The Long-run Economic Effects of School Desegregation*

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Abstract

I estimate the effect of school desegregation on long-run economic outcomes by studying a natural experiment in Jefferson County, KY. In 1975, the district, under a court order, developed a unique busing assignment plan to merge the majority-white County district and the majority-black City district. Under this plan, students were assigned to be bused to new schools (versus stay at their home school and have new students bused in) based on their race and the first letter of their last name. Using this plausibly conditional random assignment and confidential data from the US Census Bureau, I find black students assigned busing to former County schools live in better neighborhoods (e.g. neighborhoods with higher tract-level income) at adulthood than black students assigned to remain in former City schools. This effect is strongest for students bused in earlier grades and is increasing in the total number of years a student is assigned busing. Busing assignment has small to zero effect on white students. I explore the implications of white disenrollment from the district (i.e. "white flight") by using a novel dataset of archival yearbook records. I find the effect for white students remains small even after preliminary accounting for disenrollment. These results suggest that school desegregation in this setting had positive long-run effects for black students by giving them access to better schools (e.g. schools with more capital investment, more credentialed teachers, lower drop-out rates, etc.).

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I. Introduction

In Brown v. Board of Education, the Supreme Court ruled de jure segregation of schools unconstitutional because "separate educational facilities are inherently unequal." That decision in 1954 ultimately set off a wave of desegregation plans over the next 30 years, many of which were court-ordered due to resistance from local school districts.[1] In terms of integrating schools, these plans were successful–by 1988, about 44% of black children were attending majority white schools. In the early 1990s, however, the Supreme Court issued three decisions which led to the dissolution of many court-ordered desegregation plans. Currently, only 23% of black children are attending majority white schools, a level of segregation not seen in the United States since 1968 (Orfield et al. 2014).[2]

Numerous studies document beneficial short term effects of school desegregation (e.g. Guryan 2004; Reber 2010; Johnson 2015; Bergman 2018). Recent work finds harmful short-run effects of re-segregation (Lutz 2011; Billings, Deming, and Rockoff 2014; Cook 2016). However, there is little evidence about the long term effects of either on final educational attainment, earnings, or neighborhood quality in adulthood (aside from Johnson 2015). Furthermore, the existing literature has primarily produced estimates of the **net** effect of desegregation by studying district-level changes induced by court orders (Guryan 2004; Reber 2010; Lutz 2011; Johnson 2015). Within-cohort evaluations of desegregation are particularly scant, making it difficult to understand the mechanisms through which school desegregation has positive effects. That is, are the positive effects due to changing peers, changing resources, or something else?

In this paper, I use within-cohort variation in busing assignment from a unique desegregation plan in Jefferson County, KY to estimate the long-run economic effect of busing. In 1975, the primarily white Jefferson County Public Schools (JCPS, "County" schools) district was ordered to integrate with the primarily black Louisville City Schools (LCS, "City" schools) district. To fix language, I will refer to the merged district as JCPS, "the merged district", or "the district", and I will refer to schools in the merged district that were in LCS prior to the merger as "former City"

[1] As detailed below, court-ordered desegregation primarily followed the Civil Rights Act in 1964 and two additional cases–Green v. Kent in 1968 and Swann v. Charlotte-Mecklenburg in 1971. Brown v. Board and Brown II laid foundation for these later decisions, but did not, themselves, induce wide-scale school integration.

[2] Caetano and Maheshri (2017) find that demographic shocks explain only 60% of this change. Lutz (2011) provides causal evidence that the dissolution of a desegregation court order for a district increases segregation in that district.

schools and schools in the merged district that were in JCPS prior to the merger as "former County" schools. The merged district is the union of the former County and the former City schools.

To achieve the target level of integration in each school, the merged district followed a busing plan designed by the federal district court judge. White students from minority-black, former County schools were taken by bus from their home school to a former City school, and black students from minority-white, former City schools were taken by bus from their home school to a former County school. Not all students were assigned busing in the same grades. For example, some white students were bused in 5th grade and 10th grade while others were bused in 3rd and 8th. Likewise, some black students were bused in 2nd grade through 9th grade while others were bused in 4th grade through 12th grade. The grades in which a student was assigned busing were based on the first initial of the student's last name and their race.

This conditionally random assignment procedure creates a series of natural experiments allowing me to study the causal effect of busing assignment among students within the same graduating cohort. I start by estimating the intent-to-treat effect of busing assignment relative to no busing assignment. This is possible because black students in 10th grade in 1975, for example, who have an assignment of busing in 2nd through 9th grade are not bused because they have completed those grades when busing begins. On the other hand, black students in 10th grade in 1975 who have an assignment of busing in 4th through 12th grade are bused because they have not completed 10th through 12th grade when busing begins. Furthermore, the busing assignment scheme induces random variation in the number of years assigned to busing and variation in the age at which a student is first assigned busing.[3]

To measure the long-run outcomes of students affected by this busing plan, I link confidential data from the Social Security Administration's Numident file on place of birth with confidential data from the 2000 Decennial Census and a special extract from the 2000 Decennial containing each individual's "alphabet group." I analyze characteristics of the individual's neighborhood in adulthood to study outcomes for all Census respondents as opposed to the random sample

[3]The intensive margin variation in busing assignment is, however, correlated with the age at which a student is first assigned busing, the year in which a student is first assigned busing, whether the busing assignment is disrupted (i.e. the student is assigned busing in one grade, assigned home school in a later grade, and then assigned busing again in yet a later grade), and other busing plan components.

surveyed for questions related to income.[4]

Using the linked data and variation in busing assignment induced by first letter of last name, I find that black students assigned busing to former County schools (formerly majority-white schools) live in higher quality neighborhoods as adults than black students assigned to remain in former City schools (formerly majority-black schools). This intent-to-treat effect increases in the number of years the student is assigned busing, and it is strongest for students assigned busing at earlier ages. By comparison, I find small to zero intent-to-treat effects of busing assignment on long-run neighborhood outcomes for white students. I find qualitatively similar effects on individual earnings, educational attainment, and employment using the random sample of respondents in the long-form Census.

These differences in outcomes emerge despite the fact that former City and County schools are both equally integrated after 1975. Historical data from the Office of Civil Rights show that former City schools were approximately 25.9% black from 1976-1982 while former County schools were approximately 23.5% black.[5] Despite the roughly equal integration, anecdotal accounts and empirical evidence suggest the schools were not equal with respect to staffing, facilities, neighborhood environment, other resources, and short-run student outcomes (e.g. dropout rates). Specifically, former City schools were worse along all these margins even after the district merger. Bused and not bused students were exposed to similar racial integration, but ultimately, attended different schools with different resources. This suggests the long-run effects of busing assignment in this setting are due to improved school resources and not simply an effect of peer race.

I then explore the treatment effect of busing take-up, moving beyond the intent-to-treat estimation discussed above. Estimating the effect of busing take-up versus remaining in the home school, however, is more complicated. It is possible that students do not comply with their busing assignment and instead drop out of school, move to another district, or transfer to a private school. In the publicly available 5% sample of the 1980 Decennial Census, 66.3% of white children aged 6-17 in Jefferson County are attending a public school. Over 93.3% of black children in Jefferson County are attending a public school in 1980. To produce a student-level measure

[4] In a recent paper about the long-run economic effects of Food Stamps, Bailey, Hoynes, Rossin-Slater, and Walker (2019) also analyze the characteristics of respondents' neighborhoods to leverage the full short-form Census data.

[5] Historical data from the Office of Civil Rights were collected, digitized, and provided by Ben Denckla and Sarah Reber here: https://web.archive.org/web/20150109135107/http://l1.ccpr.ucla.edu/OCR/ocr.htm

of compliance, I use a novel dataset of archival yearbook and commencement program records from nearly twenty high schools in Jefferson County pre- and post-desegregation. Currently, I use this measure and another measure of compliance to scale the intent-to-treat estimates and estimate how large the effects might be after accounting for non-compliance. Ultimately, I will link this student-level measure of compliance directly to the 2000 Decennial Census and estimate the treatment effect of busing using an instrumental variables (IV) approach.

If non-compliance occurs equally for students assigned to busing and not assigned to busing, the IV regression will recover a local average treatment effect for those students who take up busing due to their assignment. However, the exclusion restriction is violated if assignment to busing affects non-compliance because in that case, busing assignment will affect long-run outcomes through a channel (e.g. drop-out or private school) other than actual busing. I use the yearbook and commencement program records to investigate this empirically. I find that compliance does not differ by busing assignment for black students, but that white students assigned to busing are less likely to comply with their assignment than white students assigned to remain in their home school. In Section IV, I discuss the implications of this and how it biases the estimate of the local average treatment effect for white students.

The results discussed above are based on within-cohort comparisons for cohorts in which all students are exposed to integration. Even so, I estimate similar effects to prior studies that focus on the net effect of desegregation. Johnson (2015) conducts a comprehensive study of the long-run net effect of desegregation. Using nationwide variation in desegregation court orders and data from the Panel Study of Income Dynamics, he finds that each year of exposure to school desegregation increases adult wage by 3.6 (se $= 0.019$) percent for black students. I estimate that assignment to busing increases the average income of a black student's neighborhood in adulthood by 3.4 (se $= 0.016$) percent. The effect I estimate in a setting where racial composition of schools is held constant is similar in magnitude and is not statistically different from the effect Johnson (2015) estimates in a setting where both school resources and racial composition vary. The contexts and outcomes are admittedly different, but this is suggestive evidence that the net effect of desegregation is driven by changes in school resources as opposed to changes in the racial composition of schools. I also test this directly in Jefferson County by comparing students graduating before versus after desegregation for those assigned versus not

assigned busing, but these results have not yet been approved for release.

This paper contributes to the literature on the effects of desegregation and re-segregation on student outcomes in two ways. First, I estimate long-run effects of school desegregation. Only one other paper estimates long-run effects and does so with a focus on district-level changes (Johnson 2015). Second, I estimate within-cohort effects of a school desegregation plan. Since district-level changes in segregation yield both dramatic changes in school resources and in racial integration, it is not possible to determine which change (or how much each change) affects outcomes. In Jefferson County, every student is exposed to racial integration after 1975, but there is still within-cohort variation in busing/school assignment. I isolate school resource effects by using this within-cohort variation in a setting where racial integration is held roughly constant. Other work using within-cohort variation in assignment focuses on re-segregating districts (Billings, Deming, and Rockoff 2014) or cross-district assignment lotteries (Angrist and Lang 2004; Cook 2016; Bergman 2018).

Ultimately, these results have important implications for the re-segregation of schools in the U.S.[6] I show that desegregation had long-run positive effects on economic outcomes of black students with no evidence of a strong negative effect for white students. Specifically, I find that busing leads black students to live in better neighborhoods many years later. In addition, these results shed light on the current labor market situation of black and white individuals and potential intergenerational effects of desegregation. Recent research finds that neighborhood quality has long-run effects on intergenerational mobility, suggesting that desegregation may be an important channel for improving mobility of black children.

This paper also provides suggestive evidence that the gains from desegregation are primarily due to school resource effects with peer effects playing a smaller role. While this highlights the importance of equalizing school resources for black and white students, it also suggests that merging the funding of two disparate districts is not, by itself, a sufficient remedy to educational

[6]These results also have implications for school desegregation in Jefferson County, KY. Jefferson County is one of the largest school districts in the U.S., and it has been the focus of multiple efforts to dismantle its current approach to desegregating schools, which still relies on busing students away from their neighborhood schools. For example, the Supreme Court ruled in 2007 that districts could not use race as the sole determinant for student assignment to schools (Meredith v. Jefferson County Board of Education; Parents Involved in Community Schools v. Seattle School District No. 1). And in 2017, Kentucky's state legislature took up a bill that would allow students across the state to attend the school nearest to their home (Emma Brown, "GOP bill could dismantle one of nation's most robust school desegregation efforts," The Washington Post, March 4, 2017, accessed July 15, 2019).

inequalities. Even after the districts merged, the former County schools produced better long-run outcomes for black students. This is likely the result of several lasting differences in the schools discussed in Section II.

II. Institutional Details

A. Brief History of School Desegregation in the US

The Supreme Court ruled de jure segregation of public schools unconstitutional in 1954 (Brown v. Board of Education), and the Court handed enforcement of desegregation to district courts in 1955 (Brown II). Despite this enormous shift in policy, little changed in practice. School districts adopted "freedom of choice" plans and allowed voluntary transfers that technically complied with the law but limited its effectiveness (Cascio et al. 2008). In addition, existing residential segregation and white migration or "white flight" to suburban districts also diluted the impact of Brown and Brown II.

The 1964 Civil Rights Act (CRA) and the 1965 Elementary and Secondary Education Act (ESEA) made federal funding conditional on compliance with Brown, and as a result, school districts, especially those at risk of losing large grants, desegregated "just enough" to meet federal guidelines (Cascio et al. 2010). The CRA changed the legal environment in other ways, making it possible for the U.S. Attorney General to bring suits for plaintiffs in segregated local school districts (Johnson 2015). In 1968, the Supreme Court ruled in Green v. County School Board of New Kent County that the county's "freedom of choice" plan did not eliminate the "dual system" of separate black and white schools, and mandated the district adopt a new plan that would achieve actual integration. Finally, Swann v. Charlotte-Mecklenburg Board of Education in 1971 established that mandatory busing plans were a constitutional solution to desegregate districts that were segregated as a result of residential segregation. Of the 108 court-ordered desegregation plans documented by Welch and Light (1987), 106 were ordered after 1964, 101 were ordered after 1968, and 57 were ordered after 1971. Guryan (2004) and Reber (2005) show that these court orders increased integration, even in the presence of white flight.

The CRA, ESEA, and two critical Supreme Court decisions accelerated the process of school desegregation in the US. This process was somewhat stifled by a 1974 decision in Milliken v.

Bradley that clarified schools could not be forced to desegregate across district lines unless it could be shown that the district lines were drawn with racist intentions. The Court's decision in this case meant white migration out of a desegregating district would prevent full integration because the district they migrated to could not be forced to integrate with the district they migrated from. In the early 1990s, three Supreme Court cases effectively ended court-ordered desegregation in the US. Lutz (2011) shows that when a district is released from their court order, school segregation and black dropout rates increase.

B. Busing and Desegregation in Jefferson County, KY

Like many cities in the U.S., Louisville, KY (and Jefferson County, KY) has a long history of residential and school segregation. The city charter in 1828 established public schools for white children, and in 1870, a charter established separate public schools for black children (JCPS 2019). In 1941, the Louisville City Schools (LCS) district had 57 white schools and 19 black schools. Shortly after the decision in Brown, LCS desegregated by re-drawing school attendance zones and allowing open enrollment in the high schools, subject to capacity constraints. However, the district gave students attending majority other-race schools under this plan the option to transfer to a majority same-race school. Teachers were integrated three years later in 1959. The transfer option and white migration to the Jefferson County Public Schools (JCPS) district curtailed full integration (K'Meyer 2013).

In 1972, the Kentucky Civil Liberties Union (KCLU), the local branch of National Association for the Advancement of Colored People (NAACP), and the Kentucky Commission on Human Rights (KCHR) filed a lawsuit asking the court to merge LCS, JCPS, and the small district of Anchorage to achieve de facto integration (K'Meyer 2013). Judge James Gordon initially rejected this proposal, but the Sixth Circuit Court of Appeals overturned his decision in 1973. Milliken v. Bradley put this plan in jeopardy by rejecting cross-district busing in Detroit. However, the Sixth Circuit ultimately decided the case of LCS and JCPS qualified as an exception under Milliken, and in 1975, the districts merged (K'Meyer 2013). Judge James Gordon was tasked with enforcing the desegregation order, and due to his apparent hesitation, the Sixth Circuit in July 1975 suddenly ordered he develop a plan for the school year beginning in September 1975.

