

***Post Brown vs. the Board of Education:* The Effects of the End of Court-Ordered Desegregation**

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Abstract

Nearly forty years after *Brown v. the Board of Education*, three Supreme Court decisions dramatically altered the legal environment for court-ordered desegregation in the early 1990s. Lower courts have released numerous school districts from their desegregation plans as a result. Over the same period racial segregation increased in public schools – a phenomenon which has been termed resegregation. Using a unique dataset, this paper finds that dismissal of a court-ordered desegregation plan results in a gradual, moderate increase in racial segregation and an increase in black dropout rates and black private school attendance. There is no evidence of any effect on white attendance patterns, school expenditures or property values.

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Court-ordered desegregation was one of the most ambitious and controversial government policies of the last fifty years. Beginning in 1954 with the *Brown v. Board of Education*¹ decision, the majority of the nation's large school districts were subject to mandatory desegregation plans. The plans produced dramatic increases in racial integration in the short-run. The long-run integrative effects varied from district to district. In some districts, long-run integration was achieved. In other districts the response of whites to the plans, often referred to as 'white flight', undermined the plans' ability to achieve stable integration (Rossell and Armor 1996; Reber 2002; Welch and Light 1987). There is strong evidence that the plans produced improvements in the educational outcomes of black students (Guryan 2004).²

The number of new court-ordered desegregation plans peaked in the early 1970s and declined steadily thereafter. The Supreme Court, having been largely silent on the issue of desegregation during the 1980s, issued three decisions in the early 1990s which significantly altered the legal basis for court-ordered desegregation. It became easier to terminate court-mandated plans and the return of school control to local authority became the stated goal of all desegregation cases. These decisions signaled the end of the era of court-ordered desegregation: a large and possibly accelerating number of school districts have had their desegregation plans dismissed in the post-1990 period.

Racial segregation increased in public schools over the same period – a development which has been termed resegregation (Orfield and Eaton 1996; Boger 2002; Frankenberg, Lee and Orfield 2003; for a dissenting view, see Armour and Rossell 2002). Numerous observers have assumed an explicit link between the dismissal of desegregation plans and increasing segregation in public schools. A recent *New York Times* editorial states that “much of the blame [for resegregation] goes to the courts' increased hostility to desegregation suits.”³ Many scholarly articles have made similar assumptions (e.g. Boger 2002 pg. 3; Cherminsky 2002 pg. 5; Orfield 2001 pg. 15 – 16). Despite these assertions, no attempt has been made to explicitly test the hypothesis that court dismissals are responsible for an increase in segregation in schools.⁴

¹ 347 U.S. 483 (1954).

² Guryan (2004) presents evidence that desegregation reduced black dropout rates.

³ “Fighting School Resegregation”, *New York Times* Editorial Page, January 27, 2003.

⁴ A recent exception is Clotfelter, Ladd and Vigdor (2005) – see below for a discussion of this paper.

The effect of the end of court-ordered desegregation is unclear. The dismissal of a desegregation plan does not necessarily result in increased segregation. Most plans have been in place for many years and there is evidence that a plan's ability to achieve integration erodes over time (Reber 2002). It is unclear whether or not desegregation plans are still imposing a constraint on racial segregation in the post 1990 period. If they are not imposing a constraint, segregation will not increase when a plan is dismissed.

Furthermore, even if the termination of a plan causes an increase in segregation, the termination may or may not have adverse consequences for black students. The phase-out of the plans is occurring in a very different environment than that in which they were implemented. Residential segregation has decreased significantly (Glaeser and Vigdor 2002) and funding is much more equalized across schools (Card and Payne 1998; Murray, Evans and Schwab 1998; Hoxby 2001). Given the different environment, it is not clear that the dismissals will reverse the gains achieved by implementation of the plans.

This paper answers the two questions raised above. First, does dismissal of a desegregation plan result in an increase in segregation? Second, what are the welfare implications of the end of court-ordered desegregation for black students?

The first question is answered by providing estimates of the causal link between the dismissal of court-ordered desegregation plans and changes in racial segregation in public schools. Segregation is of interest because racial integration was the primary aim of court-ordered desegregation. Examining segregation levels provides evidence on the efficacy of what has been called the most ambitious and idealistic social experiment in U.S. history (Merelman 2002).

Segregation is also of interest because of a possible link with educational outcomes. There are numerous reasons why segregation levels may affect educational outcomes. Peer effects potentially play an important role in human capital production (Boozer, Krueger and Wolkon, 1992; Hoxby 2000; Angrist and Lang 2004; Hanushek, Kain and Rivkin 2002). The degree of segregation also likely influences the distribution and level of educational resources provided to minority students – a point expressed memorably in the *Brown* decision's premise that separate schools are inherently unequal.

Segregation levels in a dismissed district may change via one of two primary mechanisms. First, students may be re-sorted among the schools within a district. For

example, the termination of a busing plan⁵ would re-sort students in a manner which increases segregation. I consider this mechanism by examining how dismissal alters the sorting of student across the attendance zones of a school district.

Second, dismissals may affect enrollment patterns by race at the district level. It is well documented that whites responded to desegregation by moving to alternative public school districts or placing their children in private schools. This response, often termed 'white flight', increased the level of segregation in many districts.

The racial sorting equilibrium across public school districts and private schools in metropolitan areas therefore reflects the presence of a court-ordered desegregation plan. The termination of a desegregation plan may break this equilibrium and effect segregation levels by changing the racial composition of a school district. I consider this mechanism by examining the response of public and private school enrollment by race to the dismissal of a desegregation plan.

A particularly interesting aspect of this potential change is the response of whites. Dismissal of a desegregation plan may cause whites to re-enter a district – a hypothesized phenomenon which I term 'reverse white flight'

The second question answered by this paper, what are the welfare implications of the end of court-ordered desegregation for black students, is addressed by examining the link between dismissal and several outcome variables including black dropout rates, black rates of private school attendance, school district expenditures and housing values.

A dismissal potentially causes a complex transformation of the school environment. The peer group a student experiences may change. In some cases long-distance bus rides are replaced by neighborhood school attendance. School districts under a court-ordered desegregation plan are monitored by the courts in regard to minority student performance. Dismissal of a desegregation plan returns a district to local control, removes the external monitoring and may therefore reduce the effort and resources expended on minority students.

These changes alter both the value and cost of education provided in a dismissed district. For example, the elimination of busing may reduce the costs associated with

⁵ Busing students to schools outside of their residential neighborhood was one technique used by the courts to achieve an increase in racial integration in districts which were under court-order.

attendance, while a change in the peer group may increase or decrease the expected return to attendance. In order to assess the net impact of these changes, the response of black dropout rates and black rates of private school attendance are examined.

Anecdotal evidence suggests that school district officials often respond to a dismissal by making substantial capital investments in the educational infrastructure of minority neighborhoods.⁶ School district expenditures are examined in order to assess the extent of this response by policy makers.

Finally, the termination of a mandatory desegregation plan potentially alters the value of the bundle of non-market goods provided by a school district. This alteration will capitalize into residential housing values (Hamilton 1976). Changes in housing prices are therefore examined because they provide a summary measure of the welfare impact of the end of a court-ordered desegregation.

My analysis uses a unique dataset, compiled from multiple sources, and an identification strategy based on the idiosyncratic timing of desegregation plan dismissals. The results suggest that dismissal induces a gradual increase in segregation levels. The magnitude of the increase is moderate. It is significantly smaller than the decrease in segregation that was achieved by the plans implementation. It should be noted, however, that the estimates reflect short-run effects. The long-run effects may be larger.

The increased segregation is the result of re-sorting of students within a dismissed district. There is no evidence of a change in the demographic composition of dismissed districts. In particular, there is no evidence of 'reverse white flight'. In independent contemporaneous work, Clotfelter, Ladd and Vigdor (2005) also explore the connection between the dismissal of court-ordered desegregation plans and racial segregation and find that post 1993 dismissals result in an increase in racial segregation – a finding broadly consistent with the results of this paper.⁷

⁶ For example, the Nashville, Tennessee school district pledged to spend \$206 million on new school construction when it was released from its desegregation plan (Goldring and Smrekar 2002). Lafayette Parish, Louisiana School Board promised to replace inadequate inner city schools after its desegregation plan ended (NAACP 2000).

⁷ There are many differences in approach between this paper and Clotfelter, Ladd and Vigdor (2005) (henceforth CLV). Three of the more significant differences are as follows. First, CLV use a sample of the largest southern school districts. This paper uses a national sample restricted to those districts under court-order in 1991. The different samples provide different counterfactuals for those districts dismissed within the sample period. CLV uses all large southern districts not dismissed in the sample period to provide a counterfactual (including districts never under court-order and those dismissed before the sample period),

The results also suggest that the end of court-ordered desegregation has negative welfare consequences for black students. Black dropout rates and black rates of private school attendance increase in response to the dismissal of a desegregation plan. These increases suggest that the value of the educational services provided to black students is reduced when a court-ordered desegregation plan is terminated. These negative welfare consequences are confined to non-southern districts. There is no change in dropout rates or private school attendance in southern dismissed districts. Dismissal has no apparent impact on school district expenditures or residential housing values.

The paper proceeds as follows. Section I provides legal background information, with an emphasis on the post-1990 period. Section II discusses the data. Section III presents the empirical model and results for the segregation, enrollment by race in public schools and school district expenditure outcome variables. Section IV presents the empirical model and results for the dropout, enrollment by race in private schools and residential housing value outcome variables. Section V concludes.

