Desegregation and Black Dropout Rates

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ABSTRACT

In 1954 the Supreme Court of the United States ruled that separate schools for black and white children were "inherently unequal." This paper studies whether the desegregation plans of the next 30 years in fact benefited the black students for whom the plans were designed. Analysis of data from the 1970 and 1980 censuses suggests that desegregation plans of the 1970's reduced the high school dropout rates of blacks by two to three percentage points during this decade. Desegregation plans can account for about half of the decline in dropout rates of blacks between 1970 and 1980. A similar analysis suggests that desegregation plans had no effect on the dropout rates of whites. The results are robust to controls for family income, parental education, and state- and region-specific trends, as well as to tests for selective migration. Endogenizing the timing of desegregation yields generally larger, though not significantly different, estimates.

(JEL: H4, I2, J1, J7, N3)

1. Introduction

From *Plessy v. Ferguson*[1] in 1896 until *Brown v. Board of Education*[2] in 1954, Southern and Border States legally segregated their school systems by race. Black schools received fewer resources and black children were taught almost exclusively by black teachers. Outside the South, migration, housing patterns, and actions by state and local leaders contributed to similar racial isolation in the schools. With the *Brown* decision, the Supreme Court deemed segregated schools "inherently unequal" and therefore unconstitutional.

Over the next 30 years, federal courts ordered the implementation of desegregation plans for many of the largest school districts in the United States. It was the intent of these court orders to provide equal educational resources to blacks by eradicating segregation on the basis of race. Indeed, the desegregation of the public schools was among the most significant innovations in the educational system of the post-World War II U.S. Nevertheless, there is little consensus on the effect of desegregation on integration's intended beneficiaries, black students. Figure 1 shows that, as the nations schools were being integrated, from the late 1960's to the early 1980's, black high school dropout rates were falling and white rates were holding constant. The contribution of court-ordered integration to this decline is an important open question.

Ideally, one would compare year-to-year changes in black and white high-school dropout rates in the years leading to and following integration. Lacking suitable annual data in the relevant time period, I compile census data and use variation in the decade of school desegregation, the result of judicial enforcement of the *Brown* decision, to estimate integration's effect on black high school dropout rates. Using the 1970 and 1980 censuses, high-school-aged blacks in districts that desegregated between 1970 and 1979 (70's desegregators) are compared to those in districts that desegregated both before and after (60's and 80's desegregators). Dropout rates among blacks declined between 1970 and 1980 by two to three percentage points in districts that desegregated in the interim relative to districts that desegregated both earlier and later. Indeed, the entire

[1] 163 U.S. 537 (1896).
[2] 347 U.S. 483 (1954).

decline in black dropout rates during the 1970's occurred in districts that desegregated during the decade. Regression analysis confirms that these estimates are robust to controls for family income, parental education levels, state- and region-specific trends, and controls for potential confounds from the selective migration of blacks. Instrumental variables estimates suggest the relative decline is not due to endogenous delay of integration though the courts.

A large number of studies, beginning with the Coleman Report in 1966, have estimated the effect of the racial composition of schools on black academic achievement. Much of this early work examined students in one school district. Notable among this work were studies that employed random assignment. In a reanalysis of one such experiment in Hartford, Crain and Strauss (1985) find that students randomly offered the chance to be bused to a suburban school were more likely to work in white-collar and professional jobs about 17 years after the experiment. Meta-analyses of these early studies, however, typically find that there was no consensus reached about the effect of school integration on black student achievement.[3]

In more recent work that examines national data, Boozer, Krueger and Wolkon (1992) show that black students who attend high schools with a higher fraction of black students complete fewer years of schooling and earn lower wages later. Acknowledging that omitted variables bias make the interpretation of these estimates difficult, Boozer et. al. present Instrumental Variables (IV) estimates. Their instruments for the racial composition of schools are interactions of state of birth indicators and an indicator for having attended school after 1964.[4] The IV estimates are, however, imprecise. The research design of this paper can be thought of as a refinement of Boozer et. al.'s estimation strategy. It replaces the state-year interactions with data on the decade of integration of each school district. The results are more precise and the research design allows for robustness checks and estimation of interaction effects such as the role of the

[3] See e.g. St. John (1975), Crain and Mahard (1981), Cook (1984), and Armor (2002).
[4] Boozer, Krueger and Wolkon choose 1964 because they show a sharp drop in the extent of racial segregation in that year. This decline is possibly explained by the passage of the 1964 Civil Rights Act which prohibited federal aid to segregated schools and allowed the Justice Department to join suits against school districts that were in violation of the *Brown v. Board* order to integrate.

length of exposure to integrated schools and a comparison of outcomes in districts where desegregation was more and less drastic.[5]

The paper is organized as follows. The next section provides a brief history of school desegregation and presents evidence on the integrative effectiveness of desegregation plans. Section 3 lays out the identification strategy. Section 4 describes the data. Section 5 presents the basic results. Section 6 presents several robustness checks. Section 7 concludes.

2. Background

There was no nationally organized campaign that desegregated the public schools in the U.S. Rather, a series of court cases brought chiefly by private civil rights groups led to the most effective desegregation efforts. Political forces dictated that the first *Brown* decision was enforced mainly in the courts. The second *Brown* decision a year later deemed that enforcement would be carried out by localized federal district courts on a case-by-case basis. As a result, the timing of integration varied at the school district level.

Two other developments shaped the school desegregation process. The passage of the 1964 Civil Rights Act prohibited federal aid for segregated schools and enabled the U.S. Department of Justice to join suit against school districts that failed to comply with the *Brown* order. Then in 1968, in *Green v. New Kent County*, the Supreme Court ruled that school districts could not comply with *Brown* by removing attendance restrictions that were based on race. Districts were forced to take positive action that would lead to effective integration of the schools. Many of the busing plans that integrated large urban school districts followed from the *Green* ruling.

Although desegregation began first in the South, tabulations of the timing of major desegregation plans in the largest school districts in the country reveal significant inter- and intra-regional variation. Table I lists the sample of school districts considered in this study.[6] School districts are listed chronologically by the year in which they

[5] Other notable studies of the relationship between the racial composition of schools and black achievement include Grogger (1996) and Rivkin (2000).

[6] I discuss the sampling procedure and data more specifically in section 4. The sample of districts used is the same as in Welch and Light (1987).

instituted a major desegregation plan. There is remarkable variation in the timing of desegregation plans not explained by regional differences. Of the 22 districts that implemented desegregation plans in the 1960's, seven are located outside of the South. Similarly, of the 77 districts that implemented plans in the 1970's, 35 are located outside of the South.

2.1. The Effectiveness of Desegregation Plans

Three questions concerning the effect of desegregation plans on the racial composition of school districts are relevant to this study. First, did desegregation plans lead to more racial integration? Second, did this integration happen immediately or over the course of several years? And third, did racial integration last for the ten years that are relevant for the analysis in this study?

Much research has addressed these and related questions. Coleman (1975) suggested that court-ordered desegregation plans increased the speed of white migration out of cities. Subsequent research confirmed Coleman's claim, but also found that induced white migration was not extensive enough to offset fully the effect of desegregation plans on the integration of schools. In particular, Welch and Light (1987) show that desegregation plans of the 1960's, 1970's and 1980's decreased the dissimilarity index in school districts by about 20 percentage points.[7] Rossell and Armor (1996) show that, net of effects on white enrollment, desegregation plans led to a 10 to 20 percentage point increase in the fraction of white students at the typical black student's school. Most recently, Reber (2002) shows immediate effects of desegregation on the racial integration of schools, followed by gradual resegregation due to white migration out of cities. Reber's estimates suggest that ten years after implementation, about two-thirds of the induced integration remains. Taken as a whole, the evidence in the literature weighs strongly that desegregation plans led to a decrease in the segregation of public school districts.

To buttress these findings, I present direct evidence addressing the three questions raised at the beginning of this section. Using data on school-level annual enrollments by

[7] The dissimilarity index, described formally later in this section, measures the number of students that would have to move from their present school to fully integrate the district, relative to the number of students that would have to change schools make a fully segregated district a fully integrated district.

race,[8] the analysis tracks two measures of racial integration in the years leading to and following the implementation of the desegregation plans under study. The two measures of racial integration are the black exposure index and the dissimilarity index. The district's black exposure index measures the fraction of white students at the typical black student's school. It is calculated for each district in each year according to

$$E_d = \sum_s \left[p_{sd}^w \times \left(\frac{n_{sd}^b}{n_d^b} \right) \right] \tag{1}$$

where p^w is the fraction of white students, n^b is the count of black students, s indexes schools and d indexes districts.[9] The exposure index is sensitive to the racial composition of the district. As the fraction of students in the district that are white falls, the exposure index falls. In contrast, the dissimilarity index measures school-level integration, conditional on the racial composition of the district. The dissimilarity index is the ratio of two values and is calculated as

$$D_d = \frac{\sum_s \left[n_{sd} \times \left| p_{sd}^b - p_d^b \right| \right]}{2 n_d \times p_d^b \left(1 - p_d^b \right)} \tag{2}$$

where p^b is the fraction of black students, n is total enrollment, and s and d continue to index schools and districts, respectively. The numerator is the fraction of students who would have to change schools to completely integrate the district (i.e. so that the fraction black at each school would be identical to the fraction black in the district). The denominator is the fraction of students who would have to change schools to completely integrate the district if it began completely segregated.

Both the exposure and dissimilarity indices range from zero to one. The exposure index is positively correlated with integration, while the dissimilarity index is inversely related to integration.

Figure 2 presents the results. To create the figure, the exposure and dissimilarity indices are calculated for each district in the sample in each year. The figure plots the average of each measure over time, where time is measured relative to each district's

[8] Whereas when using the census data districts are defined using individuals' county group of residence, here enrollments are counted within school district boundaries.

[9] The analysis is not substantively altered by treating all non-whites as one minority group or by ignoring non-white, non-black students.

initial date of desegregation. Thus, the figure shows the dynamics of racial integration in the years leading to and following the implementation of desegregation plans.

The top panel tracks the exposure index. In the years leading up to the implementation of desegregation plans, black exposure to whites is fairly constant. There does not seem to be any trend predating the implementation. In the initial year of the desegregation plan, black students experience a sharp increase in exposure to white students, on the order of 15 percentage points. The increase is immediate, though there seems to be additional integration in the second year of desegregation.[10] Importantly, black exposure to whites does not seem to diminish appreciably in the districts under study in the first ten years after the implementation of desegregation plans. There also does not seem to be a notable difference by the decade of desegregation.