Judge Gordon's desegregation plan was unique among desegregation plans in the U.S. As

was typical, it required each school consist of a certain percentage of black students (12 to 35 percent in this case). To achieve this, the plan adopted a traditional mandatory assignment that required the busing of black students from City schools that were formerly majority-black to County schools that were formerly majority-white and vice versa for white students. Students not assigned to busing would remain at their home school and have white or black students bused in. The busing assignment scheme, however, was not traditional (i.e. it was not based on zoning or grade restructuring).[7]

Under Judge Gordon's plan, students were quasi-randomly assigned to busing in a given grade based on the first initial of their last name and their race. For example, white students with the initials "A", "B", "F", or "Q" were assigned busing in 11th and 12th grade whereas white students with the initials "C", "P", "R", or "X" were assigned busing in 3rd and 8th grade. Black students were assigned busing in many more grades, but otherwise, were subject to the same initial-based assignment scheme. For example, black students with the initials "A", "B", "F", or "Q" were assigned busing in 2nd, 3rd, and 7th-12th grade whereas black students with the initials "C", "P", "R", or "X" were assigned busing in 2nd-9th grade. Figure 1 shows this assignment plan as displayed in a July 1975 issue of The Courier-Journal.[8]

This assignment procedure generates within-cohort variation on the extensive margin (whether a student is assigned to busing), the intensive margin (the number of years a student is assigned to busing), and age of intervention (how early in childhood they are assigned busing). Note that different children are affected by the extensive versus intensive margin variation. I study the effect of these margins on long-run economic outcomes for black and white students.

This assignment plan was used for ten years, with only a minor change for white students in 1982. In 1985, the district shifted from initial-based assignment to a zoning system for junior high schools and high schools (K'Meyer 2013).[9] In 1991, the school district fully eliminated the initial-based system, moving elementary schools to a zoning system (K'Meyer 2013). In this

[7]Welch and Light (1987) identifies grade restructuring as the primary method of desegregation for districts that desegregate by pairing formerly black and formerly white schools (the method used in Jefferson County, KY). They give an example of two schools that are grades K-6 but are racially segregated. A busing plan that desegregates these schools will typically convert one school to grades 1-3 and one school to grades 4-6 (leaving kindergarten unaffected). Under this type of plan, all students in a given cohort and school are treated with the same busing assignment.

[8]Figure A1c shows the potential variation in busing assignment induced by this plan for a student attending the merged district as of 1975-76.

[9]See Figures A1a and A1b for the 1982 and 1985 changes as documented in The Courier-Journal.

paper, I focus on students in graduating cohorts from 1990 or earlier, meaning they are exposed to the pre-1985 system and aged 28 or older by the time of the 2000 Decennial Census.

C. Persistent Differences in Former County and Former City Schools

Prior to the merger in 1975, per pupil spending was slightly higher in the County than in the City, with JCPS spending approximately 10% more per student than LCS in 1972 (Census of Local Government Finances). The goal of the City-County merger was racial integration and equalization of school finances/resources. Anecdotally, however, the former City schools and former County schools remained different post-1975. For one, former County schools had long been the beneficiary of higher facilities spending. Spending on construction was 94% higher in JCPS than in LCS in 1972 (Census of Local Government Finances). Since facilities improvements, and other resources like textbooks, are a stock, former City and County schools could not immediately equalize on that margin. In fact, the district indicates in the 1975-76 School Superintendents Survey that no major capital spending occurred in that school year.[10]

Anecdotally, former City schools also had less involvement from Parent Teacher Associations (PTAs) than former County schools. An interviewee from Tracy K'Meyer's 2013 book on busing in Jefferson County states, "PTA was hard to come by. They didn't want to do anything, not in the city schools. The white kids were bused two years, so the parents weren't going to do anything in these black schools. They're just going to put in their time and then they'll go and work at their home schools."

After the merger, teachers were also assigned to schools in an effort to desegregate faculty. Despite this, archival yearbooks show that staffing was not equal in former City and County schools and that differences in staffing persisted into the 1980s. Ballard High School (a former County school) had nearly 20% more teachers with masters degrees in 1980 than Central High School (a former City school). This is consistent with Jackson (2009), suggesting that teacher labor supply responses resulted in lower teacher quality in former City schools.

I use historical school-level data from the Office of Civil Rights (OCR) from 1976, 1978, 1980, and 1982 to compare school characteristics and student outcomes in former City and

[10]Cellini, Ferrara, & Rothstein (2010) and Goodman, Hurwitz, Park, & Smith (2018) both find school facilities are an important dimension of school quality.

County schools post-integration. Details on data construction are in Appendix B. Specifically, I estimate the following equation:

$$Y_{st} = \alpha + \beta FormerCity_s + X_s + Z_t + \varepsilon_{st} \quad (1)$$

where the dependent variable is a characteristic of school **s** in year **t** or a student outcome at school **s** in year **t** and $FormerCity_s$ is a dummy variable equal to one if school **s** was a City school prior to 1975 and equal to zero if school **s** was a County school prior to 1975. X_s is a set of fixed effects for the grades that are offered at school **s** (grades 1, 7, and/or 12) and Z_t is a set of year fixed effects.

First, despite the differences outlined above, former City schools and County schools have roughly equal racial composition of students post-1975. Column 1 of Table 1a shows that the percentage of black students is only about 2.3 percentage points higher at former City schools. Columns 2-4 show that gender composition and gender composition within race are also similar at former City and County schools. Column 5 uses a measure of classroom-level racial composition from the Office of Civil Rights surveys in 1976 and 1980. Specifically, the dependent variable is the standard deviation in the percent black in each classroom (of the 18 randomly surveyed classrooms from each school). Column 6 is the fraction of those classrooms in each school that had a particularly skewed racial composition (i.e. classroom percent black below 15% or above 35%). These results indicate that classrooms were also equally integrated at former City and former County schools.

Table 1b shows how former City and County schools differ in terms of student outcomes. These results are intended to be an indication of school quality, but admittedly, these outcomes are a function of many inputs, including student quality. Columns 1 and 7 show that former City schools have higher dropout rates and a higher rate of students referred to the courts for disciplinary action than former County schools. Former City schools did not have higher suspension rates; in fact, the coefficient suggests suspension rates were lower in these schools. Columns 2-3, 5-6, and 8-9 show the relevant outcomes (dropout, suspensions, court referral) by race. Former City schools perform especially poorly for black students in terms of dropouts and court referrals, yet they are also worse for white students. Since students are quasi-randomly assigned to former City and County schools, these effects can be attributed to the school as

opposed to the student body, absent any major differences in compliance between students assigned to former City versus County schools.

Table 1c explores a few measures of school resources. Column 1 shows that former City schools are less likely to have a "Gifted and Talented" program for students. Column 2 indicates that there are no major differences in terms of whether these schools offer additional honors courses or other enrichment courses. Column 3 compares the student-teacher ratio at former City and former County schools. I use the number of classrooms as a proxy for the number of teachers in 1978 and 1980 because in those years, teacher data is not available and classroom data is. There are no statistically significant differences, but the coefficient implies the ratio is slightly higher in former City schools. Column 4 finds similar results using total number of teachers as the dependent variable and controlling for total number of students.

Finally, the neighborhoods where these schools are located also differ markedly. I show this in Table 1d. Columns 1-6 use publicly available tract-level data from the 1980 Decennial Census (obtained from NHGIS). Former city schools are located in tracts with lower rates of high school completion, higher poverty, lower employment, and lower median household income (see Columns 1-4). In addition, they are also located in tracts in which the buildings are less likely to have air conditioning and are more likely to be heated using a room heater as opposed to a central heating system (see Columns 5-6).

Column 7 uses data from the CDC's 2001-2005 prediction of daily, tract-level PM2.5 pollution. Unfortunately, pollutant data is not available at the tract level in earlier years. Nevertheless, these results show that former City schools are located in areas with higher predicted PM2.5 pollution, on average, from 2001-2005. Columns 8-10 use data from the Louisville Metropolitan Police Department on zip code level crime in 2004. Again, this the earliest year in which crime data is available at sub-county geographies. These results show that former City schools are located in zip codes with higher violent, property, and drugs/other crime.[11] Recent work finds that schools' neighborhood environments are an important input in the educational production function (e.g. Ebenstein, Lavy, and Roth 2016; Heissel, Persico, and Simon 2019).

Racial composition of schools is held roughly constant post-1975. School resources, broadly defined, likely remain different at former City and County schools. Data from several different

[11] I limit the data to crimes occurring outside of summer months and in the hours from 6am-5pm to reflect the level of crime students would be potentially exposed to near school.

sources and qualitative interviews suggest that former County schools had better facilities, more investment by Parent-Teacher Associations, more program offerings (like the Gifted and Talented program), higher quality teachers, were located in better neighborhoods, and ultimately, had better short-run student outcomes. Because of this, it is reasonable to attribute any long-run differences in student outcomes to the effect of school resource differences and not an effect of school racial composition. This interpretation becomes more complicated in the presence of disenrollment responses and non-compliance. I discuss this in Section III.B.3 and Section IV.B.

III. Data and Methodology

A. Data

1. 2000 Decennial Census and the Numident

To estimate the effect of busing assignment on long-run outcomes, I link confidential data from the Social Security Administration's Numident file on place of birth with confidential data from the 2000 Decennial Census and a special extract containing each individual's alphabet group (the busing assignment group that they are in based on the first initial of their last name in the 2000 Census).[12]

The 2000 Decennial Census can be broken into two groups: short-form respondents and long-form respondents. The short-form data contain information on age, race, sex, household structure, and residence for almost all individuals in the U.S. in 2000. The long-form data contain more detailed information on income, educational attainment, and employment for a random sample of approximately 1 in 6 households. To take advantage of the full sample from the short-form, I use the long-form with sampling weights to construct the following tract-level characteristics for every person in the short-form: average income, fraction of individuals with a high school degree, fraction of individuals with a bachelors degree, and fraction of individuals

[12]The extract file is a subset of the 2000 Decennial Census that contains a unique identifier for each individual, binary variables indicating whether their last name begins with the letters: 'A,B,F,Q', 'G,H,I', 'C,P,R,X', 'M,O,T,U,V,Y', 'D,E,N,W,Z', and 'I,J,K,S' (based on the busing assignment schemes from 1975-1984), and binary variables indicating whether their last name begins with the letters: 'I,J,K,S,W,M', 'I,J,K,S,B,W', 'A,B,F,Q,H,C,O,U,V,Y,N,Z,X,E,L,R', 'G,H,L,C,P,D', 'T,D,P,G', and 'M,T,V,R,Z,X,F,A,O,U,Y,E,Q,N' (based on the busing assignment scheme from 1985-1990). I do not observe the individual's name or even the first initial of their last name. I can only access the unique identifier and these alphabet group indicator variables.

working in the last year or last week. Since women often change their last name at marriage, making matching problematic, I limit the main sample to men aged 28 and above, and as such, I calculate the tract-level statistics for men aged 28-55. Results are robust to various methods of calculating neighborhood characteristics.

Since I attach these tract-level characteristics to each individual in the short-form sample based on their reported tract, these tract-level characteristics are individual-level outcomes–they represent the quality of the neighborhood where the individual lives. I use the neighborhood quality results as the main results in this paper because they are estimated on the full short-form sample, maximizing statistical power. Results using individual income, education, and employment responses from the long-form sample are, in general, qualitatively similar. Table 2 shows summary statistics on men aged 28-55 and living in Jefferson County, KY from the publicly available sample of the 2000 Decennial Census.[13,14]

I use place of birth from the Numident as a proxy for childhood school district. This leads to some mismeasurement that will attenuate the results. Note, this measurement error differs from the issue of migration as an endogenous response to desegregation or busing assignment. For one, some students leave Jefferson County before 1975. Second, some of those students in Jefferson County at school age will be attending private school prior to 1975. Third, some white students attending public school in Jefferson County will be attending LCS prior to 1975, and some black students attending public school in Jefferson County will be attending JCPS prior to 1975. In all of these cases, these students will not actually receive a busing assignment, but by treating county of birth as childhood school district, I will still code them as receiving an assignment. Table 3 presents migration and school attendance statistics by race for school-aged children from the publicly available 5% sample of the 1980 Decennial Census and from district-level enrollment counts (see also Figures 2a-2c). In Section IV.B, I use those statistics to adjust the intent-to-treat estimates for measurement error, and the results are roughly the same.

Finally, I use year of birth, month of birth, and school entry rules (from Bedard and Dhuey 2007) to define each individual's graduating cohort.[15] For the main analysis, I focus on indi-

[13] I produce summary statistics from publicly available samples for now to minimize disclosure risk.

[14] For comparison, Table A3 shows these statistics for women aged 28-55 and living in Jefferson County, KY.

[15] Bedard and Dhuey (2007) collect detailed information on school entry rules to estimate the effect of these rules on adult earnings.

viduals in graduating cohorts from 1965-1990.[16] Those students in graduating cohorts from 1965-1974 are not exposed to the desegregation program, allowing me to include race by alphabet group controls. Students in graduating cohorts from 1975-1990 are exposed to the pre-1985 system and are at least 28 years old in the 2000 Decennial Census. To summarize, the final sample includes men born in Jefferson County, KY who are in graduating cohorts from 1965-1990.

2. Archival Yearbook Records

I supplement the data above with data on student-level enrollment collected from archival high school yearbooks and commencement programs from Jefferson County, KY pre- and post-desegregation. To my knowledge, this is one of the first economics papers to use yearbooks as a source of student-level data.[17]

Student-level enrollment data have many benefits in this setting. First, I use the enrollment data to improve the measurement of who is exposed to the desegregation plan. Second, I will link the data to birth and marriage indices obtained from the Kentucky Department of Libraries and Archives (KDLA) to measure busing assignment for eventually married women. Finally, the enrollment data allow me to observe actual take-up rather than busing assignment alone. By observing take-up, I can also evaluate how much take-up differs by race and for students assigned versus not assigned busing. Table 4a displays basic statistics about the yearbook data.

B. Methodology

1. Intent-to-Treat Estimates, Extensive and Intensive Margin Effects

For the main results in this paper, I estimate the intent-to-treat effect of assignment to busing by race. The identifying assumption is that, conditional on graduating cohort and race, busing assignment is exogenous to later-in-life outcomes. Since busing assignment is determined based on first initial of last name conditional on graduating cohort and race, this assumption

[16]Note, I use the term "graduating cohort" to refer to the year the individual would have graduated from high school if they completed school with no grade retention. I do not require individuals in the sample to complete high school or to complete without grade retention.

[17]I began collecting yearbook data under this project, but have continued it for a subsequent project that I am working on jointly with E. Kaplan and J. Spenkuch investigating the effect of busing assignment in Jefferson County on long-run political attitudes of white students.

is likely satisfied. One remaining concern is that students with certain initials perform better than students with other initials. For example, if students with initials closer to the beginning of the alphabet perform better than students with initials closer to the end, then the "A", "B", "F", "Q" group may be inherently different from the "C", "P", "R", "X" group. To account for this, I include students graduating before the desegregation plan and control for race by alphabet group fixed effects. Specifically, I estimate the following equation:

$$Outcome_{i,2000} = \alpha + \beta(BusAssign \times White)_{it} + \delta(BusAssign \times Black)_{it} + RY_{it} + RG_i + \varepsilon_{it} \quad (2)$$

where $Outcome_{i,2000}$ is the outcome variable measured in the year 2000 for individual **i**. $BusAssign_{it}$ is a dummy variable equal to one if individual **i** from graduating cohort **t** is assigned busing and equal to zero if not. $White_i$ is a dummy variable indicating individual **i** is white and $Black_i$ is a dummy variable indicating individual **i** is black. RY_{it} is a set of race by graduating cohort fixed effects, and RG_i is a set of race by alphabet group fixed effects. The outcome variables of interest for the short-form sample are: tract-level average income, fraction of people with a high school degree in the tract, fraction of people with a bachelors degree in the track, and fraction of people working last year or last week in the tract.[18,19] Again, I estimate (2) for men born in Jefferson County, KY and in graduating cohorts from 1965-1990. I find qualitatively similar results when estimating equation (2) for women who are single as of 2000.

Equation (2) estimates the extensive margin effect of busing assignment (not necessarily busing take-up). The assignment procedure yields extensive margin variation when some students in a graduating cohort are completely past their assignment grades and some students are not. For black students, there is only extensive margin variation in the first few years after 1975 and it is present only for students in high school. The busing assignment plan also yields substantial intensive margin variation. I estimate the effect of each additional year an individual is assigned busing with the following model:

[18]Again, these tract-level characteristics are based on men aged 28-55, but results are robust to using other samples.