I. Legal Background and Previous Research

Legal Background

Although *Brown v. Board of Education* was issued in 1954, widespread desegregation did not begin until the Civil Rights Act of 1964, which banned racial discrimination in schools receiving federal aid. The 1968 Green⁸ decision, which stipulated that school desegregation must begin immediately, further accelerated the process. Numerous southern districts were placed under court-ordered desegregation plans, many with mandatory busing components, and southern schools ultimately became the least segregated in the country.

while this paper uses districts which remained under court-order as the counterfactual. Second, this paper examines several outcome measures in addition to racial segregation such as dropout rates and rates of private school attendance by race. Examining these outcomes provides insight into the welfare implications of the end of court-ordered desegregation. Finally, as noted by CLV, this paper allows for more flexibility in the time pattern of effects of dismissal of a desegregation plan. This flexibility (in particular the estimation of a vector of coefficients for the period prior to dismissal – see below), as well as a set of rigorous robustness checks, is useful in assessing whether or not the estimated increase in segregation reflects the causal impact of dismissal.

⁸ *Green v. County School Board of New Kent County*, 391 U.S. 430 (1968).

The *Keyes*⁹ decision, issued in 1973, ruled that court-ordered desegregation could proceed in areas which had not practiced *de jure* segregation. Desegregation became viable in areas outside of the south and numerous northern and western school districts were placed under mandatory desegregation plans.

The Supreme Court issued no significant decisions relating to school desegregation between the mid-1970s and 1990. The flow of new desegregation orders from lower courts increased through the early 1970s and declined gradually thereafter. By 1990, the flow of new orders had virtually stopped. There has been only a single federal desegregation order that involved a mandatory student assignment plan since 1990 (Raffel 2002).

The legal environment for court-ordered desegregation changed radically with the 1991 *Board of Education of Oklahoma City v. Dowell*¹⁰ ruling. This decision defines the requirements for a school district to be declared unitary – a term indicating a district is no longer operating an illegal, racially dual school system – and stipulates that once a district achieves unitary status it must be permanently released from court control. Even immediate and complete resegregation is acceptable, as long as the school district does not state its attendance policies are aimed at achieving racial segregation. Prior to the decision, it had been widely presumed that districts released from court control had an obligation to maintain a desegregated district (Orfield 2001; Lindseth 2002).

The *Freeman v. Pitts*¹¹ decision, issued in 1992, eases the burden placed on defendant school districts in desegregation suits. Finally, *Missouri v. Jenkins*¹² limits enforcement options available to federal courts and states that restoration of school control to locally elected officials should be the primary goal of all desegregation cases.

These decisions collectively express the opinion that the courts have “done enough” in the area of school desegregation and that long-running desegregation cases should be moved to closure (Tushnet 1996). A large number of school districts have been released from their desegregation plans as a result of the above decisions and there is an apparent acceleration in the rate of dismissals. Most observers have concluded that the era of court-ordered desegregation is drawing to a close (Frankenburg, Lee and Orfield 2003, pg.20;

⁹ *Keyes v. Denver School District No. ,* 413 U.S. 189 (1973).

¹⁰ 498 U.S. 237 (1991).

¹¹ 503 U.S. 467 (1992).

¹² 515 U.S. 70 (1995).

Lindseth 2002, pg. 42). See Appendix A for additional information on court-ordered desegregation.

The causal impact of desegregation plan dismissal on racial segregation and other outcome variables is identified in this paper from both *whether* a district is dismissed and *when* it is dismissed. It is therefore important to examine the process of dismissal in detail.

The process of dismissal, once initiated in the courts, typically takes several years. Once initiated, virtually all districts are subsequently dismissed from court supervision. Every contested motion for unitary status post-1990 has resulted in a dismissal.¹³

A dismissal can be initiated by any one of a number of agents, including defendant school districts, plaintiffs, federal district court judges and third parties such as parents of students in affected districts and non-school governmental bodies. The variety and idiosyncrasy of who initiates the dismissal process makes it unlikely that dismissal is a function of school district or community characteristics and preferences.

A few examples illustrate this point. Pinellas County, Florida, which serves St. Petersburg, had operated under a successful desegregation plan (success being defined as achieving high, long-term levels of black-white exposure). The defendant school board moved for dismissal.¹⁴ Cleveland, Ohio, which had one of the least successful court-ordered desegregation plans, is another example of a defendant school board moving for dismissal.¹⁵

Charlotte, North Carolina is often cited as an example of successful court-ordered desegregation. The dismissal process in Charlotte began when a white parent filed suit against the district's race-based magnet school admission policy. A district court judge consolidated the magnet school case with the much older desegregation case. The district's desegregation plan was ultimately dismissed as a result.¹⁶ Prince George's County, Maryland, a district where 'white flight' undermined the ability of its desegregation plan to achieve stable integration, is another example of a third party initiating a dismissal. Over the objection of the school board, the county government, which was a major funding source for the school district, moved that the desegregation order be terminated (Lindseth 2002).

¹³ NAACP 2000. Note that although Hillsborough County, Florida is cited as an exception to this trend, it was declared unitary and its desegregation order dismissed after the publication of the NAACP 2000.

¹⁴ Ibid.

¹⁵ 179 F.3d 453 (6th Cir 1999).

¹⁶ 57 F.Supp.2d 228.

Finally, in many cases, district judges have chosen to clear their dockets of desegregation cases at their own initiative. For example, the judges in the Middle District of Alabama choose to begin active proceedings for all desegregation cases on their dockets (Parker 2000) – a decision which led to seven districts being dismissed in 2002 alone.

Once the process of dismissal begins, there is an element of randomness in the length of time it takes for a district to be dismissed. Decisions are often appealed, adding further randomness to the date of actual dismissal. A particularly striking example of the idiosyncratic nature of the timing involves Cleveland. The judge who had overseen the desegregation suit since its inception in 1973 passed away. His successor rapidly moved the case to termination.

A final relevant piece of legal background involves desegregation plans operated by districts not under court-order. Recent federal and Supreme Court rulings¹⁷ have made it more difficult to legally operate voluntary, non-court-ordered plans. Numerous school districts have terminated voluntary desegregation efforts as a result. Boston is a prominent example.

The trend toward the elimination of voluntary plans has two implications for this paper. First, school districts released from court-ordered plans have limited ability to maintain desegregation efforts (Lindseth 2002). Most dismissed districts have returned to some form of neighborhood schooling. In many cases, though, portions of the court-ordered desegregation plan have been maintained. Magnet school programs, in particular, are often retained after dismissal (Orfield and Lee 2004). Second, the trend away from voluntary desegregation plans potentially complicates the econometric identification of the effect of court-ordered desegregation plan dismissal (this point is discussed in greater detail in section III).

¹⁷ *Tuttle v. Arlington County School Bd*, 195 F.3d 698 (4th Cir. 1999); *Wessman v. Gittens*, 160 F.3d 790 (1st Cir.); *Adarand Constructors v. Pena*, 515 U.S. 200; *City of Richmond V. J.A. Croson Co.*, 488 U.S. 469.

II. Data

This paper analyzes a nationally representative sample of school districts. The primary source of school district data is the Common Core of Data (CCD) produced by the National Center for Education Statistics. It contains basic descriptive data for the universe of public schools in the U.S. from 1987 to 2002. The School District Databook (SDDB), a school district-level tabulation of the Census of Population, complements the CCD. It provides extremely detailed demographic and housing data for the geographic areas served by school districts, but is only available for 1990 and 2000. The Census Bureau's Public Elementary-Secondary Education Finance Data (PESEF) contains detailed revenue and expenditure data for school districts and is available from 1992–2001.

No accurate national statistics are available concerning the number of court-ordered desegregation plans in place or the number of dismissals of such plans. Multiple sources are therefore used to generate two variables related to court-ordered desegregation – the presence of a court-ordered plan in 1991 and the dates of dismissal of these plans.

The primary source for the presence of a court-ordered plan variable is a 1991 nationally representative survey of school districts conducted by Christine Rossell and David Armor (Steel, Levine, Rossell and Armor 1993; Steel and Levine 1994). The survey contains detailed information on school desegregation programs. Both the content of the survey and its timing are ideally suited for the estimation strategy pursued in this paper.

The primary source of information on dismissal of desegregation orders is an unpublished table produced by the Harvard Civil Rights Project (Ma 2002). To supplement both this table, which does not claim to be comprehensive, and the Rossell and Armor survey data, I use several other sources. These include electronic searches on the legal search engines *Courtlink* and *Pacer*¹⁸, an unpublished list of school districts subject to desegregation suits to which the U.S. is a party maintained by the Civil Rights Division of the Justice Department, published and unpublished legal opinions obtained via *LexisNexis* and *Westlaw*, appendix C of Welch and Light (1987), a variety of media and internet sources (in particular the electronic archives of *Education Week*), school district documents such as

¹⁸ *Courtlink* allows for electronic searches of Federal District Court dockets and *Pacer* provides electronic retrieval of these dockets. The precise methodology used for the *Courtlink* and *Pacer* searches was suggested by Harvard Law School Professor Margo Schlanger. See Appendix B for more detailed information.

budgets and minutes of school board meetings and, finally, private communications with school district officials.

