Southern Academies: The Proliferation of All-White Private Schools After *Brown* and their Legacy for Students[*]

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We document White backlash to the public school integration, and show its negative long-run consequences for White students. Segregationists responded to *Brown vs. Board of Education*, which mandated *de jure* racial integration, by establishing all-White private academies to recreate *de facto* segregation. We compile new data on the growth of these segregation academies, identifying over 650 schools established in the 1960s and 70s across the Southeastern United States. We estimate that the establishment of a segregation academy caused large and persistent declines in White public school enrollment, decreased public school integration, and eroded local and state funding for public education. To measure the long-run impacts on educational attainment and employment, we use large-scale restricted survey and administrative data along with variation in access to academies based on birth year and birth county. We find no effects on the educational attainment and employment of Black students, but negative effects on White students. This evidence suggests White families may have sacrificed educational quality to maintain racial segregation.

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1 — Introduction

> "Throughout interviews with local respondents of both races, there was only one area of clear agreement regarding the two systems: that the private schools were for whites and the public schools were for blacks [sic]."
> *Johnson and Pearson 1982*

Social institutions can perpetuate discrimination, which causes extraordinary and persistent inefficiencies. Activists and policy makers often seek to combat institutional discrimination, but in doing so, risk triggering backlash and unforeseen consequences. For instance, school districts in the U.S. South operated public school systems segregated by race for more than a century after emancipation. The 1954 Supreme Court case *Brown v. Board of Education* declared this status quo unconstitutional, fundamentally reshaping education in the United States. The typical narrative of this transformation recounts how the landmark judicial successes of the Civil Rights movement thwarted segregationists' attempts to physically and violently obstruct public school integration.[1] Less well remembered, however, is a more durable legacy of the backlash to *Brown*: the hundreds of White-only private schools established by segregationists.

Though contemporaries described these schools as "perhaps the most important kind of local resistance" (Orfield 1969), the prevalence of these segregation academies has not been well-documented. Further, we know little about the effects of the establishment of these schools on students and public schools. We explore whether, and how much, these academies – many of which exist in some form today – undercut *Brown's* mandate.

We build a data set of the locations and founding years of over 650 segregation academies across seven states in the Southeast: Alabama, Florida, Georgia, Louisiana, Mississippi, North Carolina, and South Carolina. Organizers established these segregation academies at different times, both in anticipation and in response to court-ordered integration. We use variation in timing and the location of academies to understand the effects of these segregation academies on the provision of public schooling and the long-run outcomes of Black and White students.

Strikingly, we find the most adverse effects on White students exposed to these segregation academies' establishment. Openings induce large and persistent declines in public school enrollment, driven by the exit of White students from the public sector, and decreases in public school

1. See, for example, former Arkansas Governor Orval Faubus, who temporarily blocked Black students (the "Little Rock Nine") from integrating Little Rock Central High School by mobilizing the Arkansas National Guard in 1957. Three years later, segregationists bombed the house of the youngest of these students, Carlotta Walls.

integration. For these White students, their parents sacrificed educational quality to maintain racial segregation for themselves. By contrast, Black students were largely unaffected by the establishment of local White-only segregation academies. Flipping the doctrine of "separate but equal," Black students gained access to higher-quality integrated public schools while some White families pulled their children into lower-quality White-only segregation academies.

We define segregation academies as all-White private schools founded, often by a formal or informal association of White parents, with the purpose of avoiding integration.[2] Combining historical sources, contemporary newspaper articles, existing qualitative work on segregation academies, and sports schedules, we develop and apply guidelines to categorize all schools appearing in the 1976-1980 Universe of Private Schools. The resulting novel data set contains the location and founding years of these academies across the Southeast and is the first comprehensive data set to distinguish segregation academies from other types of private schools that were not founded to perpetuate racial segregation.

The introduction of segregation academies marked a seismic shift from public to private education unique to the South: rates of private school attendance decreased from 1960 to 1980 in the Northeast, Midwest and West, but doubled in the Southeast (Manson et al., IPUMS National Historical Geographic Information System: Version 17.0 [Dataset]). Prior to 1960, few rural communities in the Southeast had private schools meant to serve the general White population. These low income, population sparse areas could not support a schooling market with both private and public options.

Segregation academies fit neatly into the gap in the schooling market created by the threatened expiration of the all-White public school. In rural areas, academies offered low tuition in an attempt to capture parents with a strong preference for segregation but had little means to relocate or afford a traditional private school (Johnson 1971b). In urban areas, academies took different approaches depending on the more competitive market landscape. Some advertised themselves as college prep schools and charged tuition in line with other schools; others acted like the rural schools and kept tuition low to capture the new entrants to the private school market; all distinguished themselves by the racial composition of their study body.

It follows that academies were established in places where parents had a higher willingness to pay for segregated schooling: pre-existing racial animus and the percentage of the population that was Black in 1950 are the most salient predictors of segregation academy placement. The preference

2. See work by historians and journalists, Nevin and Bills 1976; Walder and Cleveland 1971; Sheffield 2022; Harris 2019; Gladney 1974; Champagne 1973; Johnson 1971a.

for segregation persists to today. While most segregation academies eventually integrated, those remaining open are less diverse and have lower rates of college matriculation than other private schools, even after conditioning on area characteristics.

This lower rate of college matriculation suggests that segregation academies, which were typically not subject to accreditation standards or state monitoring, may have provided poor quality schooling, negatively impacting White attendees. Even contemporary supporters of segregation academies warned of a "rash of fly-by-night private schools established by mountebanks and quacks" (Lassiter and Lewis 1998). Alternatively, segregation academies may have consolidated resources and opportunities among the White population, which had higher average incomes and educational attainment than the Black population because of the long history of labor market and educational discrimination in the South (Margo 1990; Althoff and Reichardt 2024). Williamson and Holcomb 2025 finds suggestive evidence that segregation academy exposure preserves racial animus among younger White people. By reinforcing the racial hierarchy in a community, academies could reduce labor market competition and improve the labor market outcomes of White students and worsen those of Black students. Indirectly, segregation academies could also have negative spillovers on the public schools, potentially making hiring qualified teachers more difficult and reducing funding for public schools.

We first investigate the effect of the establishment of segregation academies on public schools. We use an event study design that relies on variation in the location and timing of establishment of segregation academies to estimate effects on public school enrollment, funding, and inputs from 1960-1985 as recorded in annual reports from State Departments of Education. This strategy compares changes in outcomes after the establishment of a segregation academy to changes over the same period in places without a segregation academy.

We interpret our estimand as the marginal public enrollment and funding changes induced by the opening of a segregation academy. To justify this interpretation, we must deal with three main potential concerns. First, the opening of a segregation academy may coincide with another event, such as court-ordered desegregation. To address this, we control for the timing of court-ordered desegregation. Second, places with segregation academies had different characteristics than those without. This means we must be careful in our selection of a control group. Places with segregation academies are compared to not-yet and never-treated places. For robustness, we conduct a matching exercise and weight never-treated places to match pre-treatment characteristics of treated observations. Third, we address the concerns raised in the literature about staggered treatment designs by following Sun and Abraham 2021 and allowing treatment effects to vary by initial year of treatment.

We show that the opening of a segregation academy led to large and persistent decreases in public enrollment among White children, but had no effect on Black public school enrollment. In states that recorded enrollment by race (Alabama and Louisiana), White enrollment decreased by approximately 31%. On average, the opening of a segregation academy induced a 9-10% overall decrease in total public school enrollment.

Effects are largest in rural counties with a history of racial animus,[3] low household income, and a high percentage of Black population. These results are consistent with a theoretical framework of taste-based discrimination and costly outside options that we develop to precisely define our target estimand. This model predicts the *marginal* effect of the opening of a segregation academy will be larger in areas where 1) White parents have a high willingness to pay for segregated schooling and 2) the family income constraint is binding in the absence of a segregation academy. Segregation academies relaxed this constraint by reducing the distance and monetary cost of a segregated option.

Segregation academies also negatively affected funding for public schools: state revenue decreases by 10% and local revenue decreases by 8% after the opening of a segregation academy. Decreases in enrollment mechanically reduce funding: state funding is often at least partially determined by average daily attendance numbers. While methods of local funding allocation varied by state and district, most methods involved some measure of voter input. Segregation academies could encourage "open opposition to public education" (Brown 1969) by racializing the public-private education divide (Alesina, Baqir, and Easterly 1999). To evaluate the share of the revenue effects explained by enrollment declines, we regress the district-level estimated funding effects on the corresponding enrollment effects after removing fixed effects. Descriptively, residual enrollment declines primarily explain residual state funding declines ($R^2 = 0.67$). Enrollment declines explain a smaller share of the local funding declines ($R^2 = 0.28$), suggesting voters may have been less willing to fund public education. Federal revenue, which we would not expect to be affected by the establishment of a segregation academy, does not change, and thus backstops the state and local declines. As a result, the decrease in total revenue is smaller than the decrease in enrollment. This translates to an increase in per pupil funding (albeit a small and statistically insignificant one).

In our second set of results, we study the long-run effect of these schools on adult labor market, educational attainment, and residential segregation outcomes on individuals who were school-aged when a segregation academy opened near them. We measure these impacts using large-scale restricted survey and administrative data from the U.S. Census Bureau. We begin by proxying for county of schooling using place of birth information from the Social Security Administration

3. Proxied by 1948 vote share for Strom Thurmond, a segregationist presidential candidate.

(SSA) Numident linked to the American Community Survey (ACS) from 2001 through 2019 and the 2000 Long Form Decennial Census. These surveys allow us to measure a variety of outcomes in prime age (ages 25-54), including human capital and labor market measures such as educational attainment, job skills, receipt of government aid, and employment. To measure neighborhood segregation in adulthood, we also use filing addresses of the near-universe of Southern-born students found later in life in the Internal Revenue Service (IRS) Information Returns.

We employ a cohort-style event study difference-in-differences specification to causally identify the effect of these academies on students' long-run outcomes, again leveraging the timings of opening of these academies across counties from 1960-1980 and adapting Sun and Abraham 2021 to account for issues with staggered treatment designs. The outcomes of treated individuals — those of school age when a segregation academy opened nearby — are compared to (1) older individuals within the same county and (2) individuals in not-yet- or never-treated counties born in the same year. Crucially, by defining exposure by birth county rather than segregation academy attendance, we mitigate within county selection concerns. We further contribute to the literature that uses birth location as recorded in the Numident as a proxy for childhood exposure by developing a novel empirical Bayes estimator that accounts for this potential differences in birth and childhood location.[4] We find our results are robust to any potential mismeasurement of childhood location resulting from the use of place of birth.

We find no effect on educational attainment or employment of exposed Black students. These results may be explained by changes in the racial composition of the teacher workforce. In South Carolina, average teacher salary decreases after the opening of a segregation academy. But evidence suggests this reflects a change in demographics[5] rather than teacher quality: White teacher employment from 1960 to 1972 was 12% lower in places with segregation academies relative to trends in places without, but Black teacher employment was 11% higher. [6] Black students could potentially benefit from this shift: there exists a literature on the positive effect of racial concordance on the student outcomes of minority students (Dee 2005, 2004).

We find that segregation academies negatively affected White students. White students aged six when a segregation academy opened are two percentage points less likely to graduate high school or obtain a GED, three percentage points less likely to enter college, and two percentage points less likely to be in the labor force in prime age. These results suggest that, ultimately, this backlash

4. See, for example, Bailey, Sun, and Timpe 2021 and Anstreicher, Fletcher, and Thompson 2022.

5. Black teachers were paid less than their White counterparts in South Carolina (Cascio and Lewis 2024).

6. "White teachers... resign... from public schools in droves to take jobs in the segregation academies" (Whiteford 1969).

to school integration harmed White students. A back of the envelope decomposition suggests this negative effect is a combination of White families sacrificing educational quality in order to maintain racial segregation — that is, direct effect of segregation academy attendance — and negative spillovers on White public school attendees.

1.1. Related Literature

The effects of school integration are a first order question in both labor and education economics. Papers focus on two themes: shifts in enrollment patterns after schools integrated and the long run effects of school integration on students. First, these papers find that court-ordered integration causes White enrollment in public schools to decline 14-24%, with the effects largest in central urban districts where parents had access to suburbs and preexisting private school networks (Baum-Snow and Lutz 2011; Reber 2005; Welch and Light 1987; Rivkin 1994; Boustan 2012; Clotfelter 2001; Reber 2005). Our results suggest the past literature has underappreciated enrollment losses related to school desegregation in rural areas because the precipitating event is the creation of an outside option (a segregation academy) rather than a court order. Our results also underscore the importance of considering the contribution of private schools to school segregation, especially in sparsely populated areas where the link between residential segregation and public school segregation is weak.

Second, papers find court-ordered integration improved the labor market and academic outcomes of Black students (Johnson 2011; Rivkin 2000; Anstreicher, Fletcher, and Thompson 2022; Ashenfelter, Collins, and Yoon 2006; Guryan 2004). The primary mechanism driving these results is funding: integration moves Black children from poorly funded all-Black schools to better funded integrated schools (Reber 2010; Cascio et al. 2010; Anstreicher, Fletcher, and Thompson 2022). Since White students already attended well-funded public schools, these studies find no effect for them; district-wide funding increases primarily benefited Black students. Our findings suggest that segregation academies temporarily reversed this dynamic, as some White families moved from relatively well-funded public schools to newly established, often underfunded private schools.

Our setting captures the first episode in U.S. history of large-scale, rapid movement into the private sector. Furthermore, because enrollment patterns splintered by characteristics observable in the Census — that is, race — we are able disentangle effects on students eligible for attending schools and spillovers on those who could not attend. Our results speak directly to two concerns: first, when a program is rapidly adopted and supply and demand are subsidized,[7] low quality

7. Voucher programs trace their origins in the South to this period: Alabama, Georgia, Louisiana, Mississippi and

schools will emerge (Ford and Andersson 2019; Abdulkadiroglu, Pathak, and Walters 2015). Second, parental preferences over student body composition can distort evaluations of schools' effect on educational outcomes, especially when these preferences are informed by social networks.

More broadly, we contribute to a literature on how the role of backlash and beliefs shape the impacts of social movements and policies. Derenoncourt 2022 shows that the reaction of destination places can mitigate the expected effect of moving to a higher-opportunity neighborhood. Similarly, we find the establishment of segregation academies — a reaction to the movement of Black students to ostensibly higher-opportunity schools — mitigates the beneficial effect of court orders. Wheaton 2022, which lays out a theoretical model and uses survey evidence to describe this dynamic, shows that backlash is strongest among men with children, who theoretically wish to preserve the threatened ideological status quo by imparting their beliefs to their children. Acemoglu and Robinson 2008 argues that the loss of *de jure* power will prompt the elite to act to concentrate *de facto* power; Ang 2019, which studies a similar time and place as our work, describes how racially conservative Whites mobilized to offset the effect of the Voting Rights Act. We contribute to this literature by measuring the evolution and effect of the backlash to one of the most significant social policies impacting Southern children and society — school integration.

1.2. Outline

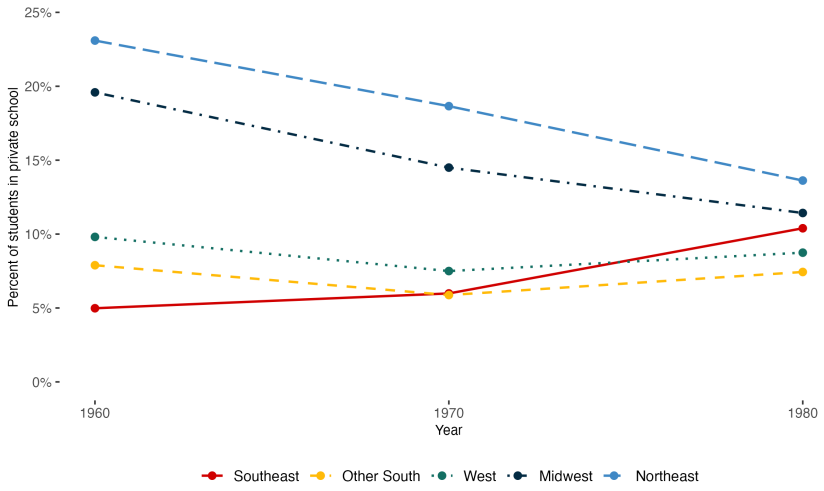
In Section 2, we give additional historical context. We describe the creation of our data set of segregation academies and descriptive characteristics of the schools in 3. We describe our definition of treatment and the spatial and temporal variation we rely on in our analysis in 4. 5 outlines a theoretical framework to precisely define our target estimand: the marginal White public enrollment loss induced by the opening of a segregation academy. 6 describes the data and empirical strategy for our measurement of the effects on public school enrollment and funding and teachers; 7 and 8 present these results. 9 describes the data and empirical strategy for our measurement of the long run effects on students; 10 and 11 present the results for White and Black students, respectively.

South Carolina all passed state tuition grants during this period (DeMatthews and Hart 2025)). While these programs were largely stayed by the courts or never fully funded, segregation academies received other explicit and implicit government support in the form of buildings, supplies, and tax credits.

2 — Historical Background

> "The outnumbered Whites 'will withdraw into an already flourishing private school system, abandoning the public schools to the blacks. . . Southern Regional Council, which works to promote better race relations, estimates 300,000 White students are attending what it calls 'segregation academies' this year–perhaps 10 times more than there were five years ago."
> *Montgomery, Jenkins, and Collins 1969*

Figure 1 — Private School Attendance, by Region



Notes: **The Southeast is our set of sample states: Alabama, Florida, Georgia, Louisiana, Mississippi, North Carolina, and South Carolina. Other South includes all other states in the South (as defined by the Census): Arkansas, DC, Delaware, Kentucky, Maryland, Oklahoma, Tennessee, Texas, Virginia, and West Virginia. Remaining regions follow the Census definition. Data *Source:* Ruggles et al. IPUMS USA: Version 14.0 [data set]. Minneapolis, MN: IPUMS, 2023.**

The term "segregation academy" first appeared in a 1969 report by the Southern Regional Council that described the segregated private schools that White students were fleeing to en masse (*The New York Times* 1969). While the report was published in 1969, at the peak of the segregation academy movement, schools began forming in the 1950s. As early as 1951, Georgia's governor, Eugene Talmadge, outlined an agenda that was "in effect . . . a plan to privatize the state's public school system" (Fitzpatrick 2023). Talmadge did not act alone: private schools received similar endorsements from the governors of other southern states.[8] From 1960 to 1980, private school attendance in the Southeast doubled (Figure 1).[9]

8. See, for example, George Wallace in Alabama (*The Montgomery Advertiser* 1963), Jimmie Davis in Louisiana (*The Shreveport Journal* 1960), and Paul Johnson in Mississippi (*The Clarksdale Press Register* 1964).

9. The shift to private schools during this time period was unique to the Southeast: because of a combination of the decline in Catholic schools and poor economic conditions, private school attendance fell in every other region.

In 1950, the Court ruled in favor of prohibiting segregation in higher education (*Sweatt v. Painter* and *McLaurin v. Oklahoma State Regents*). This set the stage for the White student exodus from public schools to schools like the Ar'lyn Worth School, established in 1950 in Atlanta, Georgia. While information about the founding circumstances of Ar'lyn Worth is scarce, the school was expelled from Georgia Association of Independent Schools in 1972 for "maintaining segregationist ties" (Press 1972).[10] The rulings of the Supreme Court did not apply to these new private schools — an 1819 case, *Trustees of Dartmouth College v. Woodward*, established that private schools are "corporations," thus prohibiting the government from interfering with their contracts.

The exodus became more formal and widespread after the 1954 Supreme Court ruling on *Brown v. Board of Education* that public schools must desegregate, and the subsequent *Brown II* ruling in 1955 that desegregation must occur "with all deliberate speed."[11] The vague phrase "all deliberate speed" gifted segregationist policy makers and interest groups time to form strategies to circumvent integration. In 1959, Prince Edward County in Virginia voted to defund its public schools as part of a political campaign against integration called "Massive Resistance."[12] The same year, a group of parents founded the private Prince Edward Academy to educate the White students of the county. This school was the quintessential segregation academy, held up as a blueprint by parents' associations forming segregation academies across the Southeast. Public schools in Prince Edward County remained closed through 1964 (Nevin and Bills 1976).[13] By 1964, 90 segregation academies had been established in the Southeast.[14]

In 1964, the Civil Rights Act mandated that school districts must be non-discriminatory in order to be eligible for federal funds. In 1965, the Elementary and Secondary Education Act made unprecedented amounts of federal funding available to compliant districts. Accordingly, in 1965, many school districts, especially high poverty districts, began submitting voluntary plans of desegregation (Cascio et al. 2010). These "freedom of choice" plans gave Black parents the option to transfer their child to formerly all-White public schools. A combination of "overcrowding" clauses, which gave schools the ability to refuse students, intimidation of Black parents by White

10. Ar'lyn Worth remained a member of the Southeastern Association of Independent Schools (SEAIS). SEAIS was one of many emergent private school associations organized during this time to offer cover to the new segregated private schools.

11. See Appendix Table B.1 for an abbreviated legal history.

12. This phenomenon was not limited to Prince Edward County. Governor Orville Faubus closed senior high schools in Little Rock, Arkansas after a court-ordered the schools to desegregate. At least nine other schools in Virginia closed, displacing over 12,000 students (Fitzpatrick 2023).

13. From 1954-1964, the NAACP Legal Defense Fund began suing individual school districts, focusing on large urban southern districts. But "even when attorneys . . . brought a school district to court, many district judges tended to approve 'almost anything' the board was willing to submit as an integration plan" (Orfield 1969).

14. Author's calculations.

residents, and lack of support by government officials meant that most of these plans amounted to tokenism (Barry and Garman 1965). In spite of the limited effect of these plans on integration, there were 170 segregation academies in the Southeast by 1967.

Two notable examples of segregation academies and their effects on public school integration come from Alabama and Mississippi. One of these schools was Lowndes Academy in Lowndesboro, Alabama. Established in 1966 by an organization of parents known as the Lowndes County Private School Foundation, it was a founding member of the Alabama Private School Association (APSA), the analogous organization to SEAIS[15]. George Wallace, governor of Alabama, publicly endorsed the school, encouraging others to donate to the $\$500,000$ fundraising effort (*Alabama Journal* 1966). 95% of the students attending Lowndes Academy today are White.[16] In contrast, 99% of the students attending Central High, the local public high school, are Black.[17] In Jackson, Mississippi, the White Citizens' Council (WCC)[18] organized a system of 12 private "Council Schools." As early as 1960, this resulted in zero White students attending Jackson's public schools.[19] 55 days after *Brown v. Board of Education*, the WCC was founded in Jackson, Mississippi with the goal of maintaining school desegregation by creating an entirely separate school system for White students. In 1968, these schools became founding members of the Mississippi Private School Association (MPSA), an organization helmed by the editor of the official publication of the WCC, *The Citizen's Council* (*Mississippi Private School Association* 2023). The reach of the WCC extended beyond Mississippi: South Carolina and Alabama had particularly strong regional presences (*Citizens' Councils* 2016; Vaught 2003).

Courts began striking down freedom of choice plans as unconstitutional and implementing more significant plans after *Green vs. Connelly (1968)*. Cited as the start of "real desegregation," this ruling applied to school districts under an active or future court order. In 1969, the Supreme Court ruled that desegregation must begin immediately in *Alexander v. Holmes County Board of Education*, putting an end to the "all deliberate speed" language of *Brown II*. In 1971, *Swann vs. Charlotte-Mecklenburg* called for even stronger enforcement by the courts. 364 additional segregation academies were established from 1968-1971.

The segregation academy movement faded in the late 1970s: in 1978, the IRS began revoking

15. Other private school organizations that provided cover to segregation academies include South Carolina Independent School Association (SCISA), the "unstated purpose [of which] was to avoid the federally court-ordered racial desegregation of the public schools" (Turnipseed, n.d.) and the Mississippi Private School Association

16. Source: PSS Private School Universe Survey data for the 2021-2022 school year.

17. Source: Common Core of Data Public school data for 2022-2023 school year.

18. For a history of the WCC, see McMillen 1994.

19. Author's calculations

the tax-exempt status of explicitly segregated private schools, though paused the revocations from 1980-1982 at the behest of the Reagan administration (Babcock 1982). Even post 1982, tax exempt status continued for schools who advertised non-discriminatory admission statements. This resulted in the peculiar habit of many all-White schools that did not admit or discouraged applications from Black students advertising non-discriminatory policies.[20] Only the most extreme of these academies (and the ones unlucky enough to be subject to an audit) lost their tax exempt status as a result of this ruling (Lowndes Academy lost its tax exempt status in 1982). However, the combination of this ruling and the decrease in federal school integration activity meant that no new segregation academies were founded after 1980.

3 — A New Data Set of Segregation Academies

In this section, we discuss how we operationalize our definition of segregation academies within the social and political context of the time. To test our classification system, we compare the characteristics of our sample of segregation academies and segregation academy locations to other private schools.

3.1. Defining Segregation Academies

The narrowest definition of a segregation academy is an all-White private school founded with the purpose of avoiding integration. Some schools unambigiously meet this criteria. Examples include the previously described Council Schools and Lowdnes Academy, as well as Marlboro Academy, established in 1969 in Bennettsville, South Carolina. One of the founders, Frank B. Rogers Jr., told *The State*: "The object of this school is quality education. It is not a flight away from integration, although that does have some bearing on it" (*The State* 1969). We label these schools as "Confirmed" segregation academies.[21]

However, all-White schools that were segregationist in effect may not have been *explicitly* segregationist in intent. New private school parents pulled their children from the public schools for a

20. Taylor 1972 is an example of an article announcing the policy.

21. Frank B. Rogers Jr.'s statement hints at the obfuscation of intent common during this period. The IRS's threat to revoke the tax-exempt status of racially discriminatory schools (Kurlander 2022) invoked pecuniary consequences for openly racist admission policies. There existed social consequences as well: schools often distanced themselves from the negative connotations carried by the term "segregation academies," which appeared widely in newspaper and academic articles by the 1970s. A 1969 *Charlotte Observer* article title captures this effort: "Parents Call Goal Quality Education."

combination of racist, religious, and politically conservative reasons. Notably, in 1962, the Supreme Court ruled in *Engel v. Vitale* (1962) that school-sponsored prayer violated the First Amendment. Schools alluded to this in their advertisements: a 1975 placement for Victory Christian Academy in Millbrook, Alabama advertised "Private, Christian, Orderly" education as a contrast to the "Public, secular, disorderly" alternative. The founders of Victory Christian and other fundamentalist Christian schools established during this time period may legitimately state their mission as providing a much more explicitly faith-based and/or conservative approach to education than possible in the public school system. But emergent conservative ideologies, namely the New Right and Religious Right, tied these motivations together, making the individual personal motivations of each parent or founding organization difficult to disentangle (Perlstein 2020; Nevin and Bills 1976). For example, Goldsboro Christian School in North Carolina did not admit Black students because "We believe that God in his plan and purpose and wisdom separated men into . . . races and that those races should be preserved"(*The New York Times* 1982).

To develop a comprehensive list of segregation academies, we consider it essential to include historically all-White schools founded during this period that occupied similar spaces as confirmed segregation academies. These include schools that shared sports leagues, private school associations, and used similar language indicative of socially conservative ideologies (Orfield 1969; Blaiklock 2022; Perlstein 2020). We categorize these schools as "Likely" segregation academies in our analysis.

3.2. Classification of Segregation Academies

We create a data set of segregation academies, the most extensive catalogue of segregation academies in the Southeast currently available.[22] We rely on primary and secondary sources, especially contemporary newspaper articles, to develop our classification criteria. Appendix C contains a full description of the criteria used to classify schools into each category:

1. *Confirmed (16%)*: Schools confirmed as founded to avoid public school integration by other scholarly work or primary sources.

2. *Likely (21%)*: Historically all-White schools that had implicit, if not explicit, racial criteria for admission. We deduce this through a combination of news coverage, name, year founded, and other observable factors. This category includes many "Christian Schools"[23] and schools founded

22. Hobbyists have compiled lists of segregation academies on Wikipedia and in Renee 2022. Their work is gratefully acknowledged.

23. See Bowler 1970.

in the late 1960s and early 1970s that recount origin stories involving "concerned parents" on their respective school history pages.

3. *Uncertain (8%)*: Schools founded during the time period of 1954-1982 for which limited information exists. This also includes schools with indeterminable founding years that share traits with "Likely" segregation academies.

4. *Likely Not (10%)*: Schools that likely did not have explicit or implicit racial criteria for admission, but for which we do not have sufficient information to make a definitive call. This category includes many Catholic schools.

5. *Definitely Not (39%)*: Schools we can definitively rule out as being segregation academies; for example, schools for students with learning disabilities.

We begin by cleaning the data set "Universe of Private Schools Survey" (UPSS), which contains all private schools in operation during 1976, 1977, 1978, or 1979 that reported statistics to the National Center of Education Statistics (NCES). This report contains enrollment data, school name, address, highest grade, lowest grade, and religious affiliation, among other characteristics, but does not contain the two pieces of information crucial for our analysis: whether or not the school is a segregation academy and year founded. We manually collect year founded during the classification process from newspaper articles, school websites, and, occasionally, tax records. If not enough information can be found about the school (for example, if founding year is indeterminable), we omit it from our sample. Appendix Figure A.1 shows the distribution of schools among labels. Catholic schools comprise the majority of schools in the survey; these are classified as "Likely Not."

It is possible that we have inadvertently omitted some segregation academies from our data set (Nevin and Bills 1976 acknowledges that some academies did not report statistics to the NCES). By comparing enrollment numbers in the UPSS to enrollment numbers in the 1980 Decennial Census, we estimate the UPSS has a private school coverage rate of elementary and high schools of approximately 80%: average yearly enrollment in segregation academies was 213,729; average yearly enrollment in all private schools was 507,297. For comparison, the 1980 census reports that 636,558 students were enrolled in private schools during this period. Additionally, we have audited our data set with a data set assembled by news organization.[24] We identified and added 22 segregation academies mentioned in other historical records or by another source that did not report statistics to the NCES and thus were not included in the UPSS report. However, some

24. This data set was compiled using algorithmic rules for a story that was never released by a news team, who shared with the authors.

districts are classified as control districts during years when they are, in fact, treated. We expect the effect of these academies and any other missing academies to attenuate the absolute value of our estimates.