[19]For the long-form sample, the outcomes of interest are: above/below median earnings, high school completion, bachelors degree completion, worked last year, and worked last week. I dichotimize earnings when using the smaller sample to increase statistical power.

$$Outcome_{i,2000} = \alpha + \beta(YearsAssign \times White)_{it} + \delta(YearsAssign \times Black)_{it} + RY_{it} + RG_i + \varepsilon_{it} \quad (3)$$

I also estimate a model including both the assignment dummy variable and the years of assignment for black students. This yields the effect of an additional year of assignment conditional on assignment. I do not do this for white students because there is not meaningful variation in both assignment and years of assignment. The number of years a student is assigned busing is correlated with the age at which they are first bused and the year in which they are first bused. Also, students assigned busing for more years are more likely to have a disruption in their busing schedule, meaning that they are assigned busing for some years, assigned to remain at their home school for some later years, and then assigned busing again for even later years. When interpreting the results from equation (3), it is important to remember these possible patterns and the fact that the students affected by the extensive margin variation are much older than the students affected by the intensive margin variation.

2. Intent-to-Treat Estimates, Early vs. Late Childhood Effects

Finally, the assignment plan yields variation in the age at which an individual is first assigned busing. Prior work has found that neighborhood interventions occurring in early childhood are especially effective (Chetty and Hendren 2018). To test for age-of-assignment effects, I estimate the following equation:

$$\begin{aligned} Outcome_{i,2000} = \alpha &+ \beta_1(BusAssign \times White)_{it} + \delta_1(BusAssign \times Black)_{it} \quad (4) \\ &+ \beta_2(GradeFirstAssign \times White)_{it} + \delta_2(GradeFirstAssign \times Black)_{it} + RY_{it} + RG_i + \varepsilon_{it} \end{aligned}$$

where $GradeFirstAssign_{it}$ is a linear term equal to the grade in which an individual is first assigned busing (1st-12th grade) and equal to zero if individual **i** is not assigned busing. Conditional on race by graduating cohort fixed effects, the first grade in which an individual is assigned busing is perfectly collinear with the first year in which they are assigned busing and highly correlated with the number of years they are assigned busing. I compare the results from

(4) to the results from the following equation:

$$
\begin{aligned}
Outcome_{i,2000} = \alpha &+ \beta_1(BusAssign \times White)_{it} + \delta_1(BusAssign \times Black)_{it} \\
&+ \beta_2(GradeFirstAssign \times White)_{it} + \delta_2(GradeFirstAssign \times Black)_{it} \\
&+ (R \times YearFirstAssign)_{it} + (R \times YearsAssign)_{it} + (R \times g(t))_{it} + RG_i + \varepsilon_{it}
\end{aligned} \tag{5}
$$

where $(R \times YearFirstAssign)_{it}$ is the interaction between the race dummy variables and a linear term in the first year an individual is assigned busing (centered at zero in 1974 and coded as zero for years earlier). $(R \times YearsAssign)_{it}$ is the interaction between the race dummy variables and a linear term in the number of years assigned. Finally, $(R \times g(t))_{it}$ is the interaction between the race dummy variables and a linear term in graduating cohort (coded as zero in 1965). I conduct several additional robustness tests that are described in Section IV.A.3.

3. LATE and the Exclusion Restriction

The prior section detailed the methodology for estimating intent-to-treat effects of busing assignment. In this section, I outline the methodology and assumptions necessary to estimate the effect of busing take-up. To estimate the local average treatment effect of busing take-up versus remaining in the home school, I employ a novel dataset of student enrollment records collected from archival yearbooks and commencement programs. As part of the busing plan, former City schools were paired with former County schools and students were bused within those pairs. For example, black students assigned busing from Central High School were assigned to one of seven County high schools that Central was paired with. Similarly, white students assigned busing from Ballard High School were assigned to Central High School.

In theory, I should be able to match a student from a pre-desegregation yearbook to a post-desegregation yearbook of the school (or schools) they would have been assigned to in a given year. However, school zone boundaries were partially re-drawn as part of the desegregation plan. For example, a black student in the Central High School zone may be re-drawn into the Louisville Male High School zone. If this occurs, I will not be able to find them in Central High School when they aren't assigned busing, and I will not be able to find them in one of

Central's paired schools when they are assigned busing. As such, I measure take-up as whether I can match the student from a pre-desegregation yearbook to any post-desegregation yearbook of the system (former City or former County) that they were assigned to in a given year. In other words, if a white student in 10th grade in a 1974-75 County yearbook should be assigned busing in 1975-76, then take-up is counted if I match them to a 1975-76 yearbook for any former City school. This measurement is not perfect and is a lower bound of take-up because not all schools have yearbooks available in a given year. I also calculate adjusted measures of take-up to account for the fact that some yearbooks were not available for data collection and for the fact that year-to-year matches are low even in pre-desegregation years. Table 4b presents statistics on these measures of take-up.

Currently, I use the individual-level estimates of take-up from the yearbook records and a district-level measure of take-up from Cunningham, Husk, and Johnson (1978) to scale the intent-to-treat estimates. Once the individual-level enrollment data is matched to the 2000 Decennial Census data, I will estimate the effect of busing by instrumenting for it with the student's busing assignment. If the assignment to busing versus not busing differentially affects enrollment in JCPS and if disenrollment affects outcomes differently than remaining in the home school, then the exclusion restriction is violated. I start with the assumption that assignment to busing does not influence enrollment in the district. Then, I explore the resulting biases from differential enrollment by assignment status. I estimate the following equations using two-stage least squares:

$$Bused_{it} = \alpha + \beta(BusAssign \times White)_{it} + \delta(BusAssign \times Black)_{it} + RY_{it} + RG_i + \varepsilon_{it} \tag{6}$$

$$Outcome_{i,2000} = \gamma + \phi(\widehat{Bused} \times White)_{it} + \lambda(\widehat{Bused} \times Black)_{it} + RY_{it} + RG_i + \omega_{it} \tag{7}$$

where $Bused_{it}$ is the dummy variable for busing take-up and $BusAssign_{it}$ is the dummy variable for busing assignment. Assuming busing assignment does not influence enrollment in the district, this yields the local average treatment effect for students who take up busing (due to their assignment) relative to those who remain in their home school (and have new students

bused in).

If busing assignment does affect enrollment decisions, then the exclusion restriction is likely violated. To make the problem more clear, consider this context in the always-takers/never-takers/compliers/defiers framework (Angrist, Imbens, Rubin 1996). First, there are no always-takers or defiers in this setting. In other words, a student not assigned busing cannot choose to take up busing.[20] Students are only compliers (i.e. they take up busing when assigned it and they do not take up busing when not assigned it) or they are never-takers (i.e. they do not take up busing, regardless of assignment). Assuming that no one leaves the district only when they are not assigned busing and/or no one stays in the district only when they are assigned busing, then the never-takers can be divided into two groups: the always-leavers and the sometimes-leavers. Always-leavers disenroll from the district regardless of busing assignment. Sometimes-leavers disenroll from the district only when they are assigned busing. The presence of sometimes-leavers results in a violation of the exclusion restriction.[21]

How prevalent are sometimes-leavers in this setting? First, consider the problem for black students. Private school enrollment is low for school-aged black children in Jefferson County, KY. In addition, there is no evidence that desegregation plans increase private school enrollment or migration for black students. For these reasons, the relevant disenrollment margin is dropout. Although desegregation plans decrease black dropout rates (Guryan 2004), it is possible that assignment to busing decreases dropout more or less than assignment to remain in the home school. I do not find any evidence of this. Student-level enrollment from yearbook data suggest black students assigned versus not assigned busing are equally likely to remain enrolled in JCPS. In this case, equation (7) yields the local average treatment effect of busing compared to remaining in the home school.

Now, consider the problem for white students. Private school enrollment is high for school-aged white children in Jefferson County, KY. Reber (2005) finds compelling evidence of white flight in response to desegregation plans. This suggests that movement to another district, enrollment in private school, and dropout are all relevant margins of disenrollment. In addition, I do find evidence that busing assignment affects disenrollment for white students. White stu-

[20] This implies there are no always-takers because a student can't choose to take up busing regardless of assignment. It also implies no defiers because a student can't choose to take up busing only when they aren't assigned.

[21] This is likely an issue in many instrumental variable designs where the treatment is something individuals would like to avoid and there is a special action that can be taken to avoid treatment.

dents assigned to busing are less likely to be enrolled in JCPS in the next year than white students not assigned to busing. In this case, equation (7) yields biased estimates for white students because there is selection into disenrollment (i.e. some white students are "sometimes-leavers").

The bias is a function of the effect the instrument has on the outcome, independent of its effect on busing take up, and the strength of the first stage (Conley, Hansen, and Rossi 2012). I will conduct a bounding exercise on the effect for white students. Altonji, Elder, and Taber (2005) find that Catholic school attendance increases the probability of high school graduation by 0.05 to 0.08 and the probability of college enrollment by 0.02 to 0.15. I will ultimately use those estimates, estimates of the returns to schooling, and measures of white flight from Cunningham et al. (1978) and the yearbook records to bound the effect of busing for white students.

Finally, these enrollment responses can affect the interpretation of the results in a peers versus resources framework. Although the former City school and former County schools are equally integrated, it is possible that the white students who leave when assigned to busing are different from the white students who leave when assigned to remain in their home school and have new peers bused in. If this is the case, then those black students who are assigned to remain in their home school are exposed to a different peer group than black students assigned to busing. I present some statistics comparing white students in former City and County schools and discuss this further in Section IV.D.

4. Decomposing the Net Effect

The methodology above estimates the effect of busing assignment or busing take-up for individuals within a cohort. Prior work finds that desegregation plans have a large positive net effect for black students. In other words, black students in a cohort exposed to desegregation have higher educational attainment and earnings than black students in a cohort graduating before desegregation occurs (Johnson 2015). In this section, I estimate how much of this cross-cohort difference in Jefferson County can be explained by the within-cohort assignment differences. First, I estimate:

$$Outcome_{i,2000} = \alpha + \beta(BusAssign \times White \times Exposed)_{it} + \delta(BusAssign \times Black \times Exposed)_{it} + \eta(White \times Exposed)_{it} + \zeta(Black \times Exposed)_{it} + (R \times g(t))_{it} + RG_i + \varepsilon_{it} \quad (8)$$

where $Exposed_{it}$ is equal to one if individual **i** is graduating in a year **t** after the desegregation plan in Jefferson County is implemented. η and ζ, then, identify the effect of desegregation exposure for students not assigned to busing, and β and δ identify the additional effect of exposure for students assigned to busing. Since I am estimating cross-cohort differences in one city, I cannot control for race by year fixed effects. Instead, I control for race interacted with a linear trend in the graduating cohort. I also re-estimate equation (9) with other large counties included in the sample to control for cohort effects:

$$Outcome_{i,2000} = \alpha + \beta(BusAssign \times White \times Exposed)_{it} + \delta(BusAssign \times Black \times Exposed)_{it} + \eta(White \times Exposed)_{it} + \zeta(Black \times Exposed)_{it} + RY_{it} + RG_i + \varepsilon_{it} \quad (9)$$

IV. Results

A. Long-run Effects of Busing Assignment

1. Extensive and Intensive Margin Effects

Using Numident data on place of birth and 2000 Decennial Census data on first initial of last name, I estimate the effect of busing assignment by race on long-run outcomes for men born in Jefferson County and in graduating cohorts from 1965-1990. Tables 5-7 display results for the effect of assignment on the quality of the tract where the student lives in adulthood. Tract-level characteristics are individual-level outcomes defined for the full short-form sample and are the following: tract-level average income, fraction of respondents in the tract with a high school degree, fraction with a bachelors degree, fraction who worked last year, and fraction who worked last week. These outcomes represent the quality of the neighborhood where the individual lives as an adult.

Column 1 of Table 5 shows that by adulthood, black students assigned to busing live in

neighborhoods with average incomes approximately 3.4% higher than black students not assigned to busing (i.e. assigned to remain in their home school and have students bused in). Column 2 shows the average income result in levels (as opposed to logs in column 1). Columns 3 and 4 show that busing assignment causes black students to live in neighborhoods with more high school graduates ($\beta = 0.0055$, $se = 0.0036$) and in neighborhoods with more college graduates ($\beta = 0.0173$, $se = 0.0058$). Busing assignment does not lead black students to live in neighborhoods with higher employment (see columns 5 and 6).

The effects for white students (also in Table 5) are small and in no specification, statistically different from zero. For example, the coefficient in column 1 suggests that busing assignment leads white students to live in neighborhoods with average income about 0.15% lower than white students not assigned to busing. The 90% confidence interval for the estimate in column 1 is -0.58% to 0.29%. All of the estimated results in Table 5 are attenuated because some individuals born in Jefferson County have migrated out of the county before the desegregation plan is implemented, some are enrolled in private school in Jefferson County before the plan is implemented, and some are attending a school where they are in the minority-race. In Section IV.B, I scale the estimates in Table 5 to account for this attenuation.

Tables 6 and 7 show the effect of intensive margin increases in busing assignment. In Table 6, the busing assignment dummy variable is replaced with a linear term for years assigned busing. The results show that black students assigned more years of busing live in better neighborhoods as adults. As before, the effects for white students are small and are not statistically different from zero. Table 7 estimates the extensive margin and intensive margin effects simultaneously for black students. I only do this for black students because white students are only bused zero to two years whereas black students are bused zero to nine years. Table 7 suggests that the positive effects of busing are due to increases on the intensive margin and are not simply due to an extensive margin shock of busing assignment.

2. Early vs. Late Childhood Effects

I test for age-of-assignment effects by estimating how the first grade in which a student is assigned busing affects outcomes. Table 8 shows these results. Columns 1 and 3 show that, for black students, being first assigned busing in an earlier grade does lead to better outcomes

(higher tract-level average income, higher fraction of college graduates in the tract) than first assignment in a later grade. Again, the estimates for white students are near zero.

The estimates in columns 1 and 3 are from a model that includes race by graduating cohort fixed effects because that is the level of randomization. However, when race by graduating cohort fixed effects are included, then the first grade in which a student is assigned busing is perfectly collinear with the first year in which they are assigned busing and highly correlated with their number of years assigned busing. I address this by estimating models that replace the race by cohort fixed effects with a race by cohort linear trend and include a linear term in first year assigned busing and a linear term in number of years assigned busing. These results, in columns 2 and 4, are similar to the results with race by cohort fixed effects, suggesting the results in columns 1 and 3 are due to an age of intervention effect rather than a year of intervention effect.

It is unlikely that these results are driven by differential compliance or measurement error by age. First, measurement error from defining the sample based on individuals born in Jefferson County should not differ much by age. From the 1980 Census, 86% of black children aged 6-8 and born in KY are still living in KY, and 81% of black children aged 15-17 and born in KY are still living in KY. This age profile (shown in Figure 2c) of the measurement error cannot explain the coefficient on grade first assigned busing. Similarly, enrollment in public school (shown in Figure 2b) does not appear to differ much by age. From ages 7-15, approximately 94-98% of black children in Jefferson County are attending a public school. Enrollment is lower for 6 year olds as well as 16 and 17 year olds, but the results are robust to dropping individuals first bused at these ages. These patterns also hold for white school-aged children from the 1980 Census.

3. Robustness Tests

I evaluate the robustness of the main extensive margin results by conducting placebo tests using similarly sized counties that are also under a court order for desegregation after 1968 (Welch and Light 1987). Specifically, I choose approximately 50 counties that are at least half the size of Jefferson County, KY. Then, I estimate the main extensive margin regression on each of those cities. Since the initial-based busing assignment plan is only used in Jefferson County, KY, then the "assigned busing" dummy variable should be near zero for all other cities. I conduct a similar

placebo test in which I randomly assign each student to a new alphabet group. The main results are robust to these placebo tests.

Finally, the results are also robust to excluding the race-by-alphabet group fixed effects or including race-by-alphabet group fixed effects interacted with a linear trend in graduating cohort. This provides support for the identifying assumption that busing assignment (based on alphabet group) is exogenous to later-in-life economic outcomes.