As seen in the bottom panel of figure 2, an analysis of the dissimilarity index shows a similar pattern. The dissimilarity index is remarkably constant in the years leading up to desegregation. Districts did not seem to change school assignments in anticipation of desegregation orders or court proceedings. In the first year of desegregation, however, there is a sharp decline in the average dissimilarity index. This decline remains in tact for at least ten years. This pattern is seen in districts that desegregated in the 1960's, 1970's and 1980's.

2.2. The Role of Legal Precedent

Causes of the timing of desegregation are crucial to the identification strategy that will be explained in the following section. Desegregation plans were principally the ultimate result of court proceedings that were initiated by private civil rights groups. A concern that will arise is that these groups focused their energy and resources on school districts where desegregation would provide the greatest local benefit. If this were the case, then the timing of desegregation was at least in part a function of time-varying determinants of student achievement.

This argument overlooks the role of precedent in the U.S. legal process. Legal precedent played a major role in the determination of where to focus the resources of

[10] It is perhaps not surprising that integration occurs so immediately since the definition of the year of implementation is based in part on the plan being effective at integration.

national civil rights groups, which were the primary force behind most legal challenges. Any agent working to benefit students nationally through desegregation must consider the long-standing effects of a legal failure early in the process. Thus, such national civil rights organizations chose to bring suit early in school districts where they felt there was a higher probability of victory, rather than in school districts where desegregation would benefit the largest number of students.

The most significant private organization in the legal process of school desegregation was the National Association for the Advancement of Colored People (NAACP). In 1929 the NAACP received a large pledge of funding from the American Fund for Public Service. The funds were intended to support an organized campaign to improve the legal status of black Americans. The campaign explicitly was to include an effort to eliminate racial inequities in public education. The NAACP, the legal arm of which later became the Legal Defense and Educational Fund (LDF), described its long-term legal strategy, which included a goal of eliminating segregation in education, as

> "… a carefully planned one to secure decisions, rulings and public opinion on the broad principle instead of being devoted merely to miscellaneous cases."[11]

The LDF campaign has since been described as one that

> "…followed a strategic approach that rejected simple accumulation of big cases, in favor of incremental victories that built a favorable legal climate …."[12,13]

For instance, the LDF brought suit for the integration of professional and graduate schools before elementary and high schools because it felt "judges would be more likely to reach favorable decisions in such cases than in cases involving elementary or high

[11] 1934 NAACP Annual Report, reprinted in Greenberg (1959) p. 35.
[12] Council for Public Interest Law (1976), p. 37.
[13] For instance, the LDF brought suit for the integration of professional and graduate schools before elementary and high schools because it felt "judges would be more likely to reach favorable decisions in such cases than in cases involving elementary or high schools" (Finch, 1981). Jack Greenberg, the former director-counsel of the LDF explained, "the marginal consequences of one or two students entering a graduate school were not of sufficient immediate *practical* importance to warrant the effort; only long-term law making was." (Greenberg, 1974).

schools."[14] Jack Greenberg, the former director-counsel of the LDF explained, "the marginal consequences of one or two students entering a graduate school were not of sufficient immediate *practical* importance to warrant the effort; only long-term law making was."[15]

A model presented in the Appendix illustrates why this strategy may have been effective in the long run. The model shows that when precedent has a strong effect on the subsequent probability of success—as was the case with desegregation—an agent acting to desegregate the nation's schools should optimally choose to weight the probability of success almost to the exclusion of any local benefits of desegregation when choosing where to bring legal challenges. The role of precedent suggests that any strategy to identify the effect of desegregation on dropout rates should focus on controlling for the district characteristics that made legal victory more likely and thus led some districts to be desegregated earlier, while others were desegregated later.

3. Identification

Desegregation plans affect black dropout rates through three main channels. First, the reassignment of students within the school district affects the set of peers with which students attend school. New student assignment plans may cause parents to withdraw their children from the public schools, or to move out of the district altogether. Net of effects on the total enrollment and the racial composition of the district, desegregation plans alter the set of peers with which black children attend school.

Second, desegregation plans may move black students to better schools. If whites attended better schools than blacks did before integration, then on average desegregation should improve the quality of schools that blacks attend. Though total support for schools may decline as a result of desegregation-induced migration, integration may still lead to a change in the average quality of schools to which black students are assigned.

Third, there may be other effects of desegregation plans on black educational outcomes. Parents may become more involved in their children's education as a result of increased information, or in order to reap the benefits of the fight they have recently won.

[14] Finch, 1981.
[15] Greenberg, 1974.

The legal victory that usually accompanies a desegregation plan may also make black children feel enfranchised.

The data necessary to separately identify these three effects are unavailable. Measures of parental involvement in students' education, of educational inputs at the district, school and classroom level, and of peer characteristics and actions would be needed. In the absence of these data, I measure the net effect of desegregation.

The estimation of the net effect of desegregation plans on black educational outcomes is not completely straightforward, however. A comparison of integrated and segregated school systems at any point in time confounds the effect of the desegregation plans with the effect of factors that led to the imposition of the plan.

Ideally, one would compile annual data on black and white dropout rates in large urban school districts. A compelling research design would match changes in these outcomes to the exact timing of integration. Comparisons of dropout rate trends in the years leading up to desegregation would be compared to rule out competing hypotheses. Unfortunately, the data necessary for such an event-study research design are not available. Annual data sets are generally too small to precisely estimate year-to-year changes in dropout rates.[16] Further, administrative data typically do not report dropout rates separately by race over the relevant time period.[17]

In lieu of annual data, I use census data from 1970 and 1980 to measure changes in black and white high school dropout rates. The analysis focuses on a sample of large school districts, 86 percent of which implemented desegregation plans between 1961 and 1982. I compare changes in black and white dropout rates during the 1970's in districts that integrated during the decade to changes in the same outcomes in districts that integrated both earlier and later.

In its simplest form the identification strategy is to use a difference-in-differences estimator. An example should help to clarify. Consider two school districts: Birmingham, Alabama, which desegregated in 1970, and St. Louis, Missouri, which

[16] The October Current Population Survey (CPS) has an education supplement but the smallest geographic identifier is the MSA. While many of the school districts in the sample can be matched by MSA, there are too few black 15-17 year olds in the CPS to precisely estimate annual dropout rates.

[17] Further, to account for endogenous migration, which will be discussed in section 5, one would have to collect data from surrounding school districts in addition to that from the districts that implemented desegregation plans.

desegregated in 1980. High-school-aged blacks are compared in the 1970 and 1980 censuses. In 1970, 17-year-olds in both cities had attended segregated schools all their lives. In 1980, 17-year olds in Birmingham had attended integrated schools since 2nd grade, while those in St. Louis had attended segregated schools throughout their education. The experiences in St. Louis are used to represent what would have happened in Birmingham in the absence of desegregation. A comparison of the change in the dropout rate in Birmingham relative to the change in the dropout rate in St. Louis is an estimate of the effect of desegregation.

In the full analysis, the treatment group comprises districts that implemented desegregation plans between 1970 and 1979 (70's desegregators). In these districts, high school students in April of 1980 attended at least one year of school after the implementation of a desegregation plan, while high school students in April of 1970 attended segregated schools throughout their education.[18] The change in dropout rates in these districts is compared to that in districts that desegregated before 1970 (60's desegregators) and after 1979 (80's desegregators). In the control districts, no desegregation plan was implemented between the time the 1970 and 1980 censuses were conducted.

The simple difference-in-differences (DID) estimator can be written

$$\begin{aligned}(3)\quad \hat{\delta} = &\left\{E\left[D_{idt}\,\middle|\, d = 1970's, t = 1980\right] - E\left[D_{idt}\,\middle|\, d = 1970's, t = 1970\right]\right\} \\ &-\left\{E\left[D_{idt}\,\middle|\, d \in (1960's, 1980's), t = 1980\right] - E\left[D_{idt}\,\middle|\, d \in (1960's, 1980's), t = 1970\right]\right\}\end{aligned}$$

where D is an indicator that equals one if a high-school-aged individual is not enrolled in school, i indexes individuals, $d \in (1960's, 1970's, 1980's)$ indexes the decade in which the individual's school system implemented a desegregation plan, and $t \in (1970, 1980)$ indexes time.

The parameter of interest can be estimated in a regression framework by regressing D on a constant, an indicator that equals one if $t = 1980$, an indicator that equals one if $d = 1970's$, and an interaction of those two indicators. The estimated

[18] Census data refer to April 1st of the census year.

coefficient on the interaction is an estimate of the effect of desegregation on dropout rates. This specification is presented in column (1) of each table of results. A specification that adds regression controls is presented in column (2).

A more general specification allows each district a different time-invariant dropout rate. This fixed-effect specification can be written

$$(4) \qquad D_{ist} = \beta_t + \gamma_s + \delta T_{dt} + \pi_t X_{ist} + \varepsilon_{ist}$$

where *s* indexes school districts, *X* is a vector of individual and district characteristics, π_t is vector of coefficients that are allowed to vary by *t*, and ε is a random error term. The regressor of interest is T_{dt}, which is equal to one for observations in 1980 in districts that desegregated in the 1970's. In other words, it is the interaction term in the DID regression model. The fixed-effect model is estimated by running a regression of the dropout indicator on a set of district dummies, an indicator that equals one if $t = 1980$, the T_{dt} interaction, and time-varying individual and district controls. Versions of this specification are reported in columns (3) – (5) throughout the results section. The specification in column (3) restricts π_t to be equal across decades. The specification in column (4) allows π_t to vary across decades. And, the specification in column (5) adds state-year interactions to allow state-specific trends in dropout rates.

The model is identified under the assumption that T_{dt}, X_{ist}, β_t, and γ_s are independent of ε_{ist}. In other words, the identifying assumptions are that the decade of desegregation is not correlated with trends in dropout rates and that the error term is drawn from the same distribution over time for students in integrated and segregated schools. To test this assumption ideally one would compare trends in dropout rates in the years preceding desegregation. It is not possible to examine these trends because dropout rate data by race are not available prior to 1970.

As an imperfect substitute, I present trends in other characteristics of districts, by the decade of desegregation. Data are collected from the City and County Data Book, which computes aggregate statistics at the county level using data from the census and

other sources. These county-level aggregates are then weighted by population and combined to form aggregates at the school district level.[19] Figure 3 shows trends in the means of three characteristics: real median income, the fraction of employment in manufacturing, and the fraction of votes cast for the winning party in presidential elections. Trends are plotted separately for districts that desegregated during the 1960's, 1970's and 1980's.