Two panel datasets, organized at the school district-year level, are constructed from the above sources. Court-ordered desegregation cases operate almost exclusively at the school district level.¹⁹ The school district is therefore the appropriate level at which to organize the data.

The first panel spans the 1987 – 2001 period and uses outcome variables constructed from the CCD. The second panel contains two periods, 1990 and 2000, and uses outcome variables generated from the SDDDB. The datasets are restricted to the districts in the nationally representative sample in the Rossell and Armor survey data which were under a court-ordered desegregation plan in 1991.²⁰ See Appendix B for detailed information on the data sources and construction of these two datasets.

III. Segregation, Public School Enrollment and School District Finance Results

Outcome Variables

A primary aim of court-ordered desegregation is increasing the extent of contact between white and black students. The extent of contact between the races can be changed via one of two primary mechanisms. First, holding the racial composition of the district fixed, students may be re-sorted among the schools which comprise a district. Court-ordered desegregation achieves racial integration by this type of re-sorting. For example, magnet school programs and busing produce integration by re-sorting students among schools within a district.

The sorting of students within a district is measured using the dissimilarity index, defined as

$$(1) \quad D_t = \frac{1}{2} * \sum_{i=1}^n \left| \frac{b_{it}}{B_t} - \frac{w_{it}}{W_t} \right|,$$

¹⁹ The *Milliken v. Bradley* decision, 418 U.S. 717 (1974), limited virtually all court-ordered desegregation plans to a single school district.

²⁰ See Appendix B for a complete description of the sampling methodology used to construct the Rossell and Armor data.

where b_{it} and w_{it} refer to the number of black and white students, respectively, at school i at time t and B_t and W_t refer to the total number of black and white students, respectively, in the school district.

The dissimilarity index ranges from 0 to 1, with 1 denoting complete segregation. It is interpretable as the percent of black students who would need to be reassigned to a different school for perfect integration to be achieved given the districts overall racial composition. An increase in segregation is reflected by an increase in the dissimilarity index.

Second, the demographic composition of a district may change. As discussed in the introduction, it is possible that white students will return to districts which have been released from their desegregation plan. Such 'reverse white flight' would increase the extent of contact between white and black students. In addition, black enrollment may change if the dismissal alters the value of the educational services provided by the district to black students. District level demographic changes are examined using data on district enrollment by race.

The extent of interracial contact within a school district is measured directly by the exposure index,

$$(2) \quad E_t = \frac{1}{B_t} \sum_{i=1}^n b_{it} * \frac{w_{it}}{t_{it}},$$

where t_{it} is the total number of students in school i . It is interpretable as the percent of white students in the average black student's school. For a given district, it ranges from 0 to the percent of white students in the district as a whole. It can be viewed as a measure of the extent of contact between the two races. An increase in segregation is reflected by a *decrease* in the exposure index.

The dissimilarity index and enrollment by race at the district level can be viewed as directly measuring behavioral responses to the end of court-ordered desegregation. The dissimilarity index will primarily capture the response of policy makers. As policies which promote integration are phased out, the dissimilarity index may increase. Changes in enrollment by race at the district level will primarily reflect the response of parents and students. Policy makers have very limited ability to influence the racial composition of a school district.

The dissimilarity index and enrollment by race are therefore the appropriate measures to use in assessing how policy makers and parents, respectively, respond to the end of court-ordered desegregation. The exposure index remains of interest because it summarizes the extent of contact between whites and blacks – a primary goal of court-ordered desegregation.

Summary Statistics

There are 571 districts in the Rossell and Armor survey data, 125 of which were under court-order in 1991. Of these 125 districts, 44, or approximately 1/3, have been dismissed in the post-1990 period. Figure 1 graphs the timing of these dismissals. There were few dismissals prior to 1996 and there is an apparent acceleration in the number of dismissals over time. Legal Appendix Table L1 lists all the districts in the sample. Table L2 lists the dismissed districts in chronological order of dismissal. Southern school districts make up 65% of the dismissals, reflecting the fact that a majority of court-ordered desegregation plans were in the South.

Table 1 presents summary statistics for 1990 and 2000 for three sets of school districts – those districts under court-ordered desegregation plans in 1991 and subsequently dismissed within the sample range of 1987 to 2002, those under a plan in 1991 and not dismissed within the sample range and those not under court-order in 1991. These groups will be referred to as the “dismissed”, “not dismissed” and “not under court-order” groups, respectively.

Comparing the three groups’ 1990 characteristics is instructive because it indicates how comparable the groups were in the pre-dismissal period. In general, the dismissed and not dismissed districts, which will form the treatment and control groups in the econometric analysis presented below, are quite similar in the pre-dismissal period. Segregation levels, dropout rates, regional composition, racial make-up and measures of district affluence such as median household income suggest the two groups are quite comparable along observable dimensions. Exceptions to this comparability are 33 percent lower average enrollment for the not dismissed group and a slightly lower probability of a not dismissed district serving a central city. The comparability of these groups suggests that dismissal of a desegregation plan is an idiosyncratic event, unrelated to district characteristics.

The districts which lacked a court-ordered plan in 1991 differ in many ways from the districts which had a plan – they have smaller enrollment, lower percentage of black students, are less likely to be located in the South and are more affluent. There is clear non-random selection into having a court-ordered plan in 1991.

Figure 2 plots the trends of the outcome variables. Tentative conclusions about the impact of the end of court-ordered desegregation can be drawn from the figure. Panel A plots the trend of the mean dissimilarity index for the three groups. The not dismissed and dismissed groups have similar trends through the early the 1990s. By the mid 1990s, the dismissed group is experiencing more rapid increase.

As shown in the figure, the relative increase in the mean dissimilarity index of the dismissed group appears associated with the cumulative number of dismissals. The figure provides suggestive evidence that the end of court-ordered desegregation has led to a re-sorting of students which increased segregation.

Panel B plots the percent of enrollment which is white. All three groups trend downward throughout the entire sample period, reflecting national demographic trends. The similar trends of the dismissed and not dismissed groups suggest that dismissal of plans does not alter the demographic composition of school districts. There is no indication of ‘reverse white flight’.

Panel C plots the black-white exposure index for the three groups. The black-white exposure indices trend downward in a similar fashion for all three groups. The similarity of the trends suggests that the decrease in black-white exposure over this period is primarily the product of the demographic trends apparent in Panel B, not the end of court-ordered desegregation.

Panel C also plots the white-white exposure index for the entire sample. The white-white exposure index is interpretable as the percent of white students in the average white student’s school. The index has a trend similar to that of the black-white exposure indices, indicating whites were experiencing a similar decrease in contact with whites as were blacks. This strengthens the claim that the downward trend in black-white exposure is primarily a product of demographic changes.

The increase in the dismissed group’s dissimilarity index apparent in Panel A mechanically decreases black-white exposure. Panel C suggests that this decrease is inconsequential compared to the decrease in exposure resulting from demographic changes.

The formal econometric analysis presented below supports the conclusions drawn from Figure 2. The end of court-ordered desegregation produces a significant re-sorting of students which increases segregation as measured by the dissimilarity index. Nevertheless, dismissals have only a limited impact on black-white exposure and no impact on school district demographic composition.

Empirical Model

The primary empirical model is:

$$(3) \quad y_{it} = \alpha + \sum_{g=-4}^4 \beta_g D_{g,it} + \delta_i + \theta_{jt} + \varepsilon_{it}$$

where y_{it} is the outcome variable for district i at time t , δ_i is a vector of district fixed-effects, θ_{jt} is a vector of Census region j - year t fixed-effects, $D_{g,it}$ is a dummy variable equaling one if district i at time t was released from its desegregation order g years ago ($g = 0$ denotes the year of dismissal). $D_{4,it}$ equals one for all years t in which it has been 4 or more years since district i was released from its desegregation order. The β vector is the parameter of interest.

The model allows the effect of dismissal to vary with the time relative to dismissal. The β vector traces out the adjustment path from the under court-ordered desegregation plan equilibrium to the new post plan equilibrium. There are several reasons why it is likely that dismissal of a court-ordered plan will result in more complex dynamics than a simple discrete shift in the outcome variable (as would be implied by a model which replaced the $D_{g,it}$ vector with a single indicator variable for dismissal).

Many of the dismissals explicitly stipulate a gradual elimination of the desegregation plan. An extreme example is Indianapolis, where the court-ordered plan is being phased out one grade at a time over a thirteen year period. There are also reasons to believe that frictions may prevent immediate adjustment. Parents may wish their children to continue to attend the school in which they were enrolled before the dismissal. Shifts in attendance patterns resulting from changes in choice of residential location will evolve slowly. The

results of the estimation strongly support the hypothesis that dismissals result in a gradual, incremental, adjustment in segregation rather than a discrete shift.

The district fixed-effects control for time-invariant characteristics of the districts such as community preference for racial integration. The Census region-year fixed-effects control for shocks common to districts at the region-year level such as demographic shifts. Time-variant variables such as demographic information may be endogenous to the dismissals and therefore do not enter the model.

The data is restricted to the set of districts under court-order in 1991; the treatment group is the set of dismissed districts; the control group is the set of undismissed districts. The model identifies the causal impact of dismissal on the outcome variable using the idiosyncratic timing of dismissal within the treatment group.²¹

The summary statistics suggest that the set of districts not under court-order would form a poor control group for the set of dismissed districts. In addition, this sample restriction avoids potential bias arising from the legal trend making voluntary desegregation plans less viable for those districts not under court-order. Districts under court-order are not affected by the legal standing of voluntary desegregation plans. If districts operating voluntary desegregation plans are experiencing changes in the outcome variable as a result of the changing legal status of voluntary plans, they would not form a valid control group for the set of treated districts. Restricting the data to those districts under court-order in 1991 likely provides a clean test of the impact of dismissal on the outcome variables.