B. Accounting for Enrollment Responses

1. Instrumental Variables Results

Currently, the student-level enrollment data from yearbooks and the data from the U.S. Census Bureau are separated. In the future, I will link these two datasets. For now, I scale the estimates in Table 5 to account for the non-compliance and measurement error statistics presented in Table 3, Tables 4a-4b, and Figures 2a-2c. Table 9 shows these results.

In column 1 of Table 9, I display the main result from column 1 of Table 5. In column 2, I adjust for measurement error in busing assignment that arises from the fact that some students born in Jefferson County, KY are not living in Jefferson County, KY prior to the district merger. I quantify this error by measuring the fraction of students born in Kentucky who are still living in Kentucky as of 1980, by race. In column 3, I further adjust for measurement error that arises from the fact that some students living in Jefferson County, KY are already attending private schools prior to the district merger. I measure this for white students using the data from Cunningham, Husk, and Johnson (1978) on the fraction of school-aged birth cohort attending public schools in 1974.[22] For black students, I use the data from 1980 on the fraction of black school-aged residents attending public schools, assuming, since non-compliance is low for black students, that the 1980 levels are a good proxy for pre-1975 levels. Finally, in column 4, I further adjust for measurement error that arises from the fact that some students are already attending a majority other-race school prior to the district merger. I measure this directly using aggregated Office of Civil Rights data available from the National Archives and Records Administration.

After adjusting for measurement error, the coefficient for black students increases to 0.0495,

[22]This is a good proxy–in 1977, 65% of the school-aged birth cohort are attending public schools and in 1980, 66% of actual residents are attending public schools.

implying that black students assigned busing live in neighborhoods with average incomes that are about 5% higher than black students not assigned busing. On the other hand, the coefficient for white students remains small at -0.0028 even after these adjustments. In this table, I also (crudely) adjust the lower bound and the upper bound of the confidence intervals by the same measurement error statistics. This exercise yields a fairly precise adjusted confidence interval for white students of -0.0113 to 0.0056. Of course, this adjustment does not account for any error in the adjustment itself.

Columns 1-4 adjust estimates based on measurement error that arises from coding busing assignment based on place of birth. In columns 5-7 of Table 9, I adjust estimates to account for non-compliance. Eventually, I will estimate the local average treatment effect directly, but for now, this adjustment is intended to give a sense of how big the effect of busing take-up may be. I use three measures of non-compliance to scale the intent-to-treat estimates.

Students can exit the district by moving to another public school district, transferring to a private school, or dropping out of school. In 1980, five years after desegregation, 66.3% of white children and 93.3% of black children in Jefferson County are attending a public school.[23] The high public school enrollment of black children suggests there is not a substantial enrollment response to desegregation. In fact, dropout rates in LCS in 1974 are approximately 9.3% and fall to 4.0% in former LCS schools in 1976. Guryan (2004) also finds that desegregation decreases black dropout rates.

To measure white flight in response to the desegregation order, Cunningham, Husk, and Johnson (1978) calculate white public school enrollment as a share of the white school-aged birth cohort in Jefferson County, KY. In the 1974-75 school year, approximately 77.8% of the white school-aged birth cohort is attending public schools in Jefferson County. In the 1975-76 school year, this drops to 74.4%, and in 1976-1977, it drops further to 66.2%. Assuming the decline in public school enrollment is entirely due to desegregation, this implies approximately 15,000 or 15% of white students left the district because of the merger.[24] Approximately one

[23] 29.4% of white children and 3.1% of black children are attending a private school. 4.3% of white children and 3.6% of black children are not enrolled in a public or private school. These statistics are calculated for individuals living in Jefferson County, KY in 1980 and aged 6-17.

[24] Total white school-aged births sums to 135,000 in the 1976-77 school year. Applying the 77.8% public school enrollment of 1974-75 to the 1976-77 birth cohort implies 105,000 white students should be enrolled. Only 90,000 white students are actually enrolled in 1976-77.

third of this decline can be explained by rising private school enrollment, leaving the remaining amount to be explained by dropout or movement out of the district (Cunningham et al. 1978). Reber (2005) also finds white enrollment in a district decreases by about 10% within 2-3 years of desegregation. Finally, anecdotal accounts suggest parents believed that removing their children would lead to a policy reversal. Another interviewee from K'Meyer (2013) shares, "My parents thought if enough people stuck together and held their kids out of school then they would have no choice but to see, well, this isn't working and there's not enough jails to hold everybody who's not sending their kids to school."

I use the Cunningham, Husk, and Johnson (1978) estimate that 15% of white students left the district due to busing as one measure of non-compliance. When using this measure, I assume that 0% of black students left the district due to busing, based on the discussion of black public school enrollment above and the results from Guryan (2004). I also use two student-level measures calculated using the archival yearbook records. The first measure is the raw match rate from the pre-1975 yearbooks to the post-1975 yearbooks, by race. The second measure accounts for the fact that the data does not include every yearbook, and thus, some students cannot be matched to a post-1975 record.

Column 5 of Table 9 adjusts the estimate from Column 4 using the raw yearbook measure of compliance. This yields the largest coefficient estimate for white students, and it suggests that white students who are bused live in neighborhoods with average incomes that are 0.7% lower than white students who remain in former County schools. This estimate is an order of magnitude lower than the estimate for black students in this column. Since only some yearbooks were available for data collection, this column represents a lower-bound on compliance. I use an adjusted compliance measure in column 6, which suggests that white students who are bused live in neighborhoods with average incomes that are only 0.4% lower than white students who remain in former County schools. Finally, in column 7, I use the Cunningham, Husk, and Johnson (1978) measure of compliance and find similar results to column 6. Ultimately, these results suggest that even after accounting for measurement error and non-compliance, the effect of busing take-up for white students remains relatively small. This exercise, however, assumes that compliance is equal for groups assigned versus not assigned to busing. This is not the case for white students, and I discuss the implications of this in the following section.

2. The Exclusion Restriction

The IV results are only unbiased if the exclusion restriction is satisfied. In other words, it must be the case that busing assignment affects outcomes only through its effect on busing take-up. If busing assignment induces disenrollment and disenrollment affects outcomes, the exclusion restriction is violated. Note, however, the intent-to-treat effects in Tables 5-8 remain unbiased in this case. Table 4b shows that disenrollment does not differ by busing assignment for black students but it does differ for white students. I will consider the three margins of disenrollment (dropout, private school enrollment, and migration out of district) and existing estimates from the literature to put a bound on the local average treatment effect for white students.

C. Decomposing the Net Effect

Prior research on school desegregation has estimated large benefits for black students exposed to court-ordered desegregation. Many educational inputs change dramatically when school districts desegregate, and comparing students in districts before and after desegregation yields an estimate of the "net effect" of all of these changes. In this paper, I focus on within-cohort comparisons. However, I can also estimate the net effect in Jefferson County, KY, and I can estimate how much of that net effect is explained by within-cohort busing assignments. These results have not yet been approved for release.

D. Peers vs. Resources and Alternative Explanations

The results in Sections IV.A suggest that the gains from busing assignment are due to improved school resources for black students. This is because City and County schools are equally integrated after 1975, but in terms school quality, they are not equal. As described in Section IV.C, I will also test whether the net benefits of desegregation in Jefferson County, KY accrue primarily to students assigned busing. Comparing the magnitude of the estimates in Table 5 to existing estimates of the long-run net effect from Johnson (2015) does suggest that peer effects play a smaller role.

The results discussed in Section IV.B, however, present a challenge to this interpretation. I find that white students assigned to busing are less likely to comply with that assignment than

white students assigned to remain in their home school (and have black students bused in). This differential compliance could result in a different unobserved composition of white students in former City versus former County schools. For example, consider a scenario in which all high-income families leave the district, regardless of assignment, but that middle-income families leave only when their child is assigned busing. This would mean that white students bused to former City schools would be from low-income families while white students remaining in former County schools would be from low- and middle-income families. These different peer compositions could explain the results for black students.[25]

To evaluate this possibility, I explore whether white students who are bused are observably different from white students who are not bused. In general, I find that white students who are bused are similar to white students who are not bused in terms of gender and standardized test scores measured one year after busing.[26] Columns 2-4 of Table 1a shows that female students, in general and by race, are equally likely to take up busing. In other words, families did not disproportionately disenroll girls or boys in response to their busing assignment. In addition, former County schools with different characteristics have similar compliance rates.[27] In terms of test scores, a report on 2nd grade test scores in Jefferson County, KY (Natkin 1980) finds that white students who are bused score only 1.0 points lower than white students who remain in former County schools. This suggests that differential compliance based on busing assignment did not result in different peer compositions along these margins.[28]

[25]Another possibility is that all extremely intolerant families leave the district, regardless of assignment, but that moderately intolerant families leave only when their child is assigned busing. In this case, white students bused to former City schools would be particularly tolerant. However, this type of sorting should lead to better outcomes for black students in the former City schools because they would be exposed to peers that are, on average, more tolerant of racial integration.

[26]Test scores measured one year after busing could be affected by busing, potentially making them a poor measure of whether the bused/not bused groups differed before busing. First, there are no reports of test score by bused/not bused for pre-desegregation years. Second, the test scores in this report are measured after only one year of busing. Third, if busing did have a negative effect on white students, then the failure to find differences in the post-busing test scores would suggest that white students who were bused had higher pre-busing test scores than those not bused. In that case, we should not expect negative peer effects for black students remaining in former City schools.

[27]Eastern High School has a 35 minute drive time to Central High School and Waggener has a 25 minute drive time, but they both have compliance rates of around 58%. At Eastern and Seneca, the student body is over 6% black pre-1975, but those schools have similar compliance rates for white students (~56-60%) as Waggener and Westport, where the student body is less than 1% black pre-1975. Jeffersontown has the highest student-teacher ratio at 25.66 and Atherton has the lowest at 23.06, but both schools have compliance rates around 40%.

[28]Data from the OCR on the percent of students in school on free or reduced price lunch also shows that, outside of a few elementary schools, former City and former County schools have an equal fraction of their students on free or reduced price lunch. I do not include these as main results because the sum of students on full price, reduced price, or free price lunch is often much lower than the sum of total students.

Finally, there is reason to believe that, even if there were some peer quality differences, they may be muted in this setting. Carrell, Sacerdote, and West (2013) shows that students sort into sub-groups when exogenously assigned to a larger group of peers. And recent work using AddHealth data on peer groups finds that school desegregation does not increase interactions with other-race peers (Mele 2019). I present suggestive evidence that this occurs in the context of school desegregation in Jefferson County by using two measures of peer interaction in schools. First, I use historical data from the Office of Civil Rights on the racial composition of randomly selected classrooms at each school. Second, I collect data on racial composition of extracurricular activities and clubs from archival yearbooks.

Figure 3 is a histogram showing the fraction of black students in each classroom and the fraction of black students in each school. Note, the fraction of black students at the classroom level is more disperse. In fact, almost half of all classrooms have a racial composition that is outside the 5th or 95th percentiles of the school-level distribution of percent black. In other words, half of all classrooms have a percentage of black students that is either below 15% or above 35% (the 5th and 95th percentiles). Even more, over one-third of all classrooms have compositions outside the 1st or 99th percentiles (i.e. a percentage of black students below 12.5% or above 42%). Finally, approximately 40% of all classrooms have a racial composition that is more than 50% different than their school's racial composition. These results suggest that even after segregation, black and white students had limited interaction at the classroom level.

This point is further highlighted in Figure 4, a histogram showing the fraction of black students in each club for the 1974-75 school year (pre-desegregation) and the same fraction for each club in the 1975-76 school year (post-desegregation). This data was collected from high school yearbooks for one City and three County high schools. Although extracurriculars become slightly more integrated after desegregation, they remain disproportionately segregated. This is suggestive evidence that black students and white students remained relatively segregated in terms of peer interaction even after the district merger. Since the evidence on peer differences suggests white students who are bused are similar to white students remaining in former County schools, and the evidence on peer interaction suggests that students were sorted into racially segregated sub-groups, it is unlikely that the main results are driven by peer effects.

V. Conclusion

In this paper, I study a unique busing assignment plan in Jefferson County, KY to estimate long-run economic effects of school desegregation. In 1975, the district (under a court order) assigned students to be bused to new schools (versus stay at their home school and have new students bused in) based on their race and the first initial of their last name. I find black students assigned busing to former County schools (formerly majority-white schools) live in better neighborhoods at adulthood than black students assigned to remain in former City schools (formerly majority-black schools). Black students in former County schools realize these gains despite continued segregation at the classroom level, limited interaction with other-race peers via extracurricular activities, and the fact that the former City and County schools are merged into one district after 1975.

This effect for black students is increasing in the total number of years a student is assigned busing and is larger for students assigned busing in earlier grades. On the other hand, busing assignment has small to zero effect on white students. Since former City and former County schools had similar racial compositions after desegregation, these results suggest school desegregation in this setting improved outcomes for black students by giving them access to better schools.

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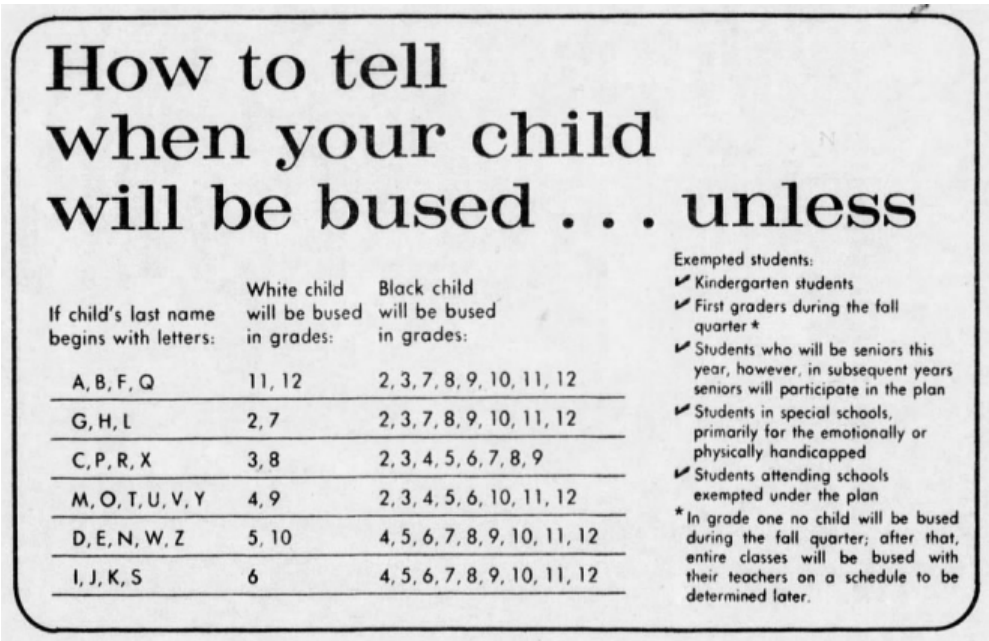
Tables and Figures

Figure 1. Busing Assignment Plan in Jefferson County, KY



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

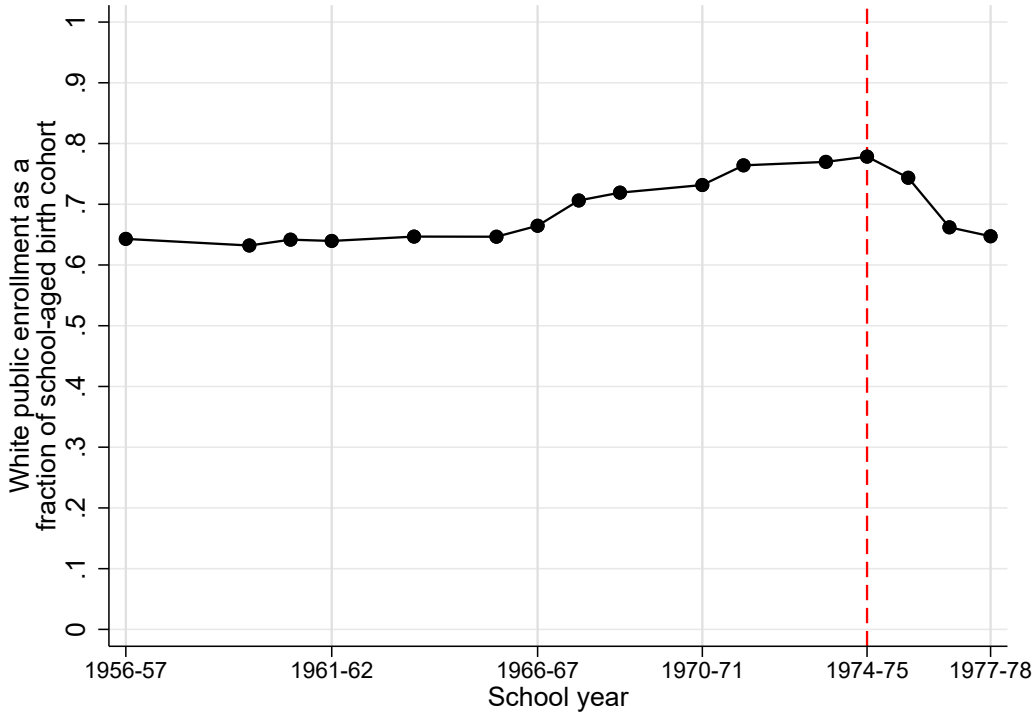
Exempted students:

- ✔ 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.