Real median income rose in all three groups of districts. Median income is slightly lower in districts that desegregated during the 1970's, but income growth is remarkably similar in all three groups. Districts that desegregated during the 1970's had a slightly smaller share of employment in manufacturing jobs than districts that desegregated during the 1980's, but a slightly larger share than districts that desegregated during the 1960's. The growth rate of manufacturing jobs was larger in 70's desegregators during the 1950's, but this growth rate was close to zero in all three districts during the 1960's. As seen in the bottom panel of the figure, the voting patterns of 60's and 70's desegregators were fairly similar between 1960 and 1972. Districts that desegregated in the 1980's were slightly more likely to vote Democrat in 1960, though they were less likely to do so in 1964.

Importantly, the trends in each of these characteristics are fairly similar from 1960 to 1970. This evidence does not, of course, rule out violations of the identifying assumption. The validity of this assumption will be discussed and tested further in subsequent sections.

4. Data

In a report commissioned by the U.S. Commission on Civil Rights, Welch and Light (1987) evaluated the effect of desegregation plans on integration. They sampled 125 school districts for this purpose. This sample represents less than one percent of U.S. school districts, but about 20 percent of total enrollment and about half of minority enrollment in 1968.[20] The analysis to follow focuses on this sample of very large school

[19] The method by which counties are matched to school districts is described in detail in the next section. The aggregates computed in this section are for the consolidated county groups that are used in the main analysis. The boundaries of these consolidated county groups lie outside the boundaries of school districts.
[20] Welch and Light (1987), p. 34.

districts. Table 1 lists the districts included in the study and when they implemented a desegregation plan. The rightmost column shows whether the district is assigned to the treatment or control group (i.e. whether or not the district integrated during the 1970's).

The empirical analysis measures changes in dropout rates using data from the 1970 and 1980 censuses. Geographic identifiers available in the 1970 and 1980 public use census data are not detailed enough to match individuals to school district boundaries. In lieu of more detailed information, I match 15-, 16- and 17-year-olds from the 1970 and 1980 censuses to the 125 districts based on individuals' county group[21] or SMSA of residence to form consolidated county groups. Individuals are matched to school district characteristics, such as the date of desegregation, based on residence in these consolidated county groups.[22] Note that the boundaries of consolidated county groups lie outside the boundaries of school districts. In the average school district, enrollment is 63.5 percent as large as the population of 15-17 year-olds in the corresponding consolidated county group.[23,24] In cases where county group boundaries are different in the 1970 and 1980 censuses, care is taken to make sure the boundaries of consolidated county groups are the same in both years.[25] For ease of exposition, for the remainder of the analysis I refer to these consolidated county groups as districts.

5. Basic Results

Summary statistics for black 15- 16-, and 17-year olds are presented in table 2. The table breaks out means of selected variables by both the decade of desegregation and

[21] County group is the smallest geographic identifier available on public use census files in 1970 and 1980.
[22] In the census, prisoners are coded as residents of the prison's county. Thus, the analysis will not track individuals who are sent to prisons outside their school district's consolidated county group. To the extent that trends in prison rates are different in treatment and control districts, this may cause a bias in the estimates.
[23] As described in section 5, one benefit of defining districts in this way is that the analysis is less sensitive to endogenous migration. One might be concerned that desegregation causes families to leave school districts. Endogenous migration of this sort would lead to changes in dropout rates at the school district level. If these families were to move within consolidated county groups, however, dropout rates measured at this level would not change in response to their migration.
[24] Because consolidated county groups are larger in size than school districts, one might argue that school district dropout rates are measured with error throughout the analysis. This mismeasurement surely leads to an underestimate of the effect of desegregation on dropout rates. Lacking a suitable way to handle this measurement error problem, while simultaneously accounting for endogenous migration, the analysis ignores the problem.
[25] Specifically, consolidated county groups are defined as the smallest geographic area that both subsumes the school district area and is identifiable in the 1970 and 1980 data.

year. The first two columns show the means in 1970 and 1980 for individuals in consolidated county groups in which the school district desegregated during the 1960's. The next four columns show means in each year for individuals residing in 70's and 80's desegregators, respectively.

The first row of the table previews the basic result of the paper. In 1970, the black high school dropout rate (defined as the fraction of black 15-, 16-, and 17-year olds not enrolled in school) was higher in districts that desegregated in the 70's than in both 60's and 80's desegregating districts. From 1970 to 1980 the dropout rate declined in districts that desegregated during the decade from 14.3 percent to 10.5 percent. Over the same period, the dropout rate remained constant in districts that did not initiate desegregation plans during the decade. The remainder of the empirical analysis centers on establishing that this decline in black high school dropout rates in 70's desegregating districts is attributable to integration.

It is instructive to compare the characteristics of high-school-aged blacks in districts according to their decade of desegregation. In 1970, the characteristics of black youths and their families were fairly similar in districts that integrated in the 1960's and 1970's. Mother's education was almost identical, though father's education was almost a half-year higher in 70's desegregators. Family size was almost identical in the three groups of districts, though the decline of 1.1 during the decade in 80's desegregators was larger than in 70's desegregators by 0.4. Districts that would desegregate after 1980 had the highest family income of the three groups in 1970 ($17,891). Family income in 70's desegregating districts was lower ($16,714), and family income in 60's desegregating districts was the lowest ($14,646) of the three groups. However, real family income actually fell during the decade in 80's desegregators by 2.1 percent. In contrast, real incomes rose in 60's and 70's desegregating districts by 4.6 and 2.6 percent, respectively.

5.1 Basic Results for Blacks

While the three groups of districts seem fairly similar, this examination of mean characteristics points out that regression controls could be important to the interpretation of the decline in dropout rates. Regression-adjusted estimates of the relative change in dropout rates in 70's desegregators during the 1970's are shown in table 3. The top panel

of the table shows estimates for the sample of black 15-, 16- and 17-year olds using 1970 and 1980 census data. The dependent variable is an indicator for whether the individual is not enrolled in school (i.e. whether he is a high-school dropout).

The first column contains the estimates of the basic difference-in-differences (DID) specification. The dropout indicator is regressed against an indicator for individuals residing in 70's desegregating districts, an indicator for observations from the 1980 census, an interaction of those two variables, and a constant. The coefficient on the interaction term, shown in the first row of the table, is an estimate of the change in dropout rates during the 1970's in 70's desegregating districts relative to 60's and 80's desegregating districts. The coefficient on the 1980 indicator is an estimate of the change in dropout rates in the comparison districts, and the coefficient on the 70's desegregator variable is an estimate of the difference in dropout rates between the two groups in 1970. The results imply that dropout rates remained unchanged in districts that did not desegregate during the decade. In contrast, dropout rates in districts that desegregated during the 1970's declined by 3.8 percentage points (standard error of 1.1 percent).

The specification in the second column of table 3 adds individual, family, and local area demographic controls. These controls include age and gender dummy variables, region dummies, family income, mother's and father's age and education, family size, the percent of employment in manufacturing in the district, and median income in the district. The estimated effect of desegregation on black high-school dropout rates declines slightly to 2.8 percentage points, but is still strongly significant. The specification in column 3 includes consolidated county group fixed effects, which subsume the 70's desegregator indicator. These fixed effects control for any permanent characteristic of districts or county groups. The estimated effect of desegregation is unchanged, and the estimates are significantly more precise. The specification in the fourth column allows the effects of the controls added in column 2 to vary by decade. The estimated effect of desegregation is a 3.0 percentage point decline and still significant. The specification in column 5 adds state-specific decade effects. The estimated effect of desegregation is slightly smaller (a 2.6 percentage point decline), though still significant.

It is worth noting that there is enough within-state variation in the timing of desegregation to allow separate controls for state-specific year effects. This result rules out that the identifying variation comes from region specific trends during the 1970's. Interestingly, the effect of desegregation is almost identical when estimated separately inside and outside the South.

5.2 Basic Results for Whites

The bottom panel of table 3 shows estimates for the same five specifications for white 15-, 16-, and 17-year olds. The results are strikingly different. The simple DID specification shows a 0.5 percentage point increase in white dropout rates in desegregating districts during the decade. The increase is not significant. Indeed, the four specifications that include controls yield smaller point estimates that center near zero. There does not seem to be any evidence of a change in white high school dropout rates in desegregating districts during the 1970's. The standard errors in the fixed effects estimates are small enough to rule out a decrease of 0.7 percentage points and an increase of 1 percentage point.[26] The contrast between the white and black results is noteworthy. The lack of a decrease in white dropout rates rules out that the black decline is driven by secular trends in dropout rates in the districts that integrated during the 1970's.

Taken together, the results for blacks and whites strongly suggest that desegregation plans led to declines in black high school dropout rates. All of the decline in black high school dropout rates between 1970 and 1980 occurred in districts that desegregated during the decade. The estimated effect is fairly robust to regression controls. Further, the lack of a similar decline in white dropout rates suggests that the black results are not driven by secular trends in dropout rates in the districts that integrated during the 1970's.

5.3 Estimating How Effects Vary with the Characteristics of Plans

It is natural to ask whether the characteristics of desegregation plans influence their effect on student outcomes. For instance, if integration were driving the

[26] Such small increases in white dropout rates, however, would indicate that that aggregate declines in high school dropout rates was not as large as implied by the results for blacks.

improvement in black outcomes, one might expect plans that were more effective at integrating students to have a more beneficial effect on black high school dropout rates. Estimates examining this question are presented in table 4. The top panel presents estimates of the effect of desegregation plans on black high school dropout rates that allow the effect to vary by the change in the dissimilarity index in the district during the decade. As described in section 2, the dissimilarity index measures the extent of racial segregation within schools in the district.[27] The specifications in columns 1-5 correspond to the specifications presented in table 3.

The first row of the table shows the estimated effect of desegregation plans on black high school dropout rates in districts with the sample average change in the dissimilarity index. The second row shows the marginal effect of a change in the dissimilarity index on this estimated change in dropout rates. The results suggest that black high school dropout rates declined more in districts with larger declines in the dissimilarity index—districts that were more effectively integrated under desegregation plans. Four of five specifications yield statistically significant estimates. The median estimate (.084) implies that the mean decline in the dissimilarity index (.164) corresponds to an additional 1.4 percentage point decline in black high school dropout rates.

