Court-Ordered Desegregation: Successes and Failures in Integrating American Schools Since *Brown*

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Abstract

The integration effort following the 1954 Supreme Court *Brown vs. Board of Education* decision is considered to be one of the most important education policies of the twentieth century. This paper uses the wide variation in the timing of court-ordered plans to examine how successful these plans were in increasing integration of the schools, given the potential behavioral response of whites. In particular, I evaluate whether racial segregation within court-ordered districts fell and whether these plans were associated with white enrollment losses from those districts. If white enrollment loss – or "white flight" – was severe, integration within these districts will be offset by the rise in between-district segregation. I find strong evidence that districts did integrate schools in the years following implementation of desegregation plans. However, desegregation plans were associated with significant white enrollment losses that offset more than one-third of the within-district reductions during the decade after plan implementation. White enrollment losses were particularly severe in districts with more districts in the same metropolitan area. This suggests that the success of the plans was limited by the decision to exclude suburban districts from the plans.

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The Supreme Court's 1954 decision in *Brown vs. Board of Education*[1], declaring segregated schools to be "inherently unequal," was a momentous piece of educational policy. As a result of *Brown* and subsequent decisions courts ordered districts around the country to desegregate their schools. But were these plans successful in integrating the schools, or was reduced segregation within these districts offset by white flight to suburban districts? What factors were associated with more and less effective plans? I address these questions using a unique dataset that tracks enrollment by race at the school level for 108 school districts with court-ordered desegregation plans. For a single district or time series, it is difficult to tell whether changes in white enrollment and segregation measures are related to desegregation plans or simply reflect underlying trends. I rely on the fact that there was substantial variation – due largely to the peculiarities of the legal process – in the timing of desegregation plan implementation to separate the effects of the plans from trends due to other factors. I also investigate which district characteristics explain the variation in white flight and long-run success in reducing segregation across districts.[2]

The question of whether desegregation plans causes white flight has been hotly debated since shortly after the first major desegregation plans were implemented in the

[1] 347 U.S. 483 (1954).

[2] Ultimately, we are interested in how these plans affected educational and other outcomes for the minority students they were designed to help. Guryan (2000) provides some of the only systematic evidence on this question. Using the 1970 and 1980 Census, he estimates that the desegregation plans of the 1970s reduced black dropout rates by one to three percentage points, explaining about half the decline in dropout rates for blacks during the 1970s. Many of the channels through which these plans could improve outcomes (for example, by increasing pressure for quality schools, changing peers, or reducing stigma) depend on increasing nonwhite exposure to whites, so the change in segregation is clearly an important intermediate outcome.

late 1960s.[3] In addition, Welch and Light – who collected some of the data used in this analysis –document reductions in segregation and percent white in the year or two following plan implementation. This early research establishes a correlation between falling segregation and white enrollment during the period when many desegregation plans were implemented.

More recent work has pointed to the problem of "resegregation" as whites have moved to the suburbs over time. Clotfelter (1999) and Clotfelter (2001) show that in the mid-1990s, a substantial portion of segregation at the school level was *between districts within metropolitan areas* and that the between-district component of segregation was growing in the late 1980s and 1990s. These papers point to the importance of white flight to the suburbs generally, but they do not address the role of desegregation plans directly.[4]

In this paper, I use a more complete dataset – spanning from the late 1960s to 1998 – to study the *long-term* effects of desegregation plans. I use the variation in implementation year across districts to identify the effects of desegregation plans. Further, most existing studies examining the variation in plans' effects on segregation employ a case-study approach or consider a relatively small number of districts with few controls. I examine the determinants of plans success in increasing long-run integration systematically, considering a variety of factors.

I present systematic evidence showing that these court-ordered plans were actually enforced. Desegregation plans substantially reduced segregation *within* affected districts, and these reductions were maintained during the 8 to 10 years following

[3] See, for example, Coleman et al. (1975), Farley (1975), and Armor (1978). A large number of case studies have also been conducted for particular districts. See, for example, Orfield and Eaton (1996) and Rossell (1978).
[4] Also see Orfield and Eaton (1996) and Orfield (2001).

implementation. However, I also find that white families responded by leaving districts that had desegregation plans. These plan-induced reductions in white enrollment offset more than one-third of the initial reductions in segregation by a decade after implementation.

There was substantial heterogeneity in long-run changes in segregation and white enrollment across districts, and the evidence suggests that the decision to exclude suburban districts from these plans had important ramifications for their success: White flight was particularly severe for districts surrounded by many alternative public school districts that were not affected by the policy. In districts where whites experienced a larger change in exposure to nonwhites, white flight was also more severe.

The paper proceeds as follows: Section II discusses the legal background and types of plans; Section III discusses the sample of districts and data sources. In Section IV, I describe the measures of segregation and enrollment I use. In Section V, I discuss the identification of the effects of desegregation plans on trends in segregation and enrollment. Section VI presents evidence on the average effects of desegregation plans on trends in segregation and enrollment. In Section VII, I present evidence that the effects of desegregation plans were heterogeneous, discuss several hypotheses that may explain this heterogeneity, and evaluate these hypotheses empirically; in particular, I consider what factors are associated with a larger white enrollment response to the policy. In Section VIII, I discuss the experiences of two districts in more detail; Section IX concludes.

II. Background

Legal History in Brief

In 1954, the landmark *Brown vs. Board of Education* decision overturned the "separate but equal" doctrine, declaring separate schools to be "inherently unequal." It was not until the 1955 decision in *Brown II*[5] that the Court started to specify *how* the dual system should be dismantled, requiring a "prompt and reasonable start" toward integration. Still, there was little large-scale integration for some time after *Brown II*, and the question of what precisely was required was unclear into the 1970s. During this period, first-generation desegregation plans typically moved just a handful of blacks to the white schools or allowed for "voluntary transfers" to different schools. These initial policies produced only small reductions in segregation.[6]

From the late 1960s to mid-1970s, a series of rulings increased the extent of integration required, expanded the set of tools to achieve it as well as the circumstances under which desegregation could be required.[7] The large-scale, court-ordered plans that I consider here were imposed following these rulings.[8] Finally, the 1974 decision in

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[5] 349 U.S. 294 (1955).

[6] Typically, southern states initially removed laws mandating segregated schools, but made no positive effort to integrate. The Little Rock case is the most famous example of early efforts at desegregation. In 1957 a handful of black students were assigned to Central High, but the dual system remained largely intact. And even this small effort toward integration required the National Guard to enforce. Similarly, the Norfolk school district accepted applications to the white schools from any student in 1957, but rejected all applications submitted by black students (Orfield and Eaton, 1996).

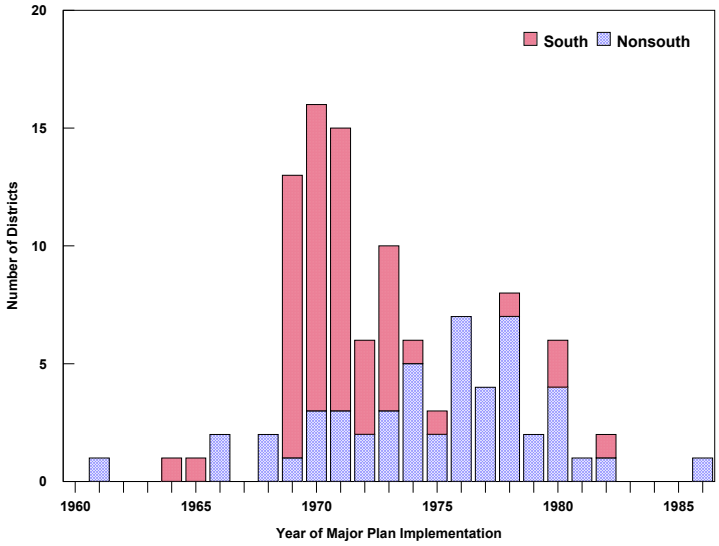
[7] In 1968, the Supreme Court ruled that "voluntary" plans were not effective and other methods should be used (*Green v. New Kent County, Virginia*). In 1971, busing and other remedies were officially sanctioned in the *Swann v. Charlotte-Mecklenburg County* decision, and the Court called for an end to identifiable "black" and "white" schools, ordering a move "from white schools and black schools to just schools." The 1973 *Keyes v. School District No.1, Denver, Colorado* decision increased the possibility of implementing plans outside the South by expanding the list of state actions that could be the basis for a desegregation order. *Keyes* made clear that specific statutes, such as those found in the South, were not the only grounds for a desegregation order. As a result, desegregation plans were required in many districts in the Northeast, Midwest, and West.

[8] Some reductions in segregation were achieved through the smaller early plans, so districts in the South had already generally moved away from perfect segregation by the late 1960s. For a few districts in the

Milliken v. Bradley[9] made it difficult to include suburban districts in a desegregation plan. This decision made it much more likely that plans could be undermined by flight as white families could move to nearby districts to avoid the plan. The results in Section VII suggest this was an important limitation.

The timing of plan implementation resulted from a complex set of factors, including the evolving case law and the peculiarities of the legal proceedings in any particular district.[10] There were often long lags between the initiation of legal proceedings and the implementation of a plan.[11] Therefore, in the empirical analysis, I use the date when major plans were actually implemented, rather than the date when the case was first known about or filed. Figure 1 shows the distribution of implementation dates for the districts in the sample.

Figure 1. Distribution of Districts by Year of Implementation



Source: Welch and Light (1987).

sample that have data available for years in the early 1960s, the data indicate that segregation was complete at that time.

[9] 418 U.S. 717 (1974).

[10] In his analysis of the effects of court-ordered plans on high school dropout rates, Guryan (2000) argues that in a legal environment where precedent is very important (as in the U.S.), the optimal strategy for groups bringing cases is to focus first on areas where they have the highest probability of a legal victory rather than areas where desegregation is expected to be most beneficial for minority students.

Because the legislated segregation practiced in the South was the first to be declared illegal, southern districts had earlier plans on average, although there is significant overlap in the timing of implementation in southern and non-southern districts.

Types of Desegregation Plans

Districts used a variety of strategies to integrate their schools. All the plans were court-ordered and therefore mandatory from the perspective of the districts; however, the "voluntary" plans allowed parents some choice over schools. There were three types of voluntary plans: "freedom of choice" (open enrollment), which allowed students to choose which schools they attended; magnet schools; and other types of voluntary transfers (for example, a transfer would be allowed if it improved the level of integration in the district). These voluntary solutions were often considered inadequate and were mostly not considered major plans. Only about 20 percent of major plans had no mandatory component.

In districts with a mandatory plan component, parents had no choice over the school their children attended (except by moving or attending private school). Plans using neighborhood attendance zones assigned students to schools in their neighborhood; these were employed primarily in the South to end the dual system and generally did not require busing. Some plans redrew the attendance zones to create schools that were more integrated, often generating noncontiguous zones. In plans that employed pairing and clustering, primarily black and primarily white schools (and their students) would be grouped together and their students shuffled among the schools to improve integration. The most common type of pairing and clustering involved grade restructuring; for

[11] See the case study for Memphis in Section VIII for one example.

example, a formerly black school would specialize in first to third grades and a formerly white school would have fourth to sixth grades.[12] Rezoning and pairing and clustering typically involved busing. Many plans incorporated several of these strategies at once.

III. Data: Enrollment by Race and Plan Implementation Dates

I use school-level data on enrollment by race for a sample of 108 districts that had at least one court-ordered desegregation plan some time between 1961 and 1986. The sample of districts was chosen for a report of the U.S. Commission on Civil Rights (Welch and Light). Detailed data on enrollment by race at the school level over time and information about court-ordered desegregation plans were then collected for these districts. Two criteria were used to select the sample of districts based on 1968 enrollment. All districts with enrollment greater than 50,000 students and minority representation between 20 and 90 percent were included (58 districts). Second, districts with 15,000 or more students and 10 to 90 percent black enrollment were chosen with sampling probabilities proportional to their size and regional representation. All other districts were excluded. The sample covered 20 percent of enrollment and about 45 percent of minority enrollment nationally in 1968.[13] The districts and implementation years are presented in the Appendix B.

Year of Implementation

This report also includes information about all court-ordered desegregation plans in each of the districts sampled, including the methods used and whether or not the plan

[12] For example, in Charlotte-Mecklenberg County, all children in grades 1-3 attended the (mostly formerly black) schools in the center city, while fourth to sixth graders went to school in the more suburban (mostly formerly white) areas of the district (Orfield and Eaton, 1996).

[13] The full Welch and Light sample includes 125 districts; I consider only the 108 that had a court-ordered plan at some time. Districts with relatively large black enrollment that did not ever have a desegregation plan are rare and therefore likely to have been different from those with plans.

was considered "major." Some districts implemented multiple plans, although many of these plans were first attempts or follow-ups. Most districts (96 of 108) are characterized as having only one major plan. In this analysis, I use the year of the first major plan as the implementation date. While the scope of major plans varied considerably, the enrollment data show sharp changes in the racial composition of schools in the year identified as a "major" plan for nearly all districts; in other words, it appears that many students were, in fact, reassigned in the year of major plans as identified by Welch and Light.