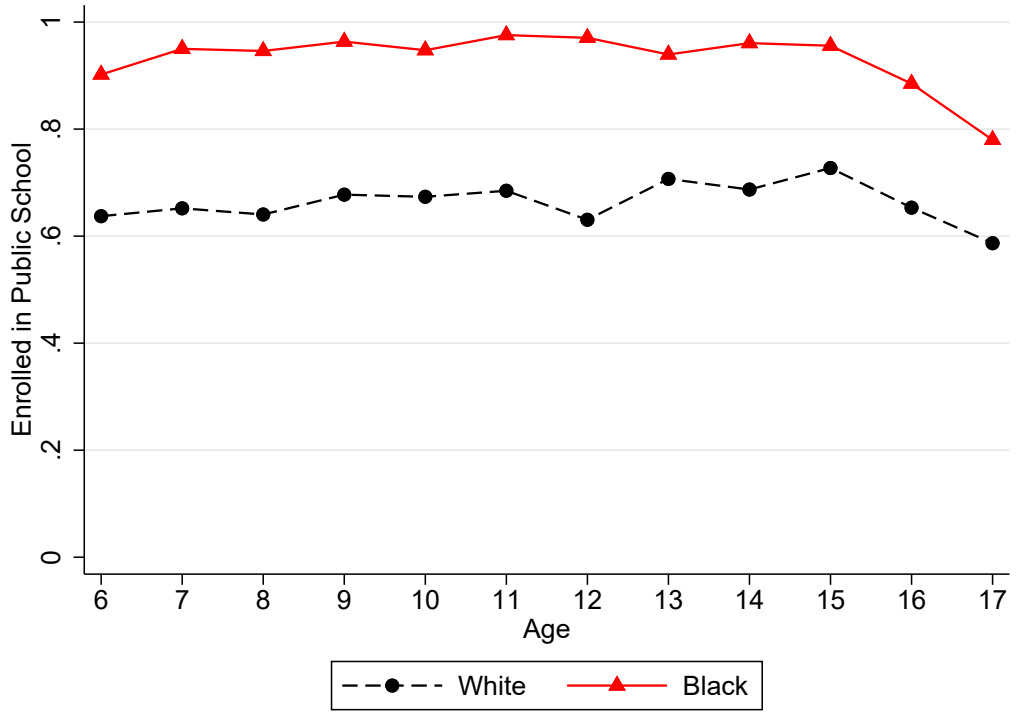
Notes: The plan depicted above was printed in the July 31, 1975 issue of The Courier-Journal. This plan, known as 'the alphabet plan', was unchanged from 1975-1982. A minor change was made for white students in 1982. In 1985, the district adopted a zoning system for middle and high school students, abandoning the alphabet plan for those students. In 1991, the district moved to a zoning system for elementary school students. The 1982 plan and the 1985 plan are displayed in Figures A1a and A1b. Figure A1c depicts the potential variation in busing assignment induced by this plan for a student in school by the 1975-76 school year.

Figure 2a. White Public School Enrollment as a Share of White School-aged Birth Cohorts



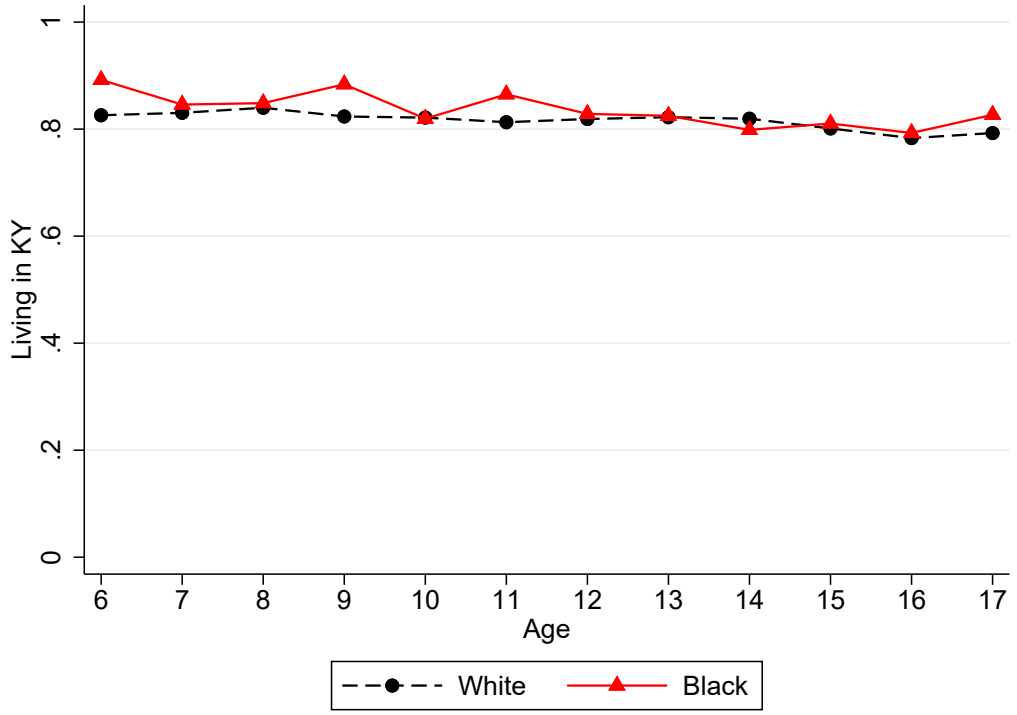
Notes: Figure 2a is derived from Cunningham et al. (1978). The authors measure white public school enrollment in Jefferson County, KY for several years from 1956 to 1977. They also calculate the school-aged birth cohort, which is the number of people who should be school aged in Jefferson County, KY based on birth records alone. In their paper, they argue that white flight is not as stark as it seems. Part of the decline in the level of white enrollment is due to a decline in the school-aged birth cohort. I digitized their figure, calculated the share directly, and I plot that share here. White public school enrollment falls after the desegregation order in 1975.

Figure 2b. Public School Enrollment in 1980, by Race and Age



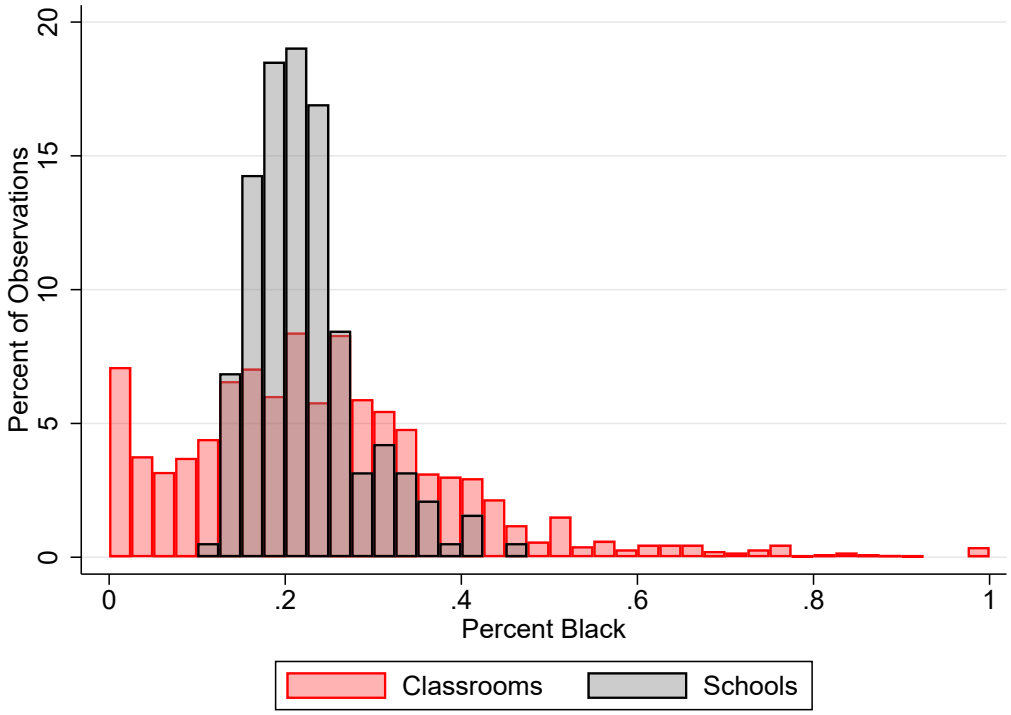
Notes: Figure 2b is calculated from publicly available Decennial Census data from 1980. In this figure, I plot the fraction of children enrolled in public school in Jefferson County, KY in 1980 for ages 6-17 and by race. Enrollment in public school is fairly constant from ages 6-15 but falls at ages 16 and 17. This is due to a rise in non-enrollment. Figures A3a and A3b show private school enrollment and non-enrollment by age and race.

Figure 2c. Fraction Born in KY Who Are Still Living in KY in 1980, by Race and Age



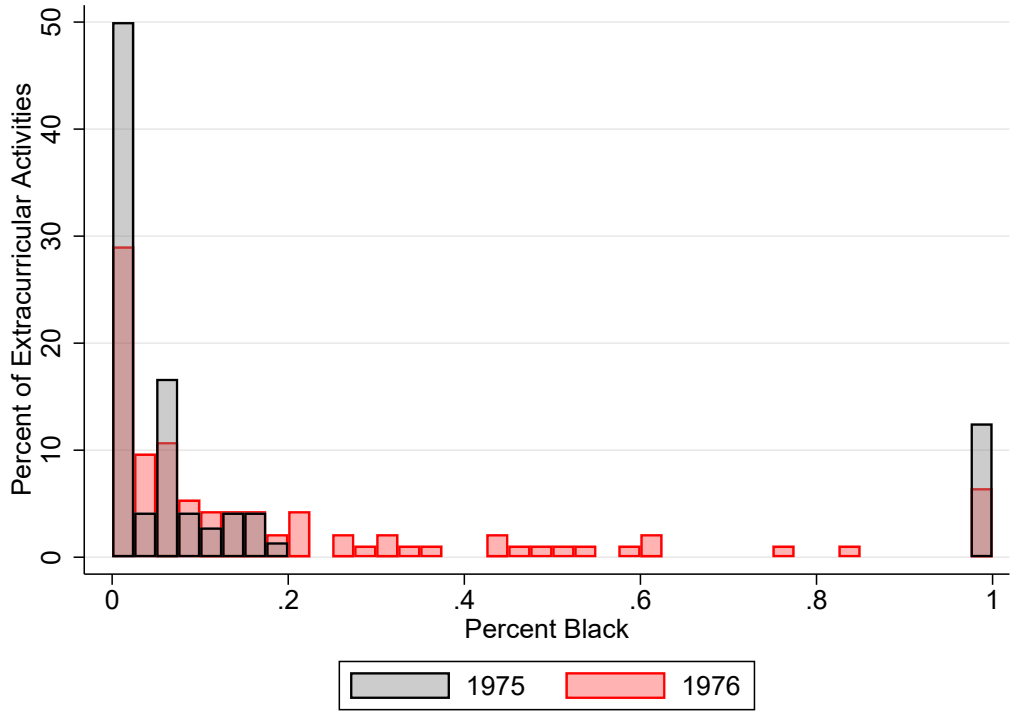
Notes: Figure 2c is calculated from publicly available Decennial Census data from 1980. In this figure, I plot the fraction of children born in KY who are still living in KY as of 1980 for ages 6-17 and by race. The probability of migrating out of the state is constant from ages 6-17. I use this as a proxy for the migration rate out of Jefferson County, KY to approximate the attenuation bias in the coefficients from Table 5 (see Table 3, Table 5, and Table 9).

Figure 3. Classroom Racial Composition versus School Racial Composition



Notes: Figure 3 is calculated from historical Office of Civil Rights data. As part of the OCR survey, schools were instructed to randomly select 18 classrooms in their school and provide information about the racial composition of the classroom. I have coded this information from the files for 1976 and 1980. The figure above plots a histogram of the percent black in each school over a histogram of the percent black in each classroom. The percent black at the classroom-level is more disperse–almost half of all classrooms are below 15% black or above 35% black (the 5th nd 95th percentiles of percent black at the school-level).

Figure 4. Racial Composition of Extracurricular Activities, Pre- and Post-Desegregation



Notes: Figure 4 is calculated using newly collected data from archival yearbooks in Jefferson County, KY. A research assistant collected data on the racial composition of extracurricular activities from one city high school (Central High School) and three county high schools (Ballard High School, Eastern High School, and Atherton High School). I plot the percent black at the club-level for 1975, the year before desegregation, and for 1976, the year after desegregation. Although clubs become slightly more integrated in 1976, they are far more segregated than the student bodies themselves.