The middle panel of table 4 shows similar results using the exposure index in place of the dissimilarity index. The exposure index measures the fraction white at the typical black student's school. The results are less precise than those using the dissimilarity index. However, the negative point estimates in four of the five interaction terms suggest that black high school dropout rates declined more in districts with larger increases in the exposure index. In other words, in districts that increased within-school integration, black dropout rates declined more markedly. The exposure index results are consistent with the dissimilarity index results, but the standard errors are too large to rule out a lack of an interaction effect.

Similarly, it is interesting to compare plans that reassigned students to schools with and without allowing parents or students choice. Voluntary plans—those that allowed parents and students some choice—typically either included the establishment of

[27] Whereas the individual data is linked to districts based on county groups of residence, the dissimilarity and exposure indices are measured using data at the school district level.

magnet schools, which were designed to attract students of both races, or involved the lifting of restrictions on enrollment at schools based on race. Non-voluntary plans typically involved redrawing of attendance zones or busing.[28] In the course of school desegregation cases, a number of defendants argued that the lifting of explicit school enrollment restrictions was sufficient to ensure equal protection of the law for black children. Plaintiffs countered that only affirmative steps to undo such *de jure* segregation could lead to *de facto* integration and thus to positive results for black children. In response, some argued that while non-voluntary plans might be more effective at integrating the schools, they would be disruptive and impose great costs on students. It is instructive to compare the effect of these two types of desegregation plans on black educational outcomes.

The results are presented in the bottom panel of table 4. The two rows of the panel show the estimated decline in 70's desegregators with non-voluntary and voluntary plans, respectively, relative to comparison districts. While the estimated effects are not statistically different, the point estimates suggest that non-voluntary plans were associated with markedly larger declines in black high school dropout rates. The estimated declines in non-voluntary plans are large and significant, ranging from 2.9 to 5.6 percentage points. In comparison, the estimated declines in districts with voluntary plans are insignificantly different from zero. The point estimates are all negative, ranging from 1.3 to 2.5 percentage points. The difference between the effects of the two types of plans is statistically significant in the basic specification in column (1), but insignificant in the remaining columns.

5.4 Length of exposure

The interaction results in table 4 shed some light on the mechanism by which desegregation led to improvements in black high school dropout rates. It is also natural to ask whether the length of exposure to integrated schools affects student outcomes. The

[28] Welch and Light (1987) placed all desegregation plans into any of six categories: "freedom of choice", "magnets", "other voluntary transfers", "neighborhood attendance zones", "rezoning", and "pairing and clustering". The former three are voluntary techniques, while the latter three are non-voluntary techniques. Plans often used more than one of these techniques. I assign plans to voluntary or involuntary based on what Welch and Light (1987) deemed the plan's "major components". In the few cases where a plan had both a voluntary and an involuntary major component the plan is called voluntary.

specifications in tables 3 and 4 compare all districts that desegregated in the 1970's to comparison districts, regardless of when they integrated during the decade. By 1980, districts that desegregated in 1971 had been integrated for nine years, while those that desegregated in 1978 had only been integrated for two years. Students in those districts had been exposed to integration for different lengths of time. I estimate a specification that allows the effect of integration to vary by the year of desegregation. The estimated declines in black high school dropout rates, along with 95-percent confidence interval bands, are plotted against the year of desegregation in figure 4. The loss of degrees of freedom leads to a loss of precision in the annual estimates. The estimates are fairly similar, however, for districts that integrated between 1970 and 1976. Declines are larger in districts that integrated in 1977, 1978 and 1979.

There are a few things worth noting in the figure. First, even with the loss of precision the estimates are precise enough to rule out a zero effect in six of the ten individual years of desegregation. It is clear that more precision could be gained by placing some structure on the relationship between exposure and dropout rates. Second, there does not seem to be any evidence that the length of exposure to integration has a compounding effect on dropout rates. In other words, there is no evidence that exposure to integration in 3rd grade has an effect on high school dropout rates conditional on attending an integrated high school. This result supports the choice of the fixed-effects specification. And third, the immediate effect of desegregation appears to be larger than the long-run effect. One story that is consistent with these results is that there is a direct causal effect of the court order and the corresponding legal victory on black high-school dropout rates. That effect lasts only one or two years, and the changes in school quality and peers resulting from integration lead to about a 2 percentage point decline in black high school dropout rates for at least ten years.

6. Robustness

The results presented in section 5 strongly suggest that the desegregation plans of the 1970's led to significant declines in black high-school dropout rates during the decade. All of the decline in black high-school dropout rates seen in the aggregate data occurred in the districts that integrated during the decade. The estimated decline is robust

to controls for individual and family demographics, and to region- and state-specific trends. Further, the estimated decline is predictably larger in districts where integration was most pronounced. In this section I present additional checks of the robustness of these estimates.

6.1. Migration

Large-scale integration may have induced migration of blacks and whites. Assignment of observations to districts in the analysis thus far is based on residence on April 1st of the current year. Validity of the estimates presumes that migration into and out of districts is not affected by desegregation plans. At the very least the analysis assumes that desegregation-induced migrants do not have different dropout propensities from the rest of the population. While much has been written on desegregation-induced white migration—often termed "white flight"—little has been discussed about the parallel phenomenon for black families.

One might still worry that families of potential dropouts avoided desegregating areas when they moved, or that families of good students sought desegregating districts. The former concern is likely to be more relevant as movers tended to have higher dropout rates.[29]

The data allow three empirical checks of whether selective migration induces bias in the estimates. Public-use census data in both 1970 and 1980 include information on whether individuals have moved during the previous five years. In the top panel of table 5, estimates are presented from the basic specifications, but with a discrete dependent variable that indicates whether the individual has moved from the county group where he lived five years ago. The first row shows estimates on the entire sample, while the second row shows estimates only on the sample of districts that integrated within five years before a census (i.e. 1965-1969 and 1975-1979). These results do not suggest that desegregation had any effect on migration. The results using the full sample are all very close to zero. The estimates using the five-year sample are larger in magnitude and are

[29] The dropout rate in the sample among those who had moved from their county group of residence from five years ago is 17.6 percent, while that of non-movers is 11.5 percent.

significant in some cases. However, three of the point estimates are negative, while two are positive.

The bottom panel of table 5 presents estimates from models that again use dropout status as the dependent variable, but control for whether the individual moved from his county group of five years ago. Estimates of the effect of desegregation are negative and significant. The estimates in columns 2 and 3 are slightly smaller in magnitude than those from the basic specifications in table 3. However, the estimate in column 4 is larger in magnitude. There does not seem to be evidence that migration affects the results in a systematic way.

A further check is to assign individuals to consolidated county groups using their county group of residence in 1975 rather than in 1980.[30] Estimates of the basic specifications using these district assignments are presented in the second row of the bottom panel of table 5. Estimates of the effect of desegregation on black high school dropout rates are larger but more imprecise due to smaller sample sizes. Differential migration patterns into desegregating districts in the period from 1975 to 1980 do not seem to explain the estimated effects from the standard specifications in Table 3.

The questions available on the public use census files address migration into, as opposed to migration out of districts. Welch and Light (1987) and Rossell and Armor (1996) show that the implementation of desegregation plans led to an increase in the speed of migration of whites out of urban school districts. Reber (2002) shows similar evidence and suggests that white migration eroded about one-third of the integration caused by court orders after ten years. If there is a similar phenomenon among blacks, the previous estimates are compromised. In fact, the problem is not as serious as it may seem because the districts referred to in this analysis are larger in physical area than actual school districts. In practice, consolidated county groups include many of the suburban areas to which migrants may have fled. Moreover, a DID estimate shows no indication that desegregation plans led to a decrease in the population of high-school-aged blacks in districts.[31] Together with the estimates presented in table 5, this suggests

[30] County group of residence five years ago is not available in the 1970 census. It is only available in the 1980 census for the half of the sample that is asked additional migration questions.
[31] See Guryan (2001).

that desegregation had little effect on migration in or out of the districts as defined in this study.

6.2. The Timing of Desegregation

As discussed briefly above, the validity of the difference-in-differences estimates rests on the assumption that, in the absence of desegregation, trends in black dropout rates would not have differed systematically with the decade of integration.[32] This section presents several tests of this assumption. Some evidence already presented suggests the assumption is reasonable. First, controlling for time-varying covariates that drive variation in dropout rates does not substantially affect the basic estimates. Second, trends in white dropout rates are identical among 60's, 70's and 80's desegregators. In fact, there is no trend in white dropout rates in the 1970's. The decline in dropout rates is a uniquely black phenomenon. Further, the entire decline in black dropout rates occurs in the districts that desegregated during the decade.

One might still be concerned that there is a monotonic relationship between the decade of desegregation and trends in black high school dropout rates. For instance, districts with the greatest potential for improvements in black outcomes may have been integrated first. One might argue that improvements in black outcomes would have occurred earlier in these districts even in the absence of desegregation. If we compared the decline in black high school dropout rates in 70's desegregators to that in 80's desegregators we would overestimate the effect of desegregation. Alternatively, if we compared the decline in black high school dropout rates in 70's desegregators to that in 60's desegregators we would underestimate the effect of desegregation.

The estimates presented in table 6 suggest bias of this sort is not important. The top panel of the table shows the five basic specifications including only 60's desegregators as comparison districts (i.e. observations in 80's desegregating districts are dropped). The bottom panel shows estimates from specifications including only 80's

[32] There are other restrictions imposed on the error term by the difference-in-differences specification. Specifically, the error term is assumed to be drawn from the same distribution for treatment and control districts and over time. A common alternative is the switching regression in which the error is drawn from a different distribution depending on which of two regimes (in this case decade of desegregation) is in place. See Roy (1951), Goldfeld and Quandt (1973), Heckman (1979), and Lee (1979) for explanation of the switching regression model.

desegregators as controls. In contrast with the predicted direction of the bias, the estimates are slightly larger when the comparison districts are 60's desegregators than when the comparison districts are 80's desegregators. The estimates are not, however, appreciably different. Taken together with the other evidence, the results in the table are consistent with the identifying assumption that the decade of desegregation is otherwise unrelated to trends in black high school dropout rates.

6.3. Instrumental Variables Estimates

Instrumental Variables (IV) estimates relax the identifying assumptions further. Estimates presented in table 7 use two instruments for the decade of desegregation: (1) the decade of the first legal opinion filed in a case concerning the desegregation of the district, and (2) the decade of the first integration plan, including minor plans that were subsequently deemed inadequate by the courts. The motivation for each instrument is that opposition groups can delay desegregation by lengthening the court proceeding or by implementing inadequate desegregation plans. Consequently, the decade of desegregation might be correlated with trends in black dropout rates independent of the effect of desegregation plans. As a result, the correlation of T_{dt} and ε_{ist} would be non-zero and ordinary least squares estimates would be inconsistent.