3.3. Characteristics of Segregation Academies

Table 1 — Descriptive Characteristics of Segregation Academies vs. Non Segregation Academies

Panel A: School Characteristics in 1976-1980

	Other Private	Segregation Academy	Difference
Enrollment	281.2	326.92	45.720*** (0.000)
Class Size	17.58	16.66	-0.920*** (0.001)
Religion	Catholic	not affiliated	
Grades	elementary	combined elementary & secondary	
N	1044	665	

Panel B: School Characteristics in 2017-2018

	Other Private	Segregation Academy	Difference
Enrollment	450.75	380.37	-70.380** (0.038)
Class Size	13.27	11.62	-1.650*** (0.000)
Percent White	69.15	83.12	13.970*** (0.000)
Percent Black	12.34	8.57	-3.770** (0.020)
College Matriculation	84.85	68.75	-16.100*** (0.000)
N	220	181	

Notes: **Data from the 1976-1980 Universe of Private Schools and 2017-2018 Public School Survey. The second panel is limited to schools that appear in both the 1976-1980 Universe of Private Schools and the 2017-2018 Private School Survey in Alabama, Florida, Georgia, Louisiana, Mississippi, North Carolina, and South Carolina. *** and ** indicate significance at the 1% and 5% levels, respectively, using a two sample t-test to test difference in means. We report modal school type in "Grades" and modal religion in "Religion." Religion consists of the following mutually exclusive categories: not affiliated, Seventh Day Adventist, Catholic, Baptist, Jewish, Episcopal; Church of Christ; Lutheran; Christian Interdenominations and Nondenominations; Amish, Mennonite, Amish-Mennonite; Assembly of God; Methodist; Presbyterian; Pentacostal; other. Grade types include elementary, middle, secondary, alternative, combined secondary and elementary, special education, and other. Differences in 2017-2018 characteristics, controlling for 1950 county characteristics, are shown in Appendix Table A.2.**

Narratives from former students and newspaper articles suggest many of the segregation academies provided poor quality education.[25] Common themes in these narratives center around poor quality

25. From a former student of Leland Academy (LA):"LA had been hastily thrown together the previous fall. . . Its 'buildings' were the borrowed Knights of Columbus hut and American Legion hut . . . gone were the public school award-winning marching band . . . I didn't get a great education, but it wasn't exactly a poor one either, in terms of basic high school classwork" (Smith, n.d.). From a 1969 article in *The Atlanta Consitutition*: "Checking a list of the 21

buildings, the loss of extracurricular offers, and unqualified teachers. While these narratives are selected, we show that schools that began as segregation academies perform worse on average today in terms of college graduation, but quality is highly heterogeneous.[26]

The descriptive evidence we present in Table 1 supports this hypothesis: segregation academies still open in 2017-2018 are poorer quality and Whiter compared to other schools that appear in both the 1976-1980 Universe of Private Schools and the 2017-2018 Private School Survey.[27] A graduate of Lamar High School in Mississippi provides an illustrative anecdote: "The school felt temporary. Built like a warehouse with aluminum siding and a flat roof. . .From a basketball coach who very unenthusiastically taught us science to a history teacher, also a coach, who left out huge chunks of world events, I had a slipshod education" (Riley 2019).

We map the locations of segregation academies in Figure 2. In order for a private school to be financially solvent, there need to be enough people who can afford to send their kids to the school. This is why most of the private schools in Figure 2 are clustered around cities. Segregation academies diverge from this model. Instead, the framework that best explains the predictors of their locations comes from the backlash literature: segregation academies are established in ideologically-homogeneous (among the White residents) places where racially conservative Whites felt threatened (Wheaton 2022; Derenoncourt 2022). Segregation academies are more likely to be located outside of cities compared to other types of private schools (Table 2).

For some communities, particularly rural counties, segregation academies were the only alternative to public education. In at least nine counties, segregation academies completely supplanted public schools for White students.[28] In urban areas, however, segregation academies had to compete with other private schools and suburban public schools. The size and number of segregation academies in each district thus varied.

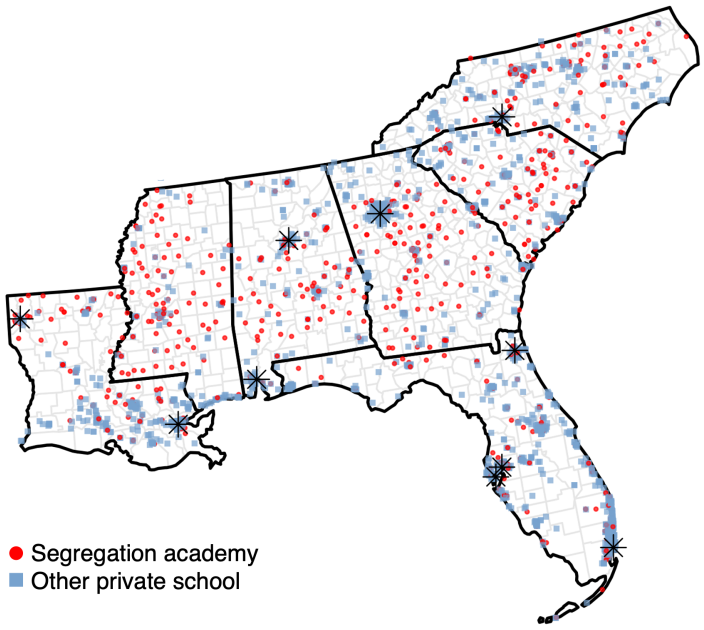
new line private schools through the state agencies revealed that only two were accredited, only five had occupancy permits from the State Fire Marshal's office indicating the buildings are safe, and four were serving lunches without health department food service permits" (Brown 1969).

26. "The new line private schools are being started in everything from church basements and old camp grounds to expensive new and modern structures. They run the gamut from obviously substandard schools to academically excellent schools whose every graduate goes to college. Their common denominator is race" (Brown 1969).

27. When comparing to all schools open in 2017 and controlling for county-level characteristics, segregation academies are significantly Whiter and perform worse in terms of 4 year college matriculation rate (Appendix Table A.2).

28. The public schools became over 95% Black in these counties: Bullock County, Alabama; Caddo Parish, Louisiana; Clarendon County, South Carolina; Coahoma County, Mississippi; Greene County, Alabama; Hinds County, Mississippi; Lowndes County, Alabama; Sumter County, Alabama; and Wilcox County, Alabama. *Source:* author's calculations derived from State Department of Education Reports. Nine represents a lower bound — it is possible that more counties met this criteria in states or years where enrollment by race was not recorded.

Figure 2 — Locations of Segregation Academies and Other Private Schools



Notes: **Data from the 1976-1980 Universe of Private School Survey (UPSS). Locations of segregation academies are indicated by dark blue circles. Locations of private schools are indicated by light blue squares. We plot the 10 biggest Southeastern cities in 1960 with starbursts. Those cities are, from left to right: Shreveport, LA; New Orleans, LA; Mobile, AL; Birmingham, AL; Atlanta, GA; Charlotte, NC; St. Petersburg, FL; Tampa, FL; Jacksonville, FL; Miami, FL.**

In Panel B, we compare characteristics of places that established a segregation academy prior to a court order vs. after a court order. Both types of places are strongly correlated with the percentage of the population that is Black. However, places that established schools prior to a court order were richer and had more population growth; places that established schools post have higher measures of preexisting racial animus. This suggests that segregation academies in low growth, low income areas may not have been financially solvent until court orders induced demand, which we expect to be positively related to racial animus.

4 — Determining Treatment Status

We define treatment status at two levels. In our first set of results describing the effect of segregation academies on public schools, we define treatment at the school district by year level. In our second set of results, treatment is defined at the county by birth year level: an individual is considered treated if she is of school age or younger when a segregation academy opens in her birth county.

A challenge of our setting is that private schools, unlike public schools, do not have a formal catchment area, meaning we cannot define treatment by school district or county boundaries. We instead adopt a spatial definition of treatment: a county or school district is "treated" if 30% of its population lives within 15 miles, or a roughly 30-45 minute commute, of a "confirmed" or "likely" segregation academy.[29] Though 15 miles may seem far, there are contemporary accounts of parents traveling this distance. A parent quoted in a 1969 article in *The Charlotte Observer* traveled 14 miles "to send her daughter . . . to another all-White private school . . . Wake Christian Academy" (Adams and Covington 1969). 1km x 1km grid level population estimates are taken from Fang and Jawitz 2018.[30]

We use straight line distance instead of network distance when defining our circles of treatment. Straight line distance may bias our estimates downward because areas considered "treated" may not be within a feasible commuting distance to school. However, this downward bias is preferable to the alternative: the potential endogeneity introduced by the use of network distance. Network distance is a measurement of modern-day commute time that can be obtained from the Google Maps API (historical commute time is not available). But modern roads have the potential to reflect a government's investment priorities, which could potentially be influenced by the location of segregation academies. For example, a local government may have built more roads to a segregation academy in order to make the school more accessible.

Once a school district or county is treated, it remains treated. We conceive of the formation of additional segregation academies as an intensification of the initial treatment over time. Figure 3 shows the spatial and temporal distribution of treatment: a large spike occurs in the school year beginning in 1970, one school year after the Supreme Court mandated immediate desegregation in *Alexander v. Holmes County Board of Education*.

29. School addresses come from the 1976-1980 UPSS. We then geolocate these addresses using Geocod.io.
30. We use the m5 model. For a detailed description of the data, see Fang and Jawitz 2018.

Table 2 — Characteristics of Places with Segregation Academies

Panel A: Compared to Places with Other Private Schools

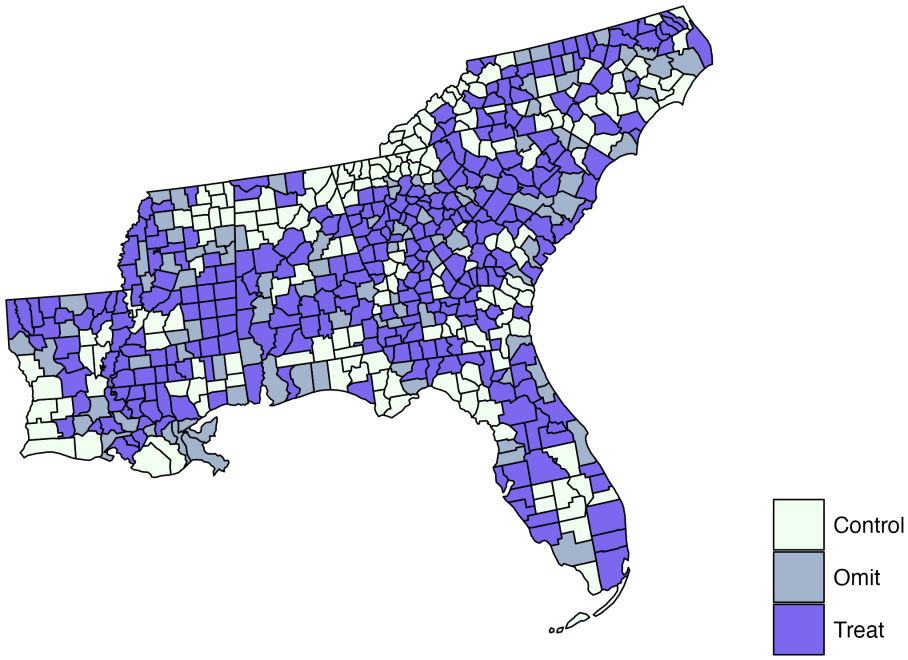
	Academy					Other Private				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Pct Black Pop (1950)	0.16***					-0.01				
	(0.02)					(0.03)				
Share Vote Strom Thurmond		0.11**					0.00			
		(0.03)					(0.02)			
Number of Lynchings Per 1950 Pop			0.04*					-0.02		
			(0.02)					(0.03)		
Population Change 1940-50				0.05*					0.16***	
				(0.02)					(0.02)	
Median Family Income: 1950					0.04					0.14***
					(0.03)					(0.02)
Observations	874	874	874	874	874	874	874	874	874	874
R^2	0.13	0.07	0.03	0.03	0.03	0.09	0.09	0.09	0.19	0.16
Within R^2	0.12	0.05	0.01	0.01	0.01	0.00	0.00	0.00	0.12	0.08
State fixed effects	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓

Panel B: Stratified by Timing Relative to Court Order

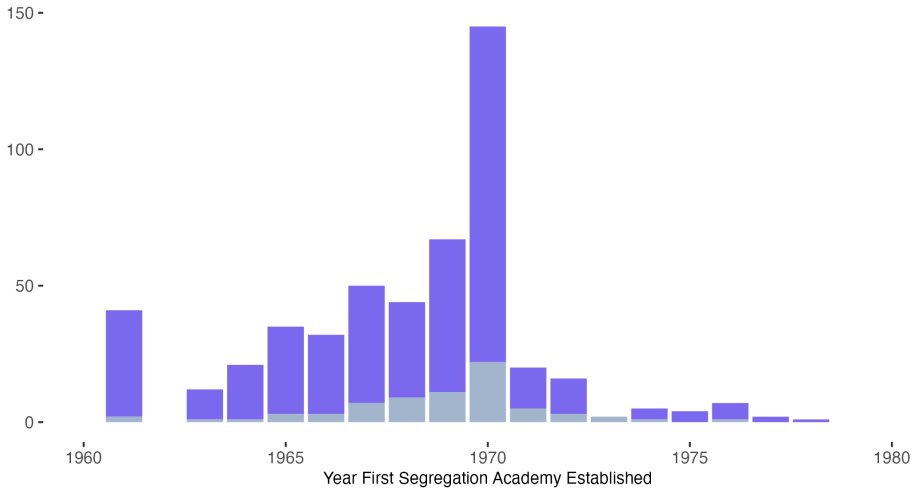
	Prior to Court Order					After Court Order				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Pct Black Pop (1950)	0.07***					0.11***				
	(0.01)					(0.02)				
Share Vote Strom Thurmond		0.02					0.09**			
		(0.01)					(0.03)			
Number of Lynchings Per 1950 Pop			-0.01					0.05**		
			(0.01)					(0.02)		
Population Change 1940-50				0.07**					-0.03	
				(0.03)					(0.02)	
Median Family Income: 1950					0.07**					-0.04
					(0.03)					(0.02)
Observations	874	874	874	874	874	874	874	874	874	874
R^2	0.05	0.03	0.02	0.05	0.05	0.10	0.08	0.06	0.05	0.05
Within R^2	0.03	0.00	0.00	0.03	0.03	0.06	0.04	0.02	0.01	0.01
State fixed effects	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓

Notes: **Table shows the correlation of characteristic in the left-most column with treatment or having at least one private school, recovered from the bivariate regression $y_c = \alpha + x_c + s_c + \epsilon_c$, where s_c is a state fixed effect and x_c is the characteristic of interest, as listed in the left column. All measures are standardized within a state. In Panel A columns (1) through (5) ("Academy"), $y_c = 1$ if at least 30% of a school district's population is within 15 miles of a segregation academy (n=531). The comparison group are never treated school districts (n=343). In columns (6) through (10) ("Other Private") $y_c = 1$ if a private school is located in the school district (n=392). In Panel B columns (1) through (5) ("Prior to Court Order"), $y_c = 1$ if a segregation academy was established prior to a court order impacting the school district (n=196). Comparison districts are districts with no segregation academies (n=343), districts where segregation academies were established after a court order (n=230), and any district with no court orders (n=105) (some, but not all, of these school districts have a segregation academy). In Panel B columns (6) through (10) ("After Court Order"), $y_c = 1$ if a segregation academy was established after a court order impacting the school district (n=230). Comparison groups are analogous to those previously described.**

Figure 3 — Spatial and Temporal Variation in Treatment



(a) Map of treated counties.



(b) Distribution of treatment timing.

Notes: **Treatment is defined as a school district or birth county with at least 30% of their population within a 15 mile radius of a "confirmed" or "likely" segregation academy. Control school districts or birth counties are not considered treated in any year. Population estimates come from Fang and Jawitz 2018, model m5. For our enrollment and funding analyses, we omit school districts with 15-30% of their population within a segregation academy. To mitigate the risk of creating implicit samples that would compromise our ability to disclose results in the future, we do not omit similar birth counties in our long run outcomes analyses that use restricted Census data.**

5 — Theoretical Framework and Target Estimand

In this section, we develop a theoretical framework to define our target estimand and motivate our measurement of public enrollment losses induced by the establishment of a segregation academy. Consider a model of taste-based discrimination inspired by Becker 1957. In Becker's canonical model, employers who have a taste for discrimination will forgo money to avoid interacting with minorities. Parents who pull their children out of free public schools to prevent their child from interacting with minority children can be thought of similarly.

For each school district, let $r^i \geq 0$ represent a parent i's taste for discrimination (or racism). Each school district d is endowed with some distribution of $r \sim F_d(r)$. Let b_{pub} represent the ratio of Black students to White students in the local public school. Let $c^i_{pub}(r^i, b)$ represent a White parent's "psychic cost" of sending their child to public school. Note $c^i_{pub}(r^i, b)$ is a function of the product of r and b such that $c^i_{pub}(0, b_{pub}) = c^i_{pub}(r^i, 0) = 0$. For example, a parent with a high taste of discrimination r^i that sent their child to a perfectly *de jure* segregated public school ($b = 0$) would incur a psychic cost of zero.

White parents with a positive r^i had three options: (1) relocate to different school district, (2) enroll their kids in an existing private school, or (3) form and/or enroll in a segregation academy.

Total White enrollment losses can thus be expressed as

$$W = W_{move} + W_{priv} + W_{segac}$$

where W_{move}, W_{priv}, and W_{segac} correspond to (1), (2), and (3), respectively.

Attending a different school than the neighborhood public school incurred costs. The cost of (1) is an increasing function of the distance to move m^i and the product of r^i and b_{alt}, the ratio of Black students to White students in the alternative school district: $c^i_{move}(m^i, r^i, b_{alt})$. The cost of (2) is increasing in ratio of private school tuition to family income $tuition^i_{priv}$, commuting distance to the nearest private school $dist^i_{priv}$, and the product of r^i and b_{priv}, the ratio of Black students to White students in the nearest private school: $c^i_{priv}(tuition^i_{priv}, dist^i_{priv}, r^i, b_{priv})$. Note that for most existing private schools, $b_{priv} \approx 0$. The cost of (3) is an increasing function in tuition and start-up costs: $c_{segac}(tuition_{segac}, startup_{segac})$.[31] Anecdotally, many segregation academies offered low tuition and instead asked parents to contribute to the school's infrastructure, either via

31. $b_{segac} = 0$ by definition, $\therefore c_{segac}(tuition_{segac}, startup_{segac}, r^i, 0) = c_{segac}(tuition_{segac}, startup_{segac})$

labor or a financial contribution to a "building fund." We thus assume $tuition^i_{segac} < tuition^i_{priv}$.

A White child exits the public school system if the cost of some outside option is less than c^i_{pub}. White enrollment losses W are equal to the sum of individuals i for which c^i_{pub} is greater than the cost c^i_{type} of at least one outside option:

$$W = \sum_{0}^{I} \mathbf{1}\{c^i_{pub} - min(c^i_{segac}, c^i_{move}, c^i_{priv}) > 0\} \tag{1}$$

The marginal White parent is shifted into leaving the public school system by the establishment of a segregation academy if

$$c^i_{segac} < c^i_{pub}(r^i, b) < min(c^i_{move}, c^i_{priv})$$

Our target estimand is the effect of segregation academies on marginal White enrollment losses, w_{segac}. It can be expressed as:

$$w_{segac} = \sum_{0}^{I} \mathbf{1}\{c^i_{segac} < c^i_{pub} < min(c^i_{move}, c^i_{priv})\} \tag{2}$$

These conditions are most likely to hold in areas where $F(r_i)$ has a long left tail (a high proportion of people are racially prejudiced) *and* the cost of attending an existing private school is high *and* the cost of moving to a racially homogeneous school district is high.

Policy makers and institutions can and did influence the scale of W and W_{segac} by lowering the cost of outside options. School district secession of racially homogeneous areas reduced the cost of moving, while private tuition voucher programs and other state laws and resolutions, some of which explicitly directed public funding for private schools, lowered the tuition burden on families (Ford, Johnson, and Partelow 2017). In 1961 Georgia, for example, $\$3.6$ million dollars (in 2013 dollars) were allocated to provide tax funded scholarships for students to attend any non-sectarian private schools (*A History of Private Schools & Race in the American South* 2016). To defray the cost of founding a new school, local governments sold segregation academies textbooks and other school supplies at bargain prices from the state. Segregation academies even operated on campuses of public schools closed via boycott or consolidation. While many of these rules were eventually rolled back (Cascio et al. 2010), funding continued via tax exemptions for individual

donations to these schools and tax exempt status for the schools as described in Section 2.

6 — Measuring the Effect on Public School Outcomes

In this section, we quantify the effect of segregation academies on the provision of public schooling. After describing our data and empirical strategy, we measure the extent of attrition from public schools to segregation academies.

6.1. Data

We combine administrative records on public schools at the district and county-level to analyze the contemporaneous effect of segregation academies on public school systems. Controls are generally collected at the county level, outcomes at the district level, and locations of segregation academies at the coordinate level. School districts are typically equivalent to counties in the Southeast. Cities sometimes, but not always, had separate school districts.

Enrollment, funding, and the numbers of teachers data come from reports compiled by southern state departments of education (DOE). State DOEs assembled reports to update the state legislature on the policy priorities, successes, and weaknesses of public schools. Though the exact format of the reports varies across states and years, they typically consist of a narrative section that describes policy areas of focus and a statistical section with tables of metrics such as enrollment and sources of funding. Each state legislature had different standards; Mississippi, for example, has exceptionally detailed data on funding, while South Carolina has extensive coverage of private school enrollment and academic outcomes. Alabama and Louisiana report enrollment by race throughout the study period, while other states either stop reporting enrollment by race in the early 1960s or report it inconsistently.

Sarah Reber compiled and generously shared the reports for 1960 through the mid 1970s, depending on the state. We extend this time series through 1985 with newly digitized data.[32] Because these data span 1960-1985, our analysis should be interpreted as the effect of opening a segregation academy *after* 1960. These data contain 871 school districts with enrollment data, 750 of which we use in our main analysis.[33] We use newly digitized annual school-level enrollment data for

32. Additional years were transcribed by the authors or an undergraduate research assistant. Ashley Sorey Dees at the University of Mississippi Library was especially helpful in acquiring additional years for Mississippi.

33. We drop partially treated districts where 15-30% of the population live within 15 miles of a segregation academy and districts where academies opened prior to 1960.

both private and public schools to decompose enrollment losses. We then link this to our data set of segregation academies by school name and county.

School-level segregation data come from Office of Civil Rights (OCR) surveys digitized by Sarah Reber and compiled by Owen Thompson. The combined data provides school-level enrollment by race data for the years 1966-1980. The purpose of these reports was to monitor compliance with desegregation rules. They thus contain a selected sample of school districts. For example, in 1967, the survey covered school districts of 3,000 or more pupils or southern school districts that were actively desegregating by either voluntary plan or court mandate. 703 out of 871 school districts have this data available, 605 of which we use in our main analysis. For a full description of the data, see Thompson 2022.

County-level demographic controls come from the 1950 census (ICPSR 2896: Haines, Historical, Demographic, Economic, and Social Data): median family income, population, percent of households that are families, and population density. We choose the 1950 census because it predates *Brown v. BOE*, and, as a result, the vast majority of segregation academies. The earliest confirmed segregation academy in our sample opened in 1952. The earliest in the sample used in our main analysis opened in 1960. We calculate percentage of the population that is Black in 1950 using population levels disaggregated by race from the City and County Databooks (ICPSR 07736 and 07735, respectively).

Proxies for preexisting racial animus come two sources. First, we use the share of county vote share for Strom Thurmond in 1948, a standard measure of racial animus in the literature (Cascio et al. 2010). Strom Thurmond was a vocal segregationist who ran under the States' Rights, or "Dixiecrat," party. However, using Strom Thurmond's vote share as an indicator of racial animus is problematic due to Alabama's decision to exclude incumbent President Harry S. Truman from the ballot. This decision distorts across-state comparisons and limits within-state variation in Alabama (Strom Thurmond won 79.7% of the vote in pre-Voting Rights Act Alabama) "1948 | The American Presidency Project," n.d. To address this limitation, we also approximate racial animus with the number of lynchings and number of Klans scaled by 1950 population. Lynching data comes from the American Lynching Project, compiled by the Equal Justice Initiative.[34] The number, founding date, and location of Ku Klux Klans were compiled by Virginia Commonwealth University as part of the Mapping the Ku Klux Klan Project (1915-1940).

Court-ordered integration data come from our assembled database of court orders, cases, and actions. These are crucial to disentangle the effect of the opening of segregation academies from

34. Martin Fiszbein and Thomas Pearson generously shared a tabular version of this data.

the effect of court-ordered integration and to estimate the interaction of these events. We draw from existing sources: the American Communities Project Logan, American Communities Project Desegregation Case Data, ProPublica (from the U.S. Department of Education, U.S. Department of Justice, Stanford University, and ProPublica research), and Anstreicher, Fletcher, and Thompson 2022. We further augment this data by adding court cases compiled by the Alabama School Connection Crain 2013, the Civil Rights Legislation Clearinghouse, the Georgia Advisory Committee to the United States Commission on Civil Rights (report published in 2007), the South Carolina Advisory Committee to the United States Commission on Civil Rights (report published in 2008), and the United States Commission on Civil Rights Civil Rights 2007. Finally, we search for any districts with missing years in legal databases such as Casetext. This yields a comprehensive data set that includes district name, year founded, and type of event. Types of events include court orders, court cases (which may or may not result in a court order, depending on the perceived quality of a district's submitted plan), and desegregation events.

To facilitate comparability of our results with previous literature, we use year of implementation as the desegregation year. In the case of multiple court cases, we use the year associated with a "major" plan, as defined by either Welch and Light 1987 or Anstreicher, Fletcher, and Thompson 2022, which expands the Welch and Light sample to 187 large ($> 15,000$ students) school districts by combining it with Logan, American Communities Project Desegregation Case Data.[35] 641 of the 871 school districts have a desegregation year defined.

School district boundaries are from 1990, as recorded in shape files compiled by NCES (National Center for Education Statistics 1995) We follow Reber 2005. Maps of school district boundaries are not available before 1990. While this practice could be potentially problematic if school district boundary changes are endogenous, Reber 2005, which studies the effect of policies implemented in the seventies, uses 1990 school district boundaries because boundaries between 1970 and 1990 changed "little, if at all."

Similarly, we use 1990 county boundaries compiled by the Census Bureau Bureau 1990. As documented by Bureau, n.d., county boundaries in Alabama, Florida, Georgia, Louisiana, Mississippi, North Carolina, and South Carolina are stable from 1970-1990.

In the Southeast, school districts in rural areas and counties typically share the same boundaries. For municipal school districts where this is not the case, we assign county characteristics based off of the county in which the school district is located. For school districts that span county borders,

35. See Appendix Table B.2 for an example of how we choose between court years. In this example, we use the year 1970 to define the $desegPost$ variable (this is also the year used by Reber 2005).

we assign county based off of share of students.

6.2. Estimation Strategy

In this section, we discuss our reduced form estimation strategy and elaborate on the definition of treatment for school districts.

Because of the inherently arbitrary nature of the spatial treatment definition, we do not expect there to be a discontinuous treatment effect 15 miles from a segregation academy. To create a clean set of control districts, we omit "partially treated" districts that have $15 - 30\%$ of population with a 15 mile radius of a segregation academy.[36]

Our treatment variable is binary and the causal parameter we recover should be interpreted as the average effect that the *introduction* of segregation academies in a school district has on school districts that have segregation academies, that is, the average effect of treatment on the treated (ATT).

Identification of our target estimand requires us to isolate the marginal effect of segregation academies on enrollment. The simplest way to distinguish these enrollment losses from the enrollment losses that would have occurred irrespective of the establishment of a segregation academy is to use a standard two-way fixed effects event study estimator:

$$Y_{d,t} = \underset{\text{district}}{\alpha_d} + \underset{\text{year}}{\lambda_t} + \sum_{\ell \neq -1} (\delta_\ell \cdot D^\ell_{d,t}) + \epsilon_{d,t} \tag{3}$$

where ℓ denotes event time and $D^\ell_{d,t} = 1$ for treated districts at event time ℓ. Take a school district where the first segregation academy was established in 1965. Years 1963, 1964, 1966, and 1967 would correspond to event times -2, -1, 1, and 2, respectively. The coefficients of interest, δ_ℓ, are interpreted as the effect of treatment at event time ℓ. We use $\ell = -1$ as our reference period to account for any possible anticipation of treatment in the year prior. We cluster standard errors at the school district level.

However, a large body of literature has shown that estimates of δ_ℓ are biased by treatment effects from other periods when treatment timing is staggered (Roth and Sant'Anna 2023; Callaway and Sant'Anna 2022; Sun and Abraham 2021). Additionally, an assumption of the standard event study is that treatment paths are homogeneous among different groups e, where e indicates the

36. In Figure A.2, we show how results differ when using different distance and percent coverage combinations to define treatment. As the definition grows more restrictive, results become noisier.

shared treatment year. This assumption is violated in our context. For example, the enrollment response in a school district where a segregation academy was established in 1959, when most public schools were de jure segregated, can reasonably be expected to differ from the enrollment response in a school district treated in 1970, when public schools were integrating.

To deal with these concerns — namely, staggered treatment timing and heterogeneous treatment effects — we adopt the method described in Sun and Abraham 2021. This method refines the traditional two-way fixed effect specification by including terms interacting relative period indicators with cohort indicators, which relaxes the treatment effect homogeneity assumption.

The estimate of the effect on White enrollment will be biased away from zero if the cost of attending a preexisting private school, the cost of moving school districts, or the psychic cost (to racially biased White parents) of attending the local public school changes *differentially* in treated versus control districts during the period of study. Court orders, which were widely implemented during this period, affect the cost of attending the local public school directly by changing the public school's ratio of Black students to White students. In all estimates, we control for the effect of court orders directly via the $desegPost$ variable. Additionally, in Appendix Figure A.4 we show the timing of court orders is distinct from the timing of the establishment of segregation academies. We thus use the following model:

$$Y_{d,t} = \underset{\text{district}}{\alpha_d} + \underset{\text{year}}{\lambda_t} + \underset{\text{year by state}}{\theta_{s(d)t}} + \gamma desegPost_{d,t} + \sum_{e} \sum_{\ell \neq -1} \delta_{e,\ell}(\mathbf{1}\{E_d = e\} \cdot D^{\ell}_{d,t}) + \epsilon_{d,t} \quad (4)$$

where $desegPost_{d,t} = 1$ once a school district has been placed under a major court order, and $\theta_{s(d)t}$ allows the time fixed effect to vary between states. We take the log of our dependent variables, enrollment and White enrollment. This allows us to measure the *percentage change* in enrollment, rather than the level change. The latter, of course, would be an artifact of the population of the school district and is not of interest. We measure changes in both the level and log of percentage White.

The coefficients on these interaction terms, $\delta_{e,\ell}$ are interpreted as the Group Average Treatment Effects on the Treated (GATT):[37]

$$GATT_{e,\ell} = E[Y_{d,e+\ell} - Y^{\infty}_{d,e+\ell} | E_d = e] \quad (5)$$

37. In Sun and Abraham 2021, these are referred to as the Cohort Average Treatment Effect on the Treated. Because cohort can also refer to birth years, we refer to these instead as GATT.

where e is the year that a segregation academy was established, ℓ is the number of years from initial treatment, $Y^{\infty}_{d,e+\ell}$ is the counterfactual outcome, and $Y_{d,e+\ell}$ is the observed outcome.

