, Jewish and many other minorities overcame prejudice and worked their way up. Blacks should do the same without any special favors.	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
<b>b.</b> Generations of slavery and discrimination have created conditions that make it difficult for blacks to work their way out of the lower class.	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
<b>c.</b> Over the past few years, blacks have gotten less than they deserve.	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
<b>d.</b> 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.	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>



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.	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
b. The U.S. government should officially apologize for slavery.	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
c. African Americans are too quick to claim that innocent remarks or behavior are signs of racism.	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>

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.	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
b. My teachers helped me better understand other people from different parts of American society.	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
c. I was impressed with my teachers.	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
d. A substantial fraction of my teachers were of a different race than I.	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>

**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.	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
b. My friends helped me better understand other people from different parts of American society.	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
c. A substantial fraction of my friends came from poor families.	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
d. A substantial fraction of my friends were of a different race than I.	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>

**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.	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
b. Attending high school in a different part of town helped me better understand other people from different parts of American society.	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
c. A substantial fraction of students came from poor families.	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
d. A substantial fraction of students were of a different race than I.	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>





## 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](mailto: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**