IV yields consistent estimates of δ, the causal effect of desegregation on dropout rates, if $E\left[Z_{dt} T_{dt} \middle| X_{ist} \right] \neq 0$ and $E\left[Z_{dt} \varepsilon_{idt} \middle| X_{ist} \right] = 0$. In other words, the instruments must be correlated with the endogenous regressor (i.e. the decade of desegregation), and must not be correlated with black dropout rates other than through their effect on the decade of desegregation. The former assumption is confirmed by the first-stage estimates reported in table 7. The decade of desegregation is strongly related to both the decade of the initial court decision and the decade of the initial attempted desegregation plan. These two instruments are particularly effective if the source of the correlation between T_{dt} and ε_{ist} is that groups opposed to integration are more effective at delay in districts where dropout rates are declining. The instruments are pre-determined relative to this delay. The instruments do not, however, generally address bias that could result from arbitrary correlation between ε_{ist} and T_{dt}.

The IV estimates are presented in the top panel of table 7. They are larger in magnitude than the OLS estimates presented in table 3. Using either instrument, IV estimates are two to four times larger in magnitude than OLS. However standard error estimates are also much larger in IV specifications. If endogenous delay were driving the OLS estimates, we would expect to see IV estimates that were smaller in magnitude. Since we see the opposite, it does not seem that endogenous delay can explain the decline in dropout rates uncovered by the OLS estimates.

7. Conclusion

Despite desegregation's prominent role in post-World War II education policy, few economists have studied its impact on the educational outcomes of the affected students. The analysis in this paper exploits variation in the timing of desegregation plans to identify the effect of these plans on the high school dropout rates of blacks. Specifically, the change in black dropout rates from 1970 to 1980 in districts that desegregated in the interim is compared to the change in black dropout rates in districts that desegregated in the 1960's and the early 1980's. Using data from the 1970 and 1980 censuses, estimates control for individual and family demographics, local area characteristics, state- and region-specific trends, and for endogenous migration.

The choice of a fixed-effect econometric specification is largely dependent on the mechanism that determined the timing of desegregation. Most desegregation plans came as a result of court orders in legal proceedings brought by private civil rights groups. A simple model of legal precedent, along with the early writings of NAACP leaders, point out that these national organizations did not just consider the local benefits that would accrue to students when choosing where to bring a legal challenge. In addition, civil rights organizations paid close attention to the likelihood that a legal challenge would result in victory. They tried hard to avoid cases that were likely to result in legal failure early on, even if the potential local benefit was large. Thus, the timing of desegregation was a function first, of permanent district characteristics—signals of the likelihood of a legal victory—and second, of recent black dropout rates—a signal of the potential local benefit of desegregation.

The results suggest that desegregation plans led to a two to three percentage point decline in the dropout rates of blacks, and that desegregation had little or no effect on the dropout rates of whites. The results are largest in districts that experienced the largest declines in racial segregation. Instrumental variables estimates based on the timing of initial legal proceedings and early integration efforts produce larger point estimates, which are less precise.

While a two to three percentage point decline can explain up to half of the decline in black dropout rates among the cohort that attended high school in the 1970's, the evaluation of the importance of desegregation for black economic development depends importantly on its long-term impacts. One must ask whether desegregation plans caused permanent improvements in black educational outcomes, or whether the resegregation described by Clotfelter (2001) and Reber (2002) led to a dissipation of the effects shown in this paper. The evaluation of desegregation's importance also depends on whether its effect was limited to those on the margin of dropping out of high school, or whether it improved outcomes relevant for all black students, such as lifetime wages. These questions are left for future research.

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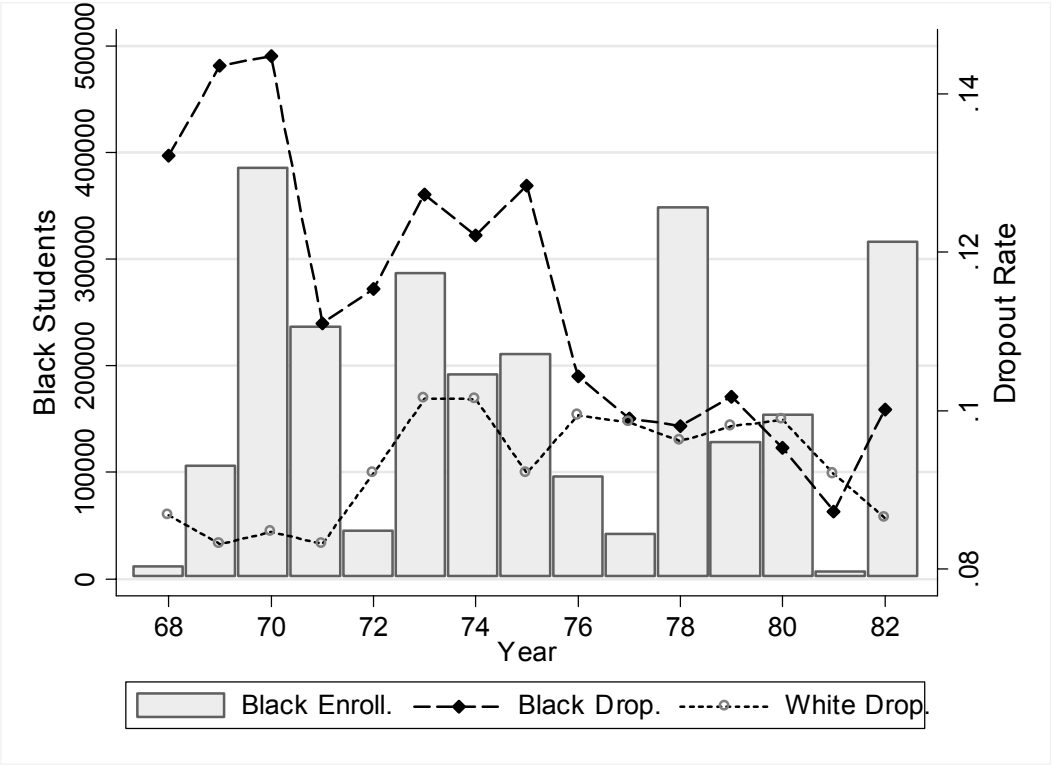
Figure 1: Aggregate Trends in Black and White Dropout Rates
and the Number of Black Students Newly Affected by Desegregation



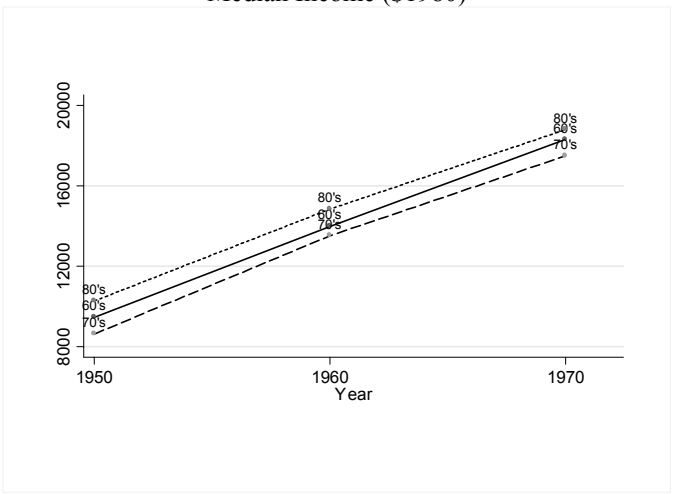
Note: The bars in the figure show the number of black students in districts that implemented a desegregation plan in each year between 1968 and 1982. Aggregate annual dropout rates for black and white 15-, 16, and 17, year olds are plotted. National dropout rates are computed in each year from the October CPS.

Figure 2: The Effect of Desegregation Plans on Racial Segregation

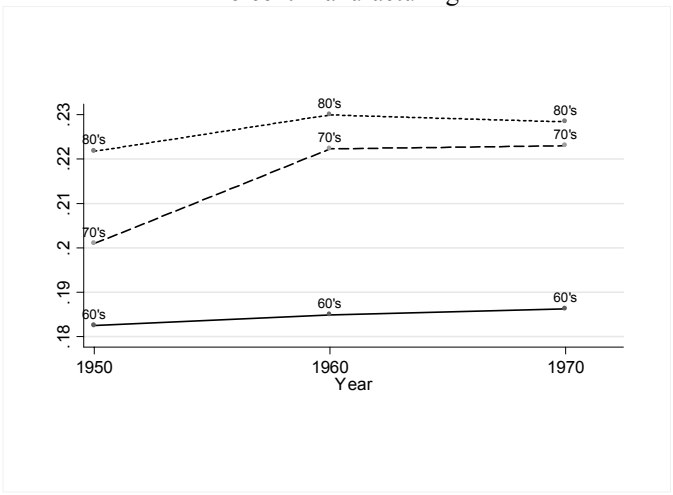
Note: The figure plots the average Exposure Index (black exposure to whites) and Dissimilarity Index over time, where time is measured in years relative to the implementation date of desegregation plans. Series are plotted separately for districts that integrated between 1970 and 1979 (70's desegregators) and for those that desegregated either from 1961 to 1969 or from 1980 to 1982 (60's and 80's desegregators).