Enrollment Data

Welch and Light compiled data on enrollment by race at the school level for years before 1986 from sources collected by the Office of Civil Rights and directly from school districts. For 1987-1998, I use similar data from the Department of Educations' Common Core of Data Public School Universe. I exclude years that have incomplete data on enrollment by race.[14]

Enrollment is divided into 5 mutually-exclusive race categories (non-Hispanic white, black, Hispanic, Asian, and American Indian). I consider two groups – non-Hispanic whites and all others[15]; for ease of exposition, I refer to these groups as whites and nonwhites (even though Hispanics generally identify themselves as white).

IV. Measures of Segregation and Enrollment

No single measure can fully capture the extent of segregation; I therefore consider several measures that have been used extensively in the literature on residential and school segregation. Many studies of segregation find that multiple measures of

[14] See Appendix B for more information.
[15] It would be interesting to consider the effects of desegregation plans on blacks and Hispanics separately; for much of the sample period, however, Hispanics are not a large enough share of enrollment to get

segregation move together, but in this case that is less likely to hold.[16] To the extent that desegregation plans induced whites to withdraw from public schools in the affected districts, they changed the racial composition in the district as a whole. Therefore, it is important to use measures that allow the consideration of the effects of changes in school assignments within a district, as well as changes in the fraction white in the district overall.

Measures of Segregation

The *dissimilarity index* captures the extent of integration, *given the fraction white in the district*. This index is calculated for two mutually-exclusive groups, in this case, whites and nonwhites. The dissimilarity index can be interpreted as the fraction of students that would have to change schools to create a racial distribution in each school equal to that of the district as a whole. The dissimilarity index ranges from 0 (each school has the same racial balance as the district overall) to 1 (complete segregation). The index is calculated for each district as follows:

$$DISSIMILARITY = \frac{\sum_s TOT_s \times \left| \%NW_s - \%NW \right|}{2 \times TOT \times \%NW \times (1 - \%NW)},$$

where TOT_s is the total enrollment in school s, $\%NW_s$ is the fraction of school s that is nonwhite, TOT is the total enrollment in the district, and $\%NW$ is the fraction of the district that is nonwhite.

If a district has a dissimilarity index of 0.7, 70 percent of nonwhites would have to be reassigned to a different school to make all schools representative of the racial composition of the whole district. The dissimilarity index thus measures "compliance" or

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precise estimates. I therefore consider all nonwhites together. All of the results are similar if only blacks and whites are considered.

"within-district" segregation; it indicates how closely the racial composition of individual schools varies from the racial composition of the entire district. This measure, however, does not fully capture the extent to which nonwhites attended the same schools as whites since the fraction white varied across districts and over time.

The *exposure index* for nonwhites measures the fraction white of the "average" nonwhite's school. This index is simply the weighted average of percent white in schools, where the weight is the school's nonwhite enrollment. Exposure of nonwhites to whites is calculated as follows:

$$NW\ EXPOSURE = \sum_s \%WH_s \times \frac{NW_s}{NW},$$

where $\%WH_s$ is the percent white in school s, NW_s is nonwhite enrollment in school s, NW is total nonwhite enrollment in the district. This measure captures the combination of the overall percent white in the district and how integrated the schools are given that composition. Ultimately, one important goal of these policies was to increase the extent to which minority students have contact with whites in schools; this was part of the logic of the *Brown* decision. Many of the channels through which desegregation plans might have improved education for minorities hinged on the notion that the plans would actually increase minority students' exposure to whites. I therefore consider nonwhite exposure to whites an important summary measure of the success of these plans. White exposure to nonwhites is calculated similarly:

$$WHITE\ EXPOSURE = \sum_s \%NW_s \times \frac{White_s}{White},$$

[16] See, for example, the analysis of trends in residential segregation in Cutler, Glaeser, and Vigdor (1998).

For a constant population, the exposure of nonwhites to whites and the exposure of whites to nonwhites must move in the same direction, while dissimilarity moves in the opposite direction. However, if the fraction white in the district falls (as was generally the case here), nonwhite exposure to whites will fall, while white exposure to nonwhites for the remaining whites in the district will rise. The relationship between fraction white and the dissimilarity index is ambiguous; in general, it depends on whether whites who depart were in schools that were more or less integrated than the average school in the district.[17]

Enrollment by Race

I consider the log of total white enrollment in the district to assess whether desegregation plans led to losses in white enrollment or "white flight." I also consider the effects of plans on the log of nonwhite enrollment.[18] Ultimately, it is the fraction of enrollment that is white that influences the exposure of nonwhite students to white students. However, desegregation plans were likely to have influenced white and nonwhite enrollments differently. Therefore, I consider the effects of the plans on white and nonwhite enrollment directly.

[17] A simple example illustrates the relationship among these measures: Consider a perfectly-integrated district (a dissimilarity index equal to 0) that is 70 percent white. Since every school is representative of the racial composition of the district, nonwhite exposure to whites is 0.7 and white exposure to nonwhites is 0.3. Now assume that whites leave schools in the district in a uniform way until the fraction white is 50 percent. Since the whites left the district uniformly, every school is still representative of the racial composition of the district, so the dissimilarity index is still 0. However, nonwhite exposure to whites has fallen to 0.5 and white exposure to nonwhites has risen to 0.5, reflecting the decline in the fraction white in the district.

[18] The value of the schools in these districts from the perspective of nonwhite students may be increased or decreased by the plans. On the one hand, nonwhites were often transferred to formerly white schools, which probably had better facilities. On the other hand, plans often involved long bus rides and other disruptions, and nonwhites were not always welcome at their newly assigned schools. In addition, to the extent that whites moved out of districts with desegregation plans, property values are expected to fall, possibly attracting more nonwhite families.

Because these data are for public school enrollment – rather than residence – in the district, I cannot distinguish between white enrollment changes due to exit from the district as opposed to private school entrance. Consistent data on private school enrollment in these districts are not available for this period. I therefore consider only the decision of whether to be in the public school in the affected district or not. The effects on integration in the public schools are the same whether whites who left (or did not come) due to the plan ended up in a suburban district or a private school.[19]

Summary Statistics

Table 1a presents the means of the outcome variables for 1968, 1970, 1980, and 1995. To maintain a consistent sample across years, I include only the 95 districts that had not yet implemented a plan in 1968 and have data available for each of the years shown. The general trends here are consistent with previous research and case studies establishing a correlation between desegregation plans and reductions in segregation *within* districts. White enrollment also fell over this period.

In 1968, these districts were highly segregated according to all the measures: The dissimilarity index averaged 0.71, the average nonwhite's school was only 28 percent white even though the average district was nearly 70 percent white. Schools in the Midwestern and Southern districts were significantly more segregated than Northern and Western districts; the fraction white was similar in all regions (Table 1b).

[19] The policy implications could be different depending on whether whites left for private or alternative public districts. For example, including nearby districts in the desegregation plan may have reduced white flight if nearby public school districts were more important (as the results in Section VII suggest). If whites who left went to private schools, the decision to exclude suburban districts is less relevant. Effects on property values in the district may depend on whether public school departures represent flight to private or alternative public schools, depending on preferences.

Southern districts that had not yet implemented a plan in 1968 were not perfectly segregated as they were before *Brown*. This suggests that the removal of laws mandating segregation and some of the smaller early plans did reduce segregation somewhat before the major plans I consider here were implemented.[20] In fact, in 1968 Southern districts look quite similar to Midwestern districts in terms of segregation.

Between 1968 and 1970, segregation (by all measures) began to fall, as the first districts began to adopt major plans; white enrollment was steady. During the 1970s, the average dissimilarity index fell substantially, indicating increasing integration. However, as measured by the exposure of nonwhites to whites, integration rose only slightly – from 37 to 43 percent – between 1970 and 1980. By 1995, nonwhite exposure in these districts had fallen below its 1970 level to 34 percent. White enrollment and the average fraction white in districts fell steadily after 1970. The regression analysis below shows that desegregation plans played an important role in explaining these trends – reducing within-district segregation, but also white enrollment, substantially.

V. Identifying Average Effects of Plans

I use the timing of plan implementation across districts to identify the effect of court-ordered desegregation plans as distinct from trends due to other factors. Figure 1 above shows the substantial variation in the timing of desegregation plan adoption. In the simplest case, one could use a difference-in-differences (DD) approach, estimating equations of the following form:

$$(1) \quad y_{it} = \alpha + \theta_i + \gamma_t + \lambda \times AFTER_{it} + \varepsilon_{it},$$

[20] For a few districts in the sample that have data available for years in the early 1960s, the data indicate that segregation was complete at that time.

where y_{it} is an outcome variable (one of the segregation measures or log of white enrollment), α is a constant, θ_i is a district fixed effect, γ_t is a year fixed effect, and $AFTER_{it}$ is a dummy variable indicating that district i has a plan in place in year t.

Equation (1) assumes that desegregation plans caused a parallel shift in trends, but that is not necessarily the case. For example, if a plan was successful in integrating the schools, but whites responded by leaving the district in the years following implementation, nonwhite exposure to whites would rise in the short term but then fall over time as whites leave. The simple DD estimator cannot capture such dynamics.

Therefore, instead of a simple "after" indicator, I use a series of dummy variables indicating time relative to implementation to estimate the dynamic effects of desegregation plans.[21] The estimating equation is straightforward in this case:

$$(2)\quad y_{it} = \alpha + \theta_i + \gamma_t + \sum_{k=-6}^{15} \lambda_k \delta_{k,it} + \varepsilon_{it}.$$

As before, α is a constant, θ_i is a district fixed effect and γ_t is a year fixed effect. $\delta_{k,it}$ is a indicator variable equal to 1 if district i is k years relative to its implementation year in year t. ε_{it} is an error term. In some specifications, I control for calendar year (t) parametrically rather than with year fixed effects; the results from both specifications are nearly identical in most cases. The time-since-implementation dummy variables ($\delta_{k,it}$) are capped at -6 years and 15 years: All years less than -6 are included in the -6 category and all years greater than 15 years are included in the plus 15 category.[22] The omitted

[21] This approach has been utilized in a variety of contexts, for example Jacobson, Lalonde, and Sullivan (1993), Grogger (1995), and Stevenson and Wolfers (2000).

[22] The district with the most years before implementation has 13 years of "pre" data; 36 is the maximum number of "post" years. About 35 percent of districts have at least 6 years of pre data, and over three-quarters of districts have at least 15 years of after data.

category is the last year prior to plan implementation. I estimate equation (2) using OLS.[23]

The pattern of the λ_ks describes the change in the trend in the left-hand-side variable associated with plan implementation. For example, $\lambda_1 - \lambda_0$ is the expected change in the dependent variable associated with moving from time 0 to time 1 (the first year of plan implementation), controlling for calendar year.

Not all districts have data available for each year relative to implementation (k). Districts that implemented earlier in the period necessarily have fewer years of data before implementation, and districts implementing later have fewer years of data after implementation. Thus, the composition of districts identifying the λ_k coefficients varies with k. If treatment effects are heterogeneous, the pattern of λ_ks could reflect the changing composition of districts in addition to the dynamics of the average treatment effect. I therefore present the results estimating equation (2) separately for the sample of districts with data available for at least 6 years before and 15 years after implementation. This reduces the sample of districts significantly, from 108 to 37.

Intuitively, this approach captures the extent to which districts that implemented desegregation plans earlier experienced earlier declines in segregation and white enrollment, compared with those having later plans. A variety of factors other than desegregation plans can influence the outcome variables considered here, for example, changes in housing policy, crime rates, employment opportunities, or district policies. As long as these factors are not *systematically* related to the year of desegregation plan

[23] I drop data for years before 1966 since very few districts have data in these very early years.

implementation, they will be picked up in the year effects and the time-since-implementation coefficients will reflect the causal effect of desegregation plans.[24]

The identifying assumption is that, absent plan implementation, the outcome variables of interest would have trended similarly in districts implementing at different times. One way of assessing the validity of this assumption is to check for pre-existing trends. If the timing of adoption is unrelated to underlying trends and individuals do not respond before implementation, there should be no trend in the λ_ks for $k \leq 0$. However, because families could have responded in anticipation of plan implementation, this may not be expected to hold for all outcomes. I discuss this further in the next section.

VI. Results: Average Effects on Segregation and Enrollment

The results of estimating equation (2) for measures of segregation provide strong evidence that desegregation plans increased integration substantially in the short run. The estimates indicate that plans reduced segregation in the long run as well; however, the magnitude of the estimated effects depends on the particular measure of segregation considered. I report the results graphically; the coefficients and standard errors are in Appendix A.