Table 1a. Differences in Student Composition between Former City and County Schools, Post-1975

	Percent Black	Percent Female	Percent Black Female	Percent White Female	Std. Dev. in Classroom Percent Black	Percent of Classrooms with Skewed Percent Black
	(1)	(2)	(3)	(4)	(5)	(6)
Former City School	0.0232	0.00292	0.00349	-0.00113	0.00839	0.0228
	(0.0176)	(0.00475)	(0.0113)	(0.00682)	(0.0130)	(0.0310)
Constant	0.251***	0.479***	0.477***	0.465***	0.164***	0.540***
	(0.0302)	(0.00723)	(0.0145)	(0.0195)	(0.0163)	(0.0514)
Years Included	'76, '78, '80, '82	'76, '78, '80, '82	'76	'76	'76, '80	'76, '80
Observations	382	382	102	102	191	191
R^2	0.152	0.045	0.038	0.071	0.098	0.084

Notes: Table 1a is derived from historical Office of Civil Rights data. In this data, I classify schools as former City schools if they were in the Louisville City Schools district and not in the Jefferson County Schools district from 1968-1974. In the 1976-82 data, I remove schools with low student populations (less than 200) and with an abnormally high percentage of black students (above 50%). On inspection, these are primarily non-traditional schools, such as vocational schools. Columns 1-4 display basic school demographics and indicate that racial composition, gender composition, and gender composition within race were roughly equal in former City and former County schools. Columns 5-6 use a measure of classroom-level racial composition. The OCR survey instructed schools to randomly select 18 classrooms in their school and provide data on the racial composition at the classroom level. I calculate the standard deviation in percent black at the classroom level for each school (column 5) and the percentage of classrooms that are below 15% black or above 35% black (the 5th and 95th percentiles of percent black at the school level). These results indicate that former City schools are also equally integrated at the classroom-level. All specifications include controls for grades offered at the school (1st grade, 7th grade, and 12th grade) and for year fixed effects (when applicable). Standard errors are clustered at the school-level.
* $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$

Table 1b. Differences in Student Outcomes between Former City and County Schools, Post-1975

	Dropouts	Black Dropouts	White Dropouts	Suspensions	Black Suspensions	White Suspensions	Court Referrals	Black Court Referrals	White Court Referrals
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Former City School	0.0100***	0.0179*	0.00862***	-0.00888	-0.0176	-0.00745	0.000602*	0.00166	0.000216
	(0.00339)	(0.00961)	(0.00322)	(0.00646)	(0.0118)	(0.00527)	(0.000338)	(0.00106)	(0.000178)
Constant	0.0123	0.0359	0.00712	0.0399**	0.0598**	0.0308**	-0.000142	-0.000902	-0.000128
	(0.00850)	(0.0239)	(0.00716)	(0.0154)	(0.0285)	(0.0119)	(0.000336)	(0.00117)	(0.000159)
Years Included	'76	'76	'76	'76, '78, '80, '82	'76, '78, '80, '82	'76, '78, '80, '82	'76	'76	'76
Observations	75	75	75	217	217	217	74	74	74
R^2	0.782	0.628	0.706	0.663	0.640	0.628	0.289	0.349	0.096

Notes: Table 1b is derived from historical Office of Civil Rights data. In this data, I classify schools as former City schools if they were in the Louisville City Schools district and not in the Jefferson County Schools district from 1968-1974. In the 1976-82 data, I remove schools with low student populations (less than 200) and with an abnormally high percentage of black students (above 50%). On inspection, these are primarily non-traditional schools, such as vocational schools. Columns 1-3 display dropout rates, in general and by race. Columns 4-6 display suspension rates, in general and by race. Columns 7-9 display court referral rates (i.e. the student is referred to court or juvenile authority for disciplinary action), in general and by race. The data reported in the OCR survey is based on the prior school year, but the measure of total students used in the denominator is from the current school year. For columns 4-6, I can interpolate the population between gap years and the results do not change. The OCR documentation also notes that data on suspensions was often prone to error because some schools reported the total number of suspensions and some reported the total number of unique students suspended. All specifications include controls for grades offered at the school (1st grade, 7th grade, and 12th grade) and for year fixed effects (when applicable). Standard errors are clustered at the school-level.
* $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$

Table 1c. Differences in Resources between Former City and County Schools, Post-1975

	Has Gifted & Talented Program	Has Additional Honors Courses	Student-Teacher Ratio	Total Teachers
	(1)	(2)	(3)	(4)
Former City School	-0.128**	0.0327	0.427	-2.422
	(0.0553)	(0.101)	(0.806)	(1.845)
Constant	-0.0432	0.577***	18.74***	16.65***
	(0.117)	(0.209)	(1.664)	(3.440)
Years Included	'76, '78, '80, '82	'76	'76, '78, '80	'76, '78, '80
Observations	382	102	246	246
R^2	0.129	0.102	0.037	0.886

Notes: Table 1c is derived from historical Office of Civil Rights data. In this data, I classify schools as former City schools if they were in the Louisville City Schools district and not in the Jefferson County Schools district from 1968-1974. In the 1976-82 data, I remove schools with low student populations (less than 200) and with an abnormally high percentage of black students (above 50%). On inspection, these are primarily non-traditional schools, such as vocational schools. Column 1 displays the likelihood the school has a Gifted and Talented program–it is considerably lower in former City schools. Column 2 looks at the probability the school offers additional honors courses, and it does not differ by former City or County school. Column 3 displays the student-teacher ratio. This is constructed using data on total number of teachers for 1976 and total number of classrooms in 1978 and 1980. Column 4 displays the level instead of the ratio, including a control for total number of students in the school. All specifications include controls for grades offered at the school (1st grade, 7th grade, and 12th grade) and for year fixed effects (when applicable). Columns 1-2 and 4 include controls for the total number of students in the school. Standard errors are clustered at the school-level.
* $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$

Table 1d. Differences in School Locations between Former City and County Schools, Post-1975

	Tract HS Completion Rate, 1980	Tract Below Poverty, 1980	Tract Employment Rate, 1980	Tract Median Household Income, 1980	Percent of Buildings in Tract with No AC, 1980	Percent of Buildings in Tract with Room Heater, 1980	Tract Predicted PM2.5 Levels, 2001- 2005 Average	Zip Code Violent Crime per Capita, 2004	Zip Code Property Crime per Capita, 2004	Zip Code Drugs/Other Crime per Capita, 2004
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Former City School	-0.154***	0.168***	-0.0801***	-9,057***	0.302***	0.157***	0.590***	0.00633***	0.0273*	0.0325***
	(0.0410)	(0.0406)	(0.0203)	(1,371)	(0.0468)	(0.0453)	(0.0518)	(0.00132)	(0.0155)	(0.00969)
Constant	0.705***	0.0889***	0.912***	22,053***	0.0995**	0.0155	15.97***	0.00316***	0.00746	0.00750
	(0.0575)	(0.0337)	(0.0246)	(1,709)	(0.0441)	(0.0320)	(0.113)	(0.00111)	(0.00910)	(0.00658)
Observations	99	99	99	99	99	99	99	99	99	99
R^2	0.153	0.385	0.316	0.547	0.309	0.256	0.386	0.415	0.135	0.335

Notes: Table 1d is derived from historical Office of Civil Rights data, publicly available Decennial Census data from 1980 (from NHGIS), recent data on predicted pollution from the CDC, and recent data on zip code level crime from the Louisville Metropolitan Police Department (LMPD). In the OCR data, I classify schools as former City schools if they were in the Louisville City Schools district and not in the Jefferson County Schools district from 1968-1974. In the 1976-82 data, I remove schools with low student populations (less than 200) and with an abnormally high percentage of black students (above 50%). On inspection, these are primarily non-traditional schools, such as vocational schools. I then geocode each school to a Census tract and match it to various tract-level or zip-level outcomes. Columns 1-6 display tract-level characteristics from the 1980 Decennial Census. Former City schools are located in neighborhoods with lower high school completion, higher poverty, lower employment rates, and lower median household income. Also, former City schools are located in areas where buildings are less likely to have air conditioning and are more likely to be heated by a room heater. Column 7 uses tract-level data on predicted PM2.5 pollution from the CDC (estimated using EPA data). I collapse this data to the average from 2001-2005. Former City schools are located in areas that have higher pollution levels as of 2001-2005 (earlier data not available). Finally, columns 8-10 use crime data from the LMPD from 2004 (earlier data not available). I collapse this to the zip code level, keeping only crimes that occur during school months and from the hours from 6am-5pm. I calculate crime rates using zip code level population. Former City schools are located in areas that have higher crime rates as of 2004. All specifications include controls for grades offered at the school (1st grade, 7th grade, and 12th grade). Standard errors are clustered at the school-level.

* $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$

Table 2. Summary Statistics on Men Aged 28-55 in Jefferson County, KY from the Public Sample of the 2000 Decennial Census

	White Men	Black Men
	(1)	(2)
Income	48735.8	25931.1
	(53685.6)	(30515.1)
High School Degree	0.908	0.851
	(0.288)	(0.356)
Bachelors Degree	0.321	0.133
	(0.467)	(0.340)
Worked Last Year	0.922	0.781
	(0.269)	(0.414)
Born in KY	0.705	0.767
	(0.456)	(0.423)
Institutionalized	0.00386	0.0198
	(0.0620)	(0.139)
Observations	5,785	1,010

Notes: Table 2 is derived from the publicly available sample of the 2000 Decennial Census. I limit the sample to men aged 28-55 and living in Jefferson County, KY as of 2000. I report averages on income, educational attainment, employment, state of birth, and institutionalization by race. Standard deviations are in parentheses.

Table 3. Summary Statistics from the Public Sample of the 1980 Decennial Census, Aggregate Office of Civil Rights Data, and Cunningham et al. (1978)

	White, 6-17 y.o.	Black, 6-17 y.o.
Panel A. Pr(Living in KY | Born in KY), 1980		
Living in KY	0.8165	0.8319
	(0.3871)	(0.3741)
Sample	Born in KY	Born in KY
Observations	17,136	1,463
Source Data	1980 Census	1980 Census
Panel B. Pr(In Public School | In Jefferson County), 1980		
Enrolled, Public	0.6709	0.9408
	(0.4700)	(0.2361)
Sample	Living in Jefferson County	Living in Jefferson County
Observations	2,647	676
Source Data	1980 Census	1980 Census
Panel C. Pr(In Public School | In Jefferson County), 1974-75		
Enrolled, Public	0.7784	–
	–	–
Sample	School-aged birth cohort	–
Observations	–	–
Source Data	Cunningham et al. (1978)	–
Panel D. Pr(In Majority Same-Race School | In Public), 1974-75		
In Majority Same-Race	0.8045	0.8382
	–	–
Sample	In Public School in Jefferson County	In Public School in Jefferson County
Observations	–	–
Source Data	NARA/OCR Aggregates	NARA/OCR Aggregates

Notes: Panel A is derived from the publicly available sample of the 1980 Decennial Census. I limit the sample to boys aged 6-17 who were born in KY, and I report the probability that they are still living in KY, by race. Panel B is also derived from the publicly available sample of the 1980 Decennial Census. I limit the sample to boys aged 6-17 who are living in Jefferson County, KY, and I report the probability that they are enrolled in public school, by race. Panel C is derived from Cunningham et al. (1978)–see notes for Figure 2a. Panel D is derived from aggregated OCR data provided by NARA. I calculate the fraction of white students in a majority white school prior to desegregation (column 1) fraction of black students in a majority-black school prior to desegregation (column 2). I adjust the coefficients from Table 5 by these numbers in Table 9.

Table 4a. Summary Statistics for Student Characteristics from Yearbooks

Panel A.	**City Schools, post-1975**	**County Schools, post-1975**
Black	0.172	0.153
	(0.377)	(0.360)
Male	0.498	0.490
	(0.500)	(0.500)
Observations	1,240	13,355
Panel B.	**Black Students, 1975**	**White Students, 1975**
'A', 'B', 'F', 'Q'	0.191	0.168
'G', 'H', 'L'	0.154	0.173
'C', 'P', 'R', 'X'	0.163	0.174
'M', 'O', 'T', 'U', 'V', 'Y'	0.155	0.154
'D', 'E', 'N', 'W', 'Z'	0.162	0.159
'I', 'J', 'K', 'S'	0.174	0.171
Observations	1,090	20,887
Panel C.	**City Schools, 1972-74**	**County Schools, 1972-74**
Continually Enrolled	0.586	0.649
–i.e. Matched to a Yearbook	(0.493)	(0.477)
Observations	752	2,867

Notes: All statistics above are derived from newly collected data from archival school yearbooks. Panel A limits the sample to schools post-desegregation and reports statistics on student race and gender. This confirms the findings in Table 1a. Former City and former County schools are equally integrated. Panel B limits to students in the year prior to desegregation and shows the fraction of students in each alphabet group. Panel C uses data on Central High School and Ballard High School from 1972-74 to calculate year-to-year match rates in the years prior to desegregation. These columns show that even in years prior to desegregation, the match rates are low. I adjust the post-desegregation match rates by these numbers in Table 9.

Table 4b. Regression Results for Student-Level Enrollment and Busing Take-up from Yearbooks

	Bused	Bused	Continually Enrolled (Matched to a Yearbook)
	(1)	(2)	(3)
Assigned Busing x White	0.934***	0.489***	-0.143***
	(0.0125)	(0.0182)	(0.0195)
Assigned Busing x Black	0.976***	0.455***	-0.0389
	(0.0134)	(0.0307)	(0.0373)
Black	0.0122*	0.00568*	-0.201***
	(0.00700)	(0.00327)	(0.0228)
Constant	0	0	0.667***
	(7.13e-11)	(3.36e-10)	(0.00700)
Sample	Matched Students	Matched+ Unmatched	Matched+ Unmatched
Observations	3,756	6,090	6,090
R-squared	0.930	0.433	0.027

Notes: All statistics above are derived from newly collected data from archival school yearbooks. I limit the sample to schools in cluster 1 (discussed in text) because we have yearbooks for almost every school in that sample. I then attempt to match everyone in a pre-desegregation yearbook to an expected post-desegregation yearbook (former City or former County). I adjust the match rates in columns 2-3 since some yearbooks are missing even when limiting to the cluster with the most complete set. Column 1 limits the sample to students who I match to a post-desegregation yearbook. This column indicates that among students who remain enrolled in the merged district, they almost always comply with their busing assignment. Column 2 runs the same regression as Column 1 but includes students who disenroll (i.e. students for whom I do not find a yearbook match). This columns indicates that approximately half of students assigned busing comply with their busing assignment. Finally, Column 3 regresses whether I find a yearbook match at all on whether the student was assigned busing. This column indicates that white students assigned busing are more likely to leave the district, but this is not the case for black students. Robust standard errors are in parentheses.

* $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$

Table 5. Extensive Margin Effect of Busing Assignment on Neighborhood Characteristics in Adulthood

	Log(Avg. Income)	Avg. Income	% with HS	% with BA	% Worked Last Year	% Worked Last Week
	(1)	(2)	(3)	(4)	(5)	(6)
Assigned Busing x Black	0.0336**	1,890***	0.00551	0.0173***	0.00187	-0.00307
	(0.0156)	(565)	(0.00360)	(0.00580)	(0.00458)	(0.00527)
Assigned Busing x White	-0.00145	-168	0.000160	-0.00128	0.000283	0.0000260
	(0.00264)	(171)	(0.000782)	(0.00168)	(0.000419)	(0.000545)
P-value, AB x B = AB x W	0.0273	0.000561	0.148	0.00227	0.730	0.560
Observations	149,000	149,000	149,000	149,000	149,000	149,000
R^2	0.0863	0.0457	0.0344	0.0257	0.131	0.161

Notes: Standard errors are clustered at the level of variation, race by grade cohort by alphabet group. The sample in all specifications is men born in Jefferson County, KY (based on the Numident) and graduating in years 1965-1990 (based on year of birth, month of birth, and school entry rules) who respond to the short-form Census in 2000. Assigned Busing x Black (AB x B) is a dummy variable equal to one if the respondent reports a non-white race and if the respondent is coded as "assigned busing" based on grade cohort, race, and alphabet group. It is equal to zero if the respondent reports race as "white" or is coded as "not assigned busing" based on grade cohort, race, and alphabet group. Assigned Busing x White (AB x W) is defined similarly. All specifications include grade cohort fixed effects interacted with race fixed effects and alphabet group fixed effects interacted with race fixed effects. The dependent variables are continuous tract-level averages derived from the 2000 Decennial long-form data using sample weights. Log(avg. income) is the natural log of the average income in the tract, avg. income is the level of the average income in the tract, % with HS is the fraction of respondents in the tract with a high school diploma or more, % with BA is the fraction of respondents in the tract with a bachelors degree or more, % worked last year is the fraction of respondents in the tract who worked last year, and % worked last week is the fraction of respondents in the tract who worked last week. These averages are based on men aged 28-55 as of the 2000 Census. For tracts with a small number of respondents, the county-level average is used. I attach these tract-level outcomes to short-form respondents to use the largest sample possible. Essentially, this measures whether busing leads people to live in "better" neighborhoods later in life (i.e. neighborhoods with higher average income, higher education levels, higher levels of employment in the prior year and prior week). The regressions themselves are not weighted.
* $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$

Table 6. Intensive Margin Effect of Busing Assignment on Neighborhood Characteristics in Adulthood

	Log(Avg. Income)	Avg. Income	% with HS	% with BA	% Worked Last Year	% Worked Last Week
	(1)	(2)	(3)	(4)	(5)	(6)
Years Assigned x Black	0.00848**	290*	0.00311***	0.00335**	0.00150*	0.000949
	(0.00393)	(167)	(0.000917)	(0.00162)	(0.000885)	(0.00117)
Years Assigned x White	-0.000601	-79.9	0.000136	-0.000323	0.000324	0.000210
	(0.00133)	(87.0)	(0.000413)	(0.000872)	(0.000212)	(0.000295)
P-value, YA x B = YA x W	0.0294	0.0507	0.00336	0.0462	0.198	0.541
Observations	149,000	149,000	149,000	149,000	149,000	149,000
R^2	0.0863	0.0457	0.0344	0.0257	0.131	0.161