Figure 3: Trends in Various Characteristics by the Decade of Desegregation

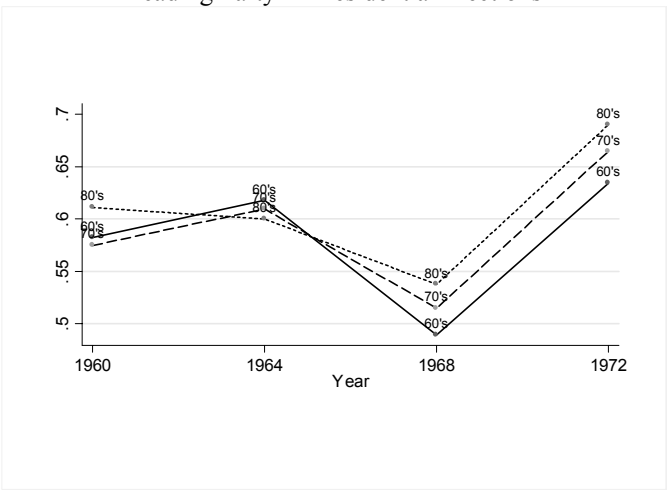
Median Income ($1980)



Percent Manufacturing

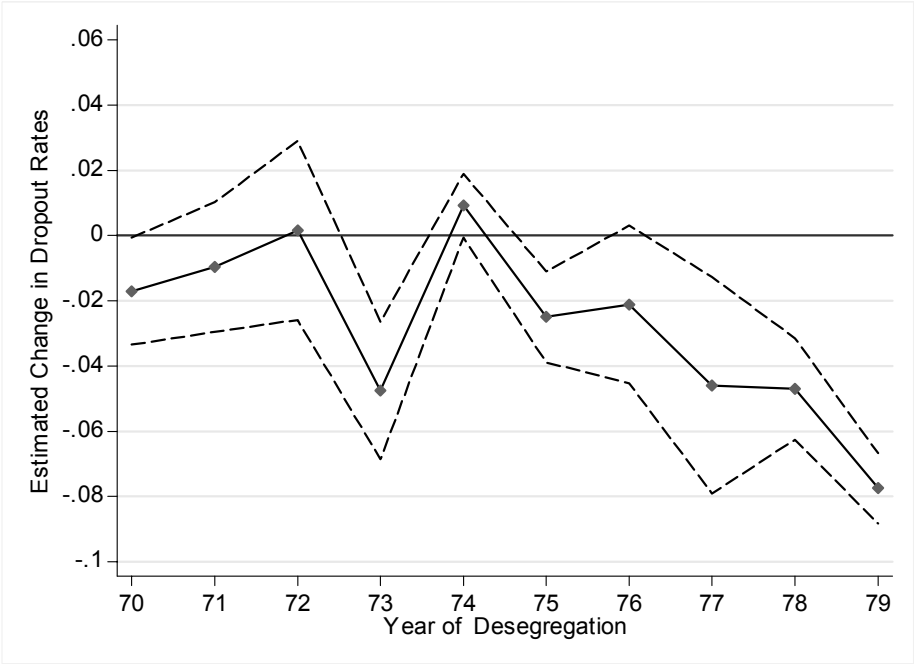


Fraction Voting for the
Leading Party in Presidential Elections



Note: The figures show trends in means of district-level characteristics over time separately by the decade of desegregation. Series are labeled by the decade in which districts desegregated. Data are drawn from the City and County Data Book, which computes county-level aggregates from census data and other sources. These county-level data are then weighted by population and then further aggregated to the consolidated county groups that form the school districts for this study.

Figure 4: Estimates of the Effect of Desegregation on Black Dropout Rates
by The Year of Desegregation



Note: The figure plots coefficient estimates on year of desegregation dummies. The dependent variable is an indicator for dropout status. The regression is estimated black for 15-, 16-, and 17-year-olds in the sample districts. The regression controls for year dummies, district fixed effects, age indicators, parental education and age, family size, region dummies, and family income. 95% confidence intervals are plotted with dashed lines. Standard errors are corrected for heteroskedasticity and for district*year correlation.

Table 1: List of School Districts in the Sample

School District		Year of Desegregation	Grade of Desegregation 1970	Grade of Desegregation 1980	Treatment or Control
NEW ORLEANS PARISH	LA	1961	Elem	Before	Control
NEWARK	NJ	1961	Elem	Before	Control
HARFORD COUNTY	MD	1965	JHS	Before	Control
OAKLAND	CA	1966	HS	Before	Control
HARTFORD	CT	1966	HS	Before	Control
GRAND RAPIDS	MI	1968	HS	Elem	Control
TACOMA	WA	1968	HS	Elem	Control
RICHMOND	CA	1969	HS	Elem	Control
BREVARD COUNTY	FL	1969	HS	Elem	Control
LEE COUNTY	FL	1969	HS	Elem	Control
PINELLAS COUNTY	FL	1969	HS	Elem	Control
POLK COUNTY	FL	1969	HS	Elem	Control
VOLUSIA COUNTY	FL	1969	HS	Elem	Control
CADDO PARISH	LA	1969	HS	Elem	Control
CALCASIEU PARISH	LA	1969	HS	Elem	Control
RAPIDES PARISH	LA	1969	HS	Elem	Control
TERREBONNE PARISH	LA	1969	HS	Elem	Control
CUMBERLAND COUNTY	NC	1969	HS	Elem	Control
NEW HANOVER COUNTY	NC	1969	HS	Elem	Control
SAN ANTONIO	TX	1969	HS	Elem	Control
PITTSYLVANIA COUNTY	VA	1969	HS	Elem	Control
BIRMINGHAM	AL	1970	After	Elem	Treatment
PASADENA	CA	1970	After	Elem	Treatment
STAMFORD	CT	1970	After	Elem	Treatment
BROWARD COUNTY	FL	1970	After	Elem	Treatment
DADE COUNTY	FL	1970	After	Elem	Treatment
PALM BEACH COUNTY	FL	1970	After	Elem	Treatment
EAST BATON ROUGE PARISH	LA	1970	After	Elem	Treatment
ROCHESTER	NY	1970	After	Elem	Treatment
GASTON COUNTY	NC	1970	After	Elem	Treatment
MECKLENBURG COUNTY	NC	1970	After	Elem	Treatment
CHARLESTON COUNTY	SC	1970	After	Elem	Treatment
GREENVILLE COUNTY	SC	1970	After	Elem	Treatment
RICHLAND COUNTY	SC	1970	After	Elem	Treatment
HOUSTON	TX	1970	After	Elem	Treatment
NORFOLK	VA	1970	After	Elem	Treatment
ROANOKE	VA	1970	After	Elem	Treatment
JEFFERSON COUNTY	AL	1971	After	Elem	Treatment
MOBILE	AL	1971	After	Elem	Treatment
LITTLE ROCK	AK	1971	After	Elem	Treatment
SAN FRANCISCO	CA	1971	After	Elem	Treatment

Note: Table lists school districts in the Welch and Light (1987) study, which are also the districts used in this study. Districts are chosen based on the following criteria, as described in Welch and Light (1987). Every district with 50,000 or more students in 1968 and 20 to 90 percent minority representation are included. Districts with 15,000 or more students in 1968 and ten to 90 percent minority representation were chosen with sampling probabilities proportional to their size and regional representation. The remaining districts—those with fewer than 15,000 students in 1968, less than ten percent minority representation—were excluded from the sample. Grade of Desegregation columns identify the grade a 17-year-old in 1970 (1980) was in when the school district desegregated. Year of Desegregation column reports the year the district's major desegregation plan was implemented according to Welch and Light (1987).

Table 1 (cont.)

School District		Year of Desegregation	Grade of Desegregation 1970	Grade of Desegregation 1980	Treatment or Control
DUVAL COUNTY	FL	1971	After	Elem	Treatment
HILLSBOROUGH COUNTY	FL	1971	After	Elem	Treatment
MUSCOGEE COUNTY	GA	1971	After	Elem	Treatment
FORT WAYNE	IN	1971	After	Elem	Treatment
WICHITA	KS	1971	After	Elem	Treatment
JEFFERSON PARISH	LA	1971	After	Elem	Treatment
TULSA	OK	1971	After	Elem	Treatment
NASHVILLE	TN	1971	After	Elem	Treatment
DALLAS	TX	1971	After	Elem	Treatment
ARLINGTON COUNTY	VA	1971	After	Elem	Treatment
ORANGE COUNTY	FL	1972	After	Elem	Treatment
FAYETTE COUNTY	KY	1972	After	Elem	Treatment
LANSING	MI	1972	After	Elem	Treatment
CLARK COUNTY	NV	1972	After	Elem	Treatment
OKLAHOMA CITY	OK	1972	After	Elem	Treatment
AMARILLO	TX	1972	After	Elem	Treatment
ATLANTA	GA	1973	After	JHS	Treatment
ROCKFORD	IL	1973	After	JHS	Treatment
INDIANAPOLIS	IN	1973	After	JHS	Treatment
PRINCE GEORGE'S COUNTY	MD	1973	After	JHS	Treatment
CINCINNATI	OH	1973	After	JHS	Treatment
LAWTON	OK	1973	After	JHS	Treatment
MEMPHIS	TN	1973	After	JHS	Treatment
FORT WORTH	TX	1973	After	JHS	Treatment
WACO	TX	1973	After	JHS	Treatment
RALEIGH COUNTY	WV	1973	After	JHS	Treatment
DENVER	CO	1974	After	JHS	Treatment
BALTIMORE	MD	1974	After	JHS	Treatment
BOSTON	MA	1974	After	JHS	Treatment
SPRINGFIELD	MA	1974	After	JHS	Treatment
MINNEAPOLIS	MN	1974	After	JHS	Treatment
PORTLAND	OR	1974	After	JHS	Treatment
JEFFERSON COUNTY	KY	1975	After	JHS	Treatment
DETROIT	MI	1975	After	JHS	Treatment
SACRAMENTO	CA	1976	After	HS	Treatment
NEW BEDFORD	MA	1976	After	HS	Treatment
OMAHA	NB	1976	After	HS	Treatment
JERSEY CITY	NJ	1976	After	HS	Treatment
DAYTON	OH	1976	After	HS	Treatment
MILWAUKEE	WI	1976	After	HS	Treatment

Note: Table lists school districts in the Welch and Light (1987) study, which are also the districts used in this study. Districts are chosen based on the following criteria, as described in Welch and Light (1987). Every district with 50,000 or more students in 1968 and 20 to 90 percent minority representation are included. Districts with 15,000 or more students in 1968 and ten to 90 percent minority representation were chosen with sampling probabilities proportional to their size and regional representation. The remaining districts—those with fewer than 15,000 students in 1968, less than ten percent minority representation—were excluded from the sample. Grade of Desegregation columns identify the grade a 17-year-old in 1970 (1980) was in when the school district desegregated. Year of Desegregation column reports the year the district's major desegregation plan was implemented according to Welch and Light (1987).

Table 1 (cont.)