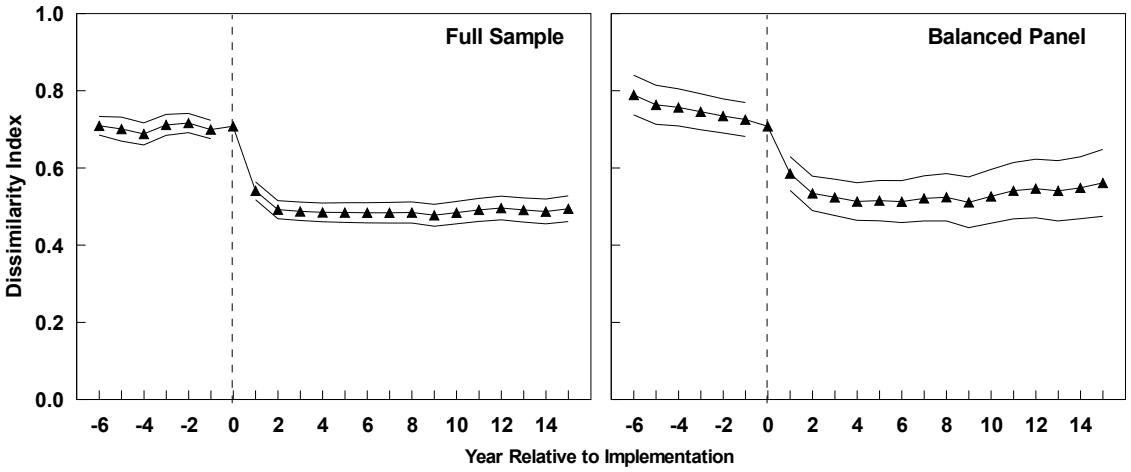
Dissimilarity Index

The λ_k coefficient estimates for the dissimilarity index, with and without controls for calendar year, are presented in Figure 2.[25] I normalize so that the outcome variable is equal to the average for the sample in 1968 (as reported in Table 1a) at time 0 (the last year before implementation and the omitted category in the regressions). I also plot the

[24] It is possible that desegregation plan implementation will be related to other outcomes empirically, but this is not necessarily a violation of the identifying assumptions. For example, if desegregation plans cause white flight which increases crime, we would observe a relationship between plans and crime.
[25] Table A.1 reports the coefficients and standard errors.

95 percent confidence interval around the coefficients. Because the last year before implementation is omitted, the confidence intervals are for the difference relative to that year.

Figure 2. Average Effects of Desegregation Plans on Dissimilarity Index



Notes: Chart plots coefficients and 95%CI from equation (2). Values are normalized so dependent variable is equal to the average value in 1968 in year 0 relative to implementation. Balanced Panel includes only districts that have data for at least 6 years before and 15 years after implementation. (See Table A.1.)

For the full sample (left panel), dissimilarity index was unchanged in the years leading up to implementation. Dissimilarity then fell substantially – by about 0.22 – in the first two years after plan implementation.[26] For the balanced panel, the estimates are similar, although there is a slight decline in the dissimilarity index in the years before implementation.[27] These estimates provide strong evidence that districts "complied" with the orders. On average, plans caused large reductions in within-district segregation. The fact that the dissimilarity index was unchanging in the years before adoption indicates that districts made little effort to reduce segregation before the plans were implemented and that implementation timing was not systematically related to underlying trends in the dissimilarity index as would be the case if, for example, plans tended to be implemented

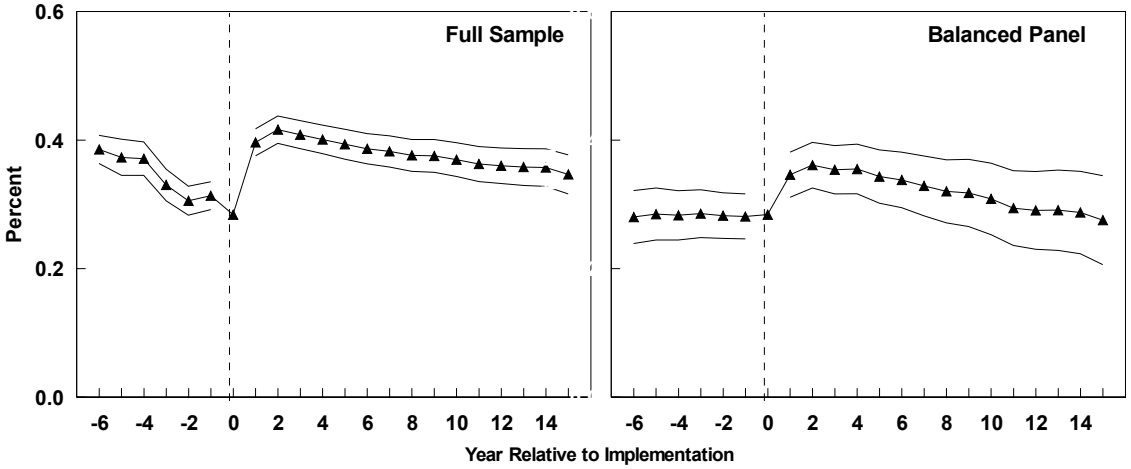
[26] These reductions are highly statistically significant, with t-statistics between 10 and 15.
[27] This decline in the pre-period is statistically significant, although it is much smaller than the reduction following plan implementation.

only after segregation started falling for other reasons or if plans were a response to rising segregation. The finding is not surprising, for had these districts been in compliance with *Brown*, the courts would not have stepped in.

Nonwhite Exposure to Whites

For the full sample, nonwhite exposure declined in the several years leading up to plan implementation (Figure 3, left panel), suggesting that white enrollment may have responded before major plan implementation. According to the estimates for the full sample, desegregation plans increased nonwhite exposure by 11 percentage points in the first 2 years after implementation, relative to the final year before implementation.

Figure 3. Average Effects of Desegregation Plans on Nonwhite Exposure



Notes: Chart plots coefficients and 95%CI from equation (2). Values are normalized so dependent variable is equal to the average value in 1968 in year 0 relative to implementation. Balanced Panel includes only districts that have data for at least 6 years before and 15 years after implementation. (See Table A.2.)

To the extent that the downward trend in exposure reflects an anticipatory white enrollment response to the plan, this is an overestimate of the short-term effect; the change from 3 years before to 2 years after implementation was only 6 percentage points. After the initial increase, exposure trended down. The estimated decline from 2 to 10 years after the policy was about 4 percentage points. In other words, more than one-third

of the initial increase in exposure was subsequently offset by white enrollment losses over time.

Estimates for the 37 districts in the balanced panel sample (right panel) also show a jump in nonwhite exposure following plan implementation, although the estimates are less precise. For this sample, nonwhite exposure was flat in the years leading up to implementation. The differences in the pre-implementation years between the full sample and the balanced panel sample could be due to compositional changes in the districts identifying the coefficients for different years relative to implementation. Alternatively, it is possible that on average in the full sample, districts did experience anticipatory white flight whereas the subset of districts in the balanced panel did not. Ultimately, the lack of more data in the years leading up to implementation for some districts in the sample make estimating the trend in the years before implementation difficult.

For both samples, however, there is a clear increase in exposure when the policy is implemented, and the long-term effect of the plans on nonwhite exposure is reduced due to white flight. The estimates for the 10-year change in nonwhite exposure to whites range from a statistically insignificant 2.4 to 8.6 percentage points, compared to 7.7 to 13.2 percentage points in the short-term. I explore the causes of white enrollment declines in greater detail in the next section.

White exposure to nonwhites

For those whites who remained in the affected districts, the plans were associated with large increases in exposure to nonwhites (Table A.3), as the reduction in within-district segregation and white enrollment losses were reinforcing. The 2-year increase in white exposure was a statistically significant 10 percentage points for both the full

sample and the balanced panel. White exposure continued to rise, reflecting continuing white enrollment losses.

Enrollment by Race

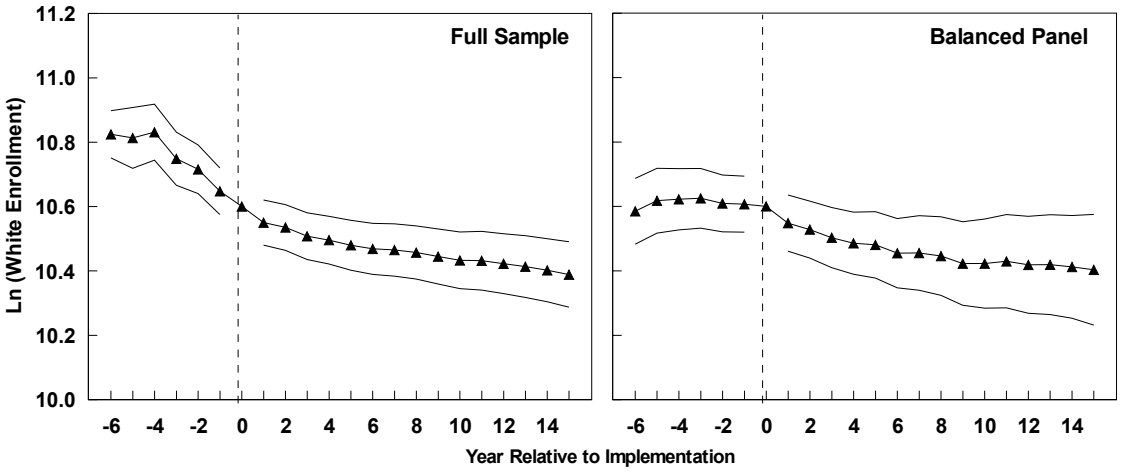
While desegregation plans reduced segregation in the short run according to all the measures, the long-run effect depends more on the measure considered. According to the dissimilarity index, segregation was flat leading up to plan implementation and fell dramatically following adoption. Exposure of nonwhites to whites also registered a substantial reduction in segregation after adoption. But there was significant reversion in this measure of segregation. White exposure to nonwhites rose after implementation and continued to rise. The decline in white enrollment following desegregation plans explains the differences in trends in segregation as captured by these different measures.

Figure 4 shows the results of estimating equation (2) for the log of white enrollment. Plan implementation is associated with a reduction in white enrollment of about 9 log points in the first 2 years, rising to about 17 points after 10 years.[28] The coefficients on the time-since-implementation variables are statistically different from the coefficient for time 0 at conventional levels by 4 years after the policy. [29]

[28] Coefficients and standard errors are presented in Table A.4. Results are qualitatively similar if percent white, instead of log of white enrollment, is considered as the dependent variable.

[29] In results not reported here, I estimate equation (3) with the log of nonwhite enrollment as the dependent variable. The point estimates indicate that log of nonwhite enrollment generally trended up before plans were implemented and continued to do so at a similar rate. This may be due to improvements in school quality for nonwhites in these districts or falling housing prices due to the departure of whites. However, the estimated coefficients on the time-since-implementation indicators are quite noisy; the trend is not statistically different from zero (t-statistics are generally less than 1).

Figure 4. Average Effects of Desegregation Plans on Log White Enrollment



Notes: Chart plots coefficients and 95%CI from equation (2). Values are normalized so dependent variable is equal to the average value in 1968 in year 0 relative to implementation. Balanced Panel includes only districts that have data for at least 6 years before and 15 years after implementation. (See Table A.4.)

As for nonwhite exposure, the estimates for the full sample indicate that white enrollment started to decline about 3 to 4 years before plans went into effect. Whites might have expected the plan and moved away (or failed to move into the district) in anticipation. I use the date of the first "major" plan. The results for the dissimilarity index indicate that this is a fairly good measure of a "plan," however, Welch and Light identify non-major plans implemented before the major plan in about half of the districts. The average time between the first plan and the major plan is 2 to 3 years.[30] Even when there were no prior plans, these policies often took years to develop and be approved by the courts.[31]

To the extent that the downward trend in white enrollment reflects a response to the plan in anticipation of its implementation, the change measured from time 0 will

[30] Limiting the sample to the 52 districts that did not have any plans before the "major" plan, the point estimates in the pre-period show less of an anticipatory decline. The estimates are quite noisy, however, so it is difficult to draw conclusions about the role of early plans based on this analysis.

[31] For example, as described in Section VIII, the first case in Memphis was filed in 1960, but the plan was not implemented until 1973. Logistical problems and continuing legal wrangling sometimes prevented approved plans from being implemented right away. Prior to desegregation, Memphis had neighborhood schools and no buses; before the plan could be put into force, the district had to not only get buses, but also

underestimate white flight due to the plans. White enrollment fell by an estimated 15 log points in the 3 years before implementation, according to the estimates for the full sample. However, the declines in the early years should be interpreted with caution since many districts have data available for only one or two years before implementation. As discussed above, this trend could reflect compositional changes in the sample of districts if treatment effects are heterogeneous (as evidence presented in the next section suggests).

The magnitude of these coefficients is substantial, suggesting that desegregation plans reduced white enrollment by about 18 to 30 log points over 10 to 15 years. Still, the plans do not appear to have been the primary cause of white flight. For comparison, average white enrollment fell by about 50 log points between 1970 and 1980 and by another 30 points between 1980 and 1995 (Table 1a); the estimated reduction in white enrollment after 10 years is about one-third the average reduction for these districts over the 1970s.