We are interested in how the $GATT_{e,\ell}$ changes over ℓ, the number of years from treatment. Thus, we again follow Sun and Abraham 2021 by estimating a weighted average of each group's average treatment effect at time from event ℓ. The vector of $GATT_{e,\ell}$ is aggregated by weighting each group-time treatment effect by $Pr\{E_d = e|E_d \in [-\ell, T-\ell]\}$, or the share of each cohort in the sample in period ℓ. This weighting addresses the contamination bias and thus allows for staggered treatment timing.

$$\nu_\ell = \sum_e GATT_{e,\ell} Pr\{E_d = e|E_d \in [-\ell, T-\ell]\} \tag{6}$$

In Appendix D, we describe what assumptions are necessary in order for ν_ℓ to be interpreted as the target estimated described in Section 5.

7 — Effects on Public School Enrollment and Integration

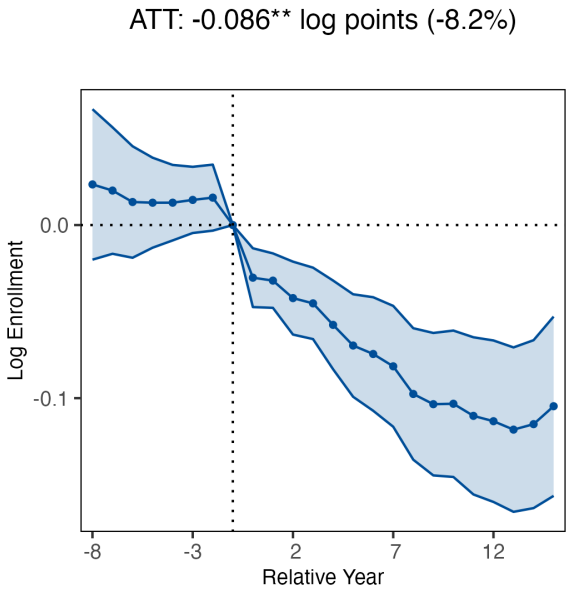
Throughout this section, we report enrollment results for all seven states of the Southeast. White enrollment results are reported for Alabama and Louisiana, private enrollment results for Louisiana and South Carolina, and segregation academy enrollment results for South Carolina only. Because institutional contexts and thus enrollment responses varied by state, estimates from sub-samples should be interpreted accordingly.

We find that the establishment of segregation academies caused a significant and persistent decline in public school enrollment. In Table 3, we report the results from Equation 4.[38] Panel A reports results on the full sample of the seven states of the Southeast (Alabama, Florida, Georgia, Louisiana, Mississippi, North Carolina, and South Carolina). Across these states, public enrollment declines .10 log points, or 10% (column (1)). Event study estimates, displayed in Figure 4, show that this effect deepens over time to 15%. Seven years post the establishment of a segregation academy, this effect is remarkably stable, suggesting a permanent gouging of public school enrollment.

In columns (2) through (5), we control for time trends interacted with a set of characteristics correlated with treatment (see Table 2 for correlations): percentage of the population that is Black, population change in the prior decade, and number of lynchings. We discuss each of these controls

38. In Appendix Table A.3, we replace $desegpost$ with indicators for the relative year from a desegregation court order. Results are largely unchanged.

Figure 4 — Public School Enrollment
Alabama, Louisiana, Mississippi, Georgia, South Carolina, North Carolina, Florida



Notes: **Y axis shows change in log enrollment for Alabama, Florida, Georgia, Louisiana, Mississippi, North Carolina, and South Carolina. Points reflect estimates from Equation 4 and Column (1) of Panel A of Table 3. The estimation equation includes year-state fixed effects, school district fixed effects, year fixed effects, and an indicator for a court order. Lines represent 95% confidence intervals. Standard errors are clustered at the school district level. Observations are at the school district by year level.**

in depth in Appendix E.1. In column (6), we run our analysis on a matched sample. A description of the matching exercise is described in Appendix E.2.[39]

In Alabama and Louisiana, states where we are able to measure enrollment by race throughout our time period of interest (1960-1985), we find that White enrollment losses drive this effect: on average, White enrollment declines by an additional 35% after the opening of a segregation academy (Panel B, Table 3). In Figure 5, we show event study estimates for Louisiana and Alabama specifically. Enrollment declines in these states follow a similar temporal pattern as enrollment declines in the full sample, suggesting they are roughly representative of the full sample. There is no effect on Black enrollment in these states, which allows us to reasonably infer that White students drive enrollment losses, though this analysis is not well powered. It follows that the composition of the public school student body changes: White share enrollment declines by 15% (Figure 5).

39. Results are also robust to using alternate event study specifications and alternate treatment definitions; see Appendix Figures A.2 and A.3, respectively.

Enrollment declines and White enrollment declines deepen over time. We attribute this to growth of the academies on both the intensive and extensive margin. Because academies opened so rapidly, space constraints often limited the number of grades they were initially able to offer. Schools then added grades in subsequent years. New schools also formed: for example, Saint James School, Montgomery's first segregation academy, opened in 1955. In the following years, four other academies opened: Montgomery Academy in 1959 and Central Alabama Academy, Stephen Spears School, and Trinity Presbyterian school in 1970. One Mississippi student's story illustrates both types of growth: she was initially turned away from Indianola Academy (established 1965) in summer of 1969 because the school was at capacity. She stayed in the public schools that fall, but then was enrolled at Leland Academy, a newly established school, in January 1970 after the court-ordered the district to integrate. Leland Academy only offered K-6, but "after the court order...added seventh and eight grades...By the start of school in the fall of 1970, Leland Academy's new cinder block building seemed to pop up overnight in a cotton field...we now [were] first through twelfth grade" (Smith, n.d.). Enrollment losses are largest in rural areas, where the costs of taking up an alternative outside option are the highest (Appendix Section F).

Conditional on having access to a segregation academy, White parents are more likely to exit public schools after a major court order than before: the effect on White public school enrollment in Alabama and Louisiana changes from a statistically insignificant -.13 to -.58 log points. The effect on total enrollment changes from negative .07 log points prior to court-ordered integration to significant negative .14 log points after court-ordered integration (Table 4).

Column (2) can be interpreted as the effect on enrollment of having a segregation academy prior to or in the absence of a court order. Column (3) can be interpreted as the effect of having a segregation academy in the presence of the court order. Note that because we limit our sample in (3) to *only districts under a court order*, we am comparing school districts under court order with a segregation academy to school districts under court order without a segregation academy. (3) is thus the *marginal* effect that segregation academies have on enrollment, not the effect of the court orders on enrollment.

Table 3 — Effect on Public School Enrollment and White Public School Enrollment

Panel A: Public Enrollment (Alabama, Florida, Georgia, Louisiana, Mississippi, North Carolina, South Carolina)

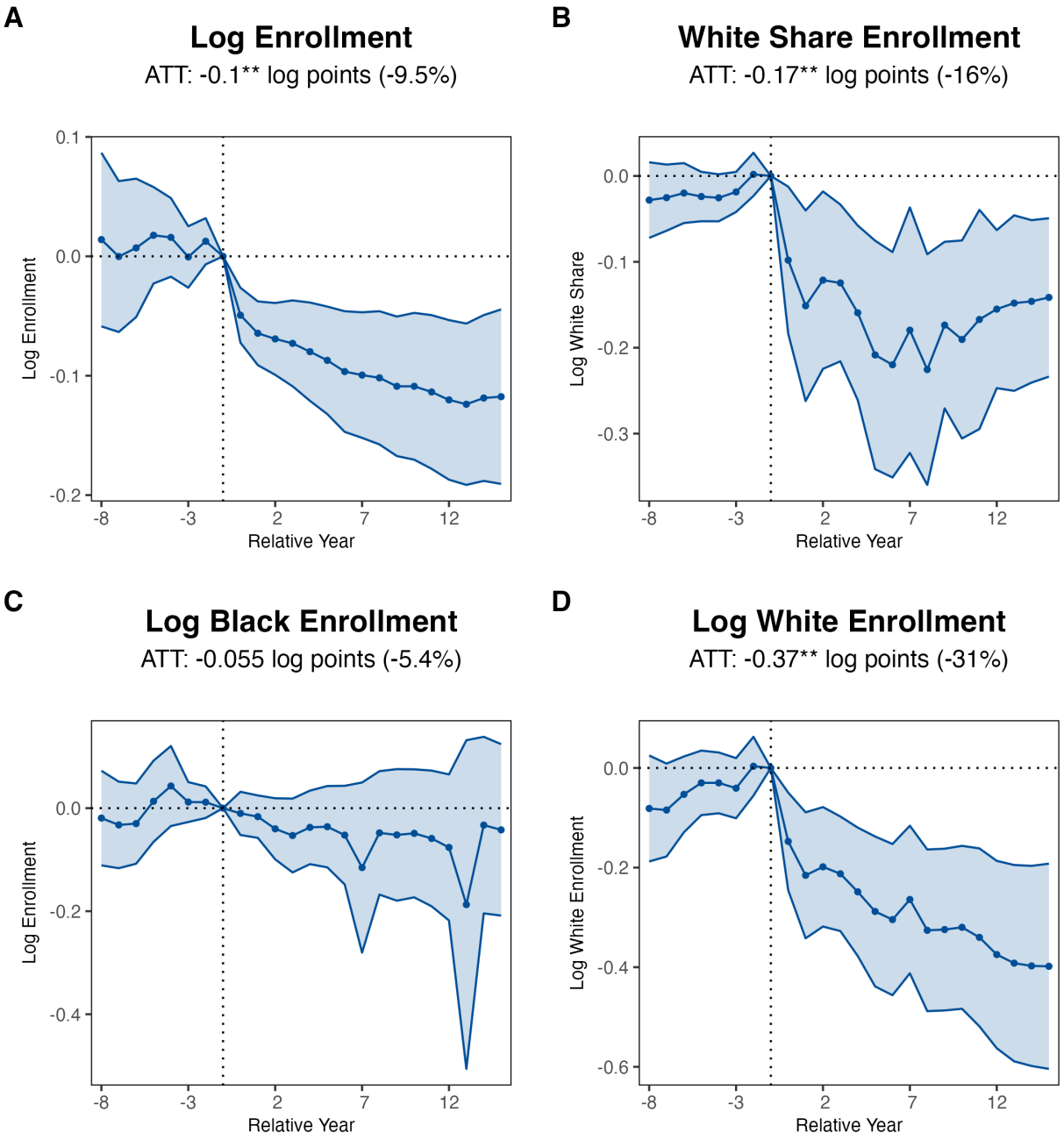
	Log Enrollment					
	(1)	(2)	(3)	(4)	(5)	(6)
ATT	-0.09***	-0.03	-0.10***	-0.08***	-0.05**	-0.06
	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.04)
Desegregation Post	-0.01	0.01	-0.02	-0.01	0.00	0.02
	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.03)
Year × Quartile Black Pop Pct		-0.01***			0.00***	
		(0.00)			(0.00)	
Year × Population Change 1940-50			0.00***		0.00***	
			(0.00)		(0.00)	
Year × Number of Lynchings				0.00*	0.00	
				(0.00)	(0.00)	
Observations	18,929	18,929	18,929	18,929	18,929	14,636
R^2	0.97	0.97	0.97	0.97	0.97	0.97
Within R^2	0.11	0.17	0.16	0.11	0.21	0.10
School District fixed effects	✓	✓	✓	✓	✓	✓
Year fixed effects	✓	✓	✓	✓	✓	✓
Year-State fixed effects	✓	✓	✓	✓	✓	✓

Panel B: White Public Enrollment (Alabama, Louisiana)

	Log White Enrollment					
	(1)	(2)	(3)	(4)	(5)	(6)
ATT	-0.37***	-0.17**	-0.40***	-0.34***	-0.21**	-0.34***
	(0.09)	(0.08)	(0.10)	(0.10)	(0.09)	(0.13)
Desegregation Post	0.16*	0.16**	0.13	0.16*	0.15*	0.26**
	(0.09)	(0.08)	(0.08)	(0.09)	(0.08)	(0.13)
Year × Quartile Black Pop Pct		-0.01***			-0.01***	
		(0.00)			(0.00)	
Year × Population Change 1940-50			0.00***		0.00***	
			(0.00)		(0.00)	
Year × Number of Lynchings				0.00	0.00	
				(0.00)	(0.00)	
Observations	6,638	6,638	6,638	6,638	6,638	4,994
R^2	0.91	0.92	0.91	0.91	0.92	0.91
Within R^2	0.17	0.24	0.21	0.18	0.26	0.17
School District fixed effects	✓	✓	✓	✓	✓	✓
Year fixed effects	✓	✓	✓	✓	✓	✓
Year-State fixed effects	✓	✓	✓	✓	✓	✓

Notes: **Event studies in Figure 4 reflect estimates from (1). Results from a matched data set are in (6). We add covariates interacted with year in columns (2) through (4) and include the full set of covariates interacted with year in column (5). Observation numbers change from (1) to (6) because of availability of covariates. Enrollment results are estimated for Alabama, Mississippi, Louisiana, Georgia, Florida, North Carolina, and South Carolina. White enrollment results are estimated for Alabama and Louisiana. Data ranges from 1960-1985. ∗ ∗ ∗, ∗∗, and ∗ indicate significance at the .01, .05, and .10 levels, respectively. Standard errors are clustered at the school district level and appear in parentheses.**

Figure 5 — Public School Enrollment by Race
Alabama and Louisiana



Notes: **Estimates for Alabama and Louisiana, where by race enrollment data is available. Points reflect estimates of the effect of the opening of a segregation academy on the outcome of interest from Equation 4 and Panel B of Table 3. The estimation equation includes year-state fixed effects, school district fixed effects, year fixed effects, and an indicator for if the school district is under court order. Dashed lines represent 95% confidence intervals. Standard errors are clustered at the school district level. Observations are at the school district by year level.**

Table 4 — Effect on Public Enrollment Pre and Post Court Order

Panel A: Total Public Enrollment (Alabama & Louisiana)

	Log Enrollment		
	All	Pre	Post
	(1)	(2)	(3)
ATT	-0.10***	-0.07	-0.14***
	(0.03)	(0.09)	(0.04)
Desegregation Post	0.03		
	(0.04)		
Observations	6,638	1,997	4,488
R^2	0.96	0.97	0.99
Within R^2	0.18	0.23	0.08
School District fixed effects	✓	✓	✓
Year fixed effects	✓	✓	✓
Year-State fixed effects	✓	✓	✓

Panel B: White Enrollment (Alabama & Louisiana)

	Log White Enrollment		
	All	Pre	Post
	(1)	(2)	(3)
ATT	-0.37***	-0.13	-0.58***
	(0.09)	(0.14)	(0.20)
Desegregation Post	0.16*		
	(0.09)		
Observations	6,638	1,997	4,488
R^2	0.91	0.97	0.94
Within R^2	0.17	0.25	0.16
School District fixed effects	✓	✓	✓
Year fixed effects	✓	✓	✓
Year-State fixed effects	✓	✓	✓

Notes: **Estimation is the same as Equation 4 in column (1). In column (2) and (3), we drop the $desegPost$ dummy variable. Instead, we split our sample to measure enrollment losses pre and post a significant court order. Column (2) limits the sample to years before a major court order. In districts where there was no court order, all years are included. Column (3) limits the sample to years after a major court order. Districts where there was no court order are omitted. In the joint sample of Alabama and Louisiana, there is no statistically significant effect on total public or White public enrollment in the years prior to a court order, but a large and significant effect on total public and White public enrollment in the years post. $***$, $**$, and $*$ indicate significance at the .01, .05, and .10 levels, respectively. Standard errors are clustered at the school district level and appear in parentheses. Observations are at the school district by year level.**

Evidence from South Carolina, where detailed private school enrollment data is available, shows that this decrease in White enrollment is primarily due to parents opting into segregation academies rather than moving school districts or switching into preexisting private schools. To further quantify the role of segregation academies in public enrollment declines, we also compare the magnitude of the public school enrollment losses to the increase in private school enrollment in Table 5. We estimate Equation 4 using the level of enrollment rather than the log so we can compare the total number of students leaving the public schools to the number entering the private schools. On average, in the Southeast, segregation academies lead to an enrollment decrease of 369 students (1), while in Louisiana and South Carolina, private school enrollment increased by 453 students (2).

Table 5 — Private Enrollment vs. Public Enrollment

	Public Enroll	Private Enroll	Public Enroll	Private Enroll	Seg Ac Enroll	Non Seg Ac Enroll
	Deep South	LA and SC	SC			
	(1)	(2)	(3)	(4)	(5)	(6)
ATT	-369.22*	453.56***	-429.93*	248.55**	288.15***	-39.60
	(209.92)	(148.63)	(220.43)	(107.13)	(105.37)	(28.98)
Desegregation Post	-393.21*	138.42	-89.96	104.79	107.61	-2.83
	(213.10)	(230.33)	(421.39)	(161.80)	(152.80)	(70.32)
Observations	11,798	2,213	1,622	983	983	983
R^2	0.96	0.97	0.98	0.84	0.80	0.90
Within R^2	0.11	0.09	0.12	0.30	0.37	0.31
School District fixed effects	✓	✓	✓	✓	✓	✓
Year fixed effects	✓	✓	✓	✓	✓	✓
State-Year fixed effects	✓					

Notes: **The table presents the effect of the establishment of a segregation academy on private and public enrollment, estimated using Equation 4. States included in the sample are indicated in the second row of the headers. Because the results in column (3) through (6) are estimated using data from South Carolina only, we drop** $state \times year$ **FE. Standard errors are clustered at the school district level and appear in parentheses. Observations are at the school district by year level.**

In Columns (3) through (6), we narrow our sample to South Carolina, where State DOE reports include enrollment data by school. This allows us to directly compare the number of students leaving the public schools to the number entering segregation academies. In Column (5), we separate out segregation academies from other private schools (Column (4)). Increases in segregation academy enrollment account for 116% of the gain in private school enrollment. We expect this percentage to smaller in Louisiana and other places where there were more preexisting private schools.

Public schools lost 430 students (3) while segregation academies gained 288 (5). The gap in this number — 142 students — can be interpreted as either the number of students that migrated out

of the school district or attended schools not included in the State Report.[40]

Declines in enrollment offset gains in court-ordered integration, thus potentially mitigating the positive effects of these orders.[41] We estimate the effect on public school integration for a subsample of 605 school districts using Equation 4. To measure integration, we calculate the Exposure Index:[42]

$$ExpIndex_d = 1 - \sum_{1}^{S}[\frac{n_{B,s}}{N_{B,d}} \times \frac{n_{B,s}}{n_{B,s} + n_{W,s}}] \tag{7}$$

Where $n_{B,s}$ is the number of Black students in school s and $N_{B,d}$ is the number of Black students in district d. The Exposure Index ranges from 0 (total segregation) to the ratio of White students in the district (total integration) and reflects the average percentage of school-mates of a Black student that are White. Changes in the level of this measure could be biased by control districts having a higher percentage White population than treated districts. Accordingly, we report the effect on log of the exposure index.

The OCR only collected data from school districts that met one of two criteria: 3,000 or greater students or school districts in the process of desegregating. As a result, small, rural districts that were not under supervision by the OCR are omitted from the analysis. We also omit "always treated" districts (districts where a segregation academy was established prior to 1966).[43] As in our main analysis, we omit districts where $15 - 30\%$ of the land area is within 10 miles of a segregation academy.

School-level data, which is required to calculate the Exposure Index at the district level, was collected by the OCR for nine years between 1966-1980.[44] Because data are not available to estimate an informative pre-period, we report the ATT for this outcome instead of a full event study.

In large districts and districts implementing desegregation plans (that is, school districts included in OCR reports), segregation academies offset approximately 93% of the gains in across-school integration induced by court-ordered desegregation. This number is calculated by dividing column

40. Some segregation academies resisted government intervention by refusing to report statistics

41. See, for example, Cascio et al. 2010; Rivkin and Welch 2006; Johnson 2011; Card and Rothstein 2007; Ashenfelter, Collins, and Yoon 2006; Guryan 2004; Anstreicher, Fletcher, and Thompson 2022, among others.

42. See Massey and Denton (1988).

43. 24% of treated districts.

44. OCR did not collect data for 1969, 1971, 1975, 1977, and 1979.

Table 6 — Effect on the Exposure Index

Treatment	Segregation Academy		Court Order	
Scale	Log	Level	Log	Level
	(1)	(2)	(3)	(4)
ATT	-0.14*	-0.04	0.15	0.03
	(0.09)	(0.02)	(0.09)	(0.02)
Observations	4,848	4,856	4,847	4,855
R^2	0.71	0.84	0.71	0.84
Within R^2	0.07	0.05	0.07	0.10
School District fixed effects	✓	✓	✓	✓
Year fixed effects	✓	✓	✓	✓

Notes: **The effect of segregation academies and court orders on the percentage of classmates of Black students that are White. Values are estimated using Equation 4 with no $desegPost$ dummy in columns (3) and (4). A positive value indicates higher relative levels of integration. $***$, $**$, and $*$ indicate significance at the .01, .05, and .10 levels, respectively. Standard errors are clustered at the school district level and appear in parentheses. Data comes from the Office of Civil Rights data collection (OCR), which provides school-level enrollment by race data for the years 1966-1980. We aggregate this up to the school-district-by-year level. OCR only collected data on school districts being monitored for compliance with desegregation orders or plans. 168 school districts in our sample are not included in the OCR reports.**

(2) of Table 6, the decrease in the Exposure Index induced by the establishment of a segregation academy, by column (4), the increase in the Exposure Index induced by a major desegregation order. columns (1) and (3) report the respective effects on the log of the Exposure Index.

8 — Effects on Public School Resources

> "Some politicians are going to do their damndest to see to it that as little money as possible is spent on schools, particularly in counties where all or most of the pupils are black."
> *The Baltimore Sun, December 8, 1969*

In this section, we ask if the exodus to private schools caused *disinvestment* from public schools. We find mixed evidence: while total funding and average teacher salary decline, the large decreases in enrollment cause per pupil funding to increase and average class size to decrease. Our results are estimated using Equation 4.

Table 7 — Education Input Outcomes

Panel A: Alabama, Louisiana, Mississippi, South Carolina, North Carolina Public School Funding

	Log Local	Log State	Log Federal	Log Total	Local PP	State PP	Federal PP	Total PP
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
ATT	-0.08**	-0.10***	-0.02	-0.06***	3.31	-4.49	8.04***	6.86
	(0.03)	(0.02)	(0.14)	(0.02)	(8.30)	(2.99)	(2.36)	(7.81)
Desegregation Post	0.03	-0.01	0.02	0.02	-6.15	-3.07	2.47	-6.74
	(0.04)	(0.02)	(0.10)	(0.02)	(10.65)	(2.61)	(2.73)	(9.86)
Observations	13,406	13,406	13,406	13,406	13,406	13,406	13,406	13,406
R^2	0.97	0.99	0.88	0.99	0.86	0.99	0.91	0.98
Within R^2	0.05	0.06	0.06	0.06	0.06	0.08	0.10	0.05
School District fixed effects	✓	✓	✓	✓	✓	✓	✓	✓
Year fixed effects	✓	✓	✓	✓	✓	✓	✓	✓
Year-State fixed effects	✓	✓	✓	✓	✓	✓	✓	✓

Panel B: Alabama

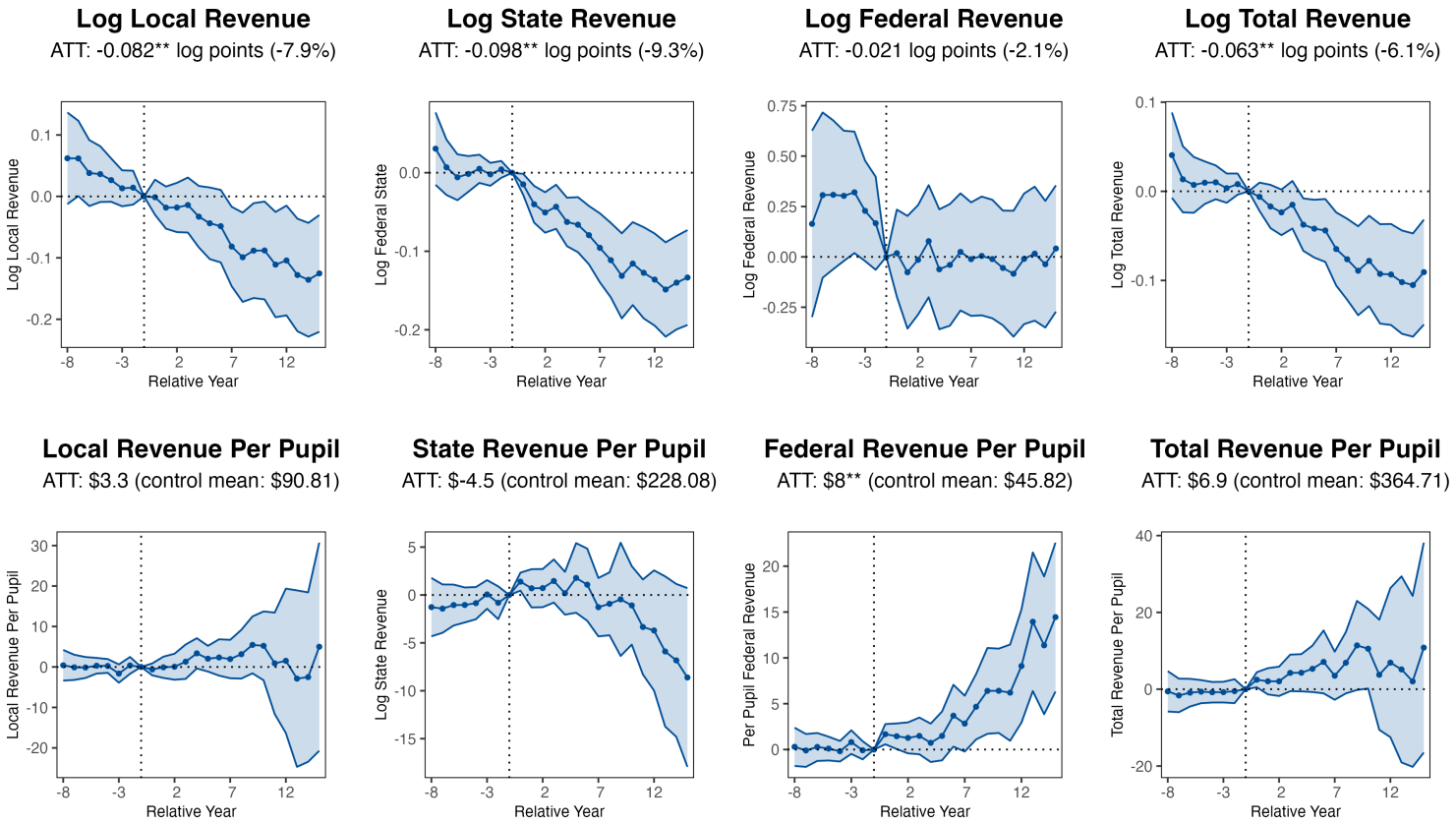
	Log Total	Total PP	Student Teacher Ratio
	(1)	(2)	(3)
ATT	-0.07*	12.06***	-0.44*
	(0.04)	(3.37)	(0.23)
Desegregation Post	0.03	-6.22**	-0.16
	(0.03)	(3.02)	(0.23)
Observations	3,675	3,675	1,516
R^2	0.98	0.96	0.89
Within R^2	0.09	0.10	0.10
School District fixed effects	✓	✓	✓
Year fixed effects	✓	✓	✓

Panel C: South Carolina

	Log Total	Total PP	Avg Teacher Salary
	(1)	(2)	(3)
ATT	0.03	-2.55	-215.47**
	(0.04)	(12.81)	(78.55)
Desegregation Post	-0.01	-6.14	-578.82***
	(0.03)	(10.77)	(197.61)
Standard-Errors	School District		County
Observations	2,028	2,028	579
R^2	0.99	0.99	0.99
Within R^2	0.18	0.13	0.17
School District fixed effects	✓	✓	
Year fixed effects	✓	✓	✓
County fixed effects			✓

Notes: **The effect of the opening of a segregation academies on public school revenue. Total revenue is computed as the sum of local, state, and federal revenue. Georgia is omitted because it only has funding data available for 11 of the 25 years of interest, and no data is available after 1976. Florida is omitted because it only has funding data available for 1960-1973. Sample is limited to observations where no funding outcomes are missing. In some cases, federal revenue is 0. In these cases, log(federal revenue) is log(federal revenue) for non zero values and 0 otherwise. In Appendix Figure A.5, we show results omitting these zeros from our sample. Results are largely unchanged. Control means for local funding (revenue) per pupil, state revenue per pupil, federal revenue per pupil, and total funding per pupil are $91, $228, $46, and $365, respectively. Corresponding event studies are in Figure 6. $***$, $**$, and $*$ indicate significance at the .01, .05, and .10 levels, respectively. Standard errors appear in parentheses. PP indicates per pupil.**

Figure 6 — Funding Outcomes
Alabama, Louisiana, Mississippi, South Carolina, North Carolina



Notes: **The effect of the opening of a segregation academy on public school revenue. Georgia is omitted because it only has funding data available for 11 of the 25 years of interest, and no data is available after 1976. Florida is omitted because there is no data available after 1973. Total revenue is calculated as local revenue + state revenue + federal revenue. Sample is limited to observations where no funding outcomes are missing. In some cases, federal revenue is 0. In these cases, log(federal revenue) is log(federal revenue) for non zero values and 0 otherwise. In Appendix Figure A.5, we show results omitting these zeros from our sample. Results are largely unchanged. Estimates correspond to Panel A of Table 7. Control means for local funding per pupil, state funding per pupil, federal funding per pupil, and total funding per pupil are $91, $228, $46, and $365, respectively. 95 percent confidence intervals are represented by the shaded area. ∗ ∗ ∗, ∗∗, and ∗ indicate significance at the .01, .05, and .10 levels, respectively.**

The establishment of segregation academies did not decrease per pupil funding. In Table 7, we present estimates of the effect of the opening of a segregation academy on revenue. We omit Florida and Georgia from these analyses because of insufficient data on funding. In column (1), we show that local revenue decreases, and in Figure 6, we show this decrease deepens over time. State funding decreases by a statistically significant .10 log points (Table 7, column (2)). Together, state and local revenue declines drive the overall decrease in total funding (Table 7, Column (4)): federal revenue does not decrease (column 3). Columns (5)-(8) of Panel A of Table 7 demonstrate the decline in enrollment more than offsets the decrease in funding *with the exception of state funding*. Total funding per pupil increases by $6 post segregation academy opening, a 2% increase relative to the control mean of $365 total funding per pupil (the control mean for local funding per pupil is $91).