School District		Year of Desegregation	Grade of Desegregation 1970	1980	Treatment or Control
SAN DIEGO	CA	1977	After	HS	Treatment
KANSAS CITY	KS	1977	After	HS	Treatment
KANSAS CITY	MO	1977	After	HS	Treatment
AKRON	OH	1977	After	HS	Treatment
FRESNO	CA	1978	After	HS	Treatment
LOS ANGELES	CA	1978	After	HS	Treatment
SAN BERNARDINO	CA	1978	After	HS	Treatment
NEW CASTLE COUNTY	DE	1978	After	HS	Treatment
PHILADELPHIA	PA	1978	After	HS	Treatment
EL PASO	TX	1978	After	HS	Treatment
LUBBOCK	TX	1978	After	HS	Treatment
SEATTLE	WA	1978	After	HS	Treatment
TUCSON	AZ	1979	After	HS	Treatment
CLEVELAND	OH	1979	After	HS	Treatment
COLUMBUS	OH	1979	After	HS	Treatment
LONG BEACH	CA	1980	After	After	Control
DOUGHERTY COUNTY	GA	1980	After	After	Control
ST. LOUIS	MO	1980	After	After	Control
BUFFALO	NY	1980	After	After	Control
TOLEDO	OH	1980	After	After	Control
PITTSBURGH	PA	1980	After	After	Control
AUSTIN	TX	1980	After	After	Control
SAN JOSE	CA	1981	After	After	Control
SOUTH BEND	IN	1981	After	After	Control
CHICAGO	IL	1982	After	After	Control
ECTOR COUNTY	TX	1982	After	After	Control
MESA	AZ	None			
MODESTO	CA	None			
VALLEJO	CA	None			
PUEBLO	CO	None			
GARY	IN	None			
SAGINAW	MI	None			
ALBUQUERQUE	NM	None			
LAS CRUCES	NM	None			
NEW YORK	NY	None			
LORAIN	OH	None			

Note: Table lists school districts in the Welch and Light (1987) study, which are also the districts used in this study. Districts are chosen based on the following criteria, as described in Welch and Light (1987). Every district with 50,000 or more students in 1968 and 20 to 90 percent minority representation are included. Districts with 15,000 or more students in 1968 and ten to 90 percent minority representation were chosen with sampling probabilities proportional to their size and regional representation. The remaining districts—those with fewer than 15,000 students in 1968, less than ten percent minority representation—were excluded from the sample. Grade of Desegregation columns identify the grade a 17-year-old in 1970 (1980) was in when the school district desegregated. Year of Desegregation column reports the year the district's major desegregation plan was implemented according to Welch and Light (1987).

Table 2: Means of Selected Variables

	60's Desegregators		70's Desegregators		80's Desegregators	
	1970	1980	1970	1980	1970	1980
Dropout	.116 (.321)	.120 (.325)	.143 (.351)	.105 (.306)	.116 (.321)	.116 (.320)
Mother's Education	9.7 (3.1)	10.8 (2.9)	9.8 (2.9)	11.2 (2.7)	9.9 (2.7)	11.1 (2.6)
Father's Education	8.4 (3.7)	10.1 (3.8)	8.8 (3.5)	10.7 (3.5)	9.6 (3.3)	10.6 (3.4)
Mother's Age	42.1 (7.0)	41.9 (7.0)	41.9 (7.1)	41.8 (7.0)	41.5 (7.0)	41.8 (6.9)
Father's Age	45.9 (8.3)	46.2 (8.6)	46.3 (8.7)	45.9 (8.7)	45.1 (8.1)	46.2 (8.5)
Family Size	6.0 (2.8)	5.2 (2.1)	5.9 (2.6)	5.2 (2.1)	6.0 (2.7)	5.4 (2.3)
Family Income in 1980 $	14,646 (10,700)	15,325 (12,524)	16,714 (12,113)	17,152 (13,160)	17,891 (12,059)	17,522 (13,502)
Moved States Last 5 Years	.047 (.211)	.024 (.154)	.046 (.209)	.029 (.167)	.031 (.173)	.014 (.116)
No. Obs.	1,068	6,780	5,019	31,827	1,169	7,468

Note: The table contains unweighted means of selected variables for the sample of black 15-, 16- and 17-year olds. Means are computed separately for districts that desegregated between 1961 and 1969 (60's desegregators), between 1970 and 1979 (70's desegregators), and between 1980 and 1982 (80's desegregators). Means are also computed separately in 1970 and 1980. Parental characteristics are only computed for individuals where the respective parent is present in the household. Standard deviations are reported in parentheses. Family income is reported in 1980 dollars, inflated using the CPI-U. "Moved States Last 5 Years" is an indicator for whether the individual reports having moved from his current state of residence in the preceding 5 years.

Table 3: Basic Results—Effect of Desegregation Plans on Dropout Rates

	(1)	(2)	(3)	(4)	(5)
			Black 15-, 16-, and 17-year olds		
70's Desegregator *1980	-.038 (.011)	-.028 (.009)	-.029 (.005)	-.030 (.005)	-.026 (.008)
1980	.002 (.008)	.006 (.007)	.016 (.005)	-.057 (.065)	
70's Desegregator	.027 (.008)	.021 (.007)			
R^2	.002	.088	.094	.095	.096
N			53,331		
			White 15-, 16-, and 17-year olds		
70's Desegregator *1980	.005 (.011)	.003 (.006)	.003 (.003)	-.001 (.003)	.001 (.004)
1980	.024 (.009)	.039 (.006)	.038 (.003)	.030 (.019)	
70's Desegregator	.013 (.007)	.002 (.004)			
R^2	.003	.161	.164	.165	.165
N			203,063		
Specification	DID	DID	Fixed Effects	Fixed Effects	Fixed Effects
Controls	No Controls	Individual, Family & District Characteristics	Individual, Family & District Characteristics	Individual, Family & District Characteristics, Demog.*Year Effects	Individual, Family & District Characteristics, Demog.*Year Effects, State*Year Effects

Note: Each column of the top panel reports estimates of the effect of desegregation plans on dropout status for black 15-, 16, and 17- year olds. The bottom panel reports results for white 15-, 16, and 17- year olds. Columns (1) and (2) report results from difference-in-differences (DID) specifications; columns (3) – (5) report results from specification that include consolidated county group fixed effects, where consolidated county groups are the geographic unit used to match individuals to school districts in the census. Additional controls are listed in the bottom row of the table. Individual, Family & District Characteristics include age indicators, family income, mother's and father's education, mother's and father's age, family size, region dummies, percent manufacturing, and median income. Standard errors, corrected for heteroskedasticity and for district*year correlation, are reported in parentheses.

Table 4: Interactions of the Effect of Desegregation with Characteristics of the Plan

	(1)	(2)	(3)	(4)	(5)
			Effectiveness of Plan: Dissimilarity Index		
70's Desegregator *1980	-.037 (.012)	-.025 (.010)	-.029 (.006)	-.030 (.005)	-.025 (.006)
70's Desegregator *1980 * Δ Dissimilarity	.062 (.062)	.009 (.051)	.084 (.035)	.091 (.038)	.193 (.049)
R^2	.002	.088	.094	.095	.097
N			53,331		
			Effectiveness of Plan: Exposure Index		
70's Desegregator *1980	-.040 (.011)	-.029 (.009)	-.031 (.005)	-.031 (.006)	-.024 (.007)
70's Desegregator *1980 * Δ Exposure	-.066 (.091)	.014 (.079)	-.039 (.049)	-.015 (.057)	-.204 (.061)
R^2	.002	.088	.094	.095	.097
N			53,331		
			Type of Plan		
Non-Voluntary	-.056 (.014)	-.029 (.012)	-.040 (.010)	-.030 (.010)	-.033 (.012)
Voluntary	-.016 (.014)	-.019 (.014)	-.020 (.008)	-.025 (.008)	-.013 (.013)
R^2	.003	.088	.094	.095	.096
N			53,331		
Specification	DID	DID	Fixed Effects	Fixed Effects	Fixed Effects
Controls	No Controls	Individual, Family & District Characteristics	Individual, Family & District Characteristics	Individual, Family & District Characteristics, Demog.*Year Effects	Individual, Family & District Characteristics, Demog.*Year Effects, State*Year Effects

Note: Each column of the top panel reports estimates of the effect of desegregation plans on dropout status, and the interaction of that effect with the change in the dissimilarity index. The middle panel reports estimates of the effect of desegregation plans on dropout status, and the interaction of that effect with the change in the exposure index. Changes in the dissimilarity and exposure indices are included as deviations from means so that the main effect measures the impact of desegregation for districts with the average change in these indices. The indices are defined in the text. The bottom panel reports estimates of the effect of desegregation plans on dropout status for black 15-, 16, and 17- year olds, and the interaction of that effect with an indicator for whether the desegregation plan offered students and parents choice (Voluntary Plans). Columns (1) and (2) report results from difference-in-differences (DID) specifications; columns (3) – (5) report results from specification that include consolidated county group fixed effects, where consolidated county groups are the geographic unit used to match individuals to school districts in the census. Additional controls are listed in the bottom row of the table. Individual, Family & District Characteristics include age indicators, family income, mother's and father's education, mother's and father's age, family size, region dummies, percent manufacturing, and median income. Standard errors, corrected for heteroskedasticity and for district*year correlation, are reported in parentheses.

Table 5: Controlling for and Checking for Endogenous Migration

	(1)	(2)	(3)	(4)	(5)
			Dep. Var.: Moved		
All Districts	.001 (.010)	.001 (.008)	-.001 (.006)	-.003 (.004)	-.010 (.006)
Desegregated 1965-1969 or 1975-1979	-.010 (.019)	-.010 (.014)	-.015 (.009)	.025 (.009)	.021 (.011)
			Dep. Var.: Dropout		
Control for Migration	-.036 (.008)	-.022 (.006)	-.021 (.004)	-.033 (.006)	-.026 (.007)
Use 1975 Residence for 1980	-.050 (.015)	-.029 (.013)	-.033 (.009)	-.038 (.010)	-.028 (.014)
Specification	DID	DID	Fixed Effects	Fixed Effects	Fixed Effects
Controls	No Controls	Individual, Family & District Characteristics	Individual, Family & District Characteristics	Individual, Family & District Characteristics, Demog.*Year Effects	Individual, Family & District Characteristics, Demog.*Year Effects, State*Year Effects

Note: Each entry in the top panel of table reports the estimated effect of desegregation on dropout status for black 15-, 16, and 17- year olds. The first row reports estimates from the same specifications as reported in table 3, with an additional control for whether the individual moved from his county group of residence in the past 5 years. The second row reports estimates from these same specifications, but defines individuals' residence in 1980 based on their residence in 1975. Each entry in the bottom panel of table reports the estimated effect of desegregation on an indicator for whether the individual moved from his county group of residence in the past 5 years. The sample includes black 15-, 16, and 17- year olds. The first row reports coefficients estimated from individuals in all districts. The second row reports coefficients estimated only from districts that desegregated between 1965 and 1969 or between 1975 and 1979. Standard errors, corrected for heteroskedasticity and for district*year correlation, are reported in parentheses.