Evidence presented in Section VII provides additional support for the conclusion that these plans were important contributors to white flight. Controlling for a variety of district characteristics, white flight *around the time of plan implementation* was more substantial for districts with plans that achieved larger short-term reductions in segregation. Further, the effect was stronger for districts in metropolitan areas with more nearby alternative school districts. If the estimates here were simply picking up general trends in white enrollment, we would not expect features of desegregation plans or the

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negotiate with the drivers unions (who were concerned not only about wages and hours, but also safety) (Egerton, 1973).

supply of alternative school districts to be important predictors of white flight around the time of plan implementation.

Overall, the success of court-ordered plans was mixed. While the plans were quite successful at reducing segregation within the affected districts, they were also important contributors to white flight from these districts. These white enrollment declines substantially, although not entirely, offset the within-district desegregation efforts.

VII. Determinants of White Flight and Long-Term Plan Effectiveness

The results presented thus far reflect the average effects of desegregation plans for the sample. However, as Table 2a demonstrates, the success of plans varied considerably across districts. For example, nonwhite exposure to whites increased on average by about 6 percentage points in the ten years following plan implementation, with a standard deviation of 14 percentage points. Changes in white enrollment also varied considerably: The average ten-year loss was 23 log points, with a standard deviation of 31 points.[32]

Why were some districts more effective in increasing nonwhite exposure to whites? Understanding why some had more white flight than others is critical to answering this question. To evaluate empirically the contribution of these different factors to white flight, I examine the relationship between long-term changes in white enrollment following plan implementation and pre-existing district characteristics, as well as features of the desegregation plans.

Determinants of White Flight: Supply and Demand for Alternative Schools

How whites responded to a court-ordered desegregation plan depended on both the demand for and supply of alternatives – that is, how much they disliked the plan and

[32] Standard deviations are based on residual changes; see below for a description of how these are constructed. Mean changes are the changes implied by the coefficients in equation (3). See Table 2a.

their ease of moving or paying for private schools. From the perspective of white families, a desegregation plan affected the quality of the schools along a number of dimensions. The plans increased contact of white students to nonwhite students. Desegregation plans often required children to travel to schools outside their neighborhoods or to attend schools with inferior facilities. These factors all work to increase demand for alternative schools among whites.

Before *Brown*, demand for segregated schools could be more easily satisfied within a school district, either with explicitly segregated schools as in the South or by drawing attendance zones to produce segregation. After a district had a court-ordered plan, however, this was no longer possible. To avoid going to an integrated school, white families would have to move to another district or enroll their children in private school.[33] For historical reasons, some metropolitan areas already had many districts, and suburban districts were generally not part of desegregation plans. Therefore, the availability of alternative nearby public school districts varied considerably. Some affected districts covered an entire metropolitan area, while others were surrounded by hundreds of alternative public school districts. Furthermore, some districts had more extensive existing private school systems.

Determinants of White Flight: Empirical Evidence

To assess the importance of these factors in explaining the variation in white flight, I relate *changes* in white enrollment, following plan implementation, to preexisting characteristics of districts. I consider the effects of region and initial school segregation

[33] Due to legal constraints, creating new districts to satisfy demand for segregation was not an option. See Alesina, Baqir, and Hoxby (2000).

as proxies for attitudes,[34] the availability of other public school districts in the metropolitan area, and the extent of the private school system before plan implementation.

To estimate how much white enrollment changed in a district during the decade after implementing a plan, *relative to the ten-year change for the average district as estimated above*, I use the residuals from estimating equation (2) with the log of white enrollment at the dependent variable.

The dependent variable is

$$\Delta WhiteEnroll_i = \frac{(\hat{\varepsilon}_{i,8} + \hat{\varepsilon}_{i,9} + \hat{\varepsilon}_{i,10})}{3} - \frac{(\hat{\varepsilon}_{i,-2} + \hat{\varepsilon}_{i,-1} + \hat{\varepsilon}_{i,0})}{3},$$

where $\hat{\varepsilon}_{i,k}$ is the residual for district *i* in year *k* relative to plan implementation. Because equation (2) includes calendar year fixed effects, *ΔWhiteEnroll* reflects the change in white enrollment compared to what would have been expected given the change in calendar year and the average ten-year change for all districts following implementation. Negative values indicate that a district had more white flight than average and vice-versa.

I estimate the following equation for the 80 districts that have all the necessary data:

$$(3)\ \Delta WhiteEnroll_i = \beta_0 + \beta_1 \times PublicDistricts_i + \beta_2 \times \%PrivateSchool_i + \beta_3 \times InitialSegregation_i + \beta_4 \times NE_i + \beta_5 \times MW_i + \beta_6 \times WEST_i + X_i\beta_7 + \varepsilon_i,$$

where *PublicDistricts* is the log of the number of public school districts in the metropolitan area surrounding district *i,*[35] *%PrivateSchool* is the percent of students in

[34] Direct measures of attitudes are not available. The General Social Survey asks a variety of questions relating to attitudes towards blacks and busing in particular; however, this survey did not start until the 1970s, and the samples at the metropolitan level are very small.

the district's city that were enrolled in private schools in 1960, and *InitialSegregation* is the dissimilarity index for the district before it implemented its plan. *NE*, *MW*, and *WEST* are dummy variables for the Northeast, Midwest, or West; South is the omitted category. *X* is a vector of control variables, including the initial white share of enrollment, log of initial enrollment, residential segregation (for the district's county), district area, district density, the manufacturing share of employment in the district's city in 1960, and the percent change in population from 1960 to 1970 for this district's city.[36]

In some specifications, I add variables related to features of the plan and the short-term change in segregation produced by the plan as discussed below. All variables are described in greater detail in Appendix B. Summary statistics for the explanatory variables are in Table 2b.

The continuous explanatory variables are divided by their sample standard deviation. The coefficients can therefore be interpreted as the change in the left-hand-side variable associated with a one standard deviation change in the right-hand-side variable.

The results of estimating equation (3) are presented in Table 3. The availability of other public school districts in the same metropolitan area is an important predictor of white flight in all specifications; an increase of one standard deviation in the number of public school districts is associated with an additional reduction in white enrollment of

[35] I use the number of school districts in 1990 rather than before plans were implemented. The coefficient could be biased if more districts were created in response to a desegregation plan. However, districts were not allowed to divide, so this is not a problem.

[36] For some districts, the desegregation plan would be expected to have caused some population loss by 1970, so the change in population from 1960 to 1970 is endogenous. The results are similar if this variable is excluded or districts that implemented a plan before 1970 are excluded.

about 8 to 10 log points over the following decade – equal to almost half the average ten-year change associated with desegregation plan implementation.[37]

One might be concerned that the declining industrial metropolitan areas of the Midwest tended to have large numbers of school districts and that court-ordered districts in these cities would have experienced more white flight even without a desegregation plan. However, equation (4) includes region fixed effects, the percent of employment in manufacturing in 1960, and the population decline between 1960 and 1970.[38] None of these coefficients is statistically significant or large in magnitude, nor does including these variables change the coefficient on the number of nearby school districts. This suggests that it was the availability of outside options, rather than these other factors, that increased white flight.

There is little evidence that variation in the extent of the existing private school system explains the variation in white enrollment losses from public districts under court order. While cities with a more extensive private school system may have been better able to absorb students leaving districts under court order, the coefficient on percent in private schools is *positive* in the main specification, indicating that areas with a more extensive existing private school system experienced *less* flight from the public schools upon plan implementation. This coefficient is insignificant and small in magnitude,

[37] This is likely an underestimate of the effect of the availability of nearby public school districts. Large districts might be expected to experience *more* white flight, as these plans would involve moving students longer distances and be more disruptive. However, the coefficient on district area is positive and significant. The positive coefficient on district area is most likely also picking up some of the effects of outside options, as metropolitan areas with large numbers of districts also tend to have smaller districts. If this variable is excluded from the regression, the coefficient on *PublicDistricts* increases by about 50 percent, while the other coefficients are largely unaffected.

[38] Each district is assigned the percent of employment in manufacturing and population decline for its city (not metropolitan area), as described in Appendix B.

however. These results suggest that other public school districts, not private schools, are the most important outside option in explaining the variation in white enrollment losses.

Region and initial school segregation are not important predictors of white flight, controlling for other factors. None of the region dummies is independently statistically significant or large, nor are the West, Midwest, and Northeast jointly significantly different from the South. High initial segregation may reflect strong tastes for segregation, so districts with high starting segregation might be expected to have experienced more white flight, controlling for other factors. Surprisingly, however, the coefficient on the initial level of segregation in the schools (measured by the dissimilarity index) is small and insignificant.[39] Initial white share of enrollment was an important predictor of flight, the coefficient is large and statistically significant in all specifications. White enrollment fell more after implementation if the white share was already low.[40]

Features of desegregation plans might also have influenced white flight, both because different types of plans produced different changes in segregation and because some methods, such as busing, may have led to more white flight independent of the change in segregation. Further, plan features could be correlated with the explanatory variables in equation (3). Therefore, I add a series of variables describing features of the district's desegregation plan. The four "Plan Features" dummy variables indicate whether the major plan implemented in the district employed each of four: rezoning, pair and

[39] As Table 1b indicates, the South and Midwest were initially more segregated than the Northeast and West, so the region dummies and initial segregation are collinear; the region and initial segregation variables are also jointly insignificant, however.

[40] This is consistent with preferences that are nonlinear in the nonwhite share of enrollment. In theory, one could assess whether white enrollment was nonlinear in the nonwhite share of enrollment at the school level (for example, there may be tipping points). Such analysis is outside the scope of this paper.

cluster, magnet schools, and other voluntary;[41] many districts employed more than one method. The results are reported in column (2).

None of the plan type variables is significant, nor are they jointly significant. These four variables are highly correlated with each other; for example, districts that used pair and cluster rarely had other voluntary as well. When only the pair and cluster variable is included (column (3)), the coefficient is negative and statistically significant. This method seems to be the most important of the plan features in predicting white flight. In both specifications, the coefficients on the other variables are largely unchanged, although the negative effect of the number of nearby school districts is slightly stronger in this specification.

Plan features are expected to influence white flight in large part because of differences in how much they change whites' exposure to nonwhites (since whites are more likely to leave if the racial composition of their schools changes more). In theory, one could disentangle the effects of the change in segregation and the methods used to achieve it by adding the change in white exposure caused by the plan to the regression in columns (2) and (3). However, the observed change in white exposure to nonwhites reflects not only the effects of the policy, but also whites' response to it and is therefore endogenous.[42]

Instead, I instrument for the observed short-term change in white exposure[43]– around the time of implementation–with the plan feature variables. As described above,

[41] Voluntary plans were mandatory from the perspective of the district, but parents were allowed some choice of school.
[42] Ideally, I would include the change in white exposure that would have resulted based on the rules of the plan, assuming no behavioral response. Such detailed information is unavailable, however.
[43] The short-term change in white exposure to nonwhites (*STΔWhiteExposure*) is constructed using the residuals from estimating equation (3) for white exposure to nonwhites (Table A.4). It is the difference in

the plan features may affect white flight not only through the change in white exposure to nonwhites, but also directly.[44] The plan feature variables may therefore not be valid instruments, so the coefficient on the change in white exposure to nonwhites cannot necessarily be interpreted as causal. Rather, the change in white exposure can be interpreted as a particular parameterization of the plan features–a summary of how "onerous" the plan was. I estimate this specification using 2SLS; the results are reported in Column (4) using all four plan feature variables and in Column (5) using only the pair and cluster indicator. In the first stage, the plan features are strong predictors of the short-term change in whites' exposure to nonwhites. [45]

As predicted, white flight was more severe in districts implementing plans using methods that produced larger increases in whites' exposure to nonwhites. The coefficient is negative, statistically significant, and similar in magnitude to the effect of the number of nearby public school districts. A one standard deviation change is associated with a reduction in white enrollment of 11 log points.

Determinants of Long-Term Changes in Nonwhite Exposure to Whites

Ultimately, increasing contact between whites and nonwhites in schools was an important goal of desegregation plans, so the effect of plans on *nonwhite exposure to whites* – that is, the percent white in the average nonwhite's school – is of interest for

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the average residuals for the three years before implementation and the residuals for the three years after plan implementation:

$$ST\Delta WhiteExposure_i = \frac{(\hat{\varepsilon}_{i,1} + \hat{\varepsilon}_{i,2} + \hat{\varepsilon}_{i,3})}{3} - \frac{(\hat{\varepsilon}_{i,-2} + \hat{\varepsilon}_{i,-1} + \hat{\varepsilon}_{i,0})}{3}.$$

The results are similar if the actual change is used or if the change for a shorter period around implementation is considered.

[44] For example, Rossell (1990) argues that plans using magnet schools caused less white flight.