Local financing would be most directly affected by backlash: a White voter may be less willing to vote to fund public education (either directly or through the election of a public school friendly politician) if the proportion of students attending private school increases or if public schools are perceived as primarily serving Black students (Alesina, Baqir, and Easterly 1999). A report on 16 counties in the Black Belt of Alabama, published in 1982, describes this mechanism directly: "needed tax increases to support public schools are blocked by Whites whose children by and large attend private schools" (Johnson and Pearson 1982). Similarly, in Virginia, Orfield 1969 describes how "one county fell from eleventh to ninety-first position [in ranking of local public support for education] in the state after a private school movement took hold." In an open letter to the Caddo Parish School Board (Louisiana), a person writes: "Your attention is invited to the ... private schools ... several thousand fewer children will attend the public (federal) schools ... we then come to the small matter of a... increase in your budget ... more to spend on ... fewer students may strike some taxpayers as being slightly more than needed" (The Shreveport Journal, April 3, 1970). State funding in all five states was at least partially determined by enrollment at this time.[45].

To distinguish between the explanatory power of these two mechanisms — enrollment declines and antipathy towards public schools — we report the share of variation in declines in funding explained by enrollment declines, by revenue source, in Table 8. We describe how we calculate residual enrollment and funding declines in Appendix F.1. The R^2 between state revenue and enrollment is the highest: this supports the claim that state revenue adjusted mechanically to enrollment declines. Local revenue declines are significantly positively associated with enrollment

45. "It is clear that private schools have had a negative effect on public schools, particularly in the area of school finance. First, by siphoning off large numbers of students who would otherwise attend public schools, private schools consequently reduce average daily attendance figures on which State funding levels are based."
Johnson and Pearson 1982

declines, but a small share of the variation is explained by enrollment: 0.28, compared to 0.67 for state revenue. As expected, the share explained by federal revenue is the smallest (0.09). Federal revenue declines are actually *negatively* associated with enrollment declines, highlighting the role federal revenue played in mitigating the effect of segregation academies on funding.

We hypothesize two federal actions effectively mitigated the negative impacts of academies on public school resources. First, the 1965 Elementary and Secondary Education Act made an unprecedented amount of funds available to students allocated based partially off of the share of students in poverty. This underscores the important role of federal funding, which was the only source of revenue to not decrease after the opening of a segregation academy. Without federal funding, per pupil funding would decreased (Appendix Table A.4).

Second, beginning in the early 1970s and motivated by the equal protection clause 14th Amendment, a series of school finance reforms restricted the ability of states to curtail spending to districts and equalized spending between low and high income districts (Jackson, Johnson, and Persico 2014). Though only three states in our sample changed their financing system during the 1970s and 1980s,[46] the threat of litigation could have reasonably dissuaded states from targeting predominantly Black school districts with funding cuts. While state funding fell significantly, the declines in state funding are almost perfectly correlated with the declines in enrollment. In short, we argue that these policies mitigated the potentially negative indirect effects that segregation academies would have had on Black students.

In Panel B of Table 7, we narrow our focus to Alabama, where data on student teacher ratio (class size) exist. Per pupil funding increases even more in Alabama (column (2)) — $\$67$, or approximately 10% of the control mean for Alabama ($\$644$). A decrease of approximately $2/5$ of a student per class (control mean: 20 students) accompanies this increase. However positive, both of these results could be due to a lack of an immediate response to declining enrollments. For example, average class size may decrease in the short run, but increase in the long run as schools adjust.

Counter to this story, Figure 7 suggests average class size remains smaller in treated counties at least 10 years out. Though class size stayed small, it is possible that the average tenure of public school teachers may have decreased after the establishment of a segregation academy. For

46. Florida changed the state funding formula from a combination of minimum foundation program (MFP) and fixed grant (FG) to one that combined a MFP, equalization plan (EP), and local effort equalizations (LE) legislatively in 1973 through the Florida Education Finance Program. North Carolina changed its formula from FG to FG + EP in 1987. South Carolina changed its formula in 1977 from FG to MFP+LE. See Table D.1, Jackson, Johnson, and Persico 2014 for more detail.

Table 8 — Correlation of Funding Declines and Enrollment Declines

	Local	State	Federal
	(1)	(2)	(3)
Public Enroll	0.72***	0.78***	-1.36***
	(0.02)	(0.01)	(0.08)
Observations	3,266	3,140	2,961
R^2	0.28	0.67	0.09
Adjusted R^2	0.28	0.67	0.09

Notes: **The correlation of residual funding declines with enrollment declines. Our sample is limited to school district years post treatment. Appendix F.1 describes how these residuals are calculated. Enrollment declines are strongly positively associated with both state and local revenue declines. Inspection of the R^2 value reveals that enrollment declines explain a higher share of state revenue declines than local. State revenue was often explicitly determined by formulas based off of average daily attendance. A combination of voters and local politicians determine local revenue, which tracks less closely with enrollment. Federal revenue declines are *negatively* associated with enrollment declines. $***$, $**$, and $*$ indicate significance at the .01, .05, and .10 levels, respectively.**

example, in Monroe County, Alabama, a historian has documented that at least seven teachers left the public schools to teach at the local segregation academy (Monroe Academy).[47] Using data on public school teachers by race from the Office of Civil Rights surveys, we find that the opening of a segregation academy causes the number of White public school teachers to decrease 12% by 1972 (Table 9). This data is only available for 1960 through 1972. We expect the actual amount of attrition to be higher, assuming White teacher attrition from public schools tracks with student attrition, which continues to grow throughout the 1970s. Conversely, we find places with segregation academies increased the number of Black teachers more than places without by the early 1970s.

In Panel C of Table 7, we proxy for teacher experience by testing for changes in average teacher salary in South Carolina (teacher salary typically increases with tenure). We find a decrease of approximately $\$215$, or 3% of the control mean of $\$8,400$. This decrease deepens over time, as shown in Figure 7. However, this decrease could also reflect a change in the racial composition of teachers. Cascio and Lewis 2024 shows that Black teachers were paid systematically less than White teachers, including in South Carolina.

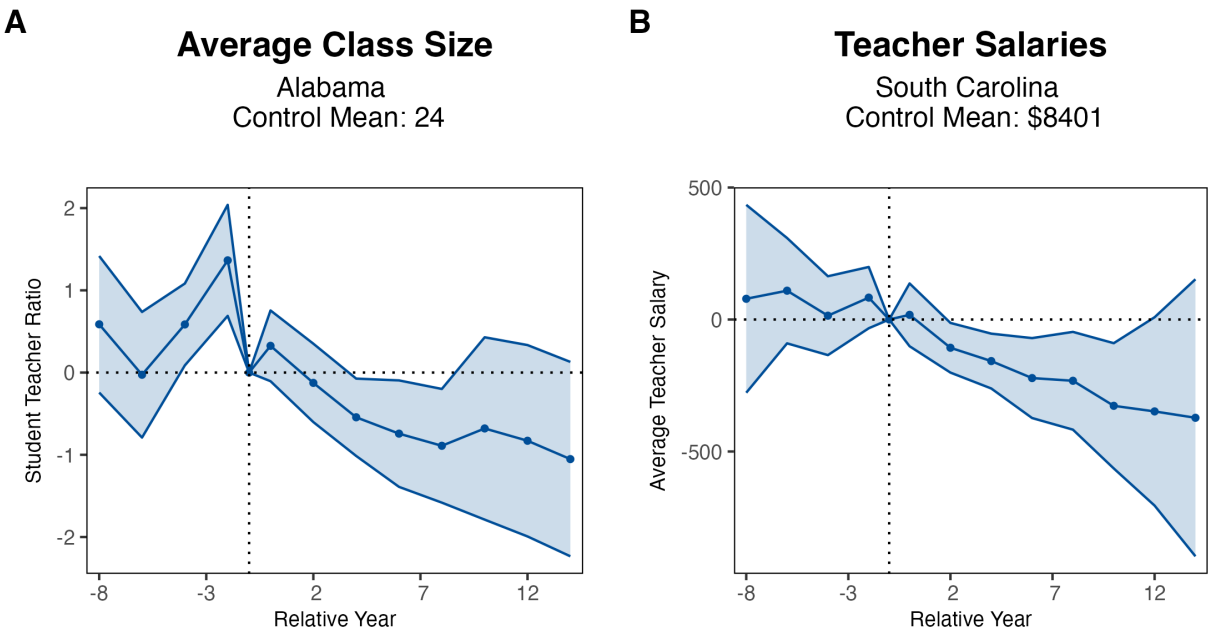
47. *Source:* Amberly Sheffield, who compared Monroe County Board of Education minutes to Monroe Academy's first yearbook.

Table 9 — Relative Trends in Employment of Teachers, by Race

	Log Black Teachers	Log White Teachers
	(1)	(2)
Post Segregation Academy × Year = 1961	-0.06*	0.11
	(0.04)	(0.09)
Post Segregation Academy × Year = 1962	0.00	0.04
	(0.05)	(0.04)
Post Segregation Academy × Year = 1963	-0.03	-0.17
	(0.04)	(0.11)
Post Segregation Academy × Year = 1964	-0.02	-0.15
	(0.03)	(0.16)
Post Segregation Academy × Year = 1967	0.09**	0.06*
	(0.04)	(0.03)
Post Segregation Academy × Year = 1968	0.05*	0.03
	(0.03)	(0.03)
Post Segregation Academy × Year = 1970	0.09**	-0.10**
	(0.04)	(0.04)
Post Segregation Academy × Year = 1972	0.11**	-0.12***
	(0.05)	(0.04)
Observations	2,732	2,732
R^2	0.96	0.92
Within R^2	0.01	0.02
School District fixed effects	✓	✓
Year fixed effects	✓	✓
State-Year fixed effects	✓	✓

Notes: **Changes in the log of teachers by race in treated vs. control school districts. To avoid infinite values, log(0) has been replaced with 0. Points represent δ_t from $Y_{i,t} = \alpha_i + \lambda_t + \theta_{s(i)t} + \sum_{t \neq 1960, \ell > 0} \delta_t t \times D^{\ell}_{i,t} + \epsilon_{i,t}$, the regression of log teachers on year interacted with an indicator = 1 if a school district is treated, controlling for school district, year, and state by year fixed effects. Standard errors are clustered at the school district level. Data comes from the Office of Civil Rights (OCR) Data Collection. OCR only collected data on school districts being monitored for compliance with desegregation orders or plans. 168 school districts in our sample are not included in the OCR reports. OCR stopped collecting data on teachers in 1972. We report the coefficient interacted with the year in place of an event study because the modal treatment year is 1970 and therefore event study estimates are not easily interpretable. Observations are at the school district by year level. $***$, $**$, and $*$ indicate significance at the .01, .05, and .10 levels, respectively.**

Figure 7 — Class Size and Teacher Salaries: Public Schools



Notes: **The effect of segregation academies on teacher inputs. 95 percent confidence intervals are represented by shaded areas. Estimates come from 4. Data come from reports from the State Departments of Education for Alabama and South Carolina, respectively.**

9 — Measuring the Effect on Adult Outcomes

We next turn to assessing the long run impacts of childhood exposure to a segregation academy on human capital and labor market outcomes using restricted Census Bureau administrative and survey data. In this section, we describe that data and our empirical strategy.

9.1. Data

Our outcome data come from 2001 to 2019 the American Community Survey (ACS), the 2000 Decennial Long Form (2000-LF), and IRS 1040 forms from 1994-1995 and 1998-2020. The ACS and 2000-LF include demographic characteristics, information on labor force participation, educational attainment, occupation, and location at the latitude and longitude level. IRS 1040s include information on filing status, location at the latitude and longitude level, and household income. We assign treatment status using birth year and place of birth at the county-level as recorded in the Census Numident, which is an administrative dataset of every person who has applied for a

social security number (SSN) shared with Census by the Social Security Administration.[48]

We keep all individuals appearing in the Numident born between 1942 to 1980 in the Southeast. We further limit our samples to individuals who we later observe in their prime-earning years: ages 25-54. The Census Bureau assigns individuals in the Numident a Protected Identification Key (PIK), which allows us to link between internal Census datasets (Wagner and Layne 2014). We then construct two main samples. The first is all of our Numident individuals who completed the ACS between 2001 and 2019 or the 2000-LF. In our second sample, we link our Numident individuals to their IRS 1040 forms. Table A.5 displays the rounded number of observations for each of our samples by race and treatment status.

As in Bailey, Sun, and Timpe 2021, we collapse our samples by birth year, survey year, county of birth, and age. For our ACS and 2000-LF samples, this collapse is done using the provided sampling weights. We then weight each cell by the number of observations. For our main results, we only keep those who have non-imputed and non-missing values for all outcomes and controls, limiting privacy issues that may arise with implicit samples to comply with the Census Bureau's disclosure process. Table 10 shows the main demographic summary statistics for our treatment and control groups by sample. We consider individuals as treated if they are age 17 or younger and born in a county where 30% of the population lives within a 15 mile radius of a segregation academy.

Long run human capital and labor market outcomes come from the ACS and 2000-LF. We measure human capital using educational attainment, high school graduation, college entrance, and 4-year college graduation. Labor market outcomes of interest include binary indicators of employment, poverty status, income from public sources, family income, and income from other nongovernmental sources. Following Bailey, Sun, and Timpe 2021, we also compute summary indices for human capital and economic self-sufficiency measures. The human capital index includes four binary indicators of educational attainment: high school or GED, some college, a four-year college degree, a professional or doctoral degree, a continuous measure of years of schooling, and a binary indicating if the individual is working in a managerial role.[49] Economic

48. County of birth is an imperfect proxy for childhood location. We address this directly through a weighting exercise in Appendix Section E.3.

49. For comparability with the results in Bailey, Sun, and Timpe 2021 and Anstreicher, Fletcher, and Thompson 2022, we adopt the Bailey, Sun, and Timpe 2021 definition of a managerial role: occupation code 1 through 359 (management, business, science and arts occupations). This excludes from managers: property, real estate, and community association managers, postmasters and mail superintendents, and miscellaneous other managers. Examples of other excluded occupations include business operations specialists; financial specialists; architecture and engineering occupations; life, physical, and social science occupations; community and social services occupations; legal occupations; education, training, and library occupations; healthcare; service.

Table 10 — Summary Statistics: Demographics

	Treat	Control
ACS Sample		
Share Black	0.29	0.20
Share Female	0.51	0.50
Birth Year Mean	1966	1965
Birth Year Std. Dev.	8.22	8.28
Birth Year Median	1966	1965
IRS Sample		
Share Black	0.32	0.22
Share Female	0.53	0.53
Birth Year Mean	1965	1965
Birth Year Std. Dev.	8.84	9.06
Birth Year Median	1966	1965

Notes: **Table shows demographic characteristics of our two main samples.** ***Source:*** **2000 Long Form Decennial Census, ACS 2001-2019, IRS 1040s 1994, 1995, 1999-2019, and Census Numident.**

self sufficiency indices include continuous measures of weeks worked, usual hours worked, the log of labor income, log of other income from nongovernmental sources, and log ratio of family income to the federal poverty threshold.

Occupation characteristics are derived from occupation as reported in the ACS or 2000-LF. We standardize all occupation codes to the `occ2010` classification system using a crosswalk compiled by IPUMS.[50] First, we create a binary indicator for working in an instructional role in the K-12 setting, `teacher`, that includes teachers in preschool, kindergarten, elementary school, secondary school, special education teachers, tutors, other educational instruction and library workers, teaching assistants, and other teachers.[51]

We also create occupation-specific variables for years of education required, job requires a 4-year college degree, underemployment, social skills, content skills, and process skills. All measures are derived from O*NET. The first three are derived from estimated level of education required; the last

50. The 2000-LF and 2001-2009 ACS samples use the 2000 SOC codes; 2010-2017 use 2010 codes; 2018-2019 use 2018 codes. A full description of changes in occupation codes and a cross walk are available at `https://usa.ipums.org/usa/volii/occtooccsoc18.shtml`

51. `occ2010` codes 2300, 2310, 2320, 2330, 2350, 2360, 2555, 2545. Though our interest is in the K-12 workforce, we include preschool teachers because they classified jointly with kindergarten teachers.

three from level of skills.[52] O*NET reports occupation characteristics at the level of 2010 National Employment Matrix SOC Occupation code (NEM-OCC). The Bureau of Labor Statistics provides a crosswalk mapping NEM-OCC to `occ2010`. Because NEM-OCC is more granular than `occ2010`, we take an average of each raw measure across `occ2010` code, weighted by the nationwide share of employment in each NEM-OCC code that can be attributed to each `occ2010` code in 2023. `Underemployed`, which we use to test for labor market discrimination, is a binary variable that is 1 if years of education, as measured by the ACS or 2000-LF, is greater than the weighted average of years of education required for each occupation. We use content and process skills, which make up the "basic" category, as another proxy for changes in school quality. "Basic" skills are typically learned in the classroom. Content skills are those associated with standardized testing: they include active listening, mathematics, reading comprehension, science, speaking, writing. Process skills include active learning, critical thinking, learning strategies, and monitoring.

Neighborhood characteristics come from 2010 census tract characteristics that we match onto the filing addresses reported for each household in the IRS data. We use this to match onto 2010 census tract characteristics. These include share White, share Black, median household income, share poor, average annual job growth from 2004 to 2013, and employment rate in 2000. These all were compiled by the Census Bureau for the Opportunity Atlas and use 2010 tract boundaries (Chetty et al. 2025). Tables A.6 and A.7 summarize the outcome variables for the ACS Sample. Table A.8 summarizes the outcome variables for the IRS sample.

9.2. Estimation Strategy

We compare changes among birth cohorts within counties in a differences-in-differences style analysis that leverages two sources of variation: age at segregation academy opening and location. Our public school analysis makes use of panel data. Our individual outcome data is instead structured as repeated cross sections. We compare individuals who are of school age or younger when a segregation opens nearby to (1) individuals within the same county who are older than school age when a segregation academy opens and (2) individuals born the same year in counties without a segregation academy. In other words, we include both birth county and birth year fixed effects. To control for state-wide policy or economic shocks that affect birth cohorts differentially across states, we also include state-birth year fixed effects. Survey year fixed effects account for macroeconomic conditions during the year of measurement. We control for the average effect of court-ordered desegregation on our outcomes of interest with $desegPost_i$.[53] This binary variable

52. O*NET reports both level and importance of skill. We follow Deming 2017 and use level.

53. For a description of the construction of $desegPost$, see Appendix B.1

indicates if individual i was younger than 18 when her birth county came under a desegregation order. We cluster standard errors at the level of treatment (county of birth). e is the year a segregation academy opened, and ℓ is the age at time of segregation academy opening:

$$Y_{i(c,t)} = \underset{\text{birth county}}{\alpha_c} + \underset{\text{birth year}}{\lambda_t} + \underset{\text{survey year}}{\zeta_{t'}} + \underset{\text{state-birth year}}{\delta_{st}} + desegPost_i + \sum_{e} \sum_{\ell \neq 18} \delta_{e,\ell}(\mathbf{1}\{E_i = e\} \cdot D_i^{\ell}) + \epsilon_i. \tag{8}$$

We interact the relative period indicator $D_{i,t}^{\ell}$ with an indicator for the treatment year, $\mathbf{1}\{E_i = e\}$, to estimate the coefficients $\delta_{e,\ell}$ for each combination of treatment year e and relative period ℓ. These coefficients are our main parameters of interest. To recover the treatment effect for each relative period ℓ, we compute a weighted average of $\delta_{e,ell}$ using the share of counties treated in year e as weights. Finally, we estimate the Average Treatment Effect on the Treated (ATT) by averaging the relative period effects over the post-treatment periods, defined as $\ell < 18$. ℓ is estimated relative to individuals aged 18 when a segregation academy opened nearby.

Treated individuals are those potentially affected by the opening of a segregation academy during their school age years (defined as age 18 or younger). Theoretically, individuals who are older than 18 could still be indirectly affected by the opening of a segregation academy. We discuss these implications for the interpretation of our results in the next section.

10 — Effects of Segregation Academies on White Students

In this section, we study the long-term effects of exposure to segregation academies on the human capital and labor market outcomes on White students. School integration expanded access to the resources previously concentrated in all-White public schools. Theoretically, creating a new private school is a response to concerns over potential erosion of the perceived value of the state-provided good. Is this perception of value determined by accurate beliefs about the effect of a school on students' later-life outcomes, such as educational attainment or socioeconomic status? Or, as in Becker 1957, is the perception of value affected by preferences over the racial composition of the student body?[54] We find evidence supporting the latter explanation: White children exposed to a segregation academy have worse outcomes in adulthood, implying that

54. "Most backers of the new schools . . . insist that their only purpose is to provide 'quality education.' Quality is jeopardized, they say, when poorly prepared kids, regardless of race, crowd into classrooms . . . Others say Negroes and whites should sit in separate schools" (Adams and Covington 1969).

White parents may have traded off school quality in order to maintain segregation.

10.1. Results

First, we find that exposed White children fare worse in terms of educational attainment. In Figure 8, we show that exposure to a segregation academy at age 6 decreases the human capital index, a composite measure that incorporates six standardized measures of human capital, by 4.2% of a standard deviation.[55] Total years of schooling declines by 0.2 of a year. As a comparison, these effects are approximately one fourth and one half as large as the (positive) effects of court-ordered integration on southern Black students, respectively (Anstreicher, Fletcher, and Thompson 2022).

Decreases in human capital translate to worse economic outcomes. We find White students exposed to segregation academies perform worse across almost all measures of economic self sufficiency. A composite measure of labor market outcomes, the economic self sufficiency index, is 5.5% lower for students exposed at age 6; this is approximately one fifth of the benefit of integration for southern Black students.[56] These measures decrease on both the intensive and extensive employment margin: exposed students participate in labor force 2% less in adulthood, earn .15 log points (14%) less labor income, and work .78 fewer weeks.[57]

10.2. Mechanisms

If segregation academies offer poor quality education, White students will be worse off. As we saw in Table 1, segregation academies have lower college matriculation rates than other private schools. In a modern setting, Abdulkadiroglu, Pathak, and Walters 2015 find a large negative effect on achievement from voucher program in Louisiana. Negative effects on math are driven by low-tuition private schools. As described in Section 3.2, there exists anecdotal evidence that segregation academies were both low-tuition (which may translate to poorly resourced) and low-quality.[58]

The effect on White students attending the public schools is more nuanced. Segregation academies

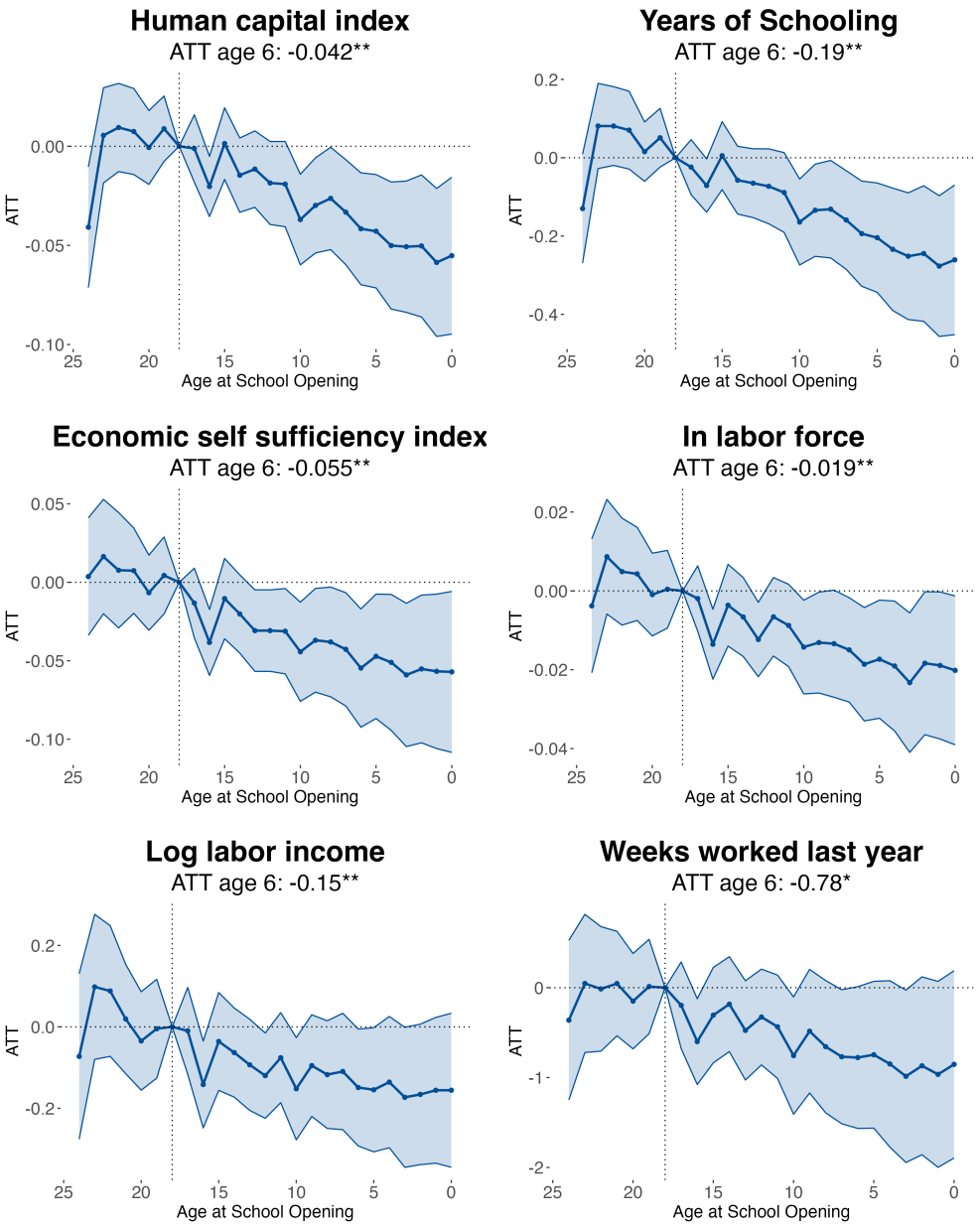
55. This index is also used by Bailey, Sun, and Timpe 2021 and Anstreicher, Fletcher, and Thompson 2022. Impacts on the components of this index are shown in Appendix Figure A.6.

56. For individuals with 0 labor income, we replace labor income with the lowest observed positive value.

57. We adjust for pre-trends in Appendix Table E.2.

58. "The Southern Regional Council reports that the caliber of teachers, many who came out of retirement, remains substandard in the segregation academies, and their curriculum far less than that offered in public schools . . . Many of the new private schools are operating on shoestring budgets, charging tuitions of $250 to $700 per child, as compared with the $1,000 and up charged by most established private schools" (Johnson 1971b).

Figure 8 — Long Term Outcomes: White Students



Notes: **Results are from estimating Equation 8. The average treatment effect is for individuals aged 6 when a segregation academy opens nearby. $*$, $**$, $***$ represent significance at the 10, 5, and 1 percent levels, respectively. Shaded areas represent 95% confidence intervals. Human capital index is a standardized measure adopted from Bailey, Sun, and Timpe 2021 that consists of years of schooling and binaries for high school degree or GED, any college, at least four years of college, professional or doctoral degree, and work in management job. Economic self-sufficiency is a standardized measure adopted from Bailey et. al 2020 consisting of binary indicators of employment, poverty status, income from public sources, family income, and income from other nongovernmental sources. Effect sizes of the Human Capital Index and Economic Self Sufficiency Index are measured in standard deviations. Components of these indices are reported in Appendix Figures A.6-A.7. We adjust the estimates for pre-trends in Appendix Table E.2.**
Source: **2000 Long Form Decennial Census, ACS 2001-2019 and Census Numident.**

shifted students' peer groups by isolating White children with (1) the resources to and (2) the desire to attend academies from children in public schools. In a summary of the peer effects literature, Sacerdote 2011 finds that high-achieving students benefit from exposure to other high-achieving peers, but that these effects are concentrated among same-race peers. A recent paper, Wu, Zhang, and Wang 2023, finds low-achieving students paired with high-achieving students perform better. These results suggest that low-achieving White students may be the most negatively affected by the sorting of high-achieving White peers from public to private schools or from private to more expensive private schools.

In the above results, we do not observe whether the individuals attended a segregation academy or a public school, and thus cannot directly estimate the effects on these different groups. To descriptively decompose the estimated treatment effect into effects on White students in private schools and White students in public schools, we develop a mixture model that leverages variation in the estimated percentage of students attending segregation academies in Appendix Section G and report results in Appendix Table G.1.[59] We that find private school attendance accounts for the majority of the effect on the human capital index and log labor income, but public school attendance accounts for the majority of the effect on the weeks worked. For all other outcomes, the contribution is approximately equal.

11 — Effects of Segregation Academies on Black Students

Segregation academies decreased integration, but inadvertently increased per pupil funding by reducing the number of students in the public schools. The existing literature on the effect of court-ordered school integration studies the effect of a bundled treatment: increases in inter-group contact accompanied by increases in funding. In our setting, we observe decreases in inter-group contact accompanied by increases in per pupil funding. Naively, we anticipate the former will negatively effect Black student outcomes — segregation academies could preserve racist cultural norms, both by limiting interracial contact and signaling the community's values, thereby implicitly encouraging open racial hostility (Williamson and Holcomb 2025). However, the literature on school finance establishes a conclusive link between increases in per pupil funding and improved outcomes in adulthood (see Jackson and Mackevicius 2024 for a summary).

59. Because this variation is endogeneous, we consider this a descriptive exercise.

11.1. Results

As shown in Figure 9, we find no effect on educational attainment or employment of school-aged Black individuals compared to 18 year-olds in treated counties and birth cohort peers in control controls.[60] We find the effect on Black students likely varied from county to county, but on average, countervailing forces led to a suggestive overall null effect on Black students.

However, those older than school age fare worse than individuals aged 18 when a segregation academy opened. This complicates our interpretation: as an intermediate step to account for this ambiguity, we de trend our estimates. We cannot rule out negative effects. We discuss this further in the mechanisms subsection.