The identifying assumption of the model is that, absent dismissal, the treated districts would have behaved similarly to set of control districts, conditional on the district and region-year fixed-effects. District-specific trends in the outcome variable are the most likely violation of the identification assumption. Specifically, if treated and untreated districts are systematically trending differently, the identifying assumption may be violated.

In order to control for such trends, the following specification is estimated

$$(4) \quad y_{it} = \alpha + \sum_{g=-4}^4 \beta_g D_{g,it} + \delta_i + \theta_{t,j} + \lambda_t * X_i + \varepsilon_{it}$$

²¹ The effect of dismissal could also be identified by restricting the sample to only those districts dismissed within the date range of the data. Identification would come solely from when a district was dismissed – i.e. there would be no control group. Such an approach, however, yields an extremely small sample size and imprecise estimates.

where X_i is a vector of district-specific characteristics as of the first year the district appears in the sample. The specification controls for district-specific trends using the base period characteristics of the districts. For example, districts with higher than average levels of poverty may experience more rapid loss of white enrollment. Such a situation would induce a negative trend in the exposure index in high poverty districts. A measure of the poverty rate from the pre-dismissal period interacted with a full set of year indicator variables controls for the presence of such a trend. These characteristics are measured *prior* to the dismissals and do not reflect endogenous adjustment to the dismissals.

The typical approach to estimating a panel data model like the one above would be to explicitly include the district fixed-effect terms, δ_i , and estimate via deviation from the mean. The estimates presented here, however, are estimated via first-differencing the data to remove the district fixed-effect.

The first-difference estimator is used in response to severe serial correlation in the model's error term. Estimation by the first-difference estimator yields considerable efficiency gains relative to estimation by deviation from the mean (Wooldridge 2002). Standard errors are clustered at the school district level in all results reported below (Betrand, Duflo and Mullainathan 2004).

Segregation Results

Table 2 presents the results for the dissimilarity index. Each column corresponds to a different specification and presents the full vector of pre and post dismissal coefficients. Pre(-4) denotes the coefficient on the indicator variable for four years prior to dismissal, while Post(0) refers to the year of dismissal. Specification (1) includes school district fixed-effects and year effects. Specification (2) replaces the year effects with region-year effects. Specification (3) includes a vector of base period school district characteristics interacted with a full set of year indicator variables (see equation (4)). The base period characteristics are location in a central city, percent of students who are white, percent of students who are hispanic, number of enrolled students and the number of enrolled students squared. Student enrollment is a crucial control given the difference in enrollment between the treatment and control groups.

Specification (4) adds additional base period characteristics taken from the SDDDB. The sample is restricted to 1990-2001 in order to utilize these data. The SDDDB characteristics are the median income of households with children, the percent of households with children below the poverty line and the percent of households with children which speak a language other than English. Numerous other district characteristics were used in unreported specifications. The results are not sensitive to the exact set of characteristics chosen.

Figure 3, Panel A graphs the results of column (3) and reveals that dismissal results in a resorting of students which increases segregation. Dismissal of a court-ordered desegregation plan has no effect on segregation in the pre-dismissal period and produces a gradual, linear increase in the post-dismissal period. The point estimates for the pre-vector are small in magnitude and estimated imprecisely. The post-vector coefficients increase with the time from dismissal. The sharp trend break around the time of dismissal suggests the estimates reflect the causal impact of dismissal.

The estimated impact of the dismissal of a court-ordered desegregation plan on the dissimilarity index grows from .007 in the year of dismissal to .058 four years after dismissal. While the post(0) coefficient is statistically insignificant, the post(1) is significant at the 15% level and the remaining coefficients are significant at the 5% level or better.

The magnitude of the effect can be interpreted in several ways. The change in the dissimilarity index four years after dismissal is equal to 16% of the index sample mean and to 32% of the 1991 cross-sectional standard deviation of the index. Using these metrics, the impact of dismissal can be viewed as quite large.

Another interpretation involves comparison to the change in the dissimilarity index resulting from the *implementation* of desegregation plans in the 1960s, 70s and 80s. Unreported results which replicate the specification estimated in Reber (2002) on the sample of school districts used here, suggest that the long-run effect of the implementation of a desegregation order on the dissimilarity index is approximately -.15 (this result is very similar to the balanced panel results presented in Reber). Using this result as a metric, the dismissal of a desegregation plan reverses approximately 1/3 of the long-run effect of the plans implementation. It needs to be emphasized that the estimates of this paper represent the short-run effect of dismissal. The long-run effect may be larger.

Appendix Table A1, Panel A displays five robustness checks. The first restricts the estimation sample to the set of districts with enrollment exceeding 10,000 in 1991. The enrollment restriction eliminates a number of small districts, primarily from the control group, and makes the control and treatment groups more comparable in terms of enrollment size. The second weights the data by student enrollment in the base period. The third includes a full set of district-specific linear trend terms to assess if district-specific trends in the outcome variable are biasing the results.

The fourth uses a balanced panel of districts and includes dismissed districts only if they contribute to the identification of the entire dismissal vector. In the standard specification, the individual coefficients of the dismissal vector are not all identified by the same set of districts. For example, districts dismissed in 2000 do not contribute to the identification of the 2 through 4 years post-dismissal coefficients. It is possible that the increase in the treatment effect with time from dismissal is a spurious result of the differing set of districts identifying the parameters. The final robustness check replicates column (3) of Table 2 using the nonwhite-white dissimilarity index. The results are robust to all of the above specifications.

The changes in the dissimilarity index can be viewed as primarily reflecting the response of school district policy makers to the dismissal of a desegregation plan. In order to assess the response of parents and children, demographic flows by race into and out of the school district are examined. Table 3 presents the results of estimating the empirical model with the log of enrollment by race as the dependent variable. The specification mirrors column (3) on Table 2 and includes district fixed-effects, region-year effects and a set of base period characteristic-year interactions.

There is no evidence of a response by black, non-white or white parents and students to the dismissal of court-ordered desegregation plans. The school enrollment equilibrium, heavily influenced by the imposition of desegregation plans in many metropolitan areas, is not broken by the dismissal of the plans in the short run. There is no evidence of 'reverse white flight'.

The exposure index measures the extent of contact between the races. It can be viewed as incorporating the net effect of changes in the sorting of students across schools

and changes in the school district wide demographic composition. Table 4 presents results for the exposure index with specifications otherwise identical to those on Table 2.

Figure 3, Panel B graphs the results of column (3). Unlike the dissimilarity index estimates, there is some indication of a downward trend in the pre-dismissal period, although the estimates in the pre-dismissal period cannot be distinguished from zero and there does appear to be a trend break around the time of dismissal.

Four years after the termination of a desegregation plan, the exposure index has decreased by .021, indicating the average black student in a dismissed district is attending a school with 2 percent fewer white students – a rather limited increase in segregation. The point estimates for one through four years after dismissal are all significant at the 5% level. The effect four years post-dismissal is equal to approximately 5% of the sample mean of the index and approximately 10% of the 1990 cross-sectional standard deviation of the index.

The estimated long-run effect of desegregation order implementation on the exposure index is .06 (again based on replication of the specifications used in Reber (2002)). Dismissal therefore reverses approximately 1/3 of the long-run effect of desegregation as measured by the exposure index.

The results for the exposure index are less robust than those for the dissimilarity index (see Appendix Table A1, Panel B). Restricting the sample to districts with a 1991 enrollment of over 10,000 in specification (5) produces small, statistically insignificant results. The district-specific linear time trends included in specification (6) produce similarly insignificant results. Note that the trends may capture part of the true effect of dismissal and bias the parameters downward.

Numerous unreported specifications assess whether the effect of dismissal on the outcome variables considered above differs by region, central city, size of enrollment and numerous other district characteristics. There is no evidence that the effect of dismissal varies by any observable characteristic. Particularly notable is a lack of heterogeneity between southern and non-southern school districts.

The dissimilarity and exposure index results may appear inconsistent with each other. While the dissimilarity index suggests a significant increase in segregation, the average exposure of blacks to whites decreased by only 2 percent. The results, however, are fully compatible.

Districts with low segregation as measured by the dissimilarity index will have low black-white exposure if the percent of students in the district who are white is relatively low. In such districts even a large change in the dissimilarity index will cause a small change in black-white exposure.

Viewed jointly, the dissimilarity and exposure index results suggest that court-ordered desegregation fails to significantly increase black-white exposure in the post 1990 period, even as it succeeds in enforcing desegregation as measured by the dissimilarity index. The failure to achieve increased exposure is explained by 'white flight' and the declining percentage of white students nationally.

It is interesting to note that federal judges often explicitly use measures such as the dissimilarity index, which measure segregation given the racial composition of the district, to assess the efficacy of desegregation plans. Measures such as the exposure index are typically not used because desegregation plans cannot influence district-level racial composition and hence have only limited ability to influence segregation defined in this manner.