Table 6: Alternative Control Groups

	(1)	(2)	(3)	(4)	(5)
			Control Desegregated Before 1970		
70's Desegregator * 1980	-.040 (.016)	-.033 (.012)	-.035 (.007)	-.032 (.008)	-.024 (.010)
1980	.004 (.014)	.011 (.011)	.014 (.006)	-.069 (.064)	
70's Desegregator	.027 (.012)	.025 (.009)			
R^2	.003	.089	.096	.097	.099
No. Obs.			44,694		
			Control Desegregated After 1980		
70's Desegregator * 1980	-.036 (.010)	-.025 (.009)	-.024 (.005)	-.030 (.007)	-.030 (.010)
1980	.000 (.006)	.003 (.006)	.002 (.002)	-.069 (.063)	
70's Desegregator	.027 (.007)	.017 (.007)			
R^2	.003	.090	.096	.097	.099
No. Obs.			45,483		
Specification	DID	DID	Fixed Effects	Fixed Effects	Fixed Effects
Controls	No Controls	Individual, Family & District Characteristics	Individual, Family & District Characteristics	Individual, Family & District Characteristics, Demog.*Year Effects	Individual, Family & District Characteristics, Demog.*Year Effects, State*Year Effects

Note: Each column of the top panel reports estimates of the effect of desegregation plans on dropout status for black 15-, 16, and 17- year olds, where the control group only includes individuals in districts that desegregated before 1970. Each column of the bottom panel reports estimates of the effect of desegregation plans on dropout status for black 15-, 16, and 17- year olds, where the control group only includes individuals in districts that desegregated after 1980. Columns (1) and (2) report results from difference-in-differences (DID) specifications; columns (3) – (5) report results from specification that include consolidated county group fixed effects, where consolidated county groups are the geographic unit used to match individuals to school districts in the census. Additional controls are listed in the bottom row of the table. Individual, Family & District Characteristics include age indicators, family income, mother's and father's education, mother's and father's age, family size, region dummies, percent manufacturing, and median income. Standard errors, corrected for heteroskedasticity and for district*year correlation, are reported in parentheses.

Table 7: Instrumental Variables Estimates

Instrument	(1)	(2)	(3)	(4)	(5)
			Instrumental Variables Estimates		
Year First Opinion Filed	-.080	-.088	-.105	-.063	-.123
	(.082)	(.069)	(.066)	(.032)	(.057)
Year Initial Plan Implemented	-.084	-.100	-.123	-.076	-.072
	(.059)	(.060)	(.061)	(.024)	(.035)
			First-Stage Estimates		
Year First Opinion Filed	.197	.178	.166	.249	.217
	(.006)	(.006)	(.004)	(.004)	(.004)
Year Initial Plan Implemented	.208	.200	.230	.397	.275
	(.006)	(.006)	(.004)	(.004)	(.004)
Specification	DID	DID	Fixed Effects	Fixed Effects	Fixed Effects
Controls	No Controls	Individual, Family & District Characteristics	Individual, Family & District Characteristics	Individual, Family & District Characteristics, Demog.*Year Effects	Individual, Family & District Characteristics, Demog.*Year Effects, State*Year Effects

Note: Each column of the top panel reports instrumental variables (IV) estimates of the effect of desegregation plans on dropout status for black 15-, 16, and 17- year olds. In the first row, the decade in which the first opinion was filed in the court proceeding that led to desegregation is used as an instrument for the decade in which the major desegregation plan was implemented. In the second column, the decade in which the initial desegregation plan was implemented is used as an instrument for the decade in which the major desegregation plan was implemented. The bottom panel reports first-stage estimates corresponding to the IV estimates in the top panel. Columns (1) and (2) report results from difference-in-differences (DID) specifications; columns (3) – (5) report results from specification that include consolidated county group fixed effects, where consolidated county groups are the geographic unit used to match individuals to school districts in the census. Additional controls are listed in the bottom row of the table. Individual, Family & District Characteristics include age indicators, family income, mother's and father's education, mother's and father's age, family size, region dummies, percent manufacturing, and median income. Standard errors, corrected for heteroskedasticity and for district*year correlation, are reported in parentheses.

Appendix 1: A Simple Model of Legal Precedent

The following model is meant to illustrate the role of legal precedent in the decision of an agent seeking to promote social change through the courts. The agent's objective is to effect some change in a set of distinct local areas. To achieve this end, the agent brings legal challenges in each of these areas, one at a time. The model points out that the agent will pay close attention to precedent when choosing the timing of legal challenges. In the absence of precedent, it is clear that the agent will choose to bring suit first in the area with the largest potential benefit from success. He will then bring suit in the area with the second largest benefit, and so on until he is finished.

Precedent creates a spillover, where the national benefits and costs of bringing a suit are no longer the same as the local benefits and costs. The agent will internalize the spillovers created by the setting of precedent and, as a result, will weigh the probability of success more heavily than the benefit from success when choosing where to bring a legal challenge early on. When precedent is important in the legal system, and when the number of cases remaining to be brought is large, the agent can virtually ignore the potential benefits from success, and base his decision solely on the probability of success.

In the model, the agent chooses to bring a legal challenge in one location in each time period. The benefits of success vary by location, and the agent seeks to maximize the expected discounted sum of benefits. The legal challenge either succeeds or fails with some probability that depends both on the facts of the case specific to the locality and on the history of legal successes and failures up to the point of the current legal challenge. This second factor represents the role of precedent in the judicial system.

To illustrate the point more formally, consider an agent seeking to bring suit in two distinct locations, denoted *A* and *B*. The agent can bring forward only one legal challenge per time period. The agent wins the legal challenge with some probability, which depends on the characteristics of the location and the history of legal successes and failures. Specifically, let each location have an inherent probability of legal success, denoted P_i, $i \in (A, B)$. Since no precedent has been set when the agent brings the first legal challenge $P_i^{t=1} = P_i$.

At $t = 2$, precedent has been set by the outcome of the case filed in the first period. Thus, the probability of success in the second case brought depends on the outcome of the first case. For ease of exposition, assume that the agent must decide the order of cases at $t = 0$. Thus, the likelihood of success in the second period depends directly on the *ex-ante* likelihood of success of the case brought in the first period. Consequently, we can write the probability of success of the second case

$$P_i^{t=2} \equiv P_i^2 = P_i + \phi(P_j),\ j \neq i$$

where $\phi(.)$ is an increasing, non-negative function that represents the effect of precedent.

A2.1. When will the agent choose to file suit in location A first?

Each district has a potential benefit from legal success. For the purposes of this model, let each location be of a different size, denoted N_i. The larger the location, the more students will benefit from a legal victory. The agent seeks to maximize the expected number of student-years under court order. Thus, the agent will choose to file suit in location *A* first if

$$2P_A N_A + [P_B + \phi(P_A)]N_B \geq 2P_B N_B + [P_A + \phi(P_B)]N_A$$
$$\Leftrightarrow$$
$$P_A N_A + \phi(P_A)N_B \geq P_B N_B + \phi(P_B)N_A$$

By making this an equality, we can take the total derivative and find an indifference relationship between the local benefit (N_i), and the likelihood of success (P_i). The total derivative becomes

$$[P_A - \phi(P_B)]dN_A + [N_A + N_B\phi'(P_A)]dP_A + [P_B - \phi(P_A)]dN_B - [N_B + N_A\phi'(P_B)]dP_B = 0$$

Holding, N_B, P_B constant we get

$$-\frac{dN_A}{dP_A} = \frac{N_A + N_B\phi'(P_A)}{P_A - \phi(P_B)}$$

We can compare how the agent trades off N_A and P_A in a world without precedent. Here, the agent chooses location *A* first if

$$P_A N_A \geq P_B N_B$$

Thus, the total derivative yields

$$-\frac{dN_A}{dP_A} = \frac{N_A}{P_A}$$

Let us compare the agent's tradeoff in a world with precedent to the agent's tradeoff in a world without precedent. The difference in the marginal rate of substitution between local benefit and the probability of success is

$$\frac{N_A + N_B\phi'(P_A)}{P_A - \phi(P_B)} - \frac{N_A}{P_A} = \frac{P_A N_B\phi'(P_A) + N_A\phi(P_B)}{P_A{}^2 - P_A\phi(P_B)}$$

$$= \frac{N_B\phi'(P_A) + (N_A / P_A)\phi(P_B)}{P_A - \phi(P_B)}$$

There is some location for which $\phi(P_i) = 0$. Otherwise, the existence of precedent in the legal system would increase the probability of success in every case after the first period. In this case, let that be location *B*. The ratio can now be written

$$\left.\frac{dN_A}{dP_A}\right|_{no\ precedent} - \left.\frac{dN_A}{dP_A}\right|_{precedent} = \frac{N_B \phi'(P_A)}{P_A}$$

The magnitude of the above expression measures the amount that precedent adds to the agent's valuation of the probability of success, relative to the local benefit. The amount by which the marginal rate of substitution increases is a measure of how much precedent adds to the agent's valuation of P_i relative to N_i.

A measure of the importance of precedent in the legal system is the size of $\phi'(.)$. When $\phi'(.)$ is large, cases that are more likely to be successful increase the probability of success in subsequent trials by large amounts relative to cases that are unlikely to be successful. This effect is weighted by N_B; the larger the potential benefit to legal success in location *B*, the larger the incentive to increase the probability of success there, by winning the case in location *A* first.

A2.2. Additional Locations, Additional Time Periods

Adding more locations to the model requires the addition of structure to the model, but the result should be intuitively clear. The effect of precedent on subsequent legal challenges magnifies as the number of cases to be tried increases. When there are two locations, precedent creates a distinction between the global and local marginal benefits of choosing a higher P_i. This distinction becomes greater as the effect of precedent is allowed to compound over the course of many cases. When there are three locations, choosing a high P_i in the first period increases the probability of success in the second period. The increased likelihood of victory in the second period is a benefit in and of itself. Additionally, the increased likelihood of victory in the second period increases the *ex-ante* likelihood of victory in the third period.

Thus, in a legal system where precedent is important and in a situation where the agent wants to eventually bring suit in a large number of locations, the value of a high P_i

will greatly outweigh the value of a high N_i. In other words, the agent seeking to pursue social change through the courts will virtually ignore the local benefit of success in the early stages of the process, and will choose to file suit in a location with a high probability of success.