However, we do find evidence consistent with a known consequence of a decrease in inter-group contact: Black individuals live in more racially segregated neighborhoods in adulthood (Figure 10). Bleemer 2021 and Bleemer and Jaynes 2025 show that Black individuals exogenously exposed to integrated environments in childhood and early adulthood live in more integrated neighborhoods in adulthood. Our finding shows the opposite is also true: segregated environments in childhood entrench segregation in adulthood.[61] We can also explain this pattern through the lens of the tipping point literature, notably Card and Rothstein 2007, which finds that White population decline accelerates after neighborhoods reach a "tipping point" in the share of minorities. Ostensibly, the increase in share Black and corresponding decrease in share White can be interpreted as an acceleration in the rate of White flight, which could be caused by some interaction of the higher initial share minority in treated counties and increases in racial animus associated with the establishment of segregation academies.[62]

11.2. Mechanisms

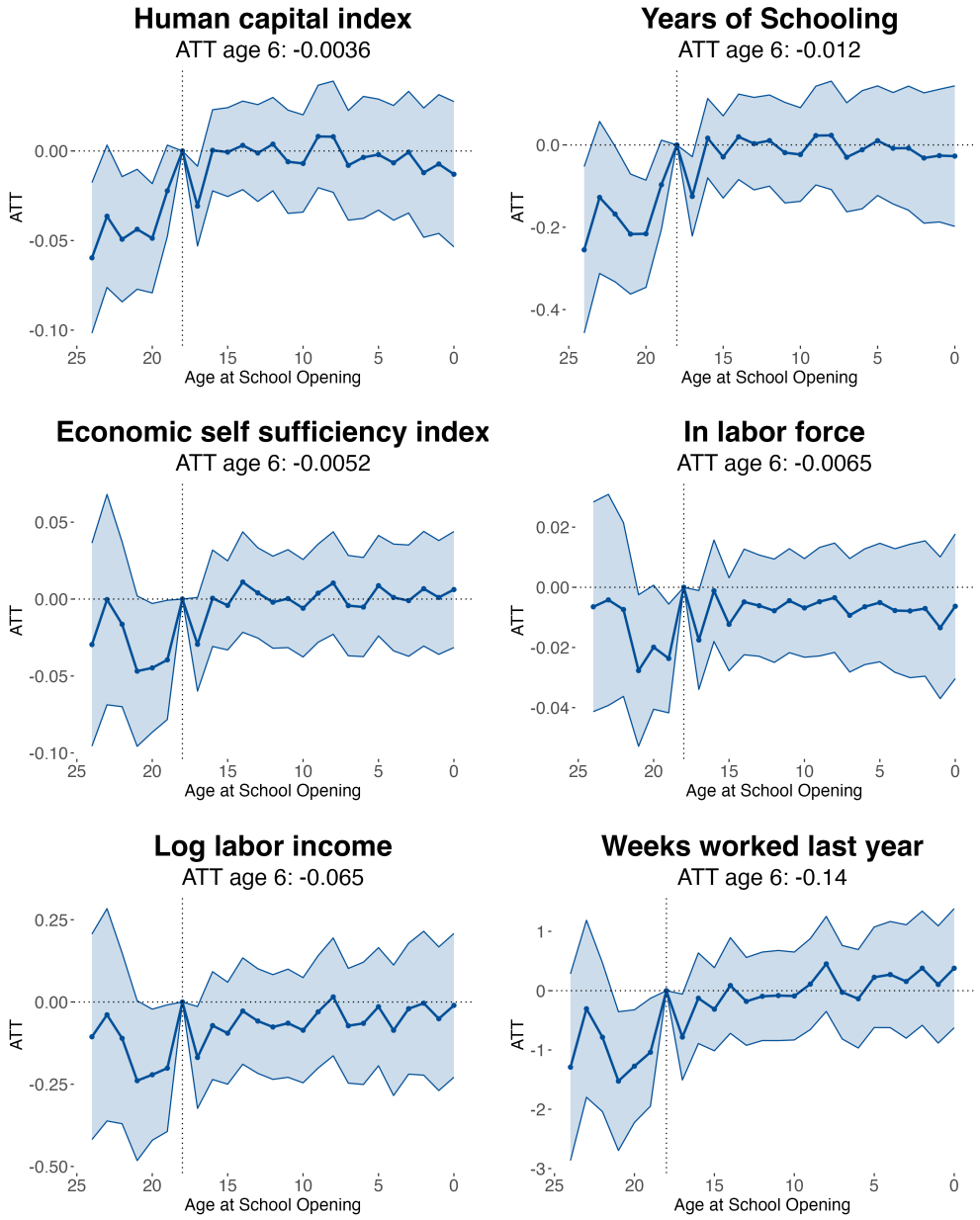
The mechanisms that affect the long-run outcomes of exposed students fit into one of two groups: "schooling" and "community." Schooling mechanisms include the quality of segregation academies

60. For additional results on Black students, see Appendix Figures A.10-A.11.

61. Perhaps surprisingly, White students do not live in more racially segregated tracts.This is likely because the level of segregation for White individuals is already remarkably high: on average, a White individual in the control sample lives in a tract that is over 80% White. Variance in share White is also remarkably low (only 16%). This is consistent with the literature on residential segregation that finds that ideal neighborhood composition for Black individuals different than for White individuals (Krysan et al. 2009), with White individuals reporting a much higher preference for same-race neighbors. Additionally, both Bleemer 2024 and Bleemer 2021 find that childhood exposure to inter-group did not changed neighborhood composition for White individuals.

62. A separate paper finds increases in racial animosity among White individuals and a decrease in the likelihood of having a Black county official after the opening of a segregation academy (Williamson and Holcomb 2025).

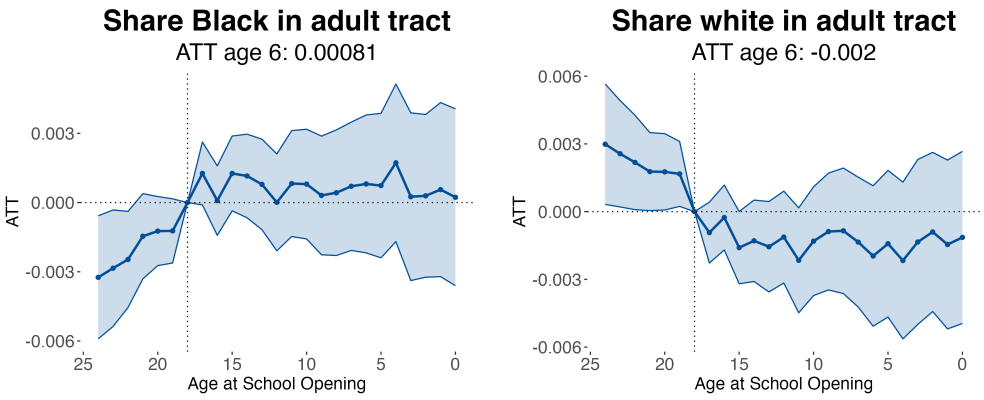
Figure 9 — Long Term Outcomes: Black Students



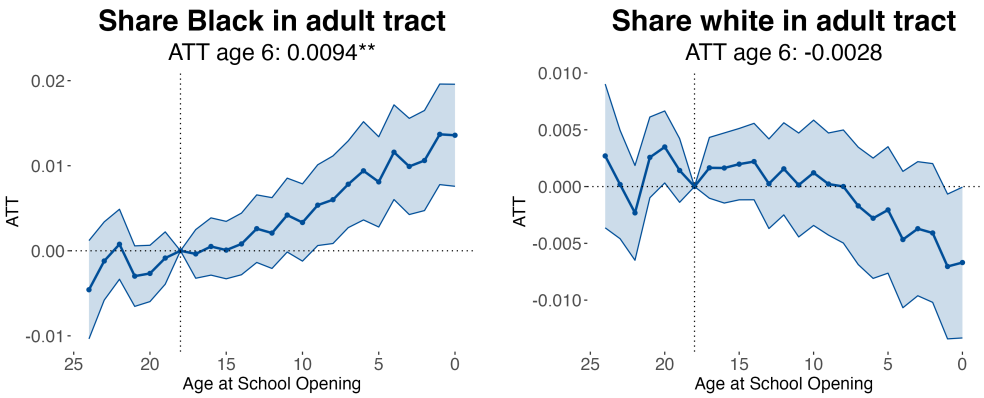
Notes: **Results are from estimating Equation 8. The average treatment effect is for individuals aged 6 when a segregation academy opens nearby. $*$, $**$, $***$ represent significance at the 10, 5, and 1 percent levels, respectively. Shaded areas represent 95% confidence intervals. Outcome variables come from the 2000 long form decennial census and 2001-2019 ACS. Human capital index is a standardized measure adopted from Bailey, Sun, and Timpe 2021 that consists of years of schooling and binaries for high school degree or GED, any college, at least four years of college, professional or doctoral degree, and work in management job. Economic self-sufficiency is a standardized measure adopted from Bailey et. al 2020 consisting of binary indicators of employment, poverty status, income from public sources, family income, and income from other nongovernmental sources. Effect sizes of the Human Capital Index and Economic Self Sufficiency Index are measured in standard deviations. Components of these indices are reported in Appendix Figures A.10-A.11. We adjust the estimates for pre-trends in Appendix Table E.2. *Source:* 2000 Long Form Decennial Census, ACS 2001-2019, and Census Numident.**

Figure 10 — Racial Composition of Tract of Residence in Adulthood

White Students



Black Students



Notes: **Results are from estimating Equation 8. The average treatment effect is for individuals aged 6 when a segregation academy opens nearby. $*$, $**$, $***$ represent significance at the 10, 5, and 1 percent levels, respectively. Shaded areas represent 95% confidence intervals. Boundaries are based on 2010 Decennial Census definitions using individual's filing address. Sample includes all individuals that filed a 1040 form in 1994, 1995, or 1999-2019 and born between 1942 and 1980.** ***Source:*** **IRS 1040s 1994, 1995, 1999-2019, and Census Numident.**

themselves, as well as the effect segregation academies have on the provision of public schooling as discussed in Section 6. Community mechanisms include the indirect impacts segregation academies may have had on an area.

Schooling mechanisms likely improved outcomes for Black children, all else held equal: in addition to the positive impacts on per pupil funding, the number of Black teachers increased relatively in places with segregation academies (Table 9).[63] Prior research shows that same-race teachers have a positive effect on minority student performance (Dee 2005, 2004). Thompson 2022 documents that the number of Black teachers decreases with the share of students attending desegregated schools. Because segregation academies decreased public school integration, it is possible that the exodus of White students from the public school system may have, in some cases, transferred more power over the school system to Black residents. In Clarke County, Alabama, home of four segregation academies,[64] a 1982 report stated "respondents familiar with the public schools believe that…the public schools compare favorably with the private academies in terms of staffing, facilities, and curriculum. They also believe that the public schools are often better off without students whose parents oppose school desegregation" (Johnson and Pearson 1982).

Community mechanisms were likely negative: school integration shifted racial attitudes (Chin 2024; Kaplan, Spenkuch, and Tuttle 2025). By decreasing school integration, segregation academies preserved racial attitudes (Williamson and Holcomb 2025) and may have emboldened racial discrimination. However, our cohort-based empirical design is not well-suited to identifying community effects. Our design rests on the assumption that segregation academies do not effect the adult outcomes of individuals who are older than age 18. This assumption is merited for the schooling mechanisms; however, young adults could have still been affected by the opening of a segregation academy. This limitation is reflected by non-zero coefficients in the event study plots presented in Figures 9.

For Black students in particular, it is worth asking why the later in life outcomes of individuals who were young adults when a segregation academy opened are worse than 18 year olds in their county of birth and their birth cohort peers in places without a segregation academy. The first plausible explanation is that some of these young adults were still attending high school: around 20% of Black students graduated high school between ages 19 and 24 during this period compared to 12% of White students (Joo 2016). These students — that is, those taking longer than 12 years to graduate — could be the most vulnerable to the impacts of segregation academies on schooling

63. However, Black individuals were no more likely to become a teacher in adulthood (Appendix Figure A.13).

64. Grove Hill Academy, Thomasville Academy, Salt Springs Academy, and Jackson Academy. The headmaster at Salt Springs agreed to be a guest speaker at a 1977 Ku Klux Klan rally, members of which financially supported the academy (Rawls 1977; *The Montgomery Advertiser* 1977)

and could have been induced to drop out.

The second plausible explanation is that segregation academies could effect local labor markets. For example, by preserving segregation, the schools could increase discrimination in the local labor force or act as signals of racial animosity, potentially dissuading outside investment in the community, prompting employers to leave, or discouraging tourism. Using the Census Bureau series of County Business Patterns, we do not find evidence that investment declined (Appendix Figure A.8). We also find no change in the likelihood that a Black individual is employed in a position that requires less education than they possess (Appendix Figure A.9).[65]

We describe two strategies to isolate schooling effects from community effects. First, we compare exposed movers to non exposed movers and exposed stayers to non exposed stayers in Figures A.14-A.15. Results are similar for Black movers and stayers across our outcomes of interest. However, labor market effects are strongest for White movers, suggesting an interaction between segregation academy establishment and local labor market conditions. We caution against over-interpretation of these results. While similar mover designs have been used in the literature to isolate the effect of an intervention from environmental factors. However, we do not observe age at move, meaning "move" is defined as lifetime migration in our data. Movers could have therefore moved during childhood or adulthood.[66] Stayers may be negatively selected: moving, especially during childhood, could be an endogeneous response to to the establishment of a segregation academy.

Second, following Dobkin et al. 2018, we de-trend our estimates by a linear approximation of the pre-trend in Appendix Section E.4. Estimates derived from this method reflect deviations in outcomes from preexisting linear trends in birth year that differ by treatment status. The advantage of this approach is that it relaxes the parallel trends assumption key to the cohort design; the disadvantage is that it makes the strong assumption that the counterfactual path of outcomes is linear in birth year. If we assume the preexisting linear trend in birth year is a reflection of negative community impacts, then the results in Panel B of Appendix Table E.2 theoretically isolate the schooling channels, which we would expect to only effect individuals school aged or younger when a segregation academy opens. Consistent with the discussion of schooling mechanisms above, economic outcomes improve for Black students, though this effect is likely overstated.[67]

65. We show in Appendix Figure A.12 that Black individuals are more likely to work in an occupation requiring a BA or more years of education. There is no difference in the skills associated with schooling (basic, process).

66. Similarly, stayers could have relocated to their birth county after moving away in childhood or adulthood.

67. As discussed in Freyaldenhoven, Hansen, and Shapiro 2019, this approach understates the influence of confounding factors for older school aged individuals (immediate post-periods) while overstating the importance for

12 — Conclusion

> "The movement itself appears to be fundamentally viable and **segregationist academies are likely to be a permanent part of the education picture in the south**."
>
> Nevin and Bills 1976

In their most extreme form, "segregation academies" emulated and perpetuated the segregated, dual-system of public education that prevailed prior to *Brown v. Board of Education*. The result of this organized resistance was that the demographics and composition of public schools radically shifted.

We create a novel data set that documents the location and nature of these schools and show that they are different than other private schools. In a counterfactual world in which White parents were not able to receive support and summon the resources to establish these segregated academies, public schools would have been more integrated 15 years out.

We contribute to the literature on school desegregation by compiling both descriptive and causal evidence that segregation academies affected effective levels of integration. Court orders are often treated as exogenous sources of variation, but many segregation academies were established prior to court orders. This implies that the measured effect of integration on outcomes such as White enrollment losses may be tainted by the presence of segregation academies. We also show that the establishment of segregation academies — a backlash by White parents to desegregating public schools — caused White flight from public schools *beyond* that induced by integration, ultimately weakening efforts to effectively integrate schools.

On average, segregation academies negatively affected White students but did not effect the long term labor market outcomes of Black students. The negative impacts on White students suggest that segregation academies were, on average, poor quality. Based off of a decomposition exercise, we hypothesize the student most negatively affected by the opening of a segregation academy is a poor White student who attends a low tuition, low quality segregation academy instead of their local public school. Federal action, in the form of both court orders and funding in public schools, likely mitigated any negative academic effects on Black students. However, Black students are more likely to live in high Black share tract in adulthood, implying lasting effects on segregation into adulthood.

younger individuals (later post-periods)). We also recenter estimates to ages 19-24. This approach replaces the linearity assumption with the assumption that community mechanisms have a constant effect on the outcomes of the working force population within each survey year.

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A — Additional Tables and Figures

A.1. Tables

Table A.1 — Data Availability by State and Source

	AL	MS	SC	NC	LA	GA	FL	Years	Source
Enrollment	✓	✓	✓	✓	✓	✓	✓	1960–1985	State DOEd
Enrollment by race	✓				✓			1960–1985	State DOEd
Funding	✓	✓	✓	✓	✓			1960–1985	State DOEd
Teachers	✓							1960–1976	State DOEd
Teacher salary			✓					1960–1981	State DOEd
Teachers by race	✓	✓	✓		✓	✓		1960–1972*	Thompson, 2022
Integration	✓	✓	✓	✓	✓	✓	✓	1966–1980**	Thompson, 2022
Enrollment by school type			✓					1960–1985	State DOEd

***Notes*: *No data 1965, 1966 and for missing OCR years: **OCR did not collect data for 1969, 1971, 1975, 1977, and 1979.**

Table A.2 — Segregation Academies vs. Other Private Schools Controling for 1950 County Characteristics

	College Matriculation Rate	Percent White	Percent Black	Percent Hispanic	Class Size 2017-18	Class Size 1976-80
	(1)	(2)	(3)	(4)	(5)	(6)
Segregation Academy	-11.77***	4.27*	-0.20	-3.45**	-0.81	-0.37
	(4.26)	(2.49)	(1.96)	(1.70)	(0.50)	(0.35)
Observations	267	390	390	390	390	1,570
R^2	0.07	0.24	0.08	0.12	0.07	0.02
Adjusted R^2	0.06	0.23	0.07	0.12	0.06	0.02

***Notes:* Differences in characteristics of segregation academies versus other schools appearing in the 2017-2018 Private School Survey. Schools are limited to those appearing in the 1976-1980 Universe of Private Schools and the 2017-2018 Private School Survey. Estimates are βs from school-level linear regressions of the form $y_s = \beta D_s + X_s$, where $D_s = 1$ if a school is a segregation academy, y_s is the indicated outcome, and $X_s(c)$ includes the county characteristics 1950 Black percentage of the population, population change from 1940-1950, and 1950 median family income as controls. Outcome data comes from the 2017-2018 Private School Survey and is reported at the school level.**

Table A.3 — Effect on Public School Enrollment and White Public School Enrollment Including Dynamic Deseg Post Variable

Panel A: Public Enrollment (Alabama, Florida, Georgia, Louisiana, Mississippi, North Carolina, South Carolina)

	Log Enrollment					
	(1)	(2)	(3)	(4)	(5)	(6)
ATT	-0.08***	-0.03	-0.10***	-0.08***	-0.05**	-0.05
	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.04)
Year × Quartile Black Pop Pct		-0.01***			0.00***	
		(0.00)			(0.00)	
Year × Population Change 1940-50			0.00***		0.00***	
			(0.00)		(0.00)	
Year × Number of Lynchings				0.00*	0.00	
				(0.00)	(0.00)	
Observations	18,929	18,929	18,929	18,929	18,929	14,636
R^2	0.97	0.97	0.97	0.97	0.97	0.97
Within R^2	0.10	0.16	0.16	0.10	0.20	0.10
School District fixed effects	✓	✓	✓	✓	✓	✓
Year fixed effects	✓	✓	✓	✓	✓	✓
Year-State fixed effects	✓	✓	✓	✓	✓	✓
Year from Court Order fixed effects	✓	✓	✓	✓	✓	✓

Panel B: White Public Enrollment (Alabama, Louisiana)

	Log White Enrollment					
	(1)	(2)	(3)	(4)	(5)	(6)
ATT	-0.37***	-0.16**	-0.40***	-0.34***	-0.20**	-0.34**
	(0.09)	(0.08)	(0.10)	(0.10)	(0.08)	(0.14)
Year × Quartile Black Pop Pct		-0.01***			-0.01***	
		(0.00)			(0.00)	
Year × Population Change 1940-50			0.00***		0.00***	
			(0.00)		(0.00)	
Year × Number of Lynchings				0.00	0.00	
				(0.00)	(0.00)	
Observations	6,638	6,638	6,638	6,638	6,638	4,994
R^2	0.91	0.92	0.92	0.91	0.92	0.91
Within R^2	0.17	0.24	0.20	0.18	0.26	0.16
School District fixed effects	✓	✓	✓	✓	✓	✓
Year fixed effects	✓	✓	✓	✓	✓	✓
Year-State fixed effects	✓	✓	✓	✓	✓	✓
Year from Court Order fixed effects	✓	✓	✓	✓	✓	✓

Notes: **Enrollment results. Specification is the same as in Table 3, but we replace** $desegpost$ **with dynamic indicators for relative year from desegregation. Results from a matched data set are in (6). We add covariates interacted with year in columns (2) through (4) and include the full set of covariates interacted with year in column (5). Observation numbers change from (1) to (6) because of availability of covariates. Enrollment results are estimated for Alabama, Mississippi, Louisiana, Georgia, Florida, North Carolina, and South Carolina. White enrollment results are estimated for Alabama and Louisiana. Data ranges from 1960-1985.** $***$**,** $**$**, and** $*$ **indicate significance at the .01, .05, and .10 levels, respectively. Standard errors are clustered at the school district level and appear in parentheses.**

Table A.4 — Effect on Revenue Sans Federal Revenue

	Log Total Less Fed	Log Total	Total Less Fed PP	Total PP
	(1)	(2)	(3)	(4)
ATT	-0.08***	-0.06***	-1.18	6.86
	(0.02)	(0.02)	(7.93)	(7.81)
Desegregation Post	0.00	0.02	-9.22	-6.74
	(0.02)	(0.02)	(10.07)	(9.86)
Observations	13,406	13,406	13,406	13,406
R^2	0.99	0.99	0.98	0.98
Within R^2	0.06	0.06	0.06	0.05
School District fixed effects	✓	✓	✓	✓
Year fixed effects	✓	✓	✓	✓
Year-State fixed effects	✓	✓	✓	✓

Notes: **Estimates of the effect on revenue, less federal revenue. Estimates come from Equation 4. Total Less Fed is calculated by subtracting federal revenue from total revenue; it is equal to state revenue added to local revenue. Georgia and Florida are excluded because of limited finance data availability. Columns (2) and (4) correspond to columns (4) and (8) of Table 7. Sample is limited to observations for which we observe local, state, and federal revenue. *Source*: Alabama, Louisiana, Mississippi, North Carolina, and South Carolina Departments of Education.**

Table A.5 — Main Samples, Ns

	Treat	Control	Total
ACS Sample			
Black	426,000	76,500	503,000
White	1,170,000	344,000	1,514,000
Total	1,596,000	420,000	2,016,000
IRS Sample			
Black	53,580,000	7,241,000	60,821,000
White	114,100,000	25,970,000	140,070,000
Total	167,680,000	33,211,000	200,891,000

Notes: **Table shows the rounded numbers of observations for each of our main analysis samples. Ns are rounded according to the Census Bureau's disclosure avoidance guidance. The ACS sample includes the 2000 Long Form Decennial Census. It is reported at the individual level. The IRS sample is at the person by tax-filing year level. *Source:* 2000 Long Form Decennial Census, ACS 2001-2019, IRS 1040s 1994, 1995, 1999-2019, and Census Numident.**

Table A.6 — ACS Sample: Human Capital and Economic Self Sufficiency Outcomes

	Black		White	
Outcome Variable	Treated	Control	Treated	Control
Human Capital Measures				
Yrs. Schooling: Mean	13.3000	13.1900	13.8600	13.5300
Yrs. Schooling: Std. Dev.	2.2230	2.1800	2.4360	2.4330
Sh. HS or GED	0.9029	0.8957	0.9310	0.9104
Sh. Any College	0.5908	0.5621	0.6755	0.6189
Sh. College Plus	0.2280	0.2080	0.3493	0.2896
Sh. Professional or Doctoral	0.0147	0.0124	0.0334	0.0278
Sh. Professional Job	0.0869	0.0847	0.1342	0.1261
Economic Self Sufficiency Measures				
Sh. In Labor Force	0.9528	0.9507	0.9566	0.9560
Sh. Worked Last Year	0.9881	0.9878	0.9923	0.9908
Weeks Worked Last Year: Mean	44.6500	44.7000	45.4500	45.3300
Weeks Worked Last Year: Std. Dev.	12.7100	12.6500	11.7200	11.9100
Usual Hrs Per Week: Mean	40.3500	40.1200	41.6300	41.6400
Usual Hrs Per Week: Std. Dev.	11.0000	10.7900	11.8600	11.9300
Labor Income (Log): Mean	9.8020	9.8290	9.8310	9.7100
Labor Income (Log): Std. Dev.	2.2310	2.1740	2.7790	2.8660
Other Income (Log): Mean	1.6920	1.7000	2.6000	2.5410
Other Income (Log): Std. Dev.	3.0500	3.0490	3.1640	3.1630
Income to Poverty Ratio: Mean	5.5210	5.5280	5.9290	5.8640
Income to Poverty Ratio: Std. Dev.	0.8534	0.8343	0.7061	0.7209
Sh. Not in Poverty	0.8817	0.8887	0.9595	0.9522
Neg. Government Income: Mean	-0.2456	-0.2513	-0.1129	-0.1186
Neg. Government Income: Std. Dev.	1.412	1.422	0.9691	0.9936
Alternative Labor Income: Mean	35,960.00	35,790.00	49,780.00	46,120.00
Alternative Labor Income: Std. Dev.	31,380.00	30,490.00	53,700.00	49,460.00

Notes: **Table shows demographic characteristics of our two main samples. Data is reported at the individual level.** ***Source:*** **2000 Long Form Decennial Census, ACS 2001-2019, and Census Numident.**

Table A.7 — ACS and 2000 LF Sample: Migration, Wealth, and Occupation Outcomes

	Black		White	
Outcome Variable	Treated	Control	Treated	Control
Migration Measures				
Share Moved States	0.3400	0.3402	0.3725	0.3400
Share Moved Counties	0.6126	0.6291	0.7448	0.6750
Share Moved Out-South	0.1496	0.1478	0.1342	0.1283
Wealth Measures				
Home Value: Mean	157300.00	155600.00	228800.00	208600.00
Home Value: Std. Dev.	173500.00	159000.00	248800.00	233500.00
Sh. Own Home	0.5634	0.5711	0.7619	0.7706
Occupation Measures				
Years Required: Mean	10.0100	9.9470	11.9300	11.5700
Years Required: Std. Dev.	6.0550	6.0290	5.0710	5.2410
Share Requires College Plus	0.2155	0.2065	0.3066	0.2787
Share Underemployed	0.3036	0.2928	0.2554	0.2478
Share Works as Teacher	0.0581	0.0585	0.0716	0.0706
Content Skills: Mean	-0.3150	-0.3387	0.0505	-0.0337
Content Skills: Std. Dev.	0.9468	0.9418	0.8566	0.8767
Process Skills: Mean	-0.2357	-0.2587	0.1836	0.0980
Process Skills: Std. Dev.	1.1250	1.1230	0.9860	1.0150
Social Skills: Mean	-0.0835	-0.1231	0.3180	0.2183
Social Skills: Std. Dev.	1.1070	1.1150	1.0060	1.0410

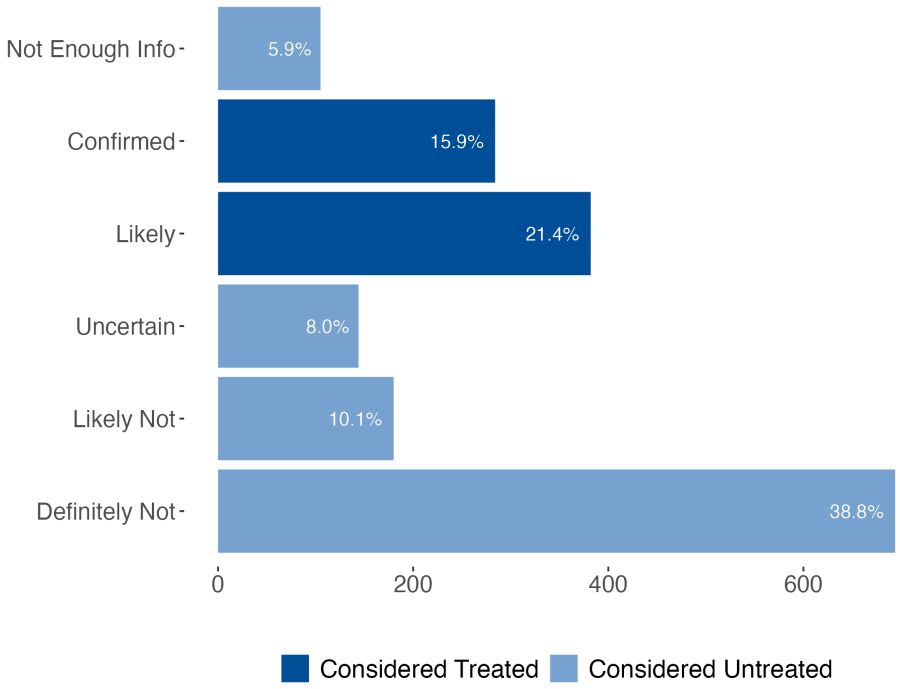
Notes: **Table shows the summary statistics of our outcomes of interest from the ACS and 2000 LF main sample. Data is reported at the individual level.** ***Source:*** **2000 Long Form Decennial Census, ACS 2001-2019, and Census Numident.**

Table A.8 — IRS Sample: Income, Migration, and Neighborhood Characteristics

	Black		White	
Outcome Variable	Treated	Control	Treated	Control
Income Measures				
WSI: Mean	42,630.00	43,720.00	77,410.00	68,140.00
WSI: Std. Dev.	58,870.00	56,520.00	153,600.00	106,100.00
WSI: Median	31,130.00	32,210.00	59,590.00	54,570.00
Max WSI: Mean	46,520.00	47,680.00	96,990.00	82,070.00
Max WSI: Std. Dev.	78,900.00	72,590.00	379,900.00	246,200.00
Max WSI: Median	32,800.00	34,03.000	67,750.00	61,330.00
Migration				
Share Moved States	0.3293	0.3681	0.3683	0.3259
Share Moved Counties	0.5853	0.6625	0.7366	0.6552
Share Moved Out-South	0.1476	0.1667	0.1275	0.1060
Distance Moved	11.11	11.29	11.54	11.20
Neighborhood Characteristics				
Share College+: Mean	0.1838	0.1743	0.2467	0.1943
Share College+: Std. Dev.	0.1281	0.1229	0.1668	0.1436
Share White: Mean	0.4329	0.5277	0.7827	0.8133
Share White: Std. Dev.	0.2935	0.2763	0.1766	0.1598
Share Black: Mean	0.4746	0.3810	0.1349	0.1182
Share Black: Std. Dev.	0.3113	0.2778	0.1546	0.1379
Annual Job Growth: Mean	0.0086	0.0079	0.0174	0.0146
Job Growth: Std. Dev.	0.0753	0.0716	0.0667	0.0667
Share Employed: Mean	0.5733	0.5730	0.6209	0.5993
Share Employed: Std. Dev.	0.1080	0.1003	0.0887	0.0844
Job Density: Mean	984.9	820.9	867.2	526.5
Job Density: Std. Dev.	5848	4456	9420	6356
Median HH Income: Mean	66260	65990	82760	73730
Median HH Income: Std. Dev.	24230	22750	31050	26140
Poverty Rate; Mean	0.1847	0.1692	0.1040	0.1187
Poverty Rate: Std. Dev.	0.1241	0.1060	0.0732	0.0687