[45] The partial F-statistic for the instruments in the first stage is 8.1 for the regression reported in Column (4); the t-statistic for the instrument (pair and cluster) in the first stage is 5.7 for the regression reported in Column (5).

policy. I therefore estimate equation (3) with the 10-year change in nonwhite exposure – constructed in a similar manner to the long-term change in white enrollment – as the dependent variable. The results are presented in Table 4. As the results for white enrollment above would suggest, starting with a higher white share of enrollment is predictive of a plan that is more successful at increasing nonwhite exposure to whites. The availability of nearby public school districts is also important. Districts with few nearby alternatives sustained larger increases in nonwhite exposure – a one standard deviation increase in the number of districts reduces the long-run change in nonwhite exposure by 5 percentage points.

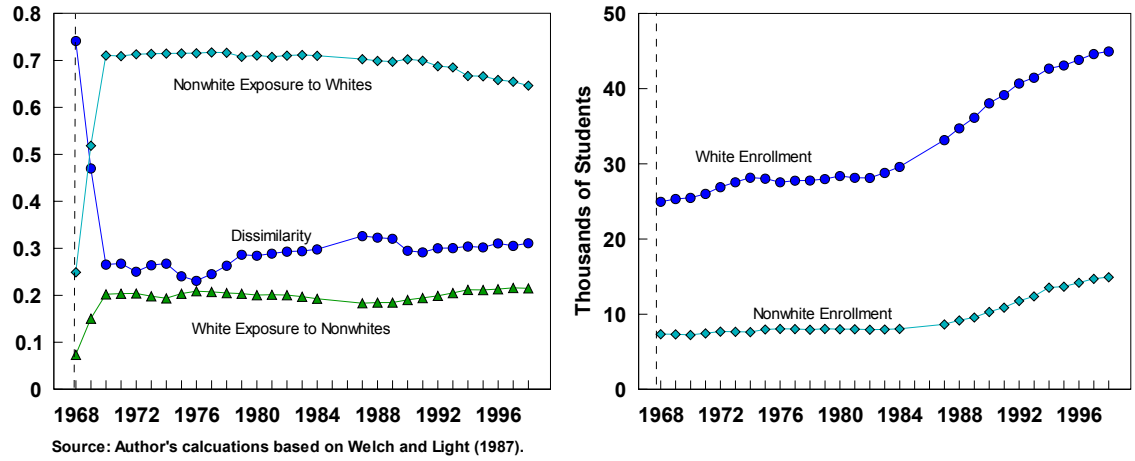
VIII. Outside Options White Enrollment and Plan Success: Two Districts' Stories

Both the Memphis, Tennessee and Volusia County, Florida districts are in the historically legally segregated South. But prior to desegregation, Memphis's enrollment was nearly half black, and the district was surrounded by ten other public school districts. In contrast, Volusia county school district covered the entire Daytona Beach metropolitan area and had enrollment that was just over 20 percent nonwhite. Memphis was unable to maintain meaningful integration due to significant white flight, while Volusia county sustained long-term reductions in segregation over many years.

Volusia County, Florida

Figure 5 shows trends in measures of segregation and enrollment by race. Before plan implementation, the schools were highly segregated by all measures; the dissimilarity index was 0.74, about average for a Southern district that had not yet implemented a plan in 1968 (Table 1b).

Figure 5. Trends in Segregation and Enrollment, Volusia County Florida



Source: Author's calcuations based on Welch and Light (1987).

The average nonwhite was in a school that was about 24 percent white, while the average white was in a school that was only 7 percent nonwhite. Fourteen of the district's 60 schools were virtually 100 percent nonwhite, and there were 27 nearly all-white schools (averaging 97 percent white).

The plan was phased in starting in 1969, reducing dissimilarity by about 0.47 and increasing nonwhite exposure by 46 percentage points in 2 years. These changes persisted for decades. Meanwhile, both nonwhite and white enrollment were stable or growing slightly. Desegregation was maintained even as the white and nonwhite populations both grew substantially in the 1980s.

How did Volusia County achieve these substantial long-term improvements in integration? Both the demand factors, such as the number of reassignments required, and the supply factors, such as the availability of outside options, worked in favor of a successful plan in this case.

The school-level data indicate that the plan involved the closing of a large number of nonwhite schools (presumably the most run-down schools), reassigning most nonwhite students to predominately white schools, and moving a small number of whites to the

remaining predominately nonwhite and mixed schools.[46] This strategy ensured that most white students did not experience large changes in the racial composition of the school they attended, very few were actually reassigned, and many were largely unaffected.

While a large fraction of nonwhite students appear to have been reassigned, this group was small compared with the number of whites in their new schools. About 18,000 of the district's 25,000 white students were in schools that experienced an increase in the nonwhite enrollment share of less than 10 percentage points. Even the small number of whites who were most affected – those who were reassigned to nonwhite schools – were in schools that were 58 percent white on average.

In short, the shortage of nearby alternatives and the relatively small impact on whites appear to have ensured the success of Volusia County's desegregation plan in achieving long-run increases in exposure.

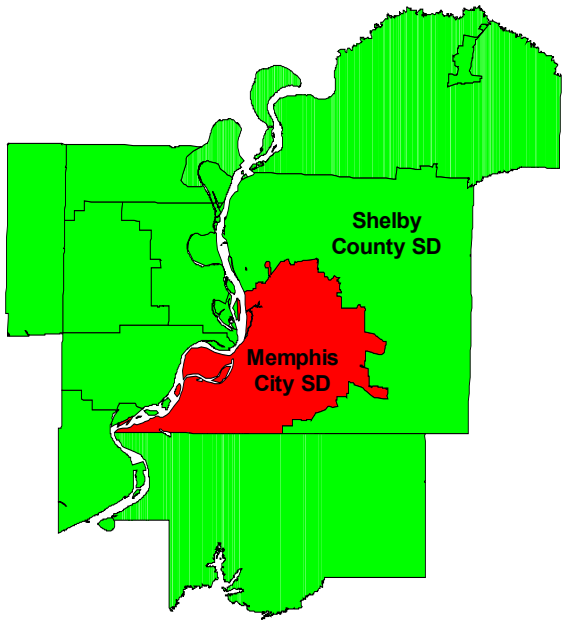
Memphis

In contrast to Volusia County, the Memphis City school district was almost evenly split between blacks and whites in the years before the implementation of a major

[46] I cannot follow individual students, so I do not know exactly what reassignments the plan required. However, I can see how enrollment by race changed in different types of schools. Because enrollment is relatively stable in Volusia County, I can infer what types of reassignments the plan probably entailed. Of the 14 nonwhite schools in 1968, 8 were closed by 1970; the number of nonwhite students attending those schools fell by about 3,500, from nearly 5,000. Nonwhite enrollment in formerly white schools (90 percent or more white in 1968) rose by about the same amount, suggesting that students from the closed schools were bused to formerly white schools. At the same time, white enrollment in formerly white schools fell by about 2,200 and white enrollment in formerly mixed schools fell by 1,100, although this represents only a small fraction of the 25,000 white students in the district. Those 3,300 white students appear to have been split about evenly between the remaining nonwhite schools and new schools. (New schools are those that did not appear in the data in 1968, before the plan was implemented. It is possible that some of them are related to the nonwhite schools that closed. However, they have changed name and identification number; further, there are no obvious matches in terms of size and racial composition. information on the location of the schools is not available; however, even if a "new" school is using the same building as a "closed" school, these changes suggest that it is sufficiently reorganized that it can be considered a new school.) White enrollment in nonwhite schools rose by 2,161 and white enrollment in new schools rose by 1,761, compared to reductions in white and mixed schools of 2,257 and 1,140, respectively. Total white enrollment in the district rose by 527 between 1968 and 1970. In districts where white enrollment is

desegregation plan in January 1973. The city of Memphis was one school district, while the much-whiter remainder of Shelby County was a separate district. There were 10 other school districts in the Memphis metropolitan area (see Figure 6). These facts, combined with a desegregation plan that left virtually no white school untouched, contributed to large and immediate white flight from the public school system in Memphis, largely undermining the improvements in nonwhite exposure.

Figure 6. Memphis Metropolitan Area School Districts



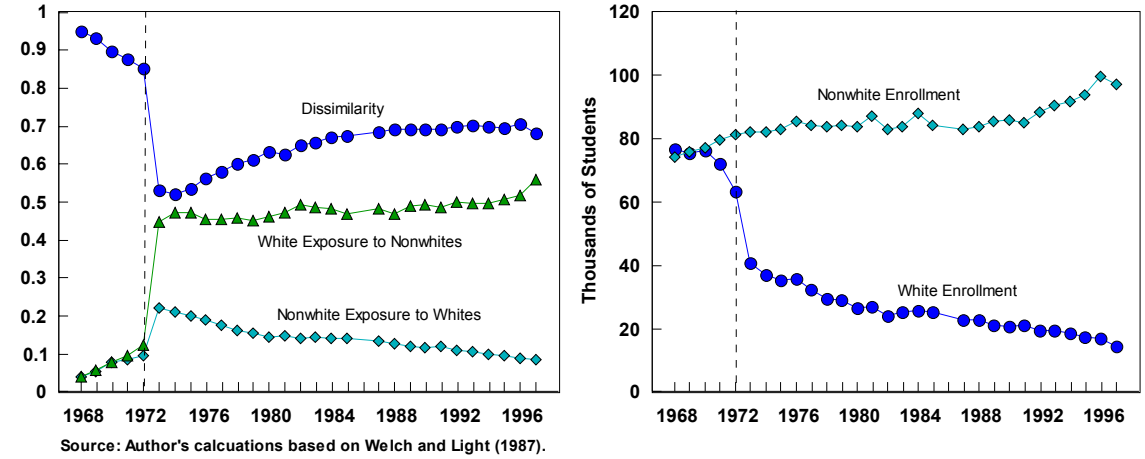
Memphis' history of desegregation involved a long and drawn-out legal process that gave white families ample time to change their behavior in anticipation of plan implementation. When the NAACP Legal Defense and Education Fund (LDF) filed the

declining, it is much more difficult to infer the reassignments, because does not know where the departing students would have been assigned had they stayed in the district.

first suit in Memphis in 1960, the district was still fully segregated. The full-scale plan was not finally implemented until January 1973.[47]

Figure 7 shows trends in segregation and enrollment for the Memphis school district. The dissimilarity index was falling slowly in the several years prior to plan implementation, from nearly perfect segregation in 1968; this trend reflects the small amounts of integration resulting from the early plans.

Figure 7. Trends in Segregation and Enrollment, Memphis



Source: Author's calcuations based on Welch and Light (1987).

A large, one-year drop in dissimilarity and increase in both nonwhite exposure and white exposure is evident in 1973, the first full year after the plan. This was accompanied by a significant drop in white enrollment; thus, nonwhite exposure to whites did not rise as much as it otherwise would have and began falling immediately after plan implementation.

[47] In 1962, a small number of black first graders were enrolled in previously all-white schools; by 1967, there was some integration in all grades. Still, integration was clearly token: The dissimilarity index was nearly 0.95 in 1968. In 1968, the LDF sought further action, arguing that the district was still predominately segregated. The integration of the faculties was ordered in 1969, and after the 1971 *Swann* decision, the appeals court ruled that Memphis had not done enough. First the Department of Health, Education, and Welfare (HEW) began drafting a plan, but the department was removed from the project by Washington and the district picked up where they left off. The District judge in charge of the case chose the plan involving the least amount of busing in the Spring of 1972, but appeals and stays postponed its implementation until January 1973. See Egerton (1973) for further information.

White enrollment fell by nearly 23,000 – over 35 percent – in a single year when the plan was implemented. This example shows how whites leave the public schools in anticipation of and in response to desegregation plans, although the white enrollment decline in Memphis was atypically sharp.

Where did these white students go? In the short term, it appears that private schools played an important role. Catholic schools are typically considered the most affordable private school alternative for less wealthy families. However, the Memphis Archdiocese issued a pastoral letter in support of integration by busing and did not make efforts to attract new students. Many leaders of Protestant sects made statements in support of the plan as well. Still, reports at the time indicate that some Protestant religious groups, as well as the anti-busing parents organization, established a substantial number of private schools enrolling some 8,000 students by February of 1973. Busing supporters hoped that whites would return to the public school system, but white enrollment only continued to fall.

While private schools played a role in the short run, suburban districts were probably the ultimate destination for those whites who otherwise might have gone to Memphis public schools. The fact that Memphis was nearly majority black made it difficult to integrate without reassigning large number of white students and affecting large changes in the presence of blacks in the schools whites attend. That and the availability of other nearby school districts led to severe white flight, undermining the desegregation plan.

XI. Conclusions

Using the variation in the timing of the implementation of court-ordered

desegregation plans, this paper estimates the effects of these plans on trends in segregation and white flight. In addition, I examine the role of the availability of nearby school districts and other factors in explaining the variation in white flight across districts. The evidence indicates that plans were enforced and produced large, short-term reductions in segregation; but the behavioral response of whites undermined the success of these plans in the long run. White enrollment fell substantially following plan implementation, offsetting about half of the initial gains in nonwhite's exposure to whites.