Notes: Standard errors are clustered at the level of variation, race by grade cohort by alphabet group. The sample in all specifications is men born in Jefferson County, KY (based on the Numident) and graduating in years 1965-1990 (based on year of birth, month of birth, and school entry rules) who respond to the short-form Census in 2000. Years Assigned x Black (YA x B) is a continuous variable equal to the number of years the respondents is coded as "assigned busing" based on grade cohort, race, and alphabet group if the respondent reports as non-white race and equal to zero if the respondent reports race as "white." Years Assigned x White (YA x W) is defined similarly. All specifications include grade cohort fixed effects interacted with race fixed effects and alphabet group fixed effects interacted with race fixed effects. The dependent variables are continuous tract-level averages derived from the 2000 Decennial long-form data using sample weights. Log(avg. income) is the natural log of the average income in the tract, avg. income is the level of the average income in the tract, % with HS is the fraction of respondents in the tract with a high school diploma or more, % with BA is the fraction of respondents in the tract with a bachelors degree or more, % worked last year is the fraction of respondents in the tract who worked last year, and % worked last week is the fraction of respondents in the tract who worked last week. These averages are based on men aged 28-55 as of the 2000 Census. For tracts with a small number of respondents, the county-level average is used. I attach these tract-level outcomes to short-form respondents to use the largest sample possible. Essentially, this measures whether busing leads people to live in "better" neighborhoods later in life (i.e. neighborhoods with higher average income, higher education levels, higher levels of employment in the prior year and prior week). The regressions themselves are not weighted.
* $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$

Table 7. Extensive and Intensive Margin Effect of Busing Assignment on Neighborhood Characteristics in Adulthood, Black Students

	Log(Avg. Income)	Avg. Income	% with HS	% with BA	% Worked Last Year	% Worked Last Week
	(1)	(2)	(3)	(4)	(5)	(6)
Assigned Busing x Black	0.0143	1,310*	-0.00238	0.0102	-0.00202	-0.00603
	(0.0180)	(700)	(0.00410)	(0.00690)	(0.00520)	(0.00603)
Years Assigned x Black	0.00787*	234	0.00321***	0.00292*	0.00158*	0.00121
	(0.00422)	(180)	(0.000993)	(0.00173)	(0.000946)	(0.00126)
Observations	149,000	149,000	149,000	149,000	149,000	149,000
R^2	0.0863	0.0457	0.0344	0.0257	0.131	0.161

Notes: Standard errors are clustered at the level of variation, race by grade cohort by alphabet group. The sample in all specifications is men born in Jefferson County, KY (based on the Numident) and graduating in years 1965-1990 (based on year of birth, month of birth, and school entry rules) who respond to the short-form Census in 2000. See Table 5 notes for a definition of Assigned Busing x Black. See Table 6 notes for a definition of Years Assigned x Black. All specifications include grade cohort fixed effects interacted with race fixed effects and alphabet group fixed effects interacted with race fixed effects. Also, the specifications above include an Assigned Busing x White dummy variable that is not reported because white students do not have meaningful variation in number of years assigned busing (they are only bused for 0-2 years). The dependent variables are continuous tract-level averages derived from the 2000 Decennial long-form data using sample weights. Log(avg. income) is the natural log of the average income in the tract, avg. income is the level of the average income in the tract, % with HS is the fraction of respondents in the tract with a high school diploma or more, % with BA is the fraction of respondents in the tract with a bachelors degree or more, % worked last year is the fraction of respondents in the tract who worked last year, and % worked last week is the fraction of respondents in the tract who worked last week. These averages are based on men aged 28-55 as of the 2000 Census. For tracts with a small number of respondents, the county-level average is used. I attach these tract-level outcomes to short-form respondents to use the largest sample possible. Essentially, this measures whether busing leads people to live in "better" neighborhoods later in life (i.e. neighborhoods with higher average income, higher education levels, higher levels of employment in the prior year and prior week). The regressions themselves are not weighted.
* $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$

Table 8. Grade-of-Assignment Effects on Neighborhood Characteristics in Adulthood

	Log(Avg. Income)	Log(Avg. Income)	% with BA	% with BA
	(1)	(2)	(3)	(4)
Grade First Assigned x Black	-0.00746	-0.0111***	-0.00331*	-0.00406***
	(0.00547)	(0.00282)	(0.00195)	(0.00117)
Grade First Assigned x White	0.000207	-0.000519	0.0000712	-0.00163***
	(0.000705)	(0.000579)	(0.000420)	(0.000386)
Assigned Busing x Black	0.109*	0.112***	0.0508**	0.0418**
	(0.0579)	(0.0399)	(0.0206)	(0.0165)
Assigned Busing x White	-0.00294	0.00505	-0.00179	0.0101*
	(0.00601)	(0.00816)	(0.00361)	(0.00563)
Year Fixed Effects	YES	–	YES	–
Year Trend + Controls	–	YES	–	YES
Observations	149,000	149,000	149,000	149,000
R^2	0.0863	0.0862	0.0257	0.0255

Notes: Standard errors are clustered at the level of variation, race by grade cohort by alphabet group. The sample in all specifications is men born in Jefferson County, KY (based on the Numident) and graduating in years 1965-1990 (based on year of birth, month of birth, and school entry rules) who respond to the short-form Census in 2000. Grade First Assigned x Black is a continuous variable equal to the first grade in which a respondent is coded as assigned busing (1st grade through 11th grade) based on grade cohort, race, and alphabet group if the respondent reports a non-white race and equal to zero if the respondent reports race as "white." Grade First Assigned x White is defined similarly. See Table 5 notes for definitions of Assigned Busing x Black and Assigned Busing x White. All specifications alphabet group fixed effects interacted with race fixed effects. The columns with year fixed effects include grade cohort fixed effects interacted with race fixed effects. The columns with a year trend and controls include linear trends in grade cohort, number of years assigned busing, and year first assigned busing interacted with race fixed effects. The dependent variables are continuous tract-level averages derived from the 2000 Decennial long-form data using sample weights. Log(avg. income) is the natural log of the average income in the tract, and % with BA is the fraction of respondents in the tract with a bachelors degree or more. These averages are based on men aged 28-55 as of the 2000 Census. For tracts with a small number of respondents, the county-level average is used. I attach these tract-level outcomes to short-form respondents to use the largest sample possible. Essentially, this measures whether busing leads people to live in "better" neighborhoods later in life (i.e. neighborhoods with higher average income, higher education levels, higher levels of employment in the prior year and prior week). The regressions themselves are not weighted.
* $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$

Table 9. Effects Adjusted to Account for Measurement Error and Non-compliance

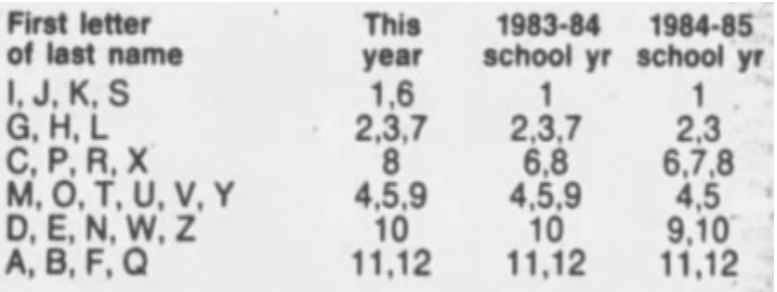
	Log(Avg. Income), Measurement Error Adjustments				Log(Avg. Income), Take-up Adjustments		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Assigned Busing x Black							
Coefficient Estimate	0.0336	0.0404	0.0429	0.0512	0.1126	0.0660	0.0512
Lower Bound of Confidence Interval	0.0080	0.0096	0.0102	0.0122	0.0269	0.0157	0.0122
Upper Bound of Confidence Interval	0.0592	0.0711	0.0756	0.0902	0.1983	0.1162	0.0902
Assigned Busing x White							
Coefficient Estimate	-0.0015	-0.0018	-0.0023	-0.0028	-0.0062	-0.0037	-0.0033
Lower Bound of Confidence Interval	-0.0058	-0.0071	-0.0091	-0.0113	-0.0248	-0.0146	-0.0133
Upper Bound of Confidence Interval	0.0029	0.0035	0.0045	0.0056	0.0124	0.0073	0.0066
Adjustment Factor	Nothing	Pr(In KY | Born in KY)	Pr(In Public School, pre-1975 | In Jeff. Co.)	Pr(In JCPS/LCS | White/Black & In JCPS or LCS)	Pr(Take-up, Yearbooks)	Pr(Take-up, Yearbooks w/ Adjustment)	Pr(Take-up, Cunningham et al. White Flight)
Black Students	–	0.8352	0.9689	0.8382	0.4250	0.7083	–
White Students	–	0.8149	0.7780	0.8045	0.4894	0.7529	0.85
Observations	149,000	149,000	149,000	149,000	149,000	149,000	149,000

Notes: See Table 5 for general notes about how the coefficients in column 1 are estimated. In column 2, I adjust the estimate in column 1 to account for measurement error induced by students migrating out of Jefferson County, KY prior to the desegregation order. The statistic used for this adjustment is from Table 3, Panel A. In column 3, I further adjust the estimate in column 2 to account for measurement error induced by students attending private school in Jefferson County, KY prior to the desegregation order. The statistic used for this adjustment is from Table 3, Panels B & C. In column 4, I further adjust the estimate in column 3 to account for measurement error induced by students attending majority-other race schools prior to the desegregation order. The statistic used for this adjustment is from Table 3, Panel D. Column 5 adjusts the estimate in column 4 by a lower-bound measure of busing take-up (from Table 4, column 2). Column 6 uses a scaled measure of take-up that accounts for the fact that yearbook-to-yearbook match rates are low even in pre-desegregation years (from Table 4a, Panel C). Finally, column 6 uses a measure of take-up derived from Cunningham et al. (1978). Note, the adjustments in columns 5-7 are not cumulative (the adjustments in columns 1-4 are). Note, this table also includes a crude adjustment of the lower and upper bounds of the confidence intervals for these coefficients. This is not ideal and does not account for error in the adjustment factors. Nevertheless, it gives some sense of how these intervals change.

* $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$

Appendix A. Supplementary Materials

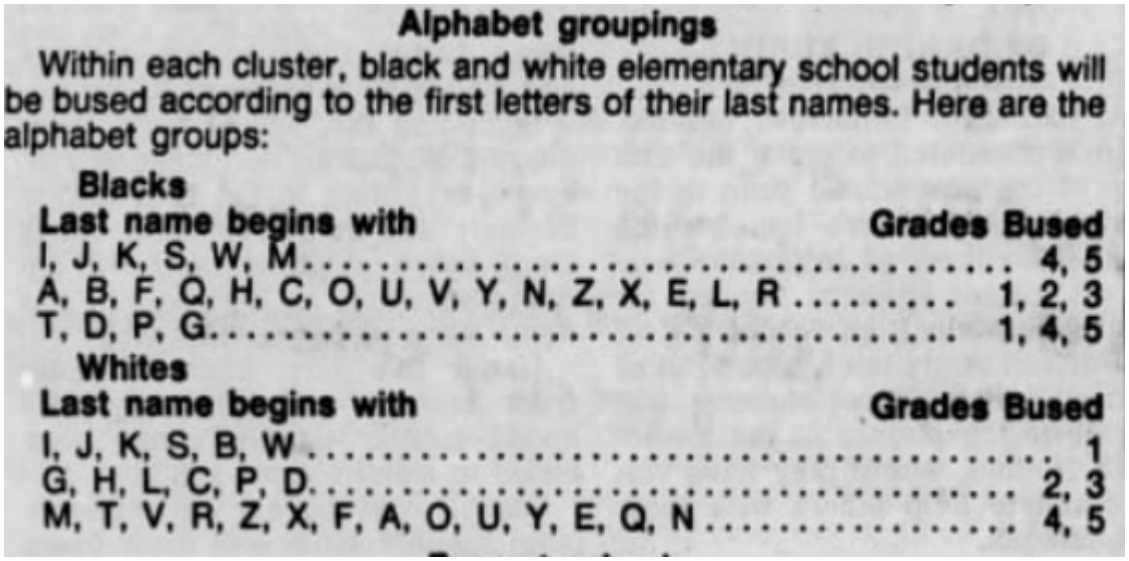
Figure A1a. Busing Assignment Plan Change in Jefferson County, KY–1982



First letter of last name	This year	1983-84 school yr	1984-85 school yr
I, J, K, S	1,6	1	1
G, H, L	2,3,7	2,3,7	2,3
C, P, R, X	8	6,8	6,7,8
M, O, T, U, V, Y	4,5,9	4,5,9	4,5
D, E, N, W, Z	10	10	9,10
A, B, F, Q	11,12	11,12	11,12

Notes: The plan depicted above was printed in an issue of The Courier-Journal. The original alphabet plan is displayed in Figure 1a. This plan details a change that was made for white students in 1982. In 1985, the district adopted a zoning system for middle and high school students, abandoning the alphabet plan for those students. In 1991, the district moved to a zoning system for elementary school students.

Figure A1b. Busing Assignment Plan in Jefferson County, KY–1985



Alphabet groupings

Within each cluster, black and white elementary school students will be bused according to the first letters of their last names. Here are the alphabet groups:

Blacks

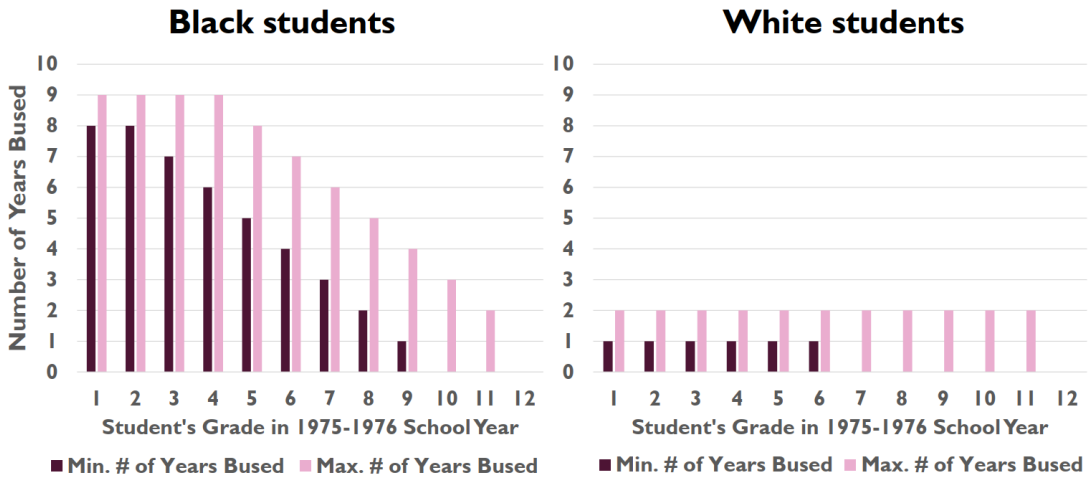
Last name begins with	Grades Bused
I, J, K, S, W, M	4, 5
A, B, F, Q, H, C, O, U, V, Y, N, Z, X, E, L, R	1, 2, 3
T, D, P, G	1, 4, 5

Whites

Last name begins with	Grades Bused
I, J, K, S, B, W	1
G, H, L, C, P, D	2, 3
M, T, V, R, Z, X, F, A, O, U, Y, E, Q, N	4, 5

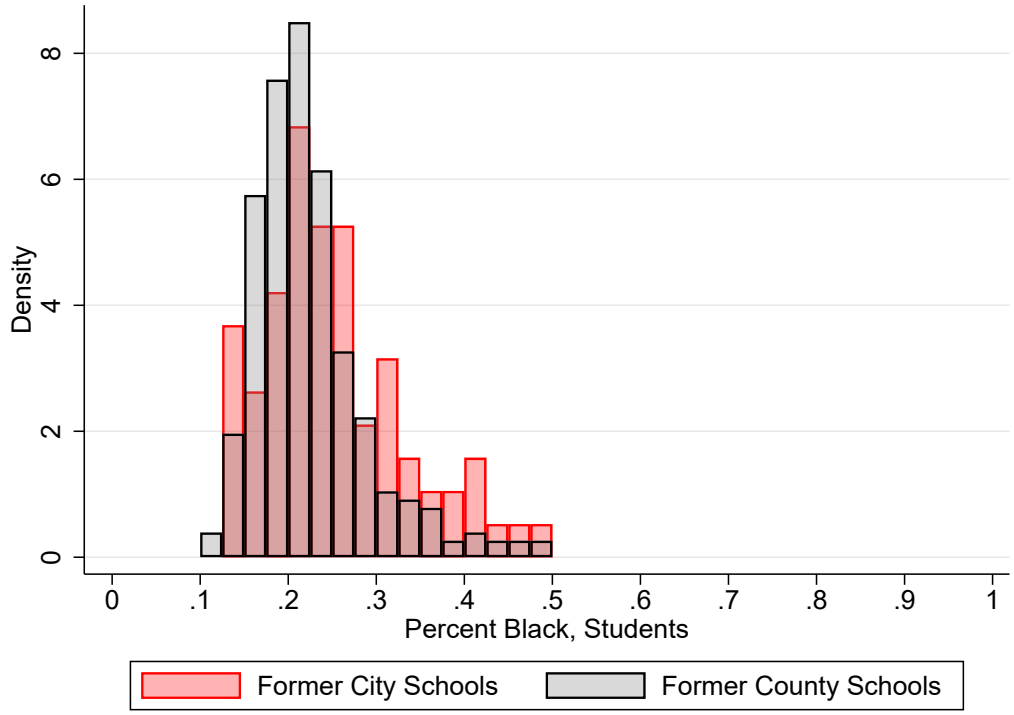
Notes: The plan depicted above was printed in an issue of The Courier-Journal. The original alphabet plan is displayed in Figure 1a. In 1985, the district adopted a zoning system for middle and high school students, abandoning the alphabet plan. The plan for elementary school students in 1985 is displayed above. In 1991, the district moved to a zoning system for elementary school students.