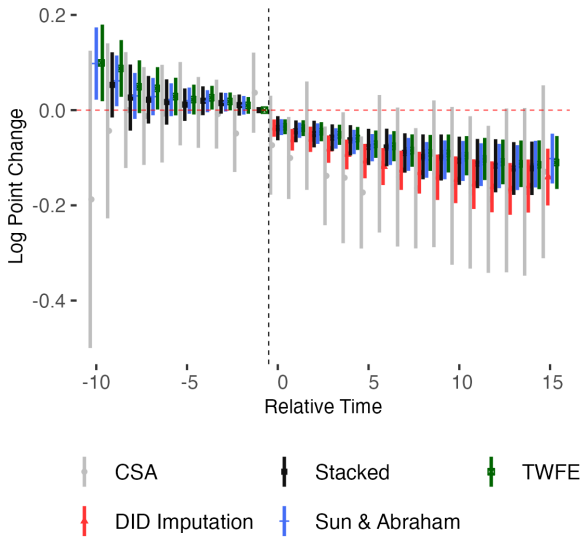
Notes: **Table shows the summary statistics of our outcomes of interest from the IRS main sample. Data is reported at the person by filing year level.** ***Source:*** **IRS 1040s 1994, 1995, 1999-2019, and Census Numident.**

Figure A.1 — Classification of Private Schools



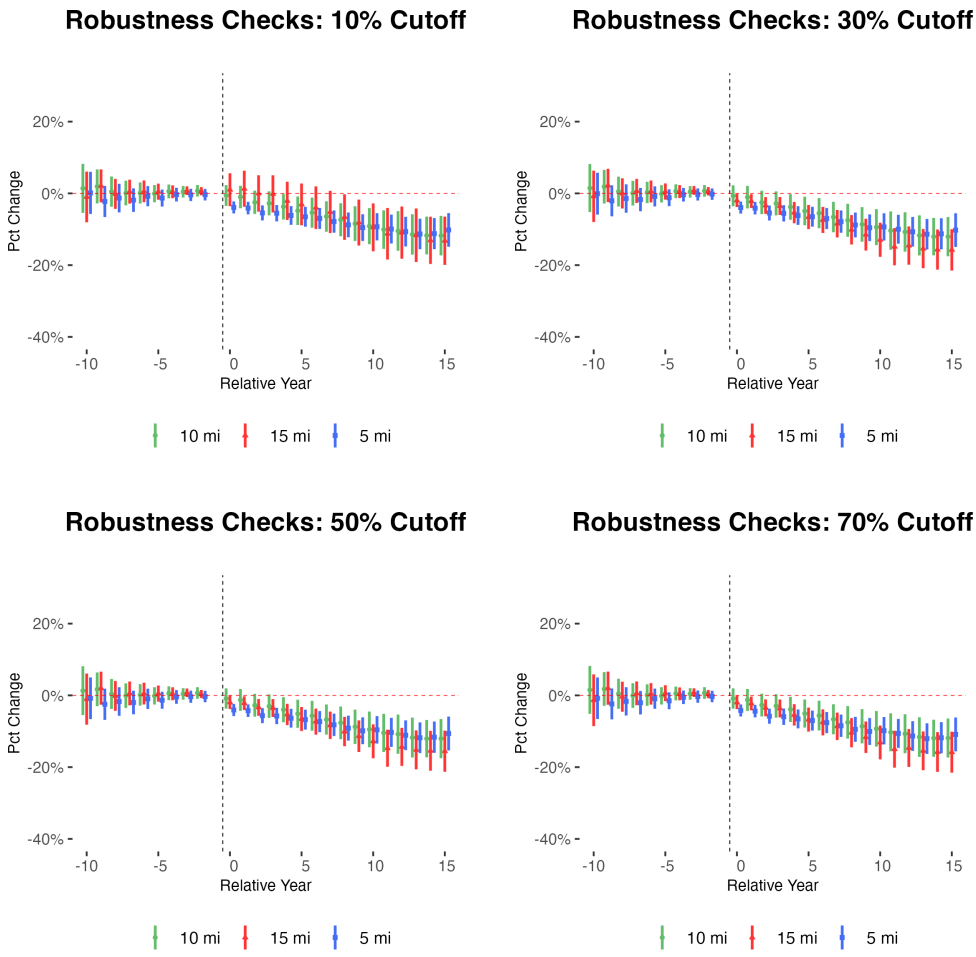
Notes: **Data come from the 1976-1980 Universe of Private School Survey. Schools are classified using a combination of primary and secondary sources. For a full description of classification criteria, see Appendix Section C.**

Figure A.2 — Alternate Event Study Specifications: Public Enrollment



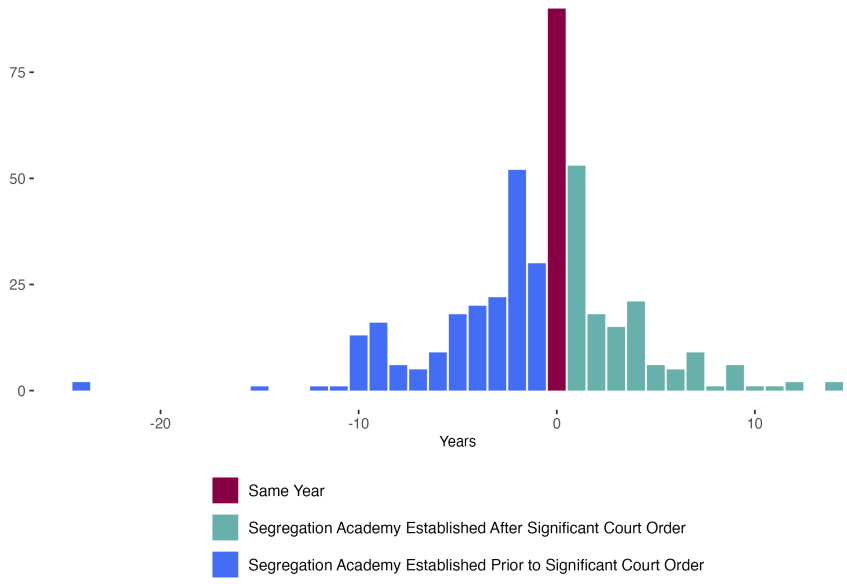
Notes: Results using alternate two-way fixed effect (TWFE) specifications. CSA refers to Callaway and Sant'Anna 2020. DID Imputation refers to Borusyak, Jaravel, and Spiess 2024. Stacked refers to a stacked differences-in-differences and Sun and Abraham refers to Sun and Abraham 2021, which corresponds to our main estimates. Points represent the estimated treatment effect for each relative period for each indicated method; bars represent the 95% confidence intervals of these estimates.

Figure A.3 — Alternate Definitions of Treatment: Public Enrollment



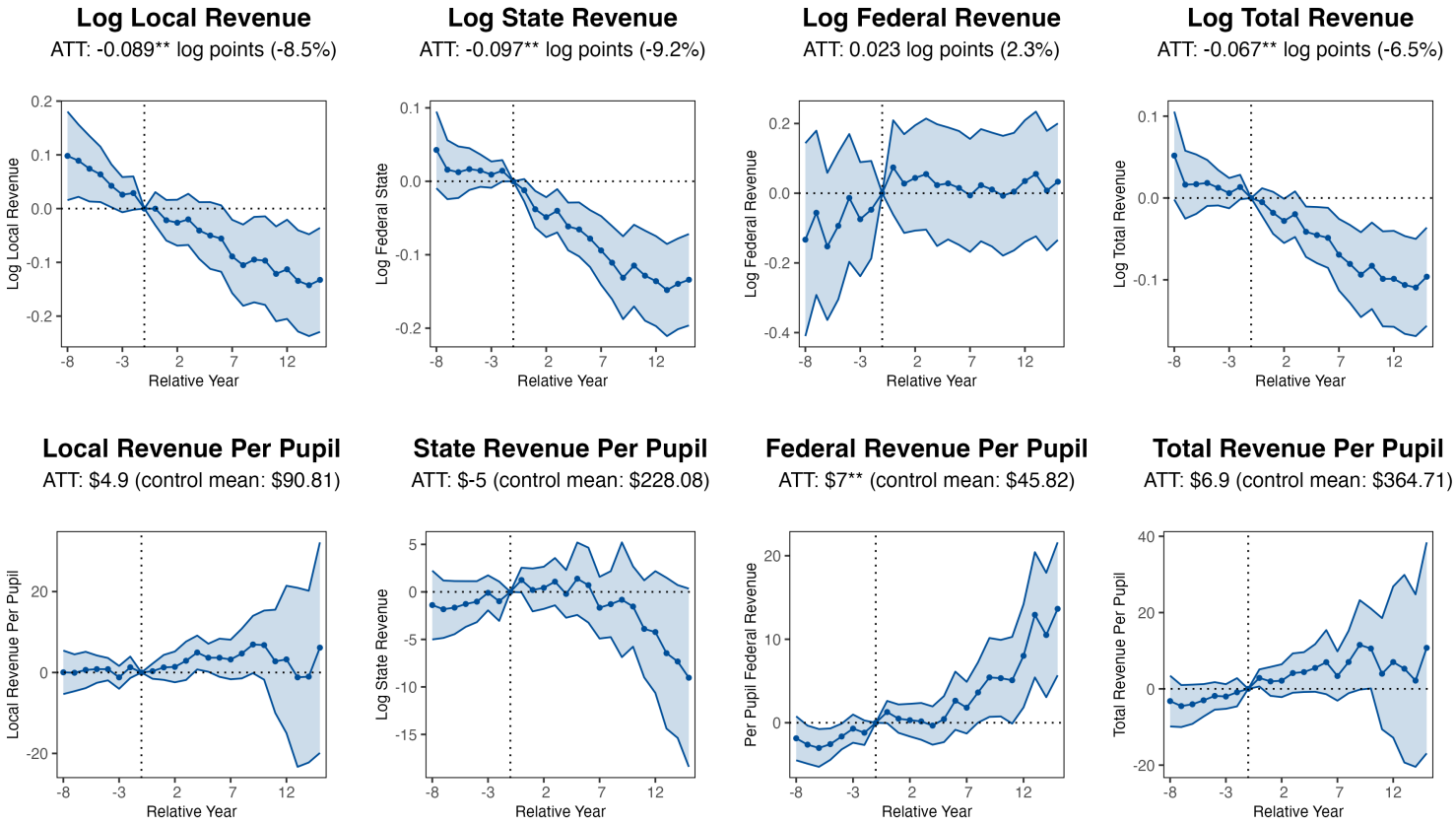
Notes: **Robustness checks. 10%, 30%, and 50% yield similar results. The more restrictive criteria of shrinking the radius of the catchment area to 5 miles or requiring 70% of the school district's population to be within the catchment area reduces the number of treated counties such that results become noisy.**

Figure A.4 — Treatment vs. Court Order Timing



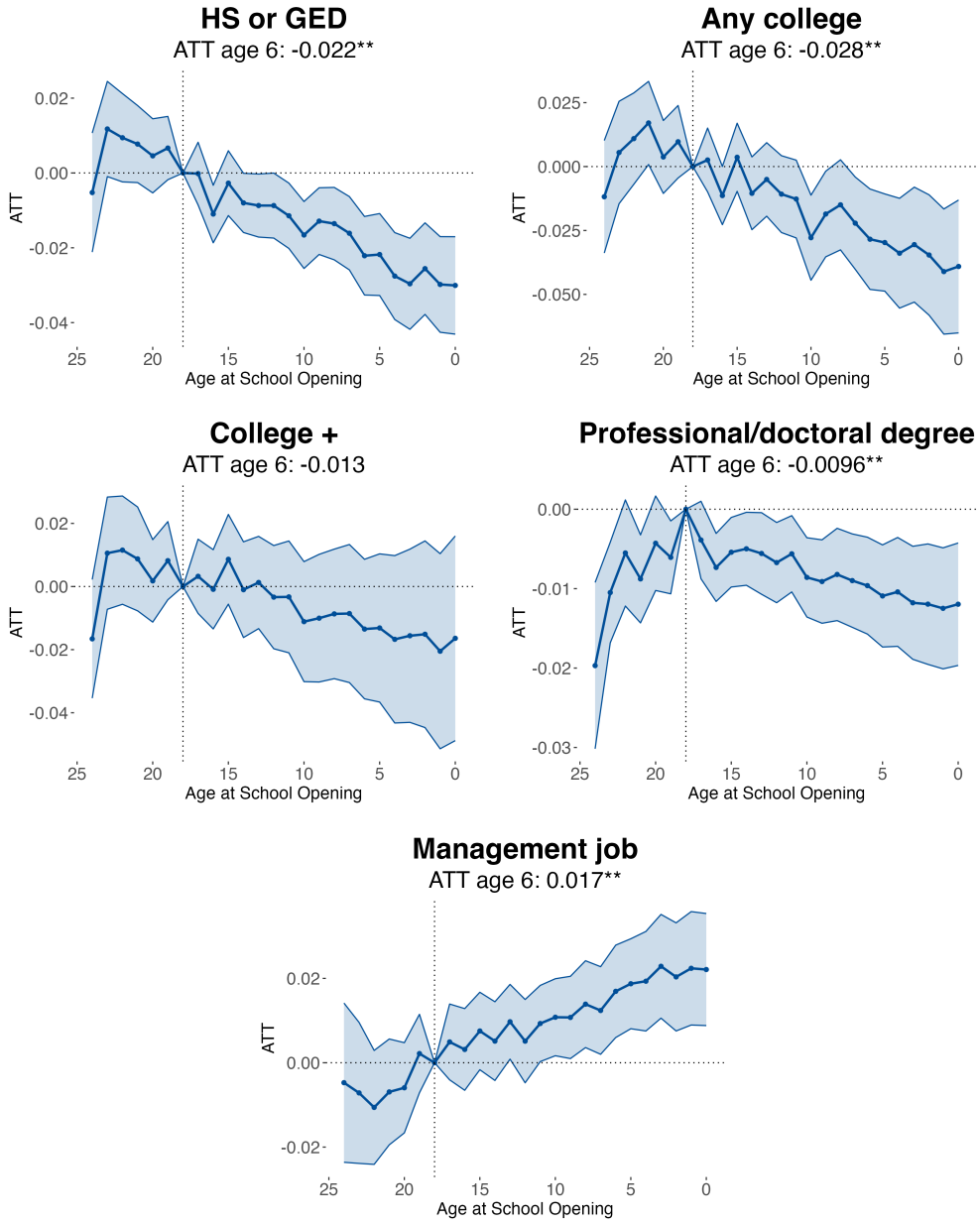
Notes: **Treatment year corresponds to the first year a school district is treated by a segregation academy. Graphs only include treated school districts that experienced a major court order.**

Figure A.5 — Funding Outcomes
Alabama, Louisiana, Mississippi, North Carolina, South Carolina



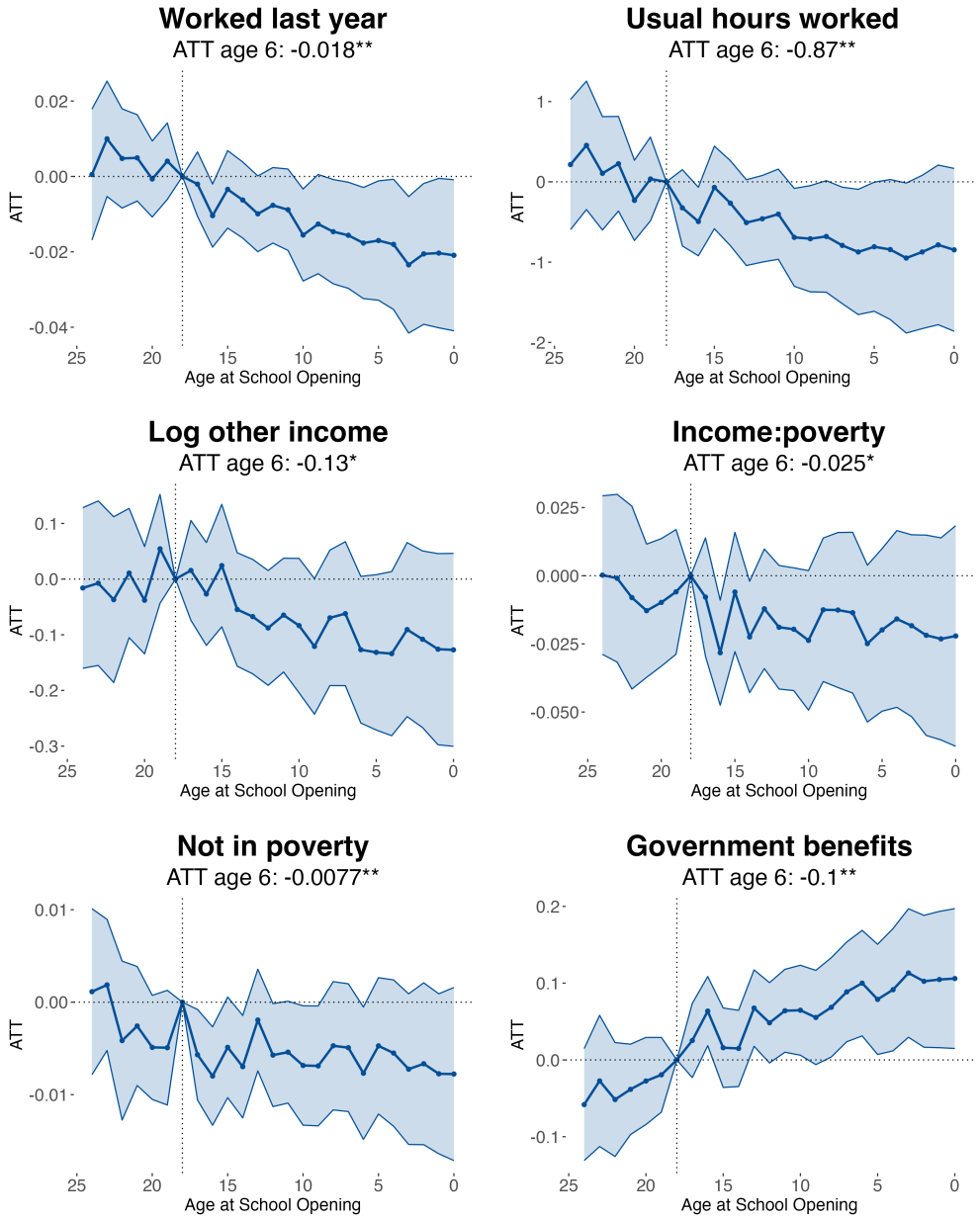
Notes: **The effect of segregation academies on funding outcomes. Georgia is omitted because it only has funding data available for 11 of the 25 years of interest, and no data is available after 1976. Florida is omitted because there is no data available after 1973. Total revenue is calculated as local revenue + state revenue + federal revenue. Sample is limited to observations where all funding outcomes are nonzero. 95 percent confidence intervals are represented by dashed lines. ∗ ∗ ∗, ∗∗, and ∗ indicate significance at the .01, .05, and .10 levels, respectively. PP indicates per pupil.**

Figure A.6 — Human Capital Index Components: White Students



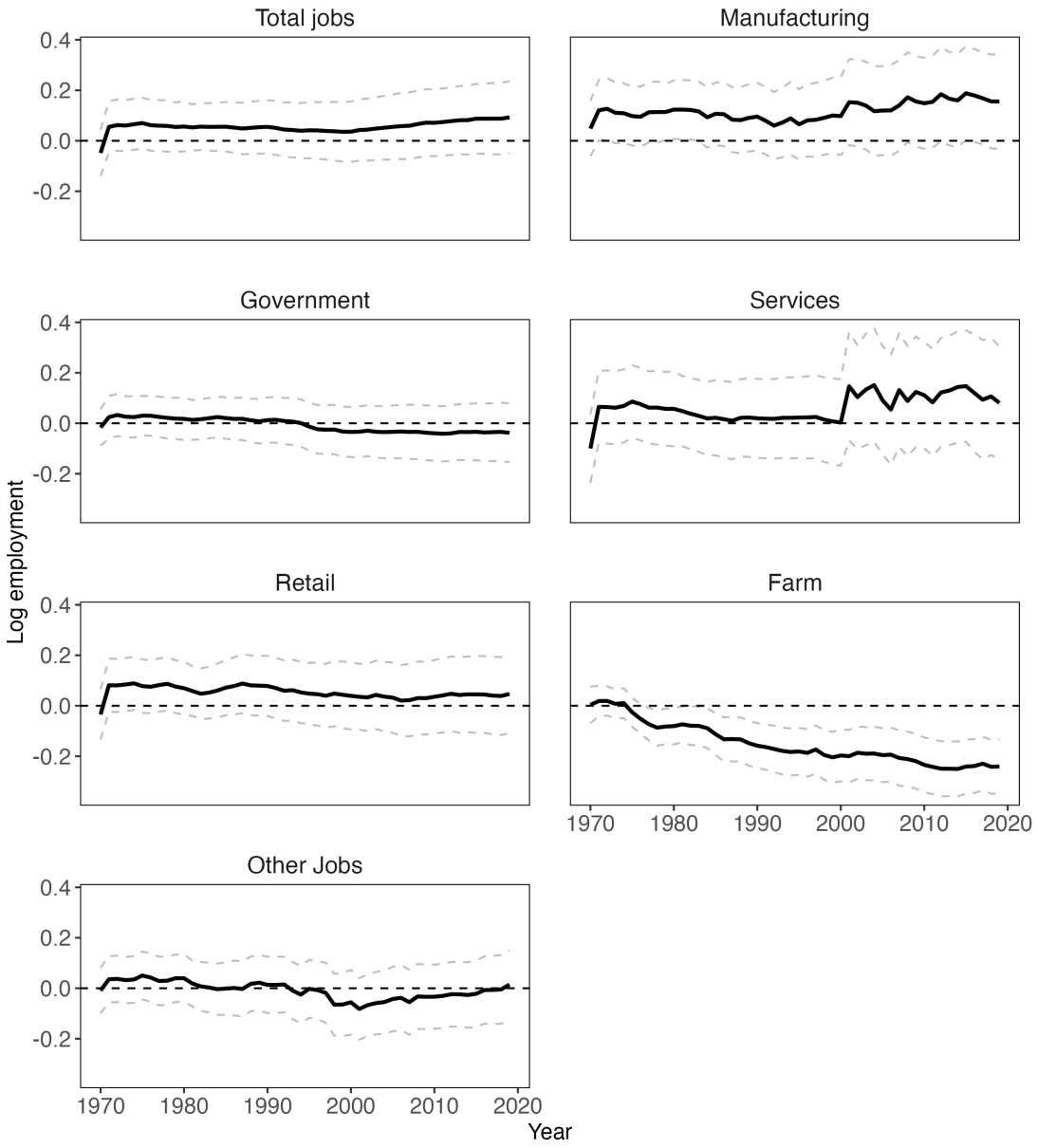
Notes: **Results are from estimating Equation 8. The average treatment effect is for individuals aged 6 when a segregation academy opens nearby. ∗, ∗∗, ∗ ∗ ∗ represent significance at the 10, 5, and 1 percent levels, respectively. Shaded areas represent 95% confidence intervals. Years of schooling is a continuous variable. HS or GED, Any College, and College + are binary indicators. Working in a management job is defined as reporting an occupation code 1 through 359 (management, business, science and arts occupations).** *Source:* **2000 Long Form Decennial Census, ACS 2001-2019, and Census Numident.**

Figure A.7 — Additional Labor Market Results: White Students



Notes: **Results are from estimating Equation 8. The average treatment effect is for individuals aged 6 when a segregation academy opens nearby. ∗, ∗∗, ∗ ∗ ∗ represent significance at the 10, 5, and 1 percent levels, respectively. Shaded areas represent 95% confidence intervals. In labor force is a binary variable. All other variables are continuous. ACS income are imputed as 0 when it is missing. In labor force, negative government income, log other income, and log labor income come from a pooled sample drawing from the 2000 long form decennial census and 2001-2019 ACS. We also include continuous measures of weeks worked, usual hours worked, the log of labor income, log of other income from nongovernmental sources, and log ratio of family income to the federal poverty threshold.** ***Source:*** **2000 Long Form Decennial Census, ACS 2001-2019, and Census Numident.**

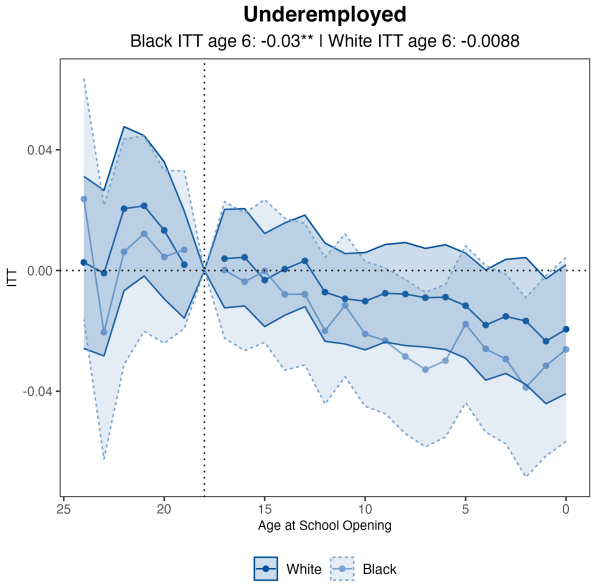
Figure A.8 — Trends in Log Employment by Sector



Notes: **Data is from Hershbein and Stuart, Data and Code For. Manufacturing, government, services, retail, and farm are shown because they were the five largest sectors by employment in 1969 in the Southeast. Points represent β from the equation $log(jobs) = \underset{\text{county}}{\alpha_c} + \underset{\text{year}}{\lambda_t} + \underset{\text{year by state}}{\theta_{s(c)t}} + \beta treatPost * t$ where treatPost = 1 if $t >$ the year of treatment for county c and t is an indicator variable for year where 1969 is the omitted category.**

B — Legislative History

Figure A.9 — Underemployment



Notes: **Results are from estimating Equation 8. The average treatment effect is for individuals aged 6 when a segregation academy opens nearby. *, **, * * * represent significance at the 10, 5, and 1 percent levels, respectively. Occupation characteristics are taken from O*NET and merged onto `occ2010`. *Source:* 2000 Long Form Decennial Census, ACS 2001-2019, and Census Numident.**

Table B.1 — Landmark School Desegregation Court Cases

Year	Case	Decision/effect
1819	*Trustees of Dartmouth College v. Woodward*	Held that private schools are corporations and that governments cannot interfere with their contracts.
1925	*Pierce v. Society of Sisters*	Held that children have the right to receive instruction from people other than public school teachers.
1936	*Murray v. Pearson*	Ordered the University of Maryland to integrate, effectively making segregation in Maryland illegal.
1938	*Missouri ex rel. Gaines v. Canada*	Held that if a state provided public education to White students, it also had to provide the same quality of education to Black students; specifically addressed law schools.

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Table B.1 – continued from previous page

Year	Case	Decision/effect
1948	*Sipuel v. Board of Regents*	Ordered the University of Oklahoma to admit Ada Lois Sipuel, a Black woman, because the state did not provide a comparable facility for Black students.
1948	*Shelley v. Kramer*	Held that courts could not enforce racially restrictive covenants in housing contracts.
1950	*Sweatt v. Painter; McLaurin v. Oklahoma State Regents*	Held that segregation was unconstitutional in professional and graduate schools.
1954	***Brown v. Board of Education of Topeka***	**Held that school segregation was unconstitutional, overturning the "separate but equal" doctrine of *Plessy v. Ferguson* (1896).**
1955	***Brown v. Board of Education (II)***	**Mandated that schools desegregate with "all deliberate speed."**
1964	Civil Rights Act	Enacted legislation conditioning federal funding on school districts' compliance with desegregation.
1965	Elementary and Secondary Education Act	Established Title I funds for high-poverty school districts.
1968	*Green v. New Kent County*	Struck down "free choice" plans that shifted the burden of desegregation onto parents and students; marked the start of "real desegregation."
1969	*Alexander v. Holmes County Board of Education*	Ordered the immediate desegregation of schools, ending the "all deliberate speed" standard from *Brown II*.
1970	Nixon administration	Ended the policy of withholding Title I funds from schools that failed to meet desegregation targets.
1971	*Swann v. Charlotte-Mecklenburg Board of Education*	Held that busing and other measures were permissible tools for enforcing school desegregation.
1971	*Green v. Connelly*	Held that racially discriminatory private schools were not eligible for federal tax exemptions or deductions.
1974	*Milliken v. Bradley*	Limited desegregation remedies by ruling that multi-district desegregation plans were unconstitutional unless intentional segregation across districts could be proven.

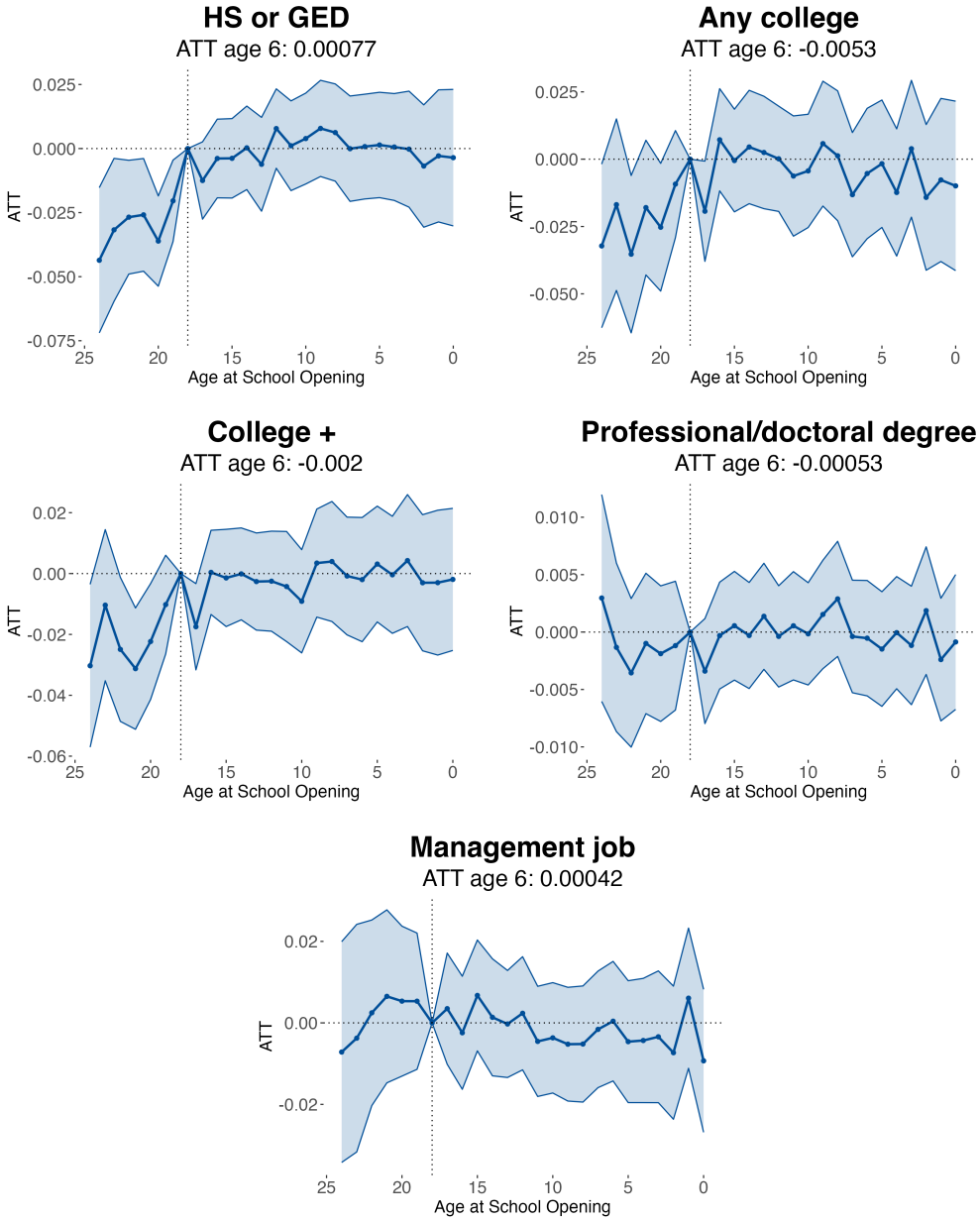
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Table B.1 – continued from previous page

Year	Case	Decision/effect
1976	*Runyon v. McCrary*	Held that private schools could not deny admission on the basis of race.
1982	Reagan administration	Relaxed enforcement of *Green v. Connelly* by allowing tax exemptions for private schools that claimed "color-blind" admissions.
1983	*Bob Jones University v. United States*	Affirmed that racially discriminatory private schools could not receive tax-exempt status.

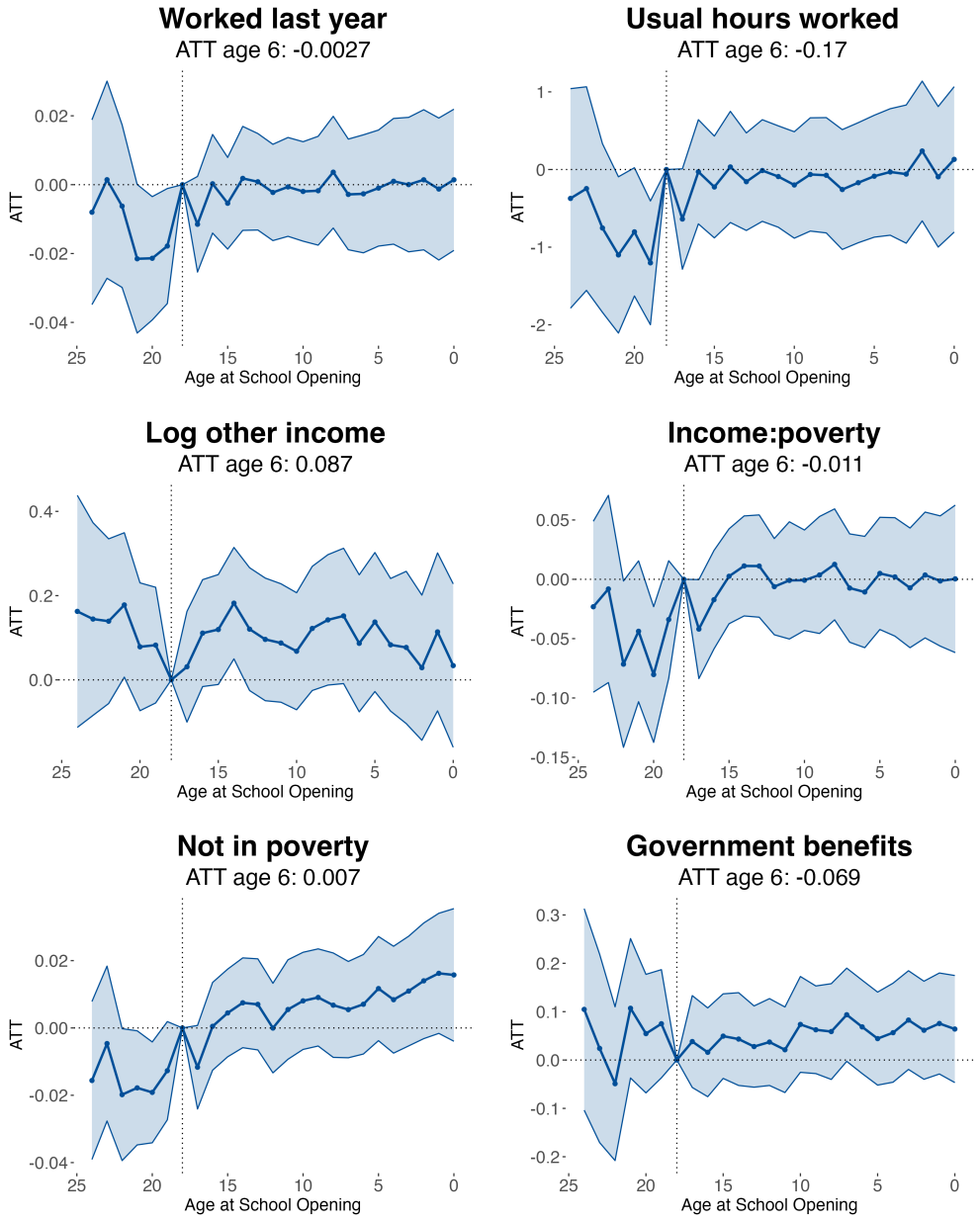
Notes: **Court case descriptions adapted from "Oyez," n.d. List expanded on from "Timeline of Events Leading to the Brown v. Board of Education Decision of 1954" 2016.**

Figure A.10 — Human Capital Index Components: Black Students



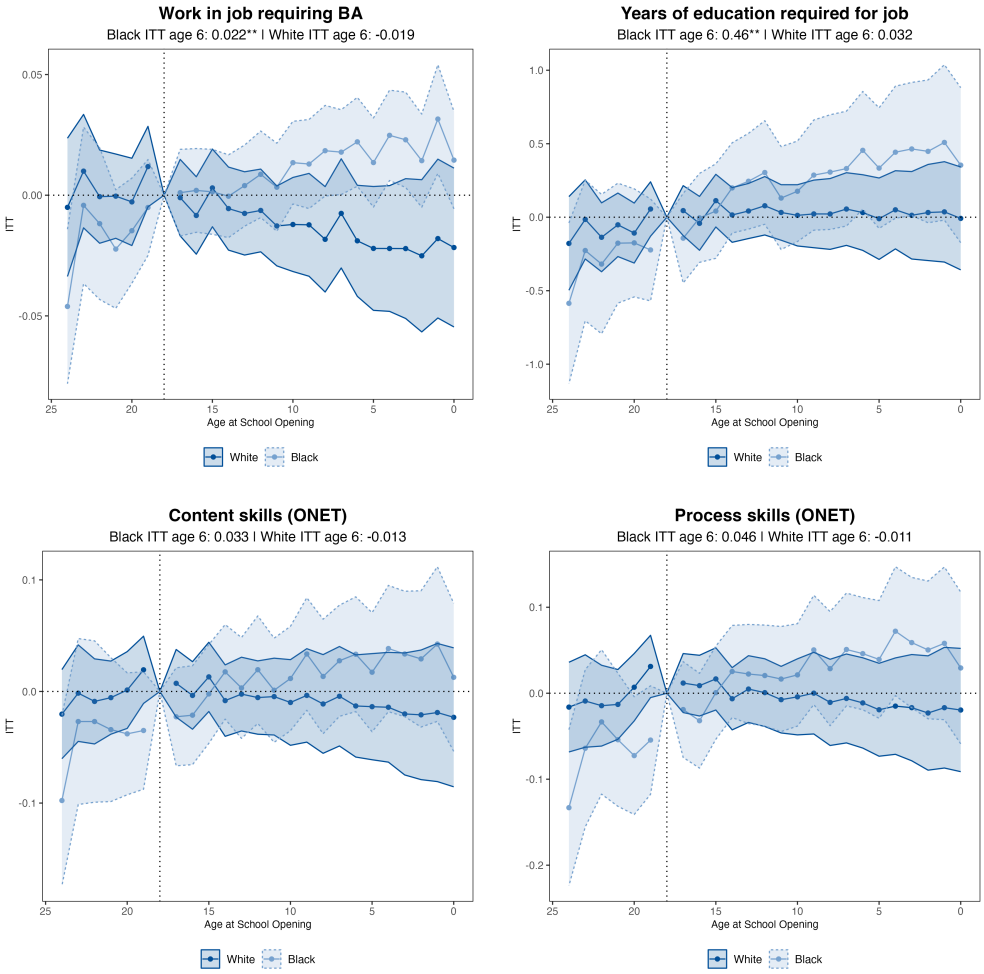
Notes: **Results are from estimating Equation 8. The average treatment effect is for individuals aged 6 when a segregation academy opens nearby. ∗, ∗∗, ∗ ∗ ∗ represent significance at the 10, 5, and 1 percent levels, respectively. Shaded areas represent 95% confidence intervals. Years of schooling is a continuous variable. HS or GED, Any College, and College + are binary indicators. Working in a management job is defined as reporting an occupation code 1 through 359 (management, business, science and arts occupations).** *Source:* **2000 Long Form Decennial Census, ACS 2001-2019, and Census Numident.**

Figure A.11 — Additional Labor Market Results: Black Students



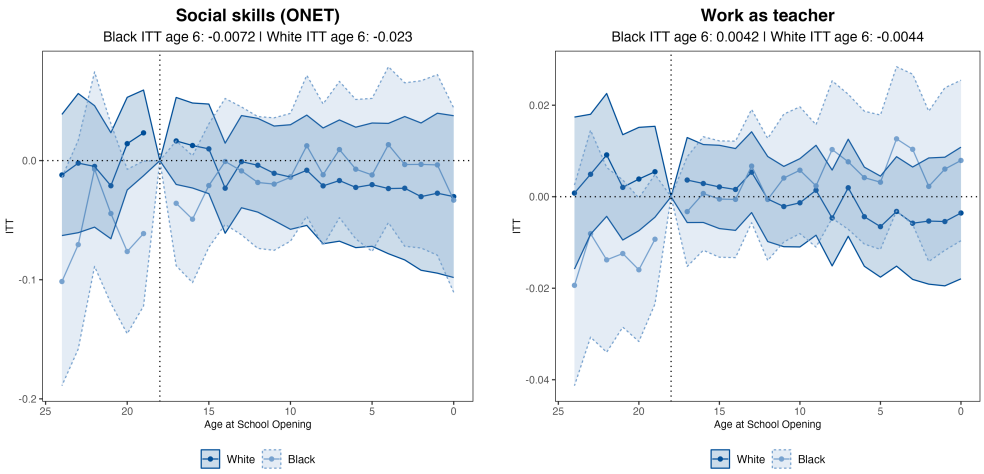
Notes: **Results are from estimating Equation 8. The average treatment effect is for individuals aged 6 when a segregation academy opens nearby. *, **, *** represent significance at the 10, 5, and 1 percent levels, respectively. Shaded areas represent 95% confidence intervals. In labor force is a binary variable. All other variables are continuous. ACS income are imputed as 0 when it is missing. In labor force, negative government income, log other income, and log labor income come from a pooled sample drawing from the 2000 long form decennial census and 2001-2019 ACS. We also include continuous measures of weeks worked, usual hours worked, the log of labor income, log of other income from nongovernmental sources, and log ratio of family income to the federal poverty threshold.** *Source:* **2000 Long Form Decennial Census, ACS 2001-2019, and Census Numident.**

Figure A.12 — Occupation Characteristics



Notes: **Results are from estimating Equation 8. The average treatment effect is for individuals aged 6 when a segregation academy opens nearby. *, **, *** represent significance at the 10, 5, and 1 percent levels, respectively. Occupation characteristics are taken from O*NET and merged onto `occ2010`. *Source:* 2000 Long Form Decennial Census, ACS 2001-2019, and Census Numident.**

Figure A.13 — Social Skills and Teaching Profession



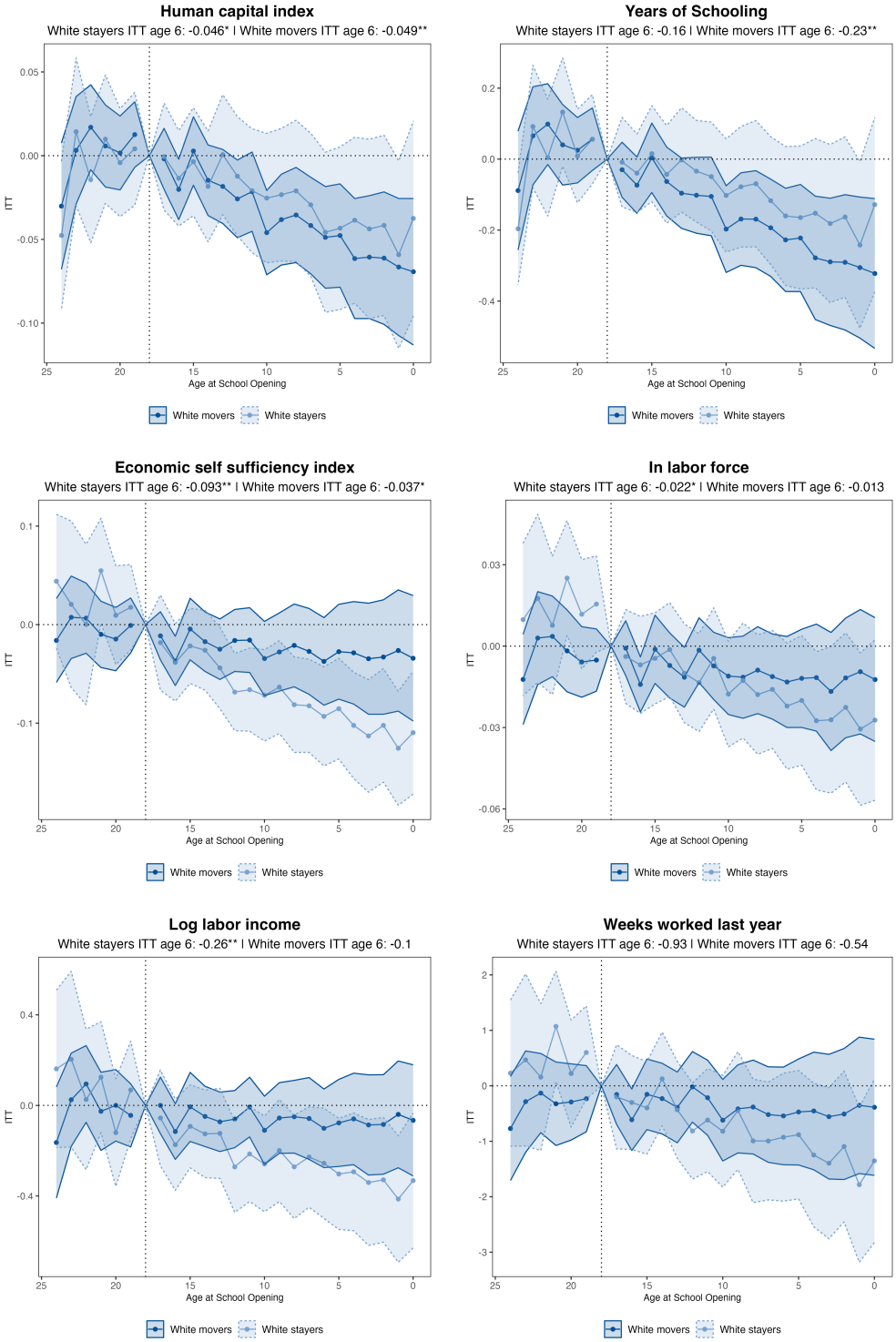
Notes: **Results are from estimating Equation 8. The average treatment effect is for individuals aged 6 when a segregation academy opens nearby. ∗, ∗∗, ∗ ∗ ∗ represent significance at the 10, 5, and 1 percent levels, respectively. Occupation characteristics are taken from O*NET and merged onto `occ2010`. Working as a teacher is defined as working in an instructional role in the K-12 setting. This includes teachers in preschool, kindergarten, elementary school, secondary school, special education teachers, tutors, other educational instruction and library workers, teaching assistants, and other teachers. This includes `occ2010` codes 2300, 2310, 2320, 2330, 2350, 2360, 2555, 2545. Though our interest is in the K-12 workforce, we include preschool teachers because they classified jointly with kindergarten teachers. *Source:* 2000 Long Form Decennial Census, ACS 2001-2019, and Census Numident.**

Table B.2 — Example Integration Events

Year	Event	Source
1960	Armstrong & U.S. v. Bd. of Educ. of City of Birmingham filed	United States Commission on Civil Rights
1961	West Birmingham Christian (segregation academy) established	Author's research (see online appendix)
1963	School board ordered to present desegregation plan	American Communities Project
1970	transfers/REZONING	Welch & Light (1987), Table A3
1976	magnets/rezoning/pair	Welch & Light (1987), Table A3
1981	magnets/rezoning/pair	Welch & Light (1987), Table A3

Notes: **Integration events for the city of Birmingham. Rows 3-5 are copied directly from Table A3 of Welch and Light 1987. Welch and Light indicated "major" plans through the use of all caps. These were the plans that resulted in the largest change in the dissimilarity index.**

Figure A.14 — Long Run Outcomes: White Movers and Stayers



Results are from estimating Equation 8. The average treatment effect is for individuals aged 6 when a segregation academy opens nearby. ∗, ∗∗, ∗ ∗ ∗ represent significance at the 10, 5, and 1 percent levels, respectively. Shaded areas represent 95% confidence intervals. *Source:* 2000 Long Form Decennial Census, ACS 2001-2019, and Census Numident.

Figure A.15 — Long Run Outcomes: Black Movers and Stayers



Results are from estimating Equation 8. The average treatment effect is for individuals aged 6 when a segregation academy opens nearby. *, **, * represent significance at the 10, 5, and 1 percent levels, respectively. Shaded areas represent 95% confidence intervals. *Source:* 2000 Long Form Decennial Census, ACS 2001-2019, and Census Numident.**

C — Private School Classification Criteria

1. Confirmed

- Written in a list of segregation academies compiled by another source. The primary list is from Wikipedia (*Segregation Academy* 2022), which we independently audit by consulting each school's reference. We then validate each school through some combination of contemporary newspaper articles, the school history website, and scholarly reports on the topic (Nevin and Bills 1976, Phillip 2005, Williams 1977).
- *Founding* member of the Mississippi Private School Association (MPSA), sponsored by the White Citizens' Council, member of the Alabama Private School Association (APSA), or *founding member* South Carolina Independent School Association (SCISA). These associations explicitly catered to the segregation academy movement and openly admitted to being inspired by the Massive Resistance movement in Prince Edward County, VA.
- Confirmed as a segregation academy in an oral history, newspaper article, or one of the several theses that have been written on the topic (Sheffield 2022, Gladney 1974, Ernest Flora 2020, Phillip 2005).
- Has a mascot that is a Rebel and founded in the late sixties. List of schools with rebels as their mascot and additional context on the schools are provided by Smith 2019), who also explains that rebel mascots are often meant as homage to Confederate soldiers.

2. Likely

- Schools that have not been confirmed as segregation academies by outside sources, but are likely segregation academies. These schools often exhibit some combination of the following traits:

– Exclusively play other known or likely segregation academies in sports. We make extensive use of the Alabama High School Football Historical Society's archive[68], which documents football schedules for Alabama and Mississippi schools. SCISA Football History[69] includes schedules as well as team photos of football teams in South Carolina that allow me to determine if the football team is all White.
– All White student body in yearbook photos or historical photos on the school's website.
– Founded during the 1960s or 1970s.
– Member (but not a founding member) of MPSA or SCISA. All members of APSA are considered confirmed segregation academies.
– Opening ad for school contains suggestive language.
– Expanded rapidly after initial opening in a place not designed to house a school (typically a church).
– School history page describes a group of parents, concerned with the "current state of public education", coming together to found the school.

- The traits used in classifying each individual school are documented in the online appendix.

3. Uncertain

- Schools founded during the time period of 1954-1982 for which there is limited information. For example, consider Chester Christian School in South Carolina. Factors that point towards the school being a segregation academy include its name — "Christian schools" were part of the second wave of segregation academies (Nevin and Bills 1976; Bowler 1970) — and its founding year (1967). However, according to the

68. ahsfhs.org
69. sites.google.com/site/scisafootballhistory

local Rock Hill, South Carolina newspaper, *The Herald*, the school's tax exempt status was not threatened by the IRS rule that schools with racially discriminatory admissions policies would be denied tax exempt status. Further information about the school does not exist. Other examples can be found in the online appendix.

4. Likely Not

- Catholic schools. These schools largely predate Brown vs. Board of Education and court-ordered integration. The official stance of the Catholic Church was that schools should be integrated: "The Catholic Church in 1957 or '58 made a decision that they were going to desegregate the schools. They did it this way. The announcement was we have two programs. We have excommunication and we have integration. Make your choice by Friday.'' (*Library of Congress,* n.d.). Catholic schools in Memphis went so far as to resist the influx of White students into private schools during the 1960s and 1970s (Nevin and Bills 1976). However, we categorize these schools as "Likely Not" instead of "Definitely Not" because not all Southern Catholic churches abided by this doctrine (Newman 2020). To categorize schools as Catholic, a combination of the following traits was used: 1) the NCES religion code, 2) the school about page, 3) the name of the school (for example, all schools with "Immaculate Conception'' or "St. Mary's'' in the name are considered Catholic).
- Elementary schools. The purpose of segregation academies was to provide a place for White children to "escape" integration. Such schools either opened as K-12 schools or rapidly added grades to accommodate as many White students as possible. Schools that strictly catered to elementary grades do not fit this definition. We also note that many of these schools are Catholic or Jewish. The exception to this rule are schools that, due to a lack of resources, began as elementary schools, then rapidly added grades (such schools are classified as "Likely").

5. Definitely Not

- Schools with an explicit purpose other than religion. For example: schools for the deaf, schools for students with developmental disabilities, Montessori schools, all boys schools, and all girls schools.
- Schools with a primarily Black student body. For example, schools affiliated with the Josephites or Sisters of the Blessed Sacrament, which are Catholic orders that focus on Black congregants.
- Schools affiliated with religions not typically affiliated with the segregation academy movement (Nevin and Bills 1976). For example: Jewish, Lutheran, and Seventh Day Adventist. Religious status of these schools were determined using the NCES religion code or the school about page.
- Schools founded before 1954 (Brown vs. Board of Education). Exceptions to this rule include Indian Springs School[70] in Shelby County, Alabama and the Carolina Academy[71] in Florence, South Carolina.
- Preschools, day care centers, and kindergartens.

70. Indian Springs was founded in 1952 but became coed in the 1975 ("Indian Springs History," n.d.). Jones 2005 documents its status as a segregation academy. It is classified as a 5: Confirmed in the author's ranking system.

71. The Carolina Academy was founded in 1953, but lost its tax exempt status in 1982.*Education Week* 1982 we infer this to be due to a racially discriminatory admissions policy, likely as a result of Green vs. Connally (see Legislative Timeline in the Appendix)

D — Event Study Assumptions

ν_ℓ can be interpreted as w_{segac} if the following assumptions hold.

Assumption 1: In the absence of a segregation academy, enrollment in treated districts would have increased or decreased in a fashion parallel to untreated districts. This assumption is the most problematic in our context, and the one we will take the most time justifying in Section 6. Segregation academies were not built in random locations. As discussed in 3.2 and shown in Table 2, the location of these academies is highly correlated with the percentage of the population that is Black and measures of preexisting racial animus. Recall that the "psychic cost" of attending an integrated school, $c^i_{pub}(r^i, b_{pub})$, is increasing in both of these variables. This presents a threat to identification if c^i_{pub} is changing *differentially* in treated vs. control counties because of something other than segregation academies during this time period. Alternatively, the cost of other outside options could change differentially in treated vs. control areas. If either of these factors hold, ν_ℓ should be interpreted as some combination of W_{move}, W_{priv}, and W_{segac}. Both of these would bias our estimates of w_{segac} away from zero.

Assumption 2: Parents must not either enroll their kids in private school or take them out of public school in anticipation of the formation of a segregation academy. The former does not seem plausible. The latter does: consider a public education loving parent who hears that a group of "concerned citizens" is going to start a private school and wage war on the public schools. This parent might decide that this community is not the best place to raise their child, and leave the school district prior to the establishment of a segregation academy. By lowering enrollment before the event time, this would bias our estimates of the effect of the opening of a segregation academy on enrollment towards zero.

Assumption 3: Any differential enrollment loss in public schools in treated vs. control districts is driven by White students. If not, ν_ℓ is some combination of W and B, where B is the average difference of changes in Black enrollment in treated vs. control districts. This assumption is necessary because we only have enrollment data disaggregated by race for two of the seven states in the Southeast.

E — Robustness

E.1. Robustness of Event Study Results

In order to identify the effect on enrollment, parallel trends must hold. As noted in Sun and Abraham 2021, it is sufficient if these assumptions hold conditional on covariates. We provide a visual test of pre-trends in event-study plots in Figures 4 and 5. Additionally, we follow Sun and Abraham 2021 by using never-treated cohorts as our control group. We adopt this because it mitigates the possibility of anticipation effects distorting estimates in not-yet-treated school districts.

However, using the never-treated group as a control group can be problematic if differing baseline characteristics of the treated and never-treated population cause a divergence in trends in periods post treatment that would have occurred irrespective of treatment. Indeed, Table 2 shows that treated locations have statistically significant different pre-treatment characteristics than control districts.

Of particular concern is the possibility that the establishment of segregation academies coincided with *unobserved* integration events that changed the psychic cost (to White parents with nonzero racial animus) of attending the local public school, c^i_{pub}, by affecting the ratio of Black students in the local public school, b_{pub} (we control for observed integration events with $desegPost$). In other words, in order for **Assumption 1** to hold, c^i_{pub} must not increase differentially in treated versus control areas. This would lead to enrollment declines irrespective of the establishment of a segregation academy, because the sum of parents for whom $c^i_{pub} >$ the cost of some outside option would increase (Equation 1). Even if we observe that these parents sent their child to a segregation academy, it is possible that, had the segregation academy not existed, they may have moved their family or attended a different private school.

As shown in Table 2, academies were established in school districts with a relatively large Black share of population (38% of treated school districts were majority Black in 1950, compared to 20% of control districts). We control for a time trend specific to the quartile percentage of the population that is Black in 1950 in column (2) of Table 3.This shrinks the overall ATT from .10 log points to statistically insignificant .03 log points. There is still a statistically significant White enrollment decline of .16 log points. However, because percent Black in 1950 significantly predicts our treatment, the inclusion of this control could bias our estimates *towards* zero. Consider the case where ever-treated is perfectly associated with quartile Black. The inclusion of a quartile Black $\times$ year trend would be equivalent to including separate time trends for treated versus control districts, which would absorb some variation that is due to treatment, biasing the treatment effect towards zero.

Across our sample, majority Black counties experienced a median enrollment decline of 31%, compared to an enrollment decline of 7% in majority White school districts. However, treated school districts experienced *larger* enrollment declines: 31.5% vs. 28.1% for treated vs. control majority Black districts, respectively, and 13.2% vs. 4.4% for treated vs. control majority White districts.

To test if White population changes drive our results, we allow for a divergence in the time trend of enrollment that is specific to preexisting population changes in column (3) of Panel A and B in Table 3 . School districts experiencing population decline prior to 1960, for example, could experience a faster rate of relative enrollment decline than school districts experiencing preexisting population growth. Estimates of public enrollment losses and White public enrollment losses are the same or marginally larger, ruling out White flight unrelated to academies as a mechanism.

Our results are unchanged when we allow the time trend to vary with preexisting measures of racial animus (Column (4) of Table 3). Results are also robust to controlling for an alternate measure of racial animus, number of Klans per 1950 population.

We cannot entirely rule out the possibility that enrollment declines would not have occurred had academies not existed. However, the cost of the two alternative outside options—moving or attending a preexisting private school—would have been very high. School districts in the South, especially in low density, rural areas, typically comprise entire counties. Furthermore, it is not clear where families would have moved. (Cascio et al. 2008) analyze the extent of desegregation occurring in the South from both observed and unobserved desegregation events. They find that by 1964-1966, most Southern school districts had desegregated to some extent, and by 1970, most desegregation in the South was complete. Preexisting private schools were concentrated in cities, which could have meant a prohibitively long commute for rural working parents.

These methods, as well as a matching exercise we describe in Appendix Section E.2, thus provide a

conservative lower bound for our treatment effect.The next section includes a more detailed discussion of the matching exercise.

E.2. Matching Exercise: Enrollment

We match and weight our control school districts using estimates from the method outlined by Ho et al. 2007. This is a more flexible, nonparametric method of controlling for district characteristics. Notably, it does not impose assumptions about the functional relationship between the district characteristics and the outcome variable. This nonparametric method weights untreated observations such that the distribution of the variables we match on, X_i do not depend on treatment, that is:

$$\tilde{p}(X|T=1) = \tilde{p}(X|T=0) \tag{E.1}$$

However, matching does come with a loss of precision: there are only 570 school districts in our matched sample, compared to 750 school districts in our main analysis. Our matching dimensions of choice are the share of the population that was Black in 1950, 1950 population, population delta 1940 to 1950, population density 1950, number of lynchings scaled by 1950 population, share vote share for Strom Thurmond in 1948, median family income in 1950, and percent of households that are families in 1950. Table 2 shows the correlation of these variables with our treatment indicator. All but the Thurmond vote share are obtained from the 1950 census. The vote share for Strom Thurmond in 1948 comes from Cascio et al. 2010.

Prior research about the nature of the school districts that housed these academies inform the matching variables we choose. Percent density, percent families, and population are used to control for size of a school district and the possibility that dense, highly populated areas with preexisting private alternatives to public schools may react differently to the introduction of a private alternative than rural areas with no preexisting private schools. Population delta 1940 to 1950 controls for enrollment growth or decline due to population growth or decline. Median family income in 1950 controls for the ability of White families to afford paying for sending their child to private school and the demand for the quality of the school (median level of schooling at age 25 was originally included then dropped because of a near perfect correlation with median family income). Finally, motivated by newspaper accounts of schools being located in districts with majority Black population (*Alabama Journal* 1966), we control for the possibility that percent of non White residents could differ systematically between school districts with a segregation academy versus school districts without. In Section F, we discuss how differences in effect size for school districts with different baseline characteristics inform our understanding of the mechanisms in play.

E.3. Childhood Location vs. Birth Location

While our main analyses use place of birth to proxy for place of schooling, a child's place of birth will not always align with their residence during their childhood years: the hospital or physical location of birth could be in a different county than their residence, or the child could have moved between birth and the age of 18. Importantly in our context, parents may have chosen to move as a result of a segregation academy opening.

One major challenge in measuring children's locations at this time is the limited availability of our only

source of residential location data during the 1960s through mid-1990s: IRS 1040 forms. The IRS maintained select years of mid-twentieth century 1040s for every five tax years from 1964 to 1994 (corresponding to location years 1965-1995). Unfortunately, dependent information was not maintained until 1994, meaning that the locations of children are especially difficult to track.