The weight of the evidence suggests that desegregation plans contributed substantially to white enrollment losses in these districts. The estimates of the average effects of desegregation plans on white enrollment presented in Section VI suggests that white enrollment losses related to desegregation plans were large–about 18 to 30 log points after ten years. In addition, features of desegregation plans – such as how much they change whites' exposure to nonwhites – as well as the number of nearby school districts were important predictors of white flight. This relationship holds even controlling for other factors predictive of declining population during this period – the manufacturing share of enrollment in 1960 and the change in population between 1960 and 1970. If desegregation plan did not affect white enrollment, we would not expect characteristics of desegregation plans and the number of districts to be related to white flight.

Court-ordered desegregation plans increased racial integration in schools, dramatically in some cases. In many districts desegregation plans' ability to reduce segregation effectively in the long run was limited by the decision to exclude the much-

whiter suburban districts. The policies were generally enforced and were successful in many districts, but for districts that were surrounded by many other public school districts, short-term reductions in segregation were largely undone by the behavioral response of white families.

Although we would ultimately like to know the effect of desegregation plans on the educational and labor market outcomes of the minority students they were designed to help, changes in minorities' exposure to whites is an important intermediate measure of the success of these plans. In addition, the white enrollment losses documented here may have implications for other aspects of desegregation plans' success. For example, possible negative effects of desegregation plans on property values would make it more difficult for districts to raise revenue. I plan to explore these questions further in future work.

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Table 1a. Summary Statistics for Selected Years
Districts in Sample with Desegregation Plans

	1968	1970	1980	1995
Enrollment Measures				
Ln(White Enrollment)	10.60	10.56	10.06	9.76
Ln(Nonwhite Enrollment)	9.79	9.84	9.94	10.22
Total Enrollment	85,107	84,866	66,942	72,130
Percent White	67.7%	65.7%	53.0%	40.9%
Segregation Measures				
Dissimilarity Index	0.708	0.599	0.379	0.384
Nonwhite Exposure to Whites	28.4%	37.1%	43.4%	34.2%
White Exposure to Nonwhites	12.1%	17.3%	36.8%	48.2%
Time Relative to Implementation				
% Districts After Implementation	0%	28%	96%	100%
Avg. # Years until Implementation	-4.6	-2.6	7.4	22.5
Number of Districts	95	95	95	95

Note: Summary statistics calculated for districts with a plan implementation date after 1968 and with data available for all of the years reported here.

Table 1b. Summary Statistics by Region (1968)
Districts in Sample with Desegregation Plans

	West	Midwest	Northeast	South
Enrollment Measures				
LN(White Enrollment)	10.73	10.92	10.39	10.50
LN(Nonwhite Enrollment)	9.89	10.20	9.73	9.63
Total Enrollment	107,367	119,690	75,199	69,460
Percent White	68.6%	65.3%	64.5%	68.8%
Segregation Measures				
Dissimilarity	0.534	0.739	0.587	0.764
Nonwhite Exposure to Whites	44.4%	27.4%	38.1%	23.0%
White Exposure to Nonwhites	20.2%	11.9%	18.3%	9.0%
Time Relative to Implementation				
% Districts After Implementation	0%	0%	0%	0%
Avg. # Years until Implementation	-7.6	-7.5	-5.9	-2.6
Number of Districts	14	18	9	54

Note: Summary statistics calculated for districts with a plan implementation date after 1968 and with data available for all of the years reported in Table 1a.

Table 2a. Summary Statistics:
Long-Term Changes in Segregation and White Flight

	Mean	Standard Deviation	Interquartile Change
Long-Term Changes (1 - 3 years before to 8 - 10 years after implementation)			
Dissimilarity Index	-0.226	0.162	0.270
Nonwhite Exposure to Whites	0.064	0.144	0.210
White Exposure to Nonwhites	0.180	0.103	0.129
Ln(White Enrollment)	-0.236	0.313	0.498

Notes: Means are the mean change implied by the estimated coefficients of equation (2) for each variable. The standard deviation is the standard deviation of the residual change for each variable; the construction of these variables is described in detail in Section VII.

Table2b. Summary Statistics
Explanatory Variables

	Mean	Standard Deviation
Supply Factors		
Public School Districts in MSA (1990)	59	99
Percent in Private School (1960)	15.4	9.8
Demand Factors		
Northeast	0.10	0.30
West	0.18	0.38
Midwest	0.21	0.41
Demand Factors		
Pair and Cluster	0.54	0.50
Rezone	0.66	0.48
Magnets	0.10	0.30
Other Voluntary	0.16	0.37
School Segregation (1966-68 Dissimilarity)	0.71	0.15
Controls		
Area	304	431
Residential Segregation (Dissimilarity)	0.77	0.12
LN (Total Enrollment) (1966-68)	11.07	0.69
Percent White (1966-68)	0.67	0.14
Percent Employment in Manufacturing (1960)	21.8	9.5
Percent Change in Population 1960-1970	10.0	17.9
Plan Implementation Year	1973	3.7

Note: Sample is limited to the 80 districts for which sufficient data are available to estimate equation (3).

Table 3: Determinants of White Flight
Dependent Variable: Long-Term Residual Change Ln (White Enrollment)

	OLS (1)	OLS (2)	OLS (3)	2SLS (4)	2SLS (5)
Supply Factors					
Ln (Districts in MSA)	-0.081	-0.104	-0.092	-0.087	-0.087
	(0.046)*	(0.049)**	(0.045)**	(0.043)**	(0.043)**
% Private Sch. (1960)	0.079	0.055	0.053	0.032	0.033
	(0.036)**	(0.035)	(0.034)	(0.035)	(0.035)
Demand Factors					
School Segregation	0.008	-0.006	-0.002	0.062	0.061
(1966-68 Dissimilarity)	(0.038)	(0.041)	(0.039)	(0.043)	(0.043)
Northeast	-0.044	-0.035	0.003	0.026	0.025
	(0.188)	(0.179)	(0.174)	(0.149)	(0.149)
West	0.043	-0.017	0.025	0.085	0.085
	(0.089)	(0.103)	(0.088)	(0.082)	(0.082)
Midwest	-0.028	-0.017	-0.013	0.014	0.013
	(0.096)	(0.095)	(0.094)	(0.089)	(0.089)
Plan Features					
Pair and Cluster		-0.070	-0.101		
		(0.049)	(0.044)**		
Rezoning		0.002			
		(0.084)			
Magnets		0.009			
		(0.095)			
Other Voluntary		0.096			
		(0.116)			
Short-Term Δ White				-0.107	-0.106
Exposure to Nonwhites				(0.043)**	(0.043)**
Controls					
District Area	0.157	0.135	0.142	0.132	0.132
	(0.066)**	(0.065)**	(0.064)**	(0.056)**	(0.056)**
Residential Segregation	-0.058	-0.040	-0.043	-0.047	-0.047
	(0.032)*	(0.035)	(0.034)	(0.030)	(0.030)
Ln (Total Enrollment)	-0.042	-0.039	-0.026	-0.060	-0.059
(1966-68)	(0.037)	(0.038)	(0.035)	(0.032)*	(0.032)*
Initial White Share of	0.156	0.155	0.158	0.112	0.113
Enrollment (1966-68)	(0.034)***	(0.036)***	(0.035)***	(0.037)***	(0.038)***
% Employment in	-0.009	-0.010	-0.014	-0.007	-0.007
Manufacturing (1960)	(0.046)	(0.045)	(0.043)	(0.039)	(0.039)
% Δ City Population	0.042	0.032	0.031	0.020	0.020
(1960-1970)	(0.025)*	(0.023)	(0.023)	(0.022)	(0.022)
Constant	0.013	0.039	0.060	-0.012	-0.012
	(0.051)	(0.094)	(0.054)	(0.046)	(0.046)
Observations	80	80	80	80	80
R-squared	0.61	0.64	0.63		

Notes: Huber-White standard errors in parentheses. Dependent variable is constructed using the residuals from equation (2) reported in Table A.4 column (2) as described in Section VII. Continuous independent variables are normalized by the sample standard deviation. The short-term change in white exposure to nonwhites is instrumented with all four plan feature variables in column (4) and with "Pair and Cluster" only in column (5). * significant at the 10% level; ** significant at the 5% level; *** significant at the 1% level.

Table 4: Determinants of Nonwhite Exposure to Whites
Dependent Variable: Long-Term Residual Nonwhite Exposure to Whites

	OLS (1)	OLS (2)	OLS (3)	2SLS (4)	2SLS (5)
Supply Factors					
LN (Districts in MSA)	-0.052	-0.053	-0.047	-0.049	-0.049
	(0.022)**	(0.025)**	(0.022)**	(0.022)**	(0.022)**
% Private Sch. (1960)	-0.000	0.012	0.011	0.019	0.020
	(0.023)	(0.023)	(0.024)	(0.023)	(0.023)
Demand Factors					
School Segregation	0.085	0.079	0.090	0.059	0.057
(1966-68 Dissimilarity)	(0.022)***	(0.019)***	(0.021)***	(0.027)**	(0.027)**
Northeast	0.015	-0.050	-0.006	-0.014	-0.016
	(0.088)	(0.087)	(0.089)	(0.086)	(0.085)
West	-0.059	-0.105	-0.052	-0.077	-0.077
	(0.044)	(0.045)**	(0.041)	(0.043)*	(0.043)*
Midwest	-0.004	-0.025	-0.011	-0.022	-0.022
	(0.062)	(0.056)	(0.059)	(0.057)	(0.057)
Plan Features					
Pair and Cluster		0.084	0.044		
		(0.028)***	(0.025)*		
Rezoning		0.061			
		(0.040)			
Magnets		0.047			
		(0.038)			
Other Voluntary		0.142			
		(0.054)**			
Short-Term Δ White				0.044	0.046
Exposure to Nonwhites				(0.025)*	(0.025)*
Controls					
District Area	-0.034	-0.039	-0.028	-0.024	-0.023
	(0.034)	(0.034)	(0.033)	(0.036)	(0.036)
Residential Segregation	-0.020	-0.023	-0.026	-0.024	-0.024
	(0.018)	(0.016)	(0.017)	(0.018)	(0.018)
Ln (Total Enrollment)	0.020	-0.004	0.013	0.028	0.029
(1966-68)	(0.020)	(0.020)	(0.019)	(0.022)	(0.022)
Initial White Share of	0.073	0.068	0.072	0.091	0.092
Enrollment (1966-68)	(0.016)***	(0.015)***	(0.015)***	(0.020)***	(0.020)***
% Employment in	0.008	0.019	0.011	0.007	0.007
Manufacturing (1960)	(0.022)	(0.022)	(0.023)	(0.022)	(0.022)
% Δ City Population	-0.010	-0.006	-0.005	-0.001	-0.000
(1960-1970)	(0.015)	(0.015)	(0.015)	(0.015)	(0.015)
Constant	-0.007	-0.094	-0.027	0.007	0.008
	(0.034)	(0.048)*	(0.033)	(0.033)	(0.032)
Observations	80	80	80	80	80
R-squared	0.59	0.65	0.61		

Notes: Huber-White standard errors in parentheses. Dependent variable is constructed using the residuals from equation (2) reported in Table A.2 column (2) as described in Section VII. Continuous independent variables are normalized by the sample standard deviation. The short-term change in white exposure to nonwhites is instrumented with all four plan feature variables in column (4) and with "Pair and Cluster" only in column (5). * significant at the 10% level; ** significant at the 5% level; *** significant at the 1% level.