Figure A1c. Potential Variation in Busing Assignment for Student in 1975-76



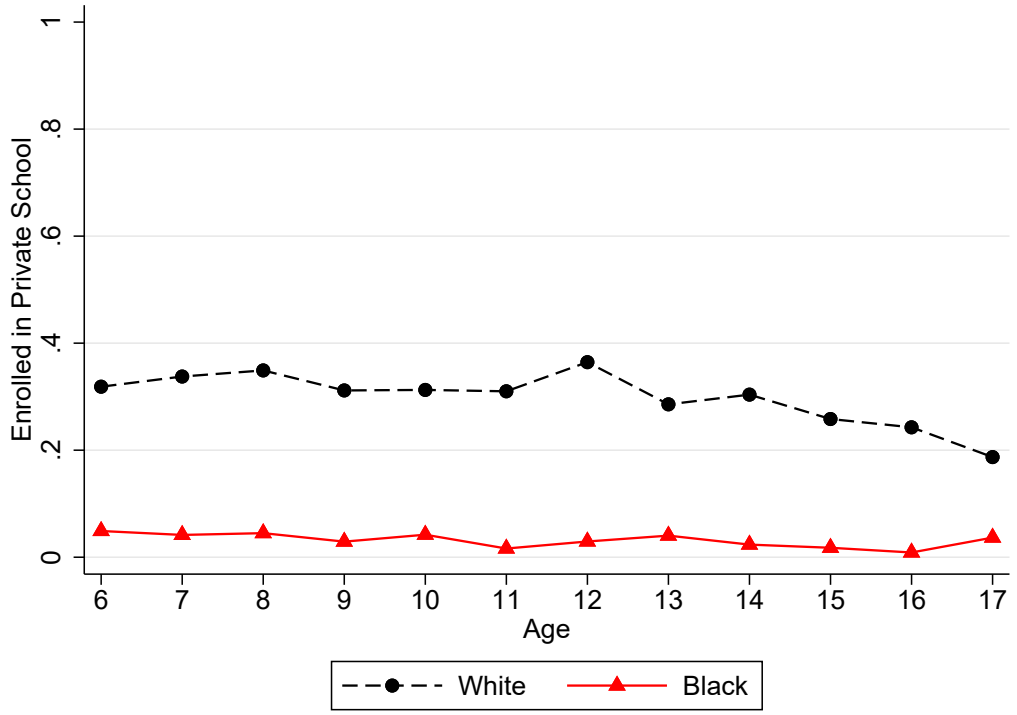
Notes: The figure above displays the potential variation in busing assignment for students attending the merged district in 1975-76. The mininum number of years assigned and the maximum number of years assigned by race are based on the plan displayed in Figure 1a.

Figure A2. Histogram of Percent Black in School, 1976-1982



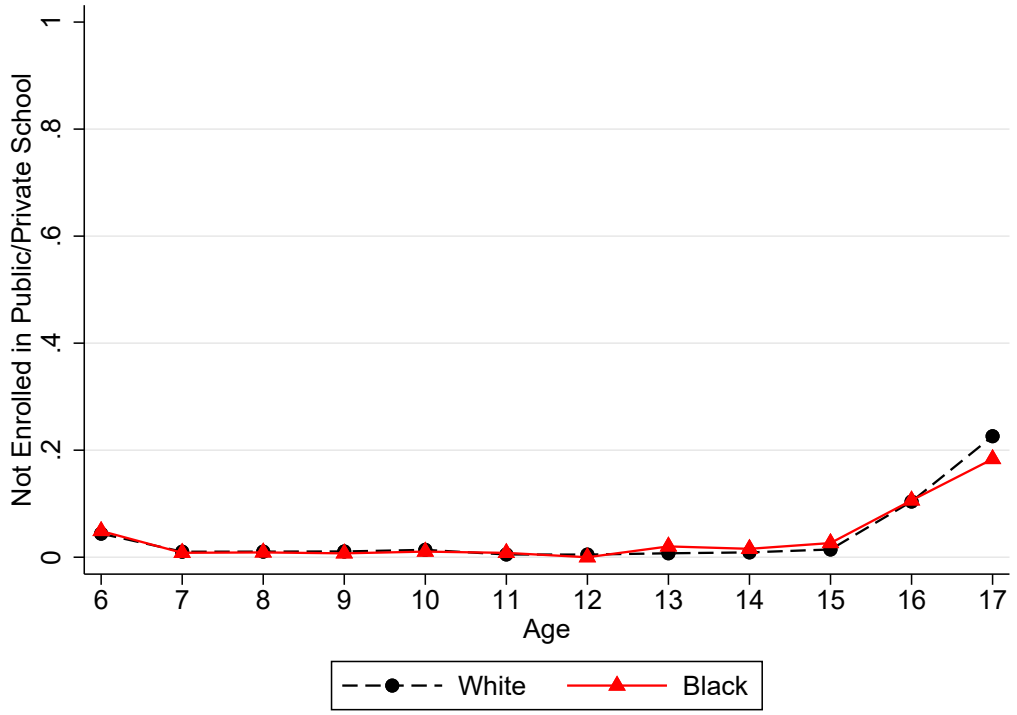
Notes: Figure A2 is calculated from historical Office of Civil Rights data. The figure above plots a histogram of the percent black in each school for former City schools (red) and former County schools (gray).

Figure A3a. Private School Enrollment in 1980, by Race and Age



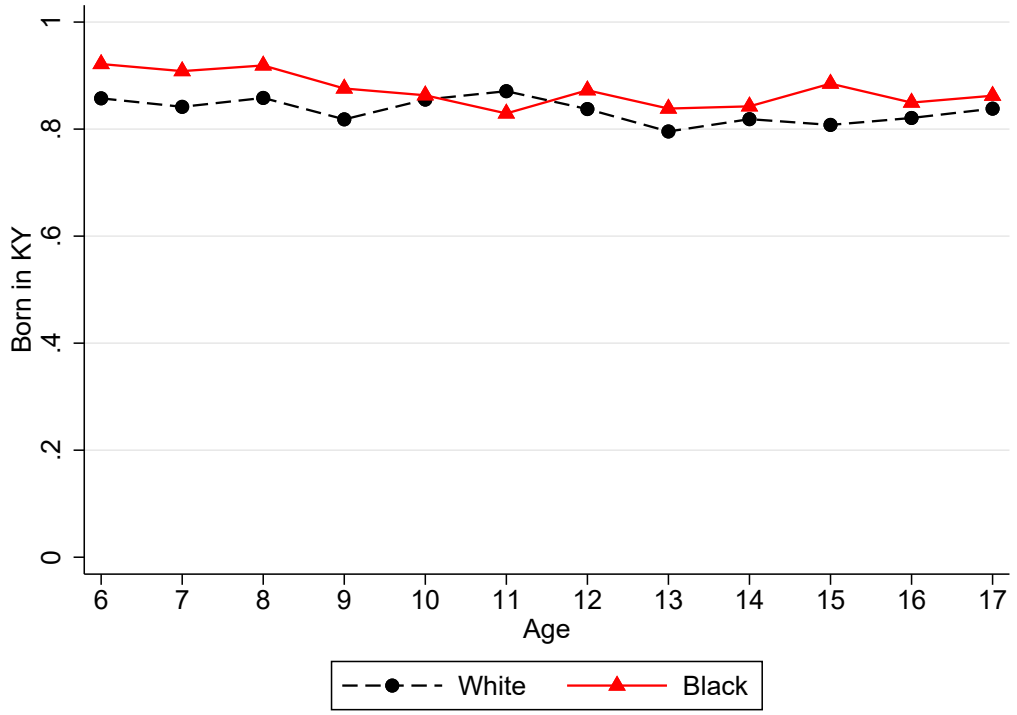
Notes: Figure A3a is calculated from publicly available Decennial Census data from 1980. In this figure, I plot the fraction of children enrolled in private school in Jefferson County, KY in 1980 for ages 6-17 and by race. Enrollment in private school is fairly constant from ages 6-15 but falls at ages 16 and 17. This is due to a rise in non-enrollment.

Figure A3b. No School Enrollment in 1980, by Race and Age



Notes: Figure A3b is calculated from publicly available Decennial Census data from 1980. In this figure, I plot the fraction of children not enrolled in school in Jefferson County, KY in 1980 for ages 6-17 and by race. Non-enrollment is fairly constant from ages 6-15 but increases at ages 16 and 17.

Figure A3c. Fraction Living in KY Who Are Born in KY in 1980, by Race and Age



Notes: Figure A3c is calculated from publicly available Decennial Census data from 1980. In this figure, I plot the fraction of children living in KY as of 1980 who were born in KY for ages 6-17 and by race.

Table A1a. Differences in Racial Composition between Former City and Former County Schools, post-1975

	Percent Black								
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Former City School	0.0265	0.0306*	0.0174	0.0184	0.00598	0.0356	0.0254	0.00772	
	(0.0189)	(0.0170)	(0.0174)	(0.0189)	(0.0197)	(0.0216)	(0.0205)	(0.0279)	
Former City School x 1976									0.0160
									(0.0180)
Former City School x 1978									0.00835
									(0.0221)
Former City School x 1980									0.0357
									(0.0217)
Former City School x 1982									0.0354
									(0.0219)
Constant	0.271***	0.227***	0.267***	0.258***	0.277***	0.335***	0.321***	0.203***	0.254***
	(0.0320)	(0.0250)	(0.0318)	(0.0324)	(0.0327)	(0.0381)	(0.0379)	(9.31e-10)	(0.0298)
Years Included	'78, '80, '82	'76, '80', '82	'76, '78, '82	'76, '78, '80	'76, '78, '80, '82	'76, '78, '80, '82	'76, '78, '80, '82	'76	'76, '78, '80, '82
Excluding Schools	-	-	-	-	Grade=1	Grade=7	Grade=12	Grade=1, Grade=7	-
Observations	280	282	293	291	130	300	312	18	382
R-squared	0.105	0.167	0.158	0.162	0.239	0.196	0.185	0.066	0.156

Notes: Table A1a is derived from historical Office of Civil Rights data. In this data, I classify schools as former City schools if they were in the Louisville City Schools district and not in the Jefferson County Schools district from 1968-1974. In the 1976-82 data, I remove schools with low student populations (less than 200) and with an abnormally high percentage of black students (above 50%). On inspection, these are primarily non-traditional schools, such as vocational schools. Columns 1-8 examine racial composition at former City (vs. former County) schools, making various retrictions outline above. Column 9 estimates the relationship by year.

* $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$

Table A1b. Differences in Free/Reduced Lunch between Former City and Former County Schools, post-1975

	Percent Free/Reduced Price Lunch		
	(1)	(2)	(3)
Former City School	0.0345	-0.00567	0.123***
	(0.0260)	(0.0231)	(0.0376)
Constant	0.310***	0.331***	0.265***
	(0.0327)	(0.0293)	(0.0460)
Observations	95	37	100
R-squared	0.358	0.670	0.389

Notes: Table A1b is derived from historical Office of Civil Rights data. In this data, I classify schools as former City schools if they were in the Louisville City Schools district and not in the Jefferson County Schools district from 1968-1974. In the 1976-82 data, I remove schools with low student populations (less than 200) and with an abnormally high percentage of black students (above 50%). On inspection, these are primarily non-traditional schools, such as vocational schools. The table above examines the percent of students with free or reduced price lunch at former City (vs. former County) schools. In column 1, I remove 5 elementary schools with outlier rates of free or reduced price lunch (above 50%). Column 2 removes all elementary schools from the sample. Column 3 shows the regression with no restrictions. One caveat with this data is that the total number of children receiving lunch (full price + reduced price + free) is often much lower than the total number of children at the school.

Table A1c. Differences in Racial Composition between Former City and Former County Schools, post-1975

	Gifted and Talented Program	Total Teachers	Student-Teacher Ratio	Suspension Rate
	(1)	(2)	(3)	(4)
Former City School x 1976	-0.0916**	-2.965**	0.495	0.00312
	(0.0406)	(1.202)	(0.811)	(0.0112)
Former City School x 1978	-0.125*	-7.398	0.160	0.00922
	(0.0639)	(5.101)	(2.438)	(0.00959)
Former City School x 1980	-0.121*	0.626	-0.566	-0.0299**
	(0.0725)	(2.807)	(1.763)	(0.0119)
Former City School x 1982	-0.176*			-0.0224***
	(0.106)			(0.00820)
Constant	-0.0551	17.49***	16.73***	0.0386**
	(0.117)	(3.674)	(3.028)	(0.0166)
Observations	382	242	242	382
R-squared	0.130	0.859	0.049	0.675

Notes: Table A1c is derived from historical Office of Civil Rights data. In this data, I classify schools as former City schools if they were in the Louisville City Schools district and not in the Jefferson County Schools district from 1968-1974. In the 1976-82 data, I remove schools with low student populations (less than 200) and with an abnormally high percentage of black students (above 50%). On inspection, these are primarily non-traditional schools, such as vocational schools. The table above examines the presence of gifted and talented programs, student-teacher ratio, total teachers, and suspension rates over time.

Table A2. Distribution of Surnames by First Initial, from Data on Top 100 Surnames

Alphabet Group	Percentage of Surnames
'A', 'B', 'F', 'Q'	0.165
'G', 'H', 'L'	0.171
'C', 'P', 'R', 'X'	0.171
'M', 'O', 'T', 'U', 'V', 'Y'	0.160
'D', 'E', 'N', 'W', 'Z'	0.157
'I', 'J', 'K', 'S'	0.175

Notes: The statistics above are calculated using publicly available data on the top 1,000 surnames in the United States. Surnames are removed if the percentage of individuals who are black or white is less than 75 percent.

Table A3. Summary Statistics on Women Aged 28-55 in Jefferson County, KY from the Public Sample of the 2000 Decennial Census

	White Women	Black Women
	(1)	(2)
Income	25448.8	22043.4
	(26890.1)	(25672.2)
High School Degree	0.924	0.886
	(0.265)	(0.318)
Bachelors Degree	0.314	0.128
	(0.464)	(0.334)
Worked Last Year	0.826	0.801
	(0.379)	(0.399)
Born in KY	0.699	0.766
	(0.459)	(0.423)
Institutionalized	0.00186	0.00379
	(0.0430)	(0.0615)
Observations	6,139	1,296

Notes: Table A3 is derived from the publicly available sample of the 2000 Decennial Census. I limit the sample to women aged 28-55 and living in Jefferson County, KY as of 2000. I report averages on income, educational attainment, employment, state of birth, and institutionalization by race. Standard deviations are in parentheses.