To help address this, the Census Bureau has created links between parents and children born between 1964 and 1979 using a restricted version of the Social Security Administration Numident, which includes names of both children and their parents (Stinson and Weiwu 2023). Matched based on name, children are linked to their parents' PIKs, which then allows us to find their parents' 1040 forms. With this dataset, we assign children born 1964 to 1979 their parents' location in 1975 and 1980. [72] While we cannot use these links to assign treatment status in our main analysis, we can approximate the rate of individuals for which treatment status would differ if childhood location, rather than birth county, was used to determine treatment status.[73] 36% of individuals born in control counties were observed in treated counties in childhood, whereas only 6% of individuals born in treated counties were observed in control counties. This measurement error would bias our estimates towards zero.

We also use this sample to test for predictors of mismatches between county of birth and childhood county (Appendix Table E.1). We find the log of parental income is negatively correlated with mismatches. This is in line with research on early childhood mobility, which typically finds that low income individuals are overrepresented among early childhood movers (Lawrence, Root, and Mollborn 2015). However, unlike this literature, we do not find Black households move counties more in our sample during childhood. We also find that mismatches are more prevalent in certain states: the rate of mistmatch is lowest in Alabama (37.56%) and highest in Georgia (56.84%).

This builds on emergent research that documents bias that arises from using county of birth, often the only variable available to researchers interested in long-term outcomes, as a proxy for childhood exposure (Sullivan, Becerra, and Genadek 2023). Mismatches between county of birth and county of residence in childhood come from two sources: measurement error (mismatch between county of hospital / birth and county of residence) and early childhood mobility. The latter is likely to be endogenous to our outcomes of interest: for example, a family may relocate once a child is of school age to be closer to "good" schools. Analysis of modern ACS data shows that early childhood moves of this type are incredibly common: 30% of families move counties before their first born turns five.[74] The prevalence of early childhood cross-county moves also suggests that using place of birth may be less endogenous than using childhood county. In other words, place of birth is closer to an intent-to-treat estimator than childhood county, which can be thought of as a treatment-on-the-treated estimator.

We incorporate variation in mismatches at the county and race level into a bounding exercise. We find our results are robust to adjusting for treatment switching during childhood.

We begin with the sample of individuals we are able to match to their parents location during childhood (hereafter referred to as the "child sample"). For each county of birth, we observe the rate of individuals

72. We first preference mother's location, then father's if mother's is not present

73. We cannot use these links to determine childhood location in our main analysis: because the last segregation academy in our sample opened in 1980, all "linkable" children (those born after 1964) were of school age when a segregation academy opened. We thus can only compute a simple difference between those observed in treated counties in childhood with those observe in control counties in childhood.

74. *Source:* Author's calculations using Mason et al., IPUMS National Historical Geographic Information System.

Table E.1 — Predictors of Childhood County Differing from Birth County

Predictor	Estimate	Std. Error	N
White (Intercept)	0.4535	0.0003484	2,040,000
Black	-0.0071	0.0006228	929,000
Alabama (Intercept)	0.3756	0.0008694	322,000
Florida	0.0346	0.001037	766,000
Georgia	0.1928	0.001101	535,000
Louisiana	0.0852	0.001126	476,000
Mississippi	0.1199	0.001435	187,000
North Carolina	0.0455	0.001141	447,000
South Carolina	0.0495	0.001338	235,000
Birth County Not Treated (Intercept)	0.5041	0.0008525	340,000
Birth County Treated	-0.0597	0.000906	2,629,000
Log of Parent Income (max WSI, AGI)	-0.0437	0.0003015	2,969,000

Notes: **Characteristics associated with childhood location differing from birth county. This is measured only on our sample of individuals that are able to be linked to parent income tax returns. The outcome variable for all regressions is a binary indicating if childhood location, as inferred from parental tax returns, is different than birth county, as recorded in the Numident. For categorical variables, the intercept is the average for the omitted category, which we specify. For example, 37.56% of Alabama children have a different birth county than childhood county, compared to 56.84% of Georgia children (=.3756+.1928). Each box is a separate regression. Estimates are rounded according to Census Bureau Disclosure Avoidance guidelines.** ***Source:*** **Census Bureau links between parents and children born between 1964 and 1979 (Stinson and Weiwu 2023). Matched based on name, children are linked to their parents' PIKs, which then allows us to find their parents' 1040 forms. With this data set, we assign children born 1964 to 1979 their parents' location in 1975 and 1980. We first preference mother's location, then father's if mother's is not present. For a full description of this data, see E.3.** ***Source:*** **Stinson and Weiwu 2023, IRS 1040s 1974, 1979, and Census Numident.**

for whom their childhood county does not match their birth county. Among these individuals, we are specifically interested in those for whom the treatment status of their birth county does not match the treatment status of their childhood county. For these "switchers," we are mis-measuring treatment. 1 - this switching rate approximates the share of individuals in each county for which binary treatment status does not change.

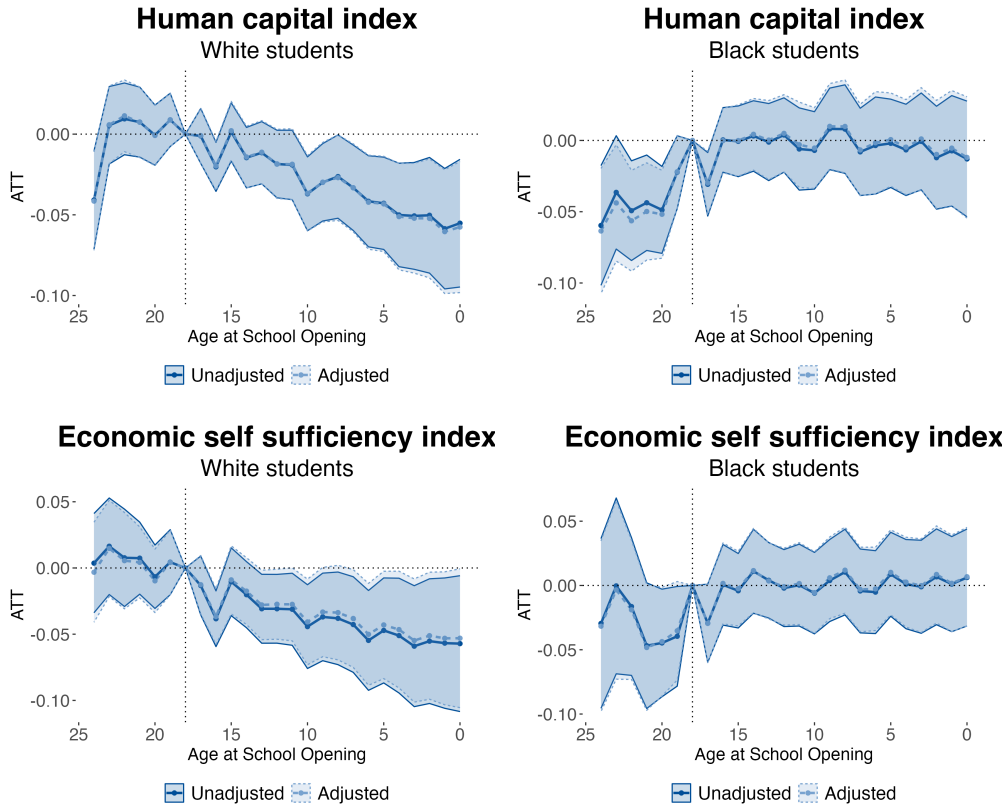
We implicitly assume that this rate of switching remains constant across the birth years represented in our sample (1942-1980). To account for sampling variation and noise, we shrink these estimates using the empirical Bayes estimator (Ramachandran and Tsokos 2021) towards the mean of the share per race:

$$1 - \text{share switch} \approx \hat{\pi}^*_{cr} \equiv \hat{\pi}_{cr} * \frac{\sigma^2(\hat{\pi}_{cr})}{\sigma^2(\hat{\pi}_{cr}) * \sigma^2(\pi_r)} + \overline{\pi}_r * \frac{\sigma^2(\pi_r)}{\sigma^2(\hat{\pi}_{cr}) * \sigma^2(\pi_r)} \tag{E.2}$$

We bootstrap the child sample with replacement using 100 repetitions to obtain $\hat{\pi}_{cr}$, the mean share of individuals of race r in each county c for whom treatment status does not change across repetitions. We then weight this by the ratio of the variance of each county-race pair $\sigma^2(\hat{\pi}_{cr})$ to the variance for each race $\sigma^2(\pi_r)$, then add a weighted mean of the share $\bar{\pi}_r$. Intuitively, this shrinks noisy estimates to the mean. We weight our estimates by this share in our decomposition exercise. These results are largely unchanged for the Human Capital and Economic Self Sufficiency indices. Figure E.1 shows the scaled estimates.

We caveat this approach by acknowledging that age at treatment could still change. Consider an individual born in 1950 county where a school opens in 1970. She appears in our data as being age 20 when a segregation academy opened, or -2 "years of exposure." However, in her true childhood county of residence, a school opened in 1965. Her true age at treatment is thus 15, or 3 years of exposure. Measurement error such as this — where individuals with nonzero years of exposure are included in the estimates of the pretreatment period, or vice versa, would bias our estimates towards zero. We could also mismeasure relative years of exposure even within the post treatment period (for example, age 10 versus age 14).

Figure E.1 — Indices Weighted for Measurement Error



Notes: **Results are from estimating Equation 8, weighted with shares calculated with Equation E.2. The average treatment effect is for individuals aged 6 when a segregation academy opens nearby.** $*$, $**$, $***$ **represent significance at the 10, 5, and 1 percent levels, respectively. Effect size is measured in standard deviations. Economic self-sufficiency is a standardized measure adopted from Bailey et al. 2021 consisting of binary indicators of employment, poverty status, income from public sources, family income, and income from other nongovernmental sources. We also include continuous measures of weeks worked, usual hours worked, the log of labor income, log of other income from nongovernmental sources, and log ratio of family income to the federal poverty threshold.** ***Source:*** **2000 Long Form Decennial Census, ACS 2001-2019, and Census Numident. Human capital index is a standardized measure adopted from Bailey, Sun, and Timpe 2021 that consists of years of schooling and binaries for high school degree or GED, any college, at least four years of college, professional or doctoral degree, and work in management job. Working in a management job is defined as reporting an occupation code 1 through 359 (management, business, science and arts occupations)** ***Source:*** **2000 Long Form Decennial Census, ACS 2001-2019, and Census Numident.**

E.4. Adjusting for Pre-trends in Cohort Design

We adjust estimates for pre-trends with two methods.

First, we recenter the event study coefficients to the entire pre-period (ages 18-24 when a segregation academy opened) by subtracting the weighted mean of the pre-treatment periods (ages 19–24), where the weights are given by the number of observations in each period n_ℓ:

$$\tilde{\beta}_\ell = \hat{\beta}_\ell - \frac{\sum_{\ell \in [19,24]} n_\ell \, \hat{\beta}_\ell}{\sum_{\ell \in [19,24]} n_\ell},$$

This is equivalent to changing the omitted period from 18 to 18-24 in the main regression (Equation 8, which is also weighted by number of observations):

$$Y_{i(c,t)} = \alpha_c + \lambda_t + \zeta_{t'} + \delta_{st} + desegPost_i + \sum_e \sum_{\ell \notin [18,24]} \delta_{e,\ell}(\mathbf{1}\{E_i = e\} \cdot D_i^\ell) + \epsilon_i \tag{E.3}$$

Effects are measured relative to ages 18 to 24, rather than age 18. The new variance can be calculated as:

$$\widehat{\mathrm{Var}}(\tilde{\beta}_\ell) = \widehat{\mathrm{Var}}(\hat{\beta}_\ell) + \widehat{\mathrm{Var}}(\bar{\beta}) - 2\,\widehat{\mathrm{Cov}}(\hat{\beta}_\ell, \bar{\beta}),$$

where $\bar{\beta}$ is the weighted average of $\hat{\beta}_{\ell \in [18,24]}$.

However, because of data restrictions, we only have access to disclosed estimates of $\hat{\beta}_\ell$ and $\widehat{SE}(\hat{\beta}\ell)$. We do not have access to the covariance matrix. This means we can only calculate standard errors from $\widehat{\mathrm{SE}}(\tilde{\beta}_\ell)$ if we assume independence ($\widehat{\mathrm{Cov}}(\hat{\beta}_\ell, \bar{\beta}) = 0$), which is unlikely to hold in our setting. This approach will overstate the true variance. Our calculated standard errors can thus be assumed to be conservative.

We thus calculate variance of the re-centered $\tilde{\beta}\ell$ as:

$$\widehat{\mathrm{Var}}(\tilde{\beta}_\ell) \approx \widehat{\mathrm{Var}}(\hat{\beta}_\ell) + \frac{\sum_{\ell \in [19,24]} n_\ell^2 \, \widehat{\mathrm{Var}}(\hat{\beta}_\ell)}{\left(\sum_{\ell \in [19,24]} n_\ell\right)^2},$$

with standard error

$$\widehat{\mathrm{SE}}(\tilde{\beta}_\ell) = \sqrt{\widehat{\mathrm{Var}}(\tilde{\beta}_\ell)}.$$

In our second approach, we address the concern that a linear trend in age at exposure by driving our results. Estimates derived from this method reflect deviations in outcomes from preexisting linear trends in age at exposure. This approach makes the strong assumption that the counterfactual path of outcomes is linear in age at exposure, which understates the importance of confounding factors (in our case, the effect a segregation academy has on its community) at older ages but overstates them at younger ages (Freyaldenhoven, Hansen, and Shapiro 2019).

We first fit a regression for the estimated $\hat{\beta}$ for ages $\ell \in [18, 24]$:

$$\hat{\beta}_{\ell \in [18,24]} = \hat{\alpha} + \hat{\delta}\ell$$

We then detrend our estimates by subtracting the fitted values from the estimated coefficients in the post period:

$$\tilde{\beta}_\ell = \hat{\beta}_\ell - (\hat{\alpha} + \hat{\delta}\ell),$$

Again, assuming independence, the variance can approximated as

$$\widehat{\text{Var}}(\tilde{\beta}_\ell) \approx \widehat{\text{Var}}(\hat{\beta}_\ell) + \widehat{\text{Var}}(\hat{\delta}\ell + \hat{\alpha})$$

and the corresponding standard error is the square root.

We show the results of these exercises in Appendix Table E.2.

Table E.2 — Preperiod Adjustments

Panel A: White Students

Measure	Base (age 6)	Recentered to ages 19-24	Linear adjustment
Human capital index	-0.042** (0.014)	-0.042** (0.018)	-0.130* (0.070)
Years of Schooling	-0.194** (0.069)	-0.226** (0.085)	-0.478 (0.334)
Economic self sufficiency index	-0.055** (0.019)	-0.059** (0.025)	-0.031 (0.046)
In labor force	-0.019** (0.007)	-0.020** (0.010)	-0.015 (0.024)
Weeks worked last year	-0.777* (0.402)	-0.715 (0.520)	-1.224 (0.945)
Log labor income	-0.149** (0.073)	-0.159 (0.106)	-0.071 (0.320)

Panel B: Black Students

Measure	Base (age 6)	Recentered to ages 19-24	Linear adjustment
Human capital index	-0.004 (0.017)	0.037 (0.024)	-0.038 (0.058)
Years of Schooling	-0.012 (0.073)	0.162 (0.106)	-0.077 (0.297)
Economic self sufficiency index	-0.005 (0.016)	0.028 (0.031)	0.118* (0.069)
In labor force	-0.006 (0.010)	0.011 (0.016)	0.074** (0.034)
Weeks worked last year	-0.136 (0.424)	0.946 (0.733)	1.883 (1.984)
Log labor income	-0.065 (0.095)	0.106 (0.157)	0.567* (0.294)

Notes: **Estimates adjusted for pre-trends as described in Section E.4.**

F — Correlates of Differential Enrollment Losses

In this section, we test the predictions of our theoretical framework. Recall that the marginal White parent is shifted into leaving the public school system by the establishment of a segregation academy if the cost of attending a segregation academy $< c^i_{pub} <$ the cost of moving to a racially homogeneous school district or the cost of enrolling in an existing private school.

We expect the marginal decrease in enrollment to be largest in areas where outside options are costly. Cost reflects commute time to the nearest private school, a family's ability to afford private school tuition, or the feasibility of moving. Recall that we assume $tuition_{priv} > tuition_{segac}$ based on anecdotal historical evidence.

We test these predictions by estimating differential enrollment losses for each school district by adapting the imputation style estimator described in Borusyak, Jaravel, and Spiess 2021.

F.1. Imputation Method for Calculating Enrollment Losses

Ideally, we would calculate unit-level treatment effects. Instead, we calculate the difference in predicted enrollment and observed enrollment. These estimates differ from unit-level treatment effects τ_i by $\sum_{l>-1} \epsilon_{i,t}$, or the sum of the error term across the post treatment periods: $\hat{\tau}_i^* = \tau_i + \sum_{\ell>-1}^{T-\ell} \epsilon_{i,t}$. This allows us to do two things. First, it allows us to create a map that shows which treated areas have the largest differential enrollment losses, allowing us to identify geographic patterns. Second, we are able to estimate correlates of losses.

First, using untreated and not yet treated observations Ω_0, we estimate coefficients: $\hat{\alpha_i}$, $\hat{\lambda_t}$,$\hat{\theta}_{s(i)t}$, $\hat{\delta}$.

$$Y_{it}(0) = \underset{\text{school district}}{\alpha_i} + \underset{\text{year}}{\lambda_t} + \underset{\text{year by state}}{\theta_{s(i)t}} + \delta desegPost_{i,t}$$

For treated observations Ω_1, we set

$$\hat{Y}_{it}(0) = \underset{\text{county}}{\hat{\theta}_i} + \underset{\text{year}}{\hat{\lambda}_t} + \underset{\text{state}\times\text{year}}{\hat{\alpha}_{s(i)t}} + \hat{\delta} desegPost_{i,t}$$

Then, we estimate the differential enrollment loss $\hat{\tau}_{it}$:

$$\hat{\tau}_{it} = Y_{it} - \hat{Y}_{it}(0) \tag{F.1}$$

The differential enrollment loss for each school district i is thus the sum of the differences in predicted enrollment and observed enrollment in each post treatment time period. We weight each time period equally.

$$\hat{\tau_i^*} = \sum_{it \in \Omega_1} w_{it} \hat{\tau_{it}} \tag{F.2}$$

F.2. Results

We graph the geographic distribution of our results in Figure F.1. In the bottom panel of Figure F.1, we regress $\hat{\tau}_{it}$ on pre-treatment characteristics. We find that enrollment losses associated with segregation academies were larger in sparsely populated, poor, and relatively Black areas with a history of racial animus. For example, a one standard deviation increase in percent Black is associated with .1 log point more children leaving the public schools for segregation academies.

We expect $c^i_{pub}(r^i, b_{pub})$ to be increasing in the three characteristics — percent of the population that was Black in 1950, the share of votes for Strom Thurmond in 1948, and lynchings per capita. The latter two act as proxies for r^i, while the former is directly related to b_{pub}. All of these characteristics are associated with larger enrollment losses, implying the intuitive result that segregation academies had a larger effect in areas where more families were opposed to integration.

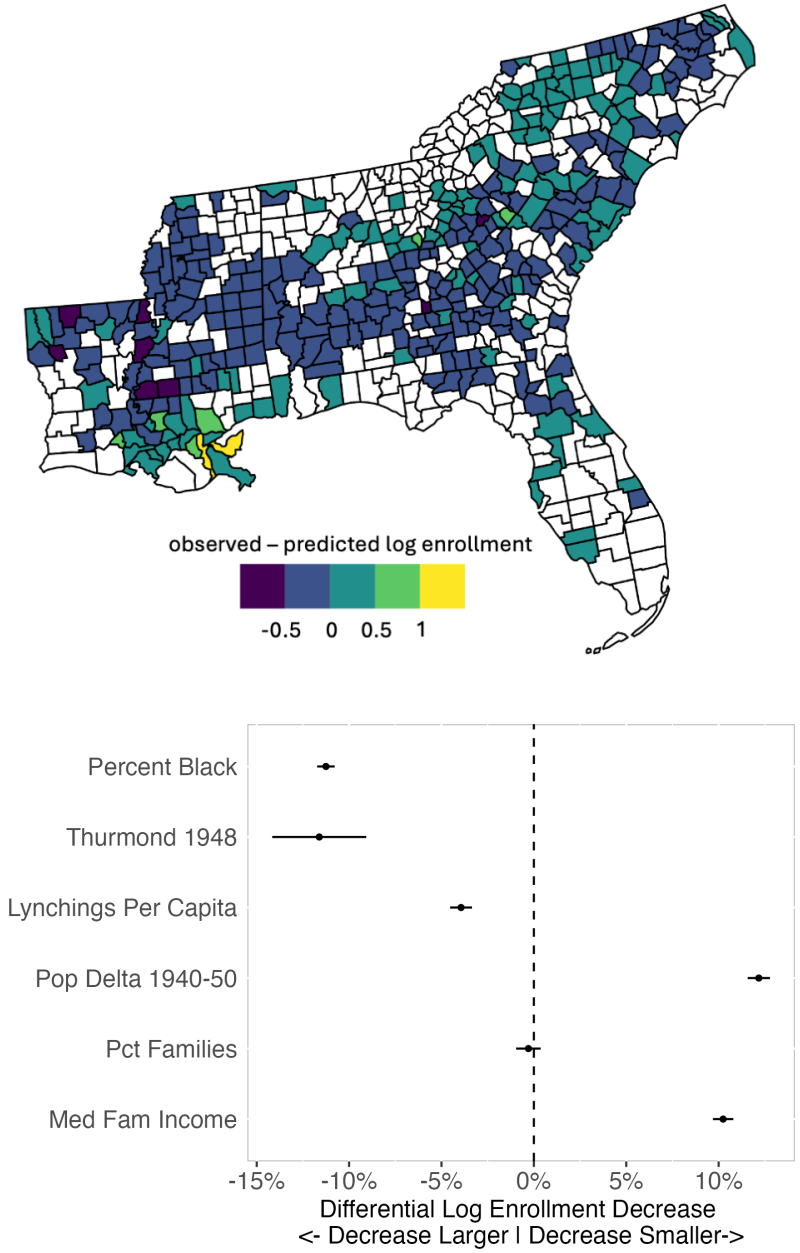
Population density, population Δ from 1940 to 1950, and median family income act as inverse proxies for distance to the nearest private school $\bar{dist}_{priv}$, average moving cost $\bar{m}$, and ratio of private school tuition to family income $\bar{tuition}_{priv}$. The cost of other outside options (moving or attending a preexisting private school) are increasing in these factors. We thus expect w_{segac} to be *decreasing* in these factors. Indeed,

enrollment losses are negatively associated with these factors. We can think of these communities as places where the cost of exiting public school would have been prohibitively high if a segregation academy had not been founded.

This sheds light on how segregation academies functioned within their communities. Anecdotal historical evidence suggests that in some communities, particularly rural communities, segregation academies completely displaced public schools for White parents. These are communities like Monroeville, Alabama, where Monroe Academy enrolls 409 students in PreK-12, 408 of which are White. The local public high school, Monroe County High School, enrolls 333 students, 93% of which are Black — there are only 23 White students. If a third of Monroe Academy high schoolers attended Monroe County High School today, the school would be 35% Black, a figure much more in line with Monroe County's demographics (54% White).[75] Contemporary examples include Lowndes Academy in Lowndes County, Alabama where in 1982 "the student body of the private system is 100 percent White, and the student body of the public school system is 99 percent black" and Warrior Academy in Greene County, Alabama: "Since the White flight to the academy, Greene's public school enrollment has ranged from 95 to 99 percent black, and the faculty has been 80 percent or more black" (Johnson and Pearson 1982). Anecdotally, to ease the cost burden for families and maximize the number of students, segregation academies would allow parents to forgo tuition in exchange for labor or scale tuition to meet each family's ability to pay. These schools saw themselves as (White) community institutions with a mission to serve the (White) children of the community.

75. Data for Monroe Academy comes from Private School Review, which retrieves its data from the NCES and is current as of 2023. We was unable to locate Monroe Academy in the NCES Private School Finder. Data for Monroe County comes from the U.S. Census and is current as of 2020.

Figure F.1 — Spatial Distribution of Enrollment Effects



Notes: **Geographic distribution and correlates of differential enrollment losses. Blue and purple shading indicate enrollment was lower than predicted and green through yellow colors indicate that enrollment was higher than predicted. Correlates are standardized within states. Enrollment losses are defined as the difference between predicted enrollment and observed enrollment. Enrollment is predicted using not-yet-treated and never-treated observations, as described in Section F.1. The x-axis indicates the change in estimated treatment effect associated with a one standard deviation change in the correlate. For example, a one standard deviation increase in Percent Black is associated with an additional .1 log point loss in enrollment due to segregation academies.**

G — Disentangling the Role of Segregation Academy Attendance versus Exposure

The results on White students include effects on both White students attending segregation academies and White students attending public schools. The former group would be affected through the quality of education offered in the new private schools; the latter, by the effect on resources for the public schools. In this section, we perform a descriptive exercise to decompose our estimates into effects on students attending segregation academies versus students attending public schools in treated counties.

We cannot estimate these effects directly because we do not observe type of school attended. This is known in the economics and political science literature as the "ecological inference" problem (King 1997 provides an overview of this field).

Conceptually, the previously discussed results are the weighted average of two latent subgroups: children who attend segregation academies, and all other children in treated counties:

$$ATT = \theta_{segac} ATT_{segac} + (1 - \theta_{segac}) ATT_{other}$$

While ATT_{other} includes children who attend public school in the county and children who attend non-segregation academy private schools. We assume $\theta_{share} = \theta_{public}$ because of the minimal presence of private schools in the South prior to advent of segregation academies: in 1960, fewer than 1.7% of children aged 5-14 attended private school in 75% of the counties in the Southeast. Only 10% of counties had rates of attendance higher than 6% (Mason et al., IPUMS National Historical Geographic Information System).

Assuming this framework holds, we conduct a simple decomposition exercise to recover ATT_{segac} and ATT_{public}:

$$Y_{ctr} = \alpha_c + \lambda_t + \theta_{s(c)t} + \text{desegPost} + \sum_{\ell \notin [18,24]} \beta_\ell D_c^\ell(1)$$

$$\beta_\ell = \theta_{segac} \beta_{1\ell} + (1 - \theta_{segac}) \beta_{2\ell}(2)$$

Where Y_{ctr} is the outcome in county c for birth year t and race r. Substituting (2) into (1):

$$Y_{ctr} = \alpha_c + \lambda_t + \theta_{s(c)t} + \text{desegPost} + \theta_{segac} \sum_{\ell \notin [18,24]} \beta_{1\ell} D_c^\ell + (1 - \theta_{segac}) \sum_{\ell \notin [18,24]} \beta_{2\ell} D_c^\ell$$

The coefficients of interest are $\beta_{1\ell \in [0,6]}$ and $\beta_{2\ell \in [0,6]}$, or the decomposition of the treatment effect between ages 0 and 6. We do not observe θ_{segac} directly: enrollment data for individual segregation academies are not widely available in our sample states. Instead, we calculate the difference in predicted public enrollment and observed public enrollment.

First, using untreated and not yet treated observations Ω_0, we regress untreated and not yet treated observations $Y_{ct}(0)$ to estimate county, year and state-by-year fixed effects $\hat{\alpha_c}$, $\hat{\lambda_t}$,$\hat{\alpha}_{s(c)t}$ and the average

effect of enrollment after a significant court order $\hat{\delta}$:

$$Y_{ct}(0) = \underset{\text{county}}{\alpha_c} + \underset{\text{year}}{\lambda_t} + \underset{\text{year by state}}{\theta_{s(c)t}} + \delta desegPost_{c,t}$$

For treated observations Ω_1, we estimate predicted log enrollment $\hat{Y}_{ct}(1)$:

$$\hat{Y}_{ct}(1) = \underset{\text{county}}{\hat{\theta}_c} + \underset{\text{year}}{\hat{\lambda}_t} + \underset{\text{state}\times\text{year}}{\hat{\alpha}_{s(c)t}} + \hat{\delta} desegPost_{c,t}$$

Then, we estimate the differential enrollment loss $\hat{\tau}_{ct}$:

$$\hat{\tau}_{ct} = Y_{ct} - \hat{Y}_{ct}(1) \tag{G.1}$$

The differential enrollment loss for each county c is thus the sum of the differences in predicted enrollment and observed enrollment in each post treatment time period. We weight each time period equally, where $w_{ct} = 1/\texttt{number of post treatment periods for county } c$

$$\hat{\tau_c^*} = \sum_{ct \in \Omega_1} w_{ct} \hat{\tau_{ct}} \tag{G.2}$$

These estimates differ from unit-level treatment effects τ_c by $\sum_{\ell > -1} \epsilon_{c,t}$, or the sum of the error term across the post treatment periods: $\hat{\tau}_c^* = \tau_c + \sum_{\ell > -1}^{T-\ell} \epsilon_{c,t}$.

This approach assumes that county of birth is a valid proxy for childhood exposure. To address this, we weight our regression by $\hat{\pi}_{cr}$, the mean share of individuals of race r in each county c for whom treatment status does not change. This estimation is described in detail in Appendix Section E.3. Because Black children were, by definition, not allowed to attend segregation academies, we only report estimates for White children.

G.1. Results

Results on students from age 0-6 are shown below.

Table G.1 — Decomposed Estimates

Measure	Base (age 0-6)	Public	Private
Human capital index	-0.050** (0.000)	-0.028** (0.009)	-0.036** (0.018)
Years of Schooling	-0.239** (0.009)	-0.120** (0.039)	-0.080 (0.072)
Economic self sufficiency index	-0.055** (0.001)	-0.023** (0.011)	-0.007 (0.021)
In labor force	-0.019** (0.000)	-0.010** (0.004)	-0.010 (0.009)
Weeks worked last year	-0.866** (0.341)	-0.358* (0.210)	-0.282 (0.450)
Log labor income	-0.156** (0.013)	-0.066 (0.049)	-0.091 (0.099)

***Notes*: Results are for White children only aged 0-6 when a segregation academy opens compared to those aged 18-24. Estimates are $\beta_{1\ell\in[0,6]}$ and $\beta_{2\ell\in[0,6]}$, or the decomposition of the treatment effect between ages 0 and 6 from the following regression: $Y_{ctr} = \alpha_c + \lambda_t + \theta_{s(c)t} + \textbf{desegPost} + \theta_{segac}\sum_{\ell\notin[18,24]}\beta_{1\ell}D_c^{\ell} + (1-\theta_{segac})\sum_{\ell\notin[18,24]}\beta_{2\ell}D_c^{\ell}$. Public and Private do not add up to the base estimate because observations are weighted to account for childhood mobility for the Public and Private estimations only (see Appendix E.3).**
***Source:* 2000 Long Form Decennial Census, ACS 2001-2019, and Census Numident.**