Table A.1. Average Effects of Desegregation Plans on Segregation
Dependent Variable: Dissimilarity Index

	Full Sample			Balanced Panel		
	(1)	(2)	(3)	(4)	(5)	(6)
Yr Controls	None	Fixed Effects	Quartic	None	Fixed Effects	Quartic
Years Since Plan Implementation						
-6 years	0.041	0.001	0.000	0.103	0.081	0.080
	(0.011)**	(0.012)	(0.012)	(0.017)**	(0.026)**	(0.025)**
-5 years	-0.000	-0.007	-0.011	0.064	0.055	0.053
	(0.016)	(0.016)	(0.016)	(0.022)**	(0.026)*	(0.024)*
-4 years	-0.011	-0.020	-0.022	0.052	0.049	0.045
	(0.015)	(0.015)	(0.014)	(0.022)*	(0.024)*	(0.023)
-3 years	0.010	0.004	0.003	0.039	0.037	0.035
	(0.014)	(0.014)	(0.014)	(0.022)	(0.024)	(0.023)
-2 years	0.022	0.008	0.011	0.028	0.027	0.025
	(0.013)	(0.013)	(0.013)	(0.022)	(0.023)	(0.022)
-1 year	-0.003	-0.009	-0.008	0.018	0.017	0.016
	(0.012)	(0.012)	(0.012)	(0.021)	(0.022)	(0.021)
0 years	--	--	--	--	--	--
	--	--	--	--	--	--
1 year	-0.182	-0.167	-0.170	-0.125	-0.123	-0.123
	(0.012)**	(0.012)**	(0.012)**	(0.022)**	(0.022)**	(0.022)**
2 years	-0.240	-0.216	-0.219	-0.175	-0.174	-0.172
	(0.012)**	(0.012)**	(0.012)**	(0.022)**	(0.023)**	(0.022)**
3 years	-0.254	-0.221	-0.225	-0.190	-0.184	-0.185
	(0.012)**	(0.012)**	(0.012)**	(0.022)**	(0.024)**	(0.023)**
4 years	-0.260	-0.223	-0.226	-0.202	-0.195	-0.195
	(0.012)**	(0.012)**	(0.012)**	(0.022)**	(0.025)**	(0.024)**
5 years	-0.263	-0.224	-0.223	-0.206	-0.193	-0.197
	(0.012)**	(0.013)**	(0.013)**	(0.022)**	(0.027)**	(0.025)**
6 years	-0.266	-0.224	-0.223	-0.209	-0.195	-0.197
	(0.012)**	(0.013)**	(0.013)**	(0.022)**	(0.028)**	(0.027)**
7 years	-0.269	-0.224	-0.223	-0.207	-0.187	-0.192
	(0.013)**	(0.014)**	(0.014)**	(0.023)**	(0.030)**	(0.028)**
8 years	-0.269	-0.224	-0.223	-0.204	-0.184	-0.185
	(0.012)**	(0.014)**	(0.014)**	(0.023)**	(0.031)**	(0.030)**
9 years	-0.278	-0.231	-0.229	-0.221	-0.197	-0.199
	(0.013)**	(0.014)**	(0.014)**	(0.024)**	(0.033)**	(0.032)**
10 years	-0.274	-0.224	-0.224	-0.209	-0.182	-0.183
	(0.013)**	(0.015)**	(0.015)**	(0.024)**	(0.036)**	(0.034)**
11 years	-0.271	-0.217	-0.220	-0.201	-0.167	-0.171
	(0.013)**	(0.015)**	(0.015)**	(0.023)**	(0.037)**	(0.036)**
12 years	-0.268	-0.212	-0.218	-0.199	-0.161	-0.166
	(0.012)**	(0.016)**	(0.015)**	(0.023)**	(0.039)**	(0.037)**
13 years	-0.274	-0.217	-0.223	-0.206	-0.167	-0.172
	(0.013)**	(0.016)**	(0.016)**	(0.022)**	(0.040)**	(0.039)**
14 years	-0.277	-0.221	-0.226	-0.200	-0.159	-0.165
	(0.013)**	(0.016)**	(0.016)**	(0.022)**	(0.041)**	(0.040)**
15 or more	-0.264	-0.214	-0.218	-0.176	-0.147	-0.151
	(0.009)**	(0.017)**	(0.017)**	(0.017)**	(0.044)**	(0.043)**
Constant	0.645	0.746	0.820	0.564	0.610	0.641
	(0.009)**	(0.031)**	(0.018)**	(0.016)**	(0.053)**	(0.043)**
# of Obs.	2996	2996	2996	1064	1064	1064
R-Squared	0.82	0.84	0.83	0.80	0.80	0.80
# of Districts	108	108	108	37	37	37

Notes: Huber-White standard errors in parentheses. Coefficients from Columns (2) and (5) are plotted in Figure 2. The Full Sample includes all districts that implemented a court-ordered plan at some time; the "Balanced Panel" includes only districts that had data available for at least 6 years before and 15 years after implementation.
** significant at 1% level. * significant at 5% level.

Table A.2. Average Effects of Desegregation Plans on Segregation
Dependent Variable: Nonwhite Exposure to Whites

	Full Sample			Balanced Panel		
	(1)	(2)	(3)	(4)	(5)	(6)
Yr Controls	None	Fixed Effects	Quartic	None	Fixed Effects	Quartic
Years Since Plan Implementation						
-6 years	0.074	0.102	0.101	-0.009	-0.004	-0.006
	(0.011)**	(0.011)**	(0.011)**	(0.014)	(0.021)	(0.020)
-5 years	0.092	0.089	0.091	-0.002	0.001	-0.001
	(0.015)**	(0.014)**	(0.014)**	(0.017)	(0.021)	(0.019)
-4 years	0.086	0.087	0.087	-0.001	-0.001	-0.000
	(0.014)**	(0.013)**	(0.013)**	(0.017)	(0.020)	(0.019)
-3 years	0.048	0.046	0.048	0.003	0.002	0.003
	(0.013)**	(0.013)**	(0.012)**	(0.017)	(0.019)	(0.018)
-2 years	0.012	0.022	0.021	-0.000	-0.002	-0.001
	(0.012)	(0.012)	(0.011)	(0.017)	(0.018)	(0.018)
-1 year	0.025	0.029	0.028	-0.002	-0.003	-0.002
	(0.012)*	(0.011)**	(0.011)**	(0.017)	(0.018)	(0.017)
0 years	--	--	--	--	--	--
	--	--	--	--	--	--
1 year	0.127	0.112	0.114	0.062	0.062	0.062
	(0.011)**	(0.011)**	(0.011)**	(0.017)**	(0.018)**	(0.017)**
2 years	0.156	0.132	0.136	0.075	0.077	0.075
	(0.011)**	(0.011)**	(0.011)**	(0.018)**	(0.018)**	(0.018)**
3 years	0.154	0.124	0.128	0.066	0.070	0.067
	(0.011)**	(0.011)**	(0.011)**	(0.017)**	(0.019)**	(0.018)**
4 years	0.150	0.117	0.119	0.066	0.071	0.066
	(0.011)**	(0.011)**	(0.011)**	(0.017)**	(0.020)**	(0.019)**
5 years	0.143	0.110	0.110	0.055	0.059	0.056
	(0.012)**	(0.012)**	(0.012)**	(0.018)**	(0.021)**	(0.020)**
6 years	0.137	0.102	0.103	0.049	0.054	0.050
	(0.012)**	(0.012)**	(0.012)**	(0.018)**	(0.022)*	(0.021)*
7 years	0.133	0.098	0.099	0.042	0.045	0.043
	(0.012)**	(0.012)**	(0.012)**	(0.018)*	(0.024)	(0.023)
8 years	0.124	0.092	0.092	0.032	0.036	0.034
	(0.012)**	(0.013)**	(0.012)**	(0.018)	(0.025)	(0.024)
9 years	0.121	0.091	0.091	0.030	0.034	0.034
	(0.012)**	(0.013)**	(0.013)**	(0.019)	(0.027)	(0.026)
10 years	0.113	0.086	0.086	0.021	0.024	0.026
	(0.012)**	(0.013)**	(0.013)**	(0.019)	(0.028)	(0.027)
11 years	0.104	0.079	0.080	0.007	0.010	0.013
	(0.012)**	(0.014)**	(0.014)**	(0.019)	(0.030)	(0.029)
12 years	0.099	0.076	0.079	0.001	0.007	0.010
	(0.012)**	(0.014)**	(0.014)**	(0.018)	(0.031)	(0.030)
13 years	0.094	0.074	0.078	-0.001	0.007	0.010
	(0.012)**	(0.015)**	(0.014)**	(0.018)	(0.032)	(0.031)
14 years	0.090	0.073	0.078	-0.008	0.003	0.006
	(0.012)**	(0.015)**	(0.015)**	(0.017)	(0.033)	(0.032)
15 or more	0.048	0.063	0.068	-0.045	-0.009	-0.007
	(0.008)**	(0.015)**	(0.015)**	(0.013)**	(0.035)	(0.035)
Constant	0.310	0.212	0.117	0.331	0.315	0.309
	(0.008)**	(0.028)**	(0.016)**	(0.013)**	(0.042)**	(0.034)**
# of Obs.	2996	2996	2996	1064	1064	1064
R-Squared	0.86	0.88	0.88	0.82	0.83	0.83
# of Districts	108	108	108	37	37	37

Notes: Huber-White standard errors in parentheses. Coefficients from Columns (2) and (5) are plotted in Figure 2. The Full Sample includes all districts that implemented a court-ordered plan at some time; the "Balanced Panel" includes only districts that had data available for at least 6 years before and 15 years after implementation.
** significant at 1% level. * significant at 5% level.

Table A.3. Average Effects of Desegregation Plans on Segregation
Dependent Variable: Exposure of Whites to Nonwhites

	Full Sample			Balanced Panel		
	(1)	(2)	(3)	(4)	(5)	(6)
Yr Controls	None	Fixed Effects	Quartic	None	Fixed Effects	Quartic
Years Since Plan Implementation						
-6 years	-0.119	-0.085	-0.083	-0.115	-0.071	-0.070
	(0.008)**	(0.009)**	(0.009)**	(0.012)**	(0.018)**	(0.017)**
-5 years	-0.087	-0.068	-0.067	-0.080	-0.054	-0.053
	(0.012)**	(0.012)**	(0.012)**	(0.015)**	(0.018)**	(0.017)**
-4 years	-0.078	-0.063	-0.062	-0.069	-0.049	-0.047
	(0.011)**	(0.011)**	(0.011)**	(0.015)**	(0.017)**	(0.016)**
-3 years	-0.059	-0.046	-0.047	-0.053	-0.038	-0.037
	(0.010)**	(0.010)**	(0.010)**	(0.015)**	(0.016)*	(0.015)*
-2 years	-0.043	-0.033	-0.034	-0.038	-0.027	-0.028
	(0.010)**	(0.009)**	(0.009)**	(0.015)*	(0.015)	(0.015)
-1 year	-0.023	-0.019	-0.019	-0.023	-0.018	-0.017
	(0.009)*	(0.009)*	(0.009)*	(0.015)	(0.015)	(0.015)
0 years	--	--	--	--	--	--
	--	--	--	--	--	--
1 year	0.079	0.073	0.074	0.075	0.068	0.071
	(0.009)**	(0.009)**	(0.009)**	(0.015)**	(0.015)**	(0.015)**
2 years	0.113	0.104	0.104	0.116	0.105	0.106
	(0.009)**	(0.009)**	(0.009)**	(0.015)**	(0.015)**	(0.015)**
3 years	0.130	0.115	0.116	0.143	0.122	0.128
	(0.009)**	(0.009)**	(0.009)**	(0.015)**	(0.016)**	(0.016)**
4 years	0.143	0.125	0.124	0.158	0.133	0.137
	(0.009)**	(0.009)**	(0.009)**	(0.015)**	(0.017)**	(0.016)**
5 years	0.151	0.128	0.127	0.169	0.135	0.143
	(0.009)**	(0.010)**	(0.010)**	(0.015)**	(0.018)**	(0.017)**
6 years	0.167	0.140	0.138	0.187	0.150	0.155
	(0.009)**	(0.010)**	(0.010)**	(0.015)**	(0.019)**	(0.018)**
7 years	0.172	0.141	0.139	0.191	0.149	0.154
	(0.009)**	(0.010)**	(0.010)**	(0.016)**	(0.020)**	(0.019)**
8 years	0.183	0.147	0.145	0.192	0.148	0.149
	(0.009)**	(0.010)**	(0.010)**	(0.016)**	(0.021)**	(0.021)**
9 years	0.196	0.154	0.153	0.215	0.166	0.166
	(0.009)**	(0.011)**	(0.011)**	(0.016)**	(0.023)**	(0.022)**
10 years	0.207	0.160	0.159	0.217	0.165	0.161
	(0.009)**	(0.011)**	(0.011)**	(0.016)**	(0.024)**	(0.023)**
11 years	0.213	0.159	0.161	0.223	0.164	0.161
	(0.009)**	(0.011)**	(0.011)**	(0.016)**	(0.025)**	(0.025)**
12 years	0.223	0.163	0.166	0.231	0.165	0.162
	(0.009)**	(0.012)**	(0.011)**	(0.016)**	(0.026)**	(0.026)**
13 years	0.234	0.169	0.171	0.242	0.170	0.167
	(0.009)**	(0.012)**	(0.012)**	(0.015)**	(0.027)**	(0.026)**
14 years	0.243	0.174	0.174	0.247	0.167	0.166
	(0.009)**	(0.012)**	(0.012)**	(0.015)**	(0.028)**	(0.027)**
15 or more	0.285	0.177	0.176	0.275	0.164	0.165
	(0.007)**	(0.013)**	(0.013)**	(0.011)**	(0.030)**	(0.030)**
Constant	0.185	0.133	0.149	0.267	0.215	0.193
	(0.006)**	(0.023)**	(0.013)**	(0.011)**	(0.036)**	(0.029)**
# of Obs.	2996	2996	2996	1064	1064	1064
R-Squared	0.89	0.90	0.90	0.90	0.91	0.90
# of Districts	108	108	108	37	37	37

Notes: Huber-White standard errors in parentheses. Coefficients from Columns (2) and (5) are plotted in Figure 2. The Full Sample includes all districts that implemented a court-ordered plan at some time; the "Balanced Panel" includes only districts that had data available for at least 6 years before and 15 years after implementation.
** significant at 1% level. * significant at 5% level.