There are several limitations to the above estimates that should be noted. The post-vector coefficients trace out the transition from the under court-order equilibrium to the new, post desegregation plan equilibrium. If the transition to the new equilibrium takes longer than four years post dismissal, then the results underestimate the full, long-run effect. Until additional data become available, this uncertainty cannot be resolved. Note, however, that the results do not indicate any deceleration in the rate of increase in segregation four years after dismissal.

Finally, the dissimilarity index estimates assesses the effect of dismissal on segregation between schools. Segregation may also occur within a school. The estimates cannot assess if the court dismissals have had an effect on within school segregation.²²

School District Expenditures

²² Court-ordered desegregation focuses heavily on segregation between schools. As a result, it seems likely that the court-order dismissals will have little impact on within school segregation. Alternatively, Clotfelter, Ladd, Vigdor (2003) presents evidence that between and within school segregation are substitutes. After a dismissal, school district officials, no longer able to implement between school desegregation, may attempt to reduce within school segregation.

There is substantial anecdotal evidence that the termination of desegregation plans is often accompanied by pledges of large scale investment in the educational infrastructure of minority neighborhoods (see footnote 6). Data obtained from the Census Bureau's Public Elementary-Secondary Education Finance Data are used to assess the prevalence of such expenditures.

Table 5, columns (1), (2) and (3) contain the estimation results for total expenditures, capital expenditures and construction expenditures, respectively. There is no evidence that expenditures respond to the dismissal of a court-ordered desegregation plan. Either the anecdotal evidence is misleading or expenditures in minority neighborhoods represent substitution away from other investments, such as school construction in non-minority neighborhoods.

Many desegregation plans involve court-mandated funding from governmental sources other than the school district. For instance, many plans mandated funding from the state government. Column (4) examines the response of school district revenues from the state to the dismissal of a plan. There is no evidence of a response. Unreported specifications which examine revenue flows from other levels of government similarly find no evidence of a change.

IV. Dropout Rate, Private School Enrollment and Property Value Results

The above results suggest that the end of court-ordered desegregation has only a limited impact on the exposure of blacks to whites. There is therefore an expectation that any effect of dismissals on black outcomes operating through peer effects will be limited. This does not mean, however, that the overall impact of dismissals on blacks will be limited.

As time passed from the *Brown* decision, desegregation cases began to focus on more than racial integration. The adequacy of financial funding for minority students and minority student achievement became explicit goals. The 1977 *Milliken II* decision allows courts to mandate spending on compensatory educational programs for minority students (Orfield and Eaton 1996). The *Freeman* decision explicitly allows courts to consider the "quality of education" in deciding whether or not to release districts from their desegregation plans (Lindseth (2002); Parker 2000).

When a district is released from its plan, it no longer has an independent body, the courts, constantly monitoring its performance in regards to the educational outcomes of minority students. This may reduce the effort and resources expended on minority students. The end of court-ordered desegregation may have a significant impact on black student outcomes even in the absence of a significant change in the exposure of blacks to whites.

Outcome Variables

The 1990 and 2000 two-period panel is used to examine the effects of court dismissal on dropout rates by race, private school attendance by race and property values. These measures are obtained from the 1990 and 2000 SDDB school district level tabulation of the U.S. Census. The status dropout rate is defined as :

$$(5) \quad S_{dt} = \frac{Drop_{dt}}{Tot_{dt}}$$

where $Drop_{dt}$ is the number of civilian 16 – 19 year olds living at time t in the area served by district d who are not enrolled in high school and do not hold a high school degree (GED holders are counted as having a high school degree) and Tot_{dt} is the total number of 16 – 19 year old civilians. The status dropout rate is a measure of the *stock* of dropouts residing in a given school district.

The SDDB is a unique source of information on private school attendance because it tabulates private school attendance by the public school district in which a student *resides*, not where the student attends school. The private school attendance rate is defined as the percent of total enrolled students residing in a district who are enrolled in private school and therefore summarizes the percent of *potential* students each public school district has enrolled in private school. Average housing values for a school district are available from the SDDB. The property value data is not available by race.

Summary Statistics

Table 6 presents summary statistics for 1990 and 2000 for both black and white students in districts under court-order in 1991. For each race, means are presented for two groups, those districts dismissed between 1991 and 1999 and those not dismissed in this

time frame. The first group forms the treatment group and the second group forms the control group.²³ The sample is again restricted to districts under court-order in 1991.

The table reinforces the conclusions drawn from Table 1 – the dismissed and non-dismissed districts are remarkably similar along observable dimensions in the 1990 pre-dismissal period. The status dropout rate declines for all groups over the course of the 1990s.²⁴ Private school attendance appears to be relatively unchanged over this period for both whites and blacks. Housing values increase moderately over the 1990s.

Empirical Model

The ideal two-period panel model would utilize the micro long form census data :

$$(6) \quad y_{kit} = \alpha + \beta(Y2_t * D_i) + \delta_i + \theta_{jt} + \lambda X_{it} + \eta M_{kit} + \varepsilon_{kit}$$

where i indexes the school district of student k at time t , $Y2_t$ is an indicator variable equaling one if $t = 2000$, D_i is a dummy variable equaling one if district i was dismissed from its desegregation order before time $t = 2000$, δ_i is a district fixed-effect, θ_{jt} is a set of census region j – year t fixed effects, X_{it} is a vector of district level time-varying covariates and M_{kit} is a vector of student level time-varying covariates. β is the coefficient of interest.

Unfortunately the micro census data does not contain a school district identifier. The SDDDB, however, allows for estimating the pooled regression which follows from (7) :

$$(7) \quad \bar{y}_{it} = \alpha + \beta(Y2_t * D_i) + \delta_i + \theta_{jt} + \lambda \bar{X}_{it} + \eta \bar{M}_{it} + \varepsilon_{it}$$

where \bar{y}_{it} denotes the district i average of y_{kit} and \bar{M}_{it} denotes the district i average of the covariates at time t .²⁵ The model assumes that dismissal results in an equal, discrete shift in

²³ Note that the treatment and control groups differ slightly from those used in section III. The 2000 Census data was collected in the spring of 2000 – during the 1999 - 2000 school year. District dismissed in the 2000, 2001 and 2002 school years, which are part of the treatment group in the 1987-2002 panel, are part of the control group for the 1990 and 2000 panel.

²⁴ These stylized facts are consistent with estimates of the trend in status dropout rates reported in a National Center for Educational Statistics report, NCES (2001). The levels reported here are somewhat higher than those cited in the NCES report, likely due to the fact that the set of districts under court-order in 1991 is not a nationally representative set of districts.

²⁵ Due to a quirk in the construction of the SDDDB and differences between the 1990 and 2000 versions of the data, the average status dropout rate for district i is measured for 16 to 19 year olds, while the time-varying covariate averages, \bar{M}_{it} , are measured for all children. See Appendix B for a more through discussion of this and related data issues.

the outcome variable for all dismissed districts, regardless of when the district was dismissed.

The segregation results from section III suggest that dismissal of a desegregation plan produces gradual changes, not a discrete shift. The results from section III therefore motivate the following model :

$$(8) \quad \bar{y}_{it} = \alpha + \beta(Y2_t * T_i) + \delta_i + \theta_{jt} + \lambda X_{it} + \eta \tilde{M}_{it} + \varepsilon_{it}$$

where T_i is equal to the years since dismissal, relative to 2000. For instance, a district dismissed in 1996 has $T_i = 4$. The effect of dismissal is a linear function of time since dismissal.

The 1990 and 2000 panel model is quite similar to the model estimated in section III. Two differences, however, bear mention. First, because the model uses pooled data, the observations are weighted by cell size. Weighting may improve the efficiency of the estimates.²⁶ In addition, weighting more closely mimics the motivating micro-level regression (equation (6)).²⁷

Second, the model here includes time-varying covariates. The interpretation of the results differs depending on whether time-varying covariates are included. If they are not included, the estimated effect is the net effect of dismissal on the outcome variable. If they are included, the estimated effect is the effect of dismissal holding all other student characteristics constant (i.e. controlling for any demographic shifts). Results of the model with and without time-varying covariates are presented.

The data contain only two periods, making it difficult to rule out the possibility that the estimates reflect selection of districts into dismissal based on black student outcomes, as opposed to the causal impact of dismissal. The extreme similarity of the dismissed and non-dismissed districts in 1990 is reassuring, but cannot rule out the possibility that the dismissed and not dismissed districts were trending differently.

²⁶ Under the assumption that the errors in the motivating micro regression are i.i.d., weighting leads to efficiency gains by reducing heteroscedasticity in the error term produced by grouping the data.

²⁷ The district level data used in the 1987 – 2001 panel model are *not* individual level data pooled to the district level. The segregation and other outcome measures are intrinsically district level measures. There is no rationale for weighting these models on efficiency grounds and they are therefore not weighted. Note that, as displayed on Table A1, columns (2) and (7), the segregation estimates are insensitive to weighting.

An alternative model to the fixed effects specification is a lagged dependent variable specification.

$$(9) \quad \bar{y}_{it} = \alpha + \beta D_i + \mu \bar{y}_{it-1} + \theta_{jt} + \lambda X_{it} + \eta \bar{M}_{it} + \varepsilon_{it}$$

The fixed effect model restricts μ to equal one, while the lagged dependent variable model allows it to vary.

Guryan (2001) demonstrates that if treatment is a function of either time-invariant characteristics or a lagged dependent variable, then a fixed effect model and a lagged dependent variable model will provide an upper and lower bound of the true treatment effect. If dismissal is positively selected on the lagged dependent variable, then the lagged dependent variable model provides the upper bound. If the treatment is negatively selected, then the fixed effects model provides the upper bound.

The sample statistics presented on Table 5, as well as the qualitative evidence presented above in Legal Background section, suggest there is neither positive nor negative selection into dismissal. Use of the lagged dependent variable model provides more formal evidence.

Results

Table 7, panel A, presents the results for the black status dropout rate. The left panel displays the fixed effect estimates, while the right panel displays the lagged dependent variable estimates. Columns (1) – (4) employ the linear dismissal indicator of equation (8). Columns (1) and (2) present the results of specifications without time-varying covariates. Column (1) contains only the district fixed effects and a year fixed effect. Column (2) adds a vector of variables interacted with an indicator for year 2000. These variables include census region, central city, 1990 enrollment and 1990 enrollment squared. These interaction terms control for trends in the outcome variable associated with the given characteristic.

Column (3) adds a vector of student level covariates. These covariates control for demographic shifts. Several of the covariates, such as the percent of parents foreign born and percent of children born out of state, attempt to control for migration. Additional student level covariates include mother’s education (specified as indicator variables for no high school degree and college degree), indicator for being beneath the poverty line and household income and household income squared. The covariate means are calculated over

the appropriate race. For instance, if the specification uses the black dropout rate as the dependent variable, the means are calculated over the population of black children.

Column (4) adds three district level covariates intended to control for shifting demographics and changing economic conditions. These include percent of total households in poverty, percent of total households with at least one adult unemployed and percent of total households who speak a language other than English. These variables pertain to the entire population of the school district (i.e. the means are calculated using all races).

Column (5) replicates column (4) using the indicator treatment variable – see equation (7). It includes a placebo indicator variable for those districts dismissed after 1999. If changes in the outcome variable for the dismissed districts are causal then there should be no evidence of a treatment effect for the placebo districts. There are 22 districts in the dismissed group and 22 in the placebo group.

Estimation of the fixed effect model suggests that dismissal results in an increase in the frequency of black students dropping out. Column (3) indicates that dismissal increases the dropout rate by .0044 for each year since dismissal. To interpret this result consider a district which was dismissed in 1996. Such a district, with four years since dismissal (the average years since dismissal in the sample is 3.5), will experience a black dropout rate approximately .017 higher than if it had not been dismissed. The mean dropout rate for dismissed districts in 1990 is .15, implying that dropout rates increase by approximately 10 percent. Virtually all of the dismissed districts were dismissed in 1994 or after, suggesting that the results should not be extrapolated beyond 6 years since dismissal.

Column (5), which employs the indicator dismissal variable, suggests that dismissal increases dropout rates by .014 – a result quite similar to that produced by the years since dismissal specification. The estimate is not precise, however. The placebo indicator for districts dismissed outside the date range of the data is extremely small in magnitude and insignificant, suggesting the results represent the causal impact of dismissal.

Panel B of Table 7 allows the effect of dismissal to vary by whether or not the district is located in the South. The parameterization is motivated by the fact that a large body of research suggests court-ordered desegregation was more successful in the south than

outside the south.²⁸ The results are striking. Across the columns, the coefficient on the dismissed indicator is larger than in Panel A. The dismissed * south interaction term is of an equal magnitude, but opposite sign. Both the interaction and main terms are estimated precisely.

These results suggest that the entire increase in dropout rates associated with dismissal of a desegregation plan is generated by non-southern districts. The placebo coefficients are again small and insignificant, suggesting the coefficients on the true dismissal variables represent the casual impact of dismissal. The placebo estimates are similarly insignificant across a range of unreported specifications. Note, however, that there are only five districts in the non-southern placebo group.

Columns (1) - (4) indicate that each year post dismissal increases black dropout rates by .01 for a non-Southern district. This suggests that a non-Southern district four years since dismissal will have experienced an increase of .04 in the rate of blacks dropping out, an increase of approximately 25% from the 1990 mean for non-southern dismissed districts.

A southern district, four years post dismissal, will have experienced no change in the rate of blacks dropping out (the effect of dismissal for southern districts is derived by adding the coefficients from the main and interaction terms).²⁹

Columns (6) – (10) replicate columns (1) – (5), but estimate the lagged dependent variable model. The lagged dependent variable results are similar to the fixed effect results, although they are estimated less precisely. There is therefore no evidence of selection into treatment based upon past dropout rates.

It would be useful, in assessing the causality of the estimates, to examine the trend in the black dropout rate in the pre-1991 period. Similar pre-trends between the treatment and

²⁸ See for example Orfield and Eaton 1996, pg. 14 – 16. A major reason for the relative success of desegregation in the south is the large average size of southern school districts. The large size of districts made 'white flight' more difficult. Note, however, that Guryan 2004 finds no difference in the effect of desegregation on black dropout rates in southern versus non-southern districts.

²⁹ Estimating the specifications without weighting by cell size produces point estimates quite similar to those on Table 7 (although they are somewhat less precise). An exception is the point estimates for the two placebo variables in column (5), Panel B. The unweighted placebo coefficients are similar in magnitude to the dismissed coefficients. Across a wide range of unweighted specifications, however, the placebo estimates never obtain statistical significance at the 5% level. An alternative to the estimates on Panel B of Table 7 are estimates identical to those in Panel A, but with the sample restricted to non-southern districts. This strategy produces an estimated effect of dismissal on non-southern districts very similar to that suggested by Panel B. Significantly, the placebo coefficient is small in magnitude and estimated very imprecisely for both weighted and unweighted estimates. These estimates are available from the author upon request.

control groups would increase the confidence that the estimates reflected the causal impact of dismissal. Unfortunately the 1980 school district tabulation of the Census (Summary File 3f) does not permit calculation of dropout rates by race.

The dropout rate for all races, however, can be consistently constructed across the 1980, 1990 and 2000 censuses. Figure 4, Panel A plots the mean of this variable for the treatment and control groups for both southern and non-southern districts. The treatment and control groups have very similar trends in the pre-period, 1980 to 1990.

Panels B and C plot the percent of blacks beneath the poverty line and the median household income. The pre-trends for the treatment and control groups are again similar. The similarity in the pre-trends of the three variables, in particular the dropout rate, increases the confidence that the estimates reflect the causal impact of dismissal

Appendix table A2 provides further evidence on the causality of the estimates. It is unlikely that dismissal causes a single discrete change in dropout rates. Desegregation plans are often phased out over time and any change in the school environment likely occurs gradually. Table A2 presents models which include *both* the dummy variable dismissal variable of equation (7) and the years since dismissal variable of equation (8) and allows for heterogeneity by southern and non-southern district.

The simple intercept shift model is decisively rejected in favor of the years since dismissal parameterization. In all specifications, the linear dismissal coefficients are statistically significant, typically at the 1% level, while the indicator dismissal coefficients are imprecisely estimated. The increase in the dropout rate is a function of time since dismissal. This provides suggestive evidence that dismissal results in a causal increase in the black dropout rate.³⁰

Table 8 presents results for black private school attendance. They indicate that dismissal increases black private school attendance in the non-south, but not in the south. Using the estimates in column (3) of panel B, the typical non-southern district four years since dismissal experiences an increase of .012 in the rate of private school attendance. Given the 1990 mean non-southern private school attendance rate of .07, this implies an increase of approximately 15 percent. The indicator treatment specification produces no

³⁰ The results are robust to excluding the two districts dismissed in 1991. The linear dismissal variable has relatively extreme values for these districts.

indication of an effect of dismissal, suggesting the result for the years since dismissal specifications (columns (1) – (4)) may primarily reflect the districts dismissed early in the sample period.

Dropout rate and private school attendance results for white and non-white children, estimated via the fixed effects model, are presented on Table 9. There is no evidence of an effect for either of these outcomes for white children. Most interesting are the results for private school attendance. The evidence from the 1987 – 2001 panel, presented in section III, similarly fails to find any evidence of a change in white school attendance patterns, suggesting dismissal of court-ordered desegregation plans does not reverse the ‘white flight’ sparked by their implementation.³¹

Table 10 presents the results for property values. There is no indication that dismissal has an impact on property values. It is unfortunate that the data used here does not allow for calculating property values by race. The results for the other outcome variables indicate the effects of a desegregation plan dismissal are restricted to black students and families. It is possible that there is an impact on black property values, but that the strategy used here, which examines average property values for all races, is not subtle enough to estimate an effect.

V. Conclusion

The results of this paper suggest that dismissal of court-ordered desegregation plan results in a causal, gradual increase in racial segregation. No evidence is found of an effect on white students along any dimension. Despite the well documented exit of whites from public schools operating under a desegregation order, it appears that the dismissal of these plans does not induce whites to return. In addition, no evidence is found of an effect on housing values or on school district expenditures. Finally, dismissal of a court-ordered plan results in the exit of black students from public schools in non-southern districts, both via dropping out and via entering private school. The increased rate of exit suggests that dismissal reduces the net value of education for black students in dismissed districts.

³¹ Although panel D, columns (1) and (2), suggest that non-southern school districts experience an *increase* in the rate of white private school attendance, the result is not robust to the inclusion of micro-level covariates which control for demographic shifts.

A crucial concern is whether this increased rate of exit is caused by the dismissals or reflects judges choosing to dismiss districts experiencing negative outcomes for minority students. The bulk of the evidence suggests that it is causal. The similarity of the dismissed and non-dismissed districts along observable dimensions in the pre-dismissal period, the similarity of the fixed effect and lagged dependent variable estimates, the placebo falsification checks and the rejection of the indicator dismissal parameterization in favor of the years since dismissal parameterization all suggest dismissal generates a causal increase in the exit of blacks from dismissed districts.

A limitation of the dropout and private school results is their reduced form nature – they cannot establish the mechanism or channel via which the dismissals are impacting educational outcomes. Despite this limitation, a discussion of possible channels is warranted. Two primary channels exist through which dismissal may negatively impact black student outcomes.

First, the increased segregation documented in section III may have a negative impact due to peer effects. The estimated decrease in exposure between whites and blacks is quite small, however. The typical black student in a dismissed school district experiences only a two percent drop in the percent of white students in his school. In addition, there is no heterogeneity in the response of racial segregation levels by southern vs. non-southern districts. It is therefore unlikely that peer effects alone explain the increased black dropout rates and private school attendance experienced in dismissed non-southern districts.

Second, while a district is under court-order, it has an independent body, the courts, constantly monitoring its performance in regards to the educational outcomes of minority students. The removal of court oversight may lead school district officials to reduce both the level of financial resources expended on black students and the level of effort expended on maintaining minority student performance. It seems likely that this second channel is at least partly responsible for the increased rate of exit of blacks from non-southern dismissed districts.

An open question raised by the results of the paper is: why do dismissals outside the south result in negative black student outcomes, whereas there is no effect of dismissal in the south? A large body of research suggests court-ordered desegregation was considerably more successful in the south than outside the south. Given the relative success of

desegregation in the south, it is possible that the court-orders in the south were no longer exercising a constraint over behavior that impacts minority student outcomes. The mandate of the court-orders may have been internalized into the utility function of school administrators. The removal of the orders therefore has no effect. In contrast, in northern districts, where the plans had been less successful, the mandate of the orders may not have been internalized by district officials.

Finally, it is important to note that the results of this paper represent the short run response to dismissal. The long run response may differ from the short run response documented here. As more data becomes available, it will become possible to estimate the long run effects of the end of court-ordered desegregation.

Appendix A

Brief History of Desegregation Law :

- **Plessy v. Ferguson, 163 U.S. 537 (1896).** Racial segregation does not constitute discrimination under the 14th Amendment. The “separate but equal” doctrine is established.
- **Brown v. Board of Education of Topeka, 347 U.S. 483 (1954).** State-imposed segregated schools are “inherently unequal”. The “separate but equal” doctrine is struck down.
- **Brown II, 349 U.S. 294 (1955).** School desegregation shall occur via plans developed by the federal judiciary. No time table for desegregation was set and the meaning of the term desegregation was left ambiguous.
- **Civil Rights Act (1964).** Discrimination banned in any school receiving federal aid. The Johnson Administration enforced this ban and had the Justice Department initiate numerous desegregation lawsuits.
- **Green v. County School Board of New Kent County, 391 U.S. 430 (1968).** Racially dual school systems must be “dismantled root and branch.” Defined the areas subject to desegregation – facilities, staff, faculty, extracurricular activities and transportation. These areas became referred to as the “Green factors.”
- **Swann v. Charlotte-Mecklenberg Board of Education, 402 U.S. I (1971).** Busing is allowed as a means of achieving desegregation.
- **Keyes v School District No.1, Denver, Colorado, 413 U.S. 189 (1973).** Requirements established for declaring school systems which lacked legally mandated segregation as having a dual system. Desegregation cases became viable in the north and west where segregation had not been legally mandated.
- **Milliken v. Bradley, 418 U.S. 717 (1974).** Inter-district desegregation orders are ruled illegal unless discrimination can be proven to have occurred across district boundaries.
- **Mid-1970s to 1991.** The Supreme Court left desegregation law essentially unchanged between the mid-1970s and 1991. The number of federal court desegregation orders, which peaked in the early 1970s, declined in the late 1970s and early 1980s. The last desegregation order for a large district was San Jose in 1986.
- **Board of Education of Oklahoma City v. Dowell, 498 U.S. 237 (1991).** The requirements for a district being declared unitary (meaning the district was no longer operating an illegal, racially dual school system) were established. Prior to the ruling, the term had no clear legal meaning. Once a district is declared unitary, it must be released from court control and is no longer legally obligated to maintain a desegregated school district. Even immediate and complete resegregation is acceptable, as long as the district does not state its attendance policies are explicitly aimed at achieving racial segregation. Prior to the decision, it has been widely presumed that districts released from court control had an obligation to maintain a desegregated district (Orfield (2001) pg. 5; Lindseth 2002 pg. 57).
- **Freeman v. Pitts, 503 U.S. 467 (1992).** Districts may be declared partially unitary by achieving one or more, but not all, of the Green factors. Factors for which the district has achieved unitary status no longer fall under court control.
- **Missouri v. Jenkins, 515 U.S. 70 (1995).** Limited enforcement options available to the district courts (for example, efforts to voluntarily induce white students from outside the district to attend district schools are not permissible). The restoration of local school control should be a primary goal of all federal desegregation cases.

Appendix B

Data Appendix

Rossell and Armor Survey Data

The sample of school districts used in this paper is restricted to the set of districts identified in the Rossell and Armor survey data. I am indebted to Christine Rossell and David Armor for providing me with their data. The original research was funded by the U.S. Department of Education from 1990 to 1993 with Christine Rossell and David Armor as co-principal investigators and Roger Levine and Lauri Steele, American Institutes for Research, as contract managers. Published works using this data file are Rossell (2003), Rossell (2002), Armor and Rossell (2002), Rossell and Armor (1996) and Steel, Levine, Rossell and Armor (1993). The sampling frame for the survey data was the set of U.S. school districts in which two or more schools offer at least one grade level (K-12) in common. 6,392 of the 16,986 districts in the 1989/1990 CCD meet this criterion. Districts with enrollment of 27,750 or greater were sampled with certainty, as were districts which were MSAP (a federal magnet school program) grantee districts. Remaining districts were sampled based on stratum for size and racial composition. Larger districts and districts with diverse racial compositions were oversampled. See Appendix A of Steel and Levine (1994) for details. The original survey sample contained 602 districts. The sample used in this paper contains 571 districts. The discrepancy arises from the fact that several districts included in the original sample do not map into the CCD. For example, the subdistricts of New York City were considered separate districts in the survey sample, but constitute a single district in the CCD. Two districts closed over the time period of the data. These districts remain in the sample until the year of closure. A single district, Chardon Public Schools, NE, had a significant change in its boundaries. The district was not under court-order in 1991 and therefore is not in sample used in most of the reported regressions – i.e. those districts under court-order in 1991. Exclusion of this district in the specifications in which it was included does not affect the results. Districts-year observations with insufficient race data were omitted from the estimation sample. Insufficient race data is defined as having the sum of enrollment by race equal to less than 90% of total enrollment. The results reported in the paper, however, are unchanged when these observations are included. All observations for Tennessee in 1997 are dropped due to clear error in the racial variables for the entire state. Tennessee ceased to report racial data in 1999 and all Tennessee districts therefore drop out of the sample from 1999 on.

Legal Variables

I construct two district level variables based on the legal status of the school district in relation to court-ordered desegregation plans. The first variable indicates the year the district was dismissed from its desegregation order if it was dismissed in 1991 or after. Many of the dismissals are unitary status declarations. Others are terminations of judicial involvement in the school district without a formal unitary status declaration. In some cases unitary declarations are made and court supervision continues for a limited period of time. The dates of dismissal attempt to reflect the date court supervision of the school district ended. Some rulings involve school districts agreeing to take a certain course of action, for instance making capital investments in minority neighborhoods, over the course of several years. These type of stipulations are not considered court supervision. The second variable indicates whether or not the district was under a court-ordered desegregation plan in 1991, the year of the first of the three early 1990s Supreme Court decisions relating to desegregation.

I use multiple sources to generate these variables :

1. Ma (2003), a spreadsheet produced by the Harvard Civil Rights Project titled “List of School Districts Previously Under Desegregation Orders Dismissed between 1990 – 2002”, is the primary source of the year of dismissal variable. A conversation with Jacinta Ma, the author of the spreadsheet, suggests it is accurate for very large districts, but may not be complete for smaller ones. As a result, I supplement the data in Ma (2002) with information from other sources.

2. The Rossell and Armor data contains a variable indicating if the school district has a desegregation plan as of Oct. 1, 1991. Another variable indicates the source of the plan, in particular whether or not it was a court-ordered plan. The Rossell and Armor data is the primary source of the under plan as of 1991 variable.
3. Appendix C of Welch and Light contains a bibliography of legal sources for each of the districts in the Welch and Light sample. For some of these districts, a date of court-order dismissal is given.
4. The Civil Rights Division of the United States Justice Department maintains a list of all school desegregation cases currently active to which the United States is a party. The list also contains the names of all school districts involved in each case. The Civil Right Division provided the author with a copy of the list current as of March 8, 2003. Historically, the Justice Department was one of the most active litigants in school desegregation cases. The list almost certainly contains a non-trivial percentage of desegregation cases still active in the federal courts.
5. Legal opinions, both published and unpublished, issued by Federal District and Appeals Courts, and available via Lexis-Nexis and Westlaw, often contain extensive information on desegregation cases.
6. The Federal District Court dockets for desegregation cases typically contain information about the status of the case and the date of dismissal if applicable. The docket numbers, required to obtain the dockets, were obtained in two ways. First, docket numbers appear on opinions issued by Federal District Courts (see above). Second, Courtlink, a service provided by Lexis-Nexis, allows for complex electronic searches of Federal District Court dockets. The dockets are available on Courtlink at varying dates for the different District Courts. Typically the dockets are available from the late 1980s or very early 1990s forward. A search using the following parameters was performed: nature of suit = "440" (denoting the case as civil rights, other), keywords = "school~ AND segregat~ OR desegregat~ OR unitary" (where the ~ is a root expander). The search provided a list of docket numbers, for both active and closed cases, meeting the above criterion. The search is the most sophisticated currently possible. However, there are several potential sources of error in the search. First, cases with no activity in the date range of the database will be missed. Second, the dockets must contain the specified keywords. A very sparse docket from a desegregation case could potential lack the keywords used in the search. Second, while all Federal District Court dockets from the relevant dates appear in the database, they are not updated unless a user specifically requests, and pays, for the update. As a result, a docket concerning a desegregation case that contains the keywords in an entry dated after the docket was initially downloaded into Courtlink and which has not been subsequently updated, will be missed by the search. As a result of these potential sources of error, the search, while the best possible, cannot be viewed as generating a comprehensive list of desegregation case dockets. The actual dockets were obtained from PACER, an electronic service maintained by the federal court system. The methodology of jointly employing Courtlink and PACER was suggested to me by Margo Schlanger, a professor at Harvard Law School and an expert on this type of empirical legal research. Professor Schlanger laid out the precise methodology employed.³²
7. A variety of published sources, including books, journal articles, newspaper articles, magazine articles, minutes of school board meetings, school budgets, etc. were utilized. In particular, the electronic archives of Education Week, the national publication with the greatest commitment to covering school desegregation issues, was used.
8. Personal communication with school district officials were used in cases when all of the aboves sources failed to provide sufficient information.

³² An example of using PACER for legal research is Schlanger (2003). Because Courtlink is a very new service, there are currently no published examples of its use for legal research (based on a conversation with Prof. Schlanger). Theodore Eisenberg & Margo Schlanger, *The Reliability of the Administrative Office of the U.S. Courts Database: An Empirical Analysis*, Notre Dame L. Rev. (forthcoming 2003). See Lynn (2002), Schlanger (2003), Shack and Yates (2002) and Westbrook (2002).

School District Data Book

The School District Data Book (SDDB) is a public school district level tabulation of the U.S. Census which focuses on children (the 2000 version is referred to as the School District Tabulation – STP2). A child is included in a districts tabulation if he/she lives within the territory of the district and his/her grade level is offered by the school district. In 2000, a child is defined as a person age 0 to 17 or a person 18 or 19 years of age who has not graduated from high school. In 1990 a child is defined as a person age 3 to 19 who has not graduated from high school. As noted in the text, the status dropout rate is calculated only for 16 to 19 year olds, while the individual level covariates are tabulated from all children. In addition, several of the individual level covariates are tabulated by household or parents of children as opposed to being tabulated by child. If the pooled regression (equation (7) in the text) was being calculated from micro data, the individual level covariates would be averaged over the total number of children in the district. The following covariates from the SDDB, however, are averaged over the set of parents with children : mother’s education and parent foreign born. Each parent with a child contributes a single observation to the calculation of the mean, regardless of the number of children the parent has. Ideally, each *child* would contribute a single observation to the calculation of the mean. Similarly, the household income variables are averaged over the set of households with children, as opposed to being average over all children. In all of the above cases, the calculated means approximate the true mean calculated over the number of children in the district. One important difference between the CCD data, used in section III, and the SDDB data, used in section IV, bears mention. The CCD maintains hispanic as a separate racial category equivalent to white, black, asian and native American. The SDDB, however, treats hispanic background as an aspect of ethnicity. For the purposes of section IV, white refers to non-hispanic white children and black refers to hispanic and non-hispanic black children. The 2000 SDDB does not contain information on black children separately tabulated by ethnicity.

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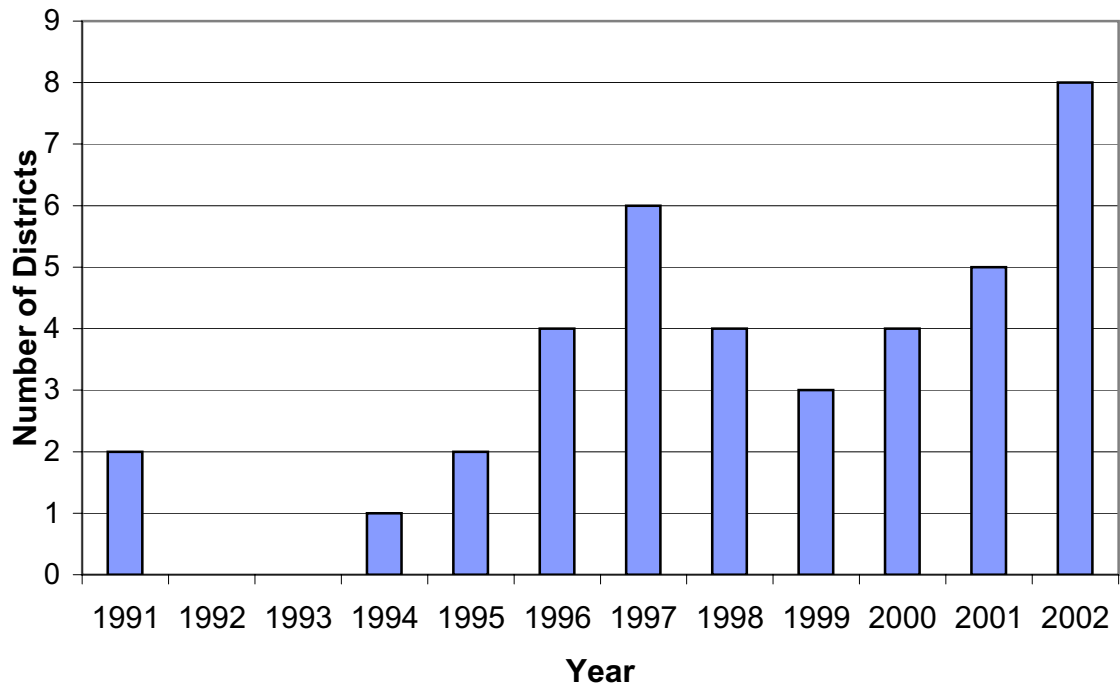
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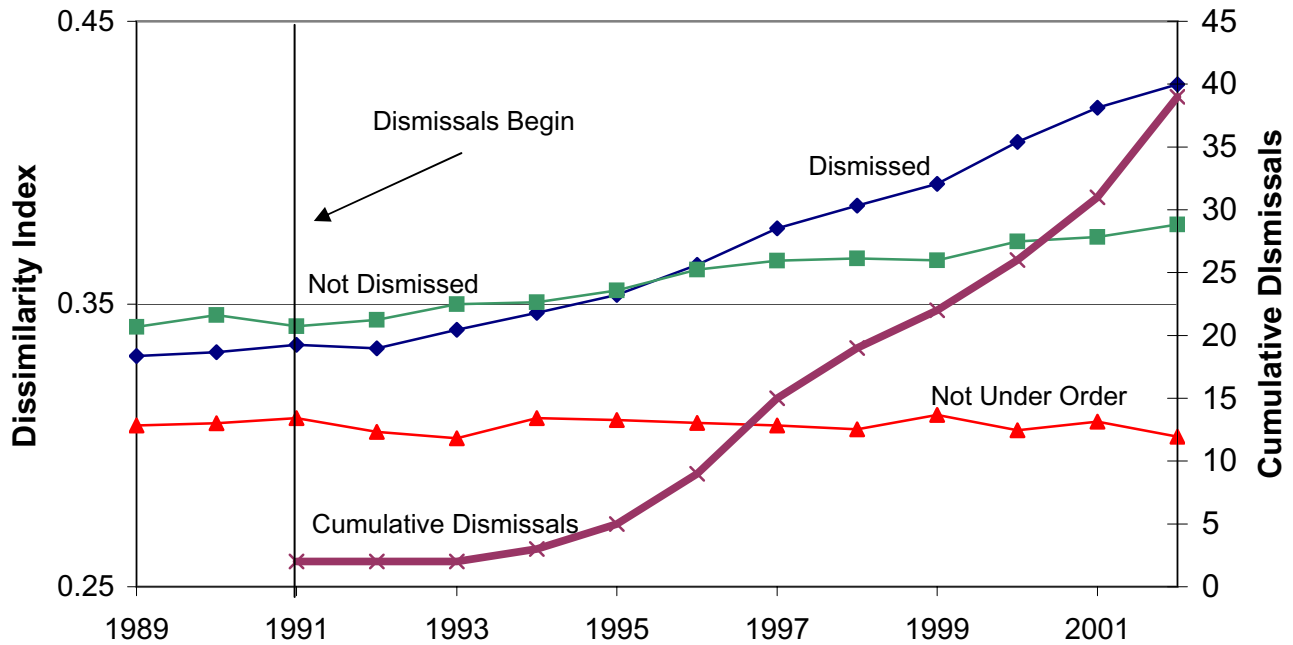
Figure 1: Desegregation Order Dismissals Post 1990