Table A.4. Average Effects of Desegregation Plans on White Flight
Dependent Variable: Ln (White Enrollment)

	Full Sample			Balanced Panel		
	(1)	(2)	(3)	(4)	(5)	(6)
Yr Controls	None	Fixed Effects	Quartic	None	Fixed Effects	Quartic
Years Since Plan Implementation						
-6 years	0.372	0.224	0.219	0.342	-0.015	0.002
	(0.034)**	(0.037)**	(0.037)**	(0.036)**	(0.052)	(0.050)
-5 years	0.307	0.213	0.207	0.263	0.017	0.033
	(0.048)**	(0.048)**	(0.048)**	(0.045)**	(0.051)	(0.049)
-4 years	0.305	0.231	0.229	0.221	0.022	0.035
	(0.044)**	(0.044)**	(0.044)**	(0.045)**	(0.049)	(0.047)
-3 years	0.204	0.148	0.150	0.174	0.025	0.035
	(0.042)**	(0.042)**	(0.042)**	(0.045)**	(0.047)	(0.045)
-2 years	0.147	0.115	0.115	0.106	0.009	0.015
	(0.039)**	(0.039)**	(0.038)**	(0.045)*	(0.045)	(0.044)
-1 year	0.062	0.047	0.048	0.057	0.007	0.010
	(0.037)	(0.037)	(0.037)	(0.045)	(0.044)	(0.043)
0 years	--	--	--	--	--	--
	--	--	--	--	--	--
1 year	-0.066	-0.050	-0.047	-0.097	-0.052	-0.055
	(0.036)	(0.036)	(0.036)	(0.045)*	(0.044)	(0.043)
2 years	-0.102	-0.065	-0.063	-0.161	-0.072	-0.077
	(0.036)**	(0.036)	(0.036)	(0.046)**	(0.045)	(0.044)
3 years	-0.154	-0.092	-0.090	-0.235	-0.097	-0.106
	(0.036)**	(0.037)*	(0.036)*	(0.045)**	(0.048)*	(0.045)*
4 years	-0.193	-0.104	-0.103	-0.289	-0.115	-0.122
	(0.036)**	(0.038)**	(0.037)**	(0.045)**	(0.049)*	(0.047)*
5 years	-0.235	-0.120	-0.118	-0.331	-0.119	-0.129
	(0.037)**	(0.040)**	(0.039)**	(0.046)**	(0.053)*	(0.050)**
6 years	-0.271	-0.132	-0.126	-0.375	-0.145	-0.143
	(0.037)**	(0.040)**	(0.040)**	(0.046)**	(0.055)**	(0.053)**
7 years	-0.299	-0.135	-0.129	-0.395	-0.145	-0.139
	(0.038)**	(0.041)**	(0.041)**	(0.048)**	(0.059)*	(0.057)*
8 years	-0.333	-0.143	-0.137	-0.418	-0.154	-0.135
	(0.037)**	(0.042)**	(0.042)**	(0.048)**	(0.062)*	(0.060)*
9 years	-0.372	-0.155	-0.151	-0.453	-0.178	-0.155
	(0.038)**	(0.044)**	(0.043)**	(0.050)**	(0.066)**	(0.064)*
10 years	-0.410	-0.167	-0.164	-0.453	-0.178	-0.142
	(0.038)**	(0.045)**	(0.044)**	(0.050)**	(0.070)*	(0.068)*
11 years	-0.441	-0.168	-0.171	-0.464	-0.170	-0.140
	(0.038)**	(0.046)**	(0.046)**	(0.049)**	(0.074)*	(0.071)*
12 years	-0.474	-0.178	-0.181	-0.490	-0.182	-0.153
	(0.037)**	(0.047)**	(0.047)**	(0.048)**	(0.077)*	(0.074)*
13 years	-0.500	-0.187	-0.188	-0.512	-0.181	-0.157
	(0.038)**	(0.049)**	(0.048)**	(0.046)**	(0.079)*	(0.077)*
14 years	-0.529	-0.198	-0.199	-0.542	-0.188	-0.172
	(0.038)**	(0.050)**	(0.049)**	(0.045)**	(0.081)*	(0.079)*
15 or more	-0.659	-0.211	-0.201	-0.663	-0.197	-0.196
	(0.027)**	(0.052)**	(0.051)**	(0.035)**	(0.088)*	(0.086)*
Constant	10.385	10.584	10.361	10.334	10.650	0.027
	(0.026)**	(0.094)**	(0.055)**	(0.033)**	(0.105)**	(0.022)
# of Obs.	2996	2996	2996	1064	1064	1064
R-Squared	0.91	0.91	0.91	0.95	0.95	0.95
# of Districts	108	108	108	37	37	37

Notes: Huber-White standard errors in parentheses. Coefficients from Columns (2) and (5) are plotted in Figure 2. The Full Sample includes all districts that implemented a court-ordered plan at some time; the "Balanced Panel" includes only districts that had data available for at least 6 years before and 15 years after implementation.
** significant at 1% level. * significant at 5% level.

Appendix B: Data

Enrollment Data

Enrollment by race at the school level for years before 1987 was obtained from Unicon Research Corporation. The data were originally collected for a report of the U.S. Commission on Civil Rights (Welch and Light, 1987). Most of these data were collected from the following sources: the Office of Civil Rights of the U.S. Department of Education, Tauber-Wilson tapes, and individual school districts. Welch and Light indicate the district-years for which there is adequate data by race at the school level to calculate segregation indexes; I include these district-years in the sample. Data for 1987 to 1998 were taken from the National Center for Education Statistics (NCES) Common Core of Data (CCD) Public School Universe (PSU), which includes enrollment by race at the school level.

Some states did not provide complete information on enrollment by race in all years (particularly the earlier years). If more than 5 percent of a district's enrollment in a particular year was in schools that reported incomplete information on enrollment by race, I dropped the observation for that district-year. A school was defined as having insufficient data on enrollment by race if the sum of enrollment by race was less than 90 percent of the reported total enrollment. (In general, entire states were missing racial breakdowns, so the precise cutoffs used are not important.)

Year of Major Plan Implementation

Welch and Light list all of the plans they identified for each district (their Appendix Table A3). I use the year of the first plan they classify as "major" with one exception. Conversations with officials familiar with the San Jose school districts' desegregation plans indicate that the district implemented a major plan in 1986 after the Welch and Light report; I therefore use 1986 as the implementation date for San Jose. The trends for the dissimilarity index show large breaks in segregation around the time of major plan implementation for most districts, suggesting that this is a reasonable measure of when the policy was implemented. The districts and implementation year are listed in Table B.1.

Number of School Districts in Metropolitan Area

I use the 1990 Census Metropolitan Statistical Area (MSA) definitions for areas outside New England and New England County Metropolitan Areas (NECMA) for New England. For each district in the sample, I count the number of school districts in the same metropolitan area in 1990. Some areas have separate elementary and secondary school districts as well as special or administrative districts. Since I do not want to double-count elementary and secondary districts that occupy the same geographic area, I use Geographic Information Systems (GIS) software and boundary files for 1990 from the Census Bureau to determine which districts cover the same area. I then count all the districts in the MSA that cover a unique geographic area.

District Area

I use GIS software to calculate the area of districts in the sample. Ideally, I would calculate the area before plan implementation. However, this is essentially equivalent to calculating the area for 1990 since these districts' boundaries changed little if at all.

Residential Segregation

The dissimilarity index for residential segregation is constructed using the Urban Institute Underclass Database tract-level Census data for. The lowest level of geographic identification in the tract-level data is the county; I therefore assign each district the residential segregation index for its county. The dissimilarity index is calculated for two mutually exclusive groups, in this case, blacks and nonblacks. Unfortunately, data limitations prevent calculating residential segregation for non-Hispanic whites and others,[1] the categories I use in the analysis of segregation of schools. However, these are likely to be highly correlated. In the residential context, the dissimilarity index indicates the fraction of nonwhites who would have to switch Census tracts so that every tract would have the same racial composition as the county as a whole.

Percent in Private Schools and Percent in Manufacturing

Percent in private schools and percent in manufacturing were taken from the 1960 City Data Book (based on Census data). Each district is assigned a value for its city. Percent in private schools is total private school enrollment through secondary school divided by total enrollment. Percent in manufacturing is manufacturing employment divided by total employment.

Percent Change in City Population

Percent change in city population is the log change in population taken from the 1960 and 1970 City Data Books (based on Census data). Each district is assigned a value for its city.

Initial Segregation, Enrollment, White Share of Enrollment, and Density

Ideally, I would measure segregation (dissimilarity index), total enrollment, and the white share of enrollment in a single year, before any districts implemented a plan, to indicate the initial conditions. Because not all districts have data available for all years, I do not use a single year as the initial conditions year. I used the latest year between 1966 and 1968 for which a district both had data and had not yet implemented a plan. Districts that did not have data satisfying these criteria were dropped from the analysis. Limiting the sample to districts that implemented after 1968 (so their initial conditions variables are measured consistently in 1968) does not affect the results. Density is the total students (in 1966-68) divided by district area.

[1] Throughout the paper, whites refers to non-Hispanic whites, and nonwhites refers to all others (not Hispanic and not white).

Table B.1
Sampled Districts and Implementation Year

	Implementation Year	State
Non-Southern Districts		
Tuscon	1978	AZ
Fresno	1978	CA
Long Beach	1980	CA
Los Angeles	1978	CA
Oakland	1966	CA
Pasadena	1970	CA
Richmond	1969	CA
Sacramento	1976	CA
San Bernadino	1978	CA
San Diego	1977	CA
San Francisco	1971	CA
San Jose	1981	CA
Vallejo	1975	CA
Denver	1974	CO
Hartford	1966	CT
Stamford	1970	CT
Wilmington	1978	DE
Chicago	1982	IL
Rockford	1973	IL
Fort Wayne	1971	IN
Indianapolis	1973	IN
South Bend	1981	IN
Kansas City	1977	KS
Witchita	1971	KS
Boston	1974	MA
New Bedford	1976	MA
Springfield	1974	MA
Detroit	1975	MI
Grand Rapides	1968	MI
Lansing	1972	MI
Minneapolis	1974	MN
Kansas city	1977	MO
St. Louis	1980	MO
Omaha	1976	NE
Jersey City	1976	NJ
Newark	1961	NJ
Clark County	1972	NV
Buffalo	1976	NY

Table B.1 (continued)

	Implementation Year	State
Rochester	1970	NY
Akron	1977	OH
Cincinnati	1973	OH
Cleveland	1979	OH
Columbus	1979	OH
Dayton	1976	OH
Toledo	1980	OH
Portland	1974	OR
Philadelphia	1978	PA
Pittsburgh	1980	PA
Seattle	1978	WA
Tacoma	1968	WA
Milwaukee	1976	WI
Southern Districts		
Birmingham	1970	AL
Jefferson County	1971	AL
Mobile	1971	AL
Little Rock	1971	AR
Brevard County	1969	FL
Broward County	1970	FL
Dade County	1970	FL
Duval County	1971	FL
Hillsborough County	1971	FL
Lee County	1969	FL
Orange County	1972	FL
Palm Beach County	1970	FL
Pinellas County	1970	FL
Polk County	1969	FL
Volusia County	1969	FL
Atlanta	1973	GA
Dougherty County	1980	GA
Muscogee County	1971	GA
Fayette County	1972	KY
Jefferson County	1975	KY
Caddo Parish	1969	LA
Calcasieu Parish	1969	LA
E. Baton Rouge Parish	1970	LA

Table B.1 (continued)

	Implementation Year	State
Jefferson Parish	1971	LA
New Orleans Parish	1961	LA
Rapides Parish	1969	LA
Terrebonne Parish	1969	LA
Baltimore	1974	MD
Harford County	1965	MD
Prince Georges County	1973	MD
Cumberland County	1969	NC
Gaston County	1970	NC
Mecklenburg County	1970	NC
New Hanover County	1969	NC
Lawton	1973	OK
Oklahoma City	1972	OK
Tulsa	1971	OK
Charleston County	1970	SC
Greeneville County	1970	SC
Richland County	1970	SC
Memphis	1973	TN
Nashville	1971	TN
Amarillo	1972	TX
Austin	1980	TX
Dallas	1971	TX
Ector County	1982	TX
El Paso	1978	TX
Fort Worth	1973	TX
Houston	1971	TX
Lubbock	1978	TX
San Antonio	1969	TX
Waco	1973	TX
Norfolk	1970	VA
Pittsylvania County	1969	VA
Roanoke	1970	VA
Raleigh County	1973	WV

Note: Sample chosen using criteria described in Welch and Light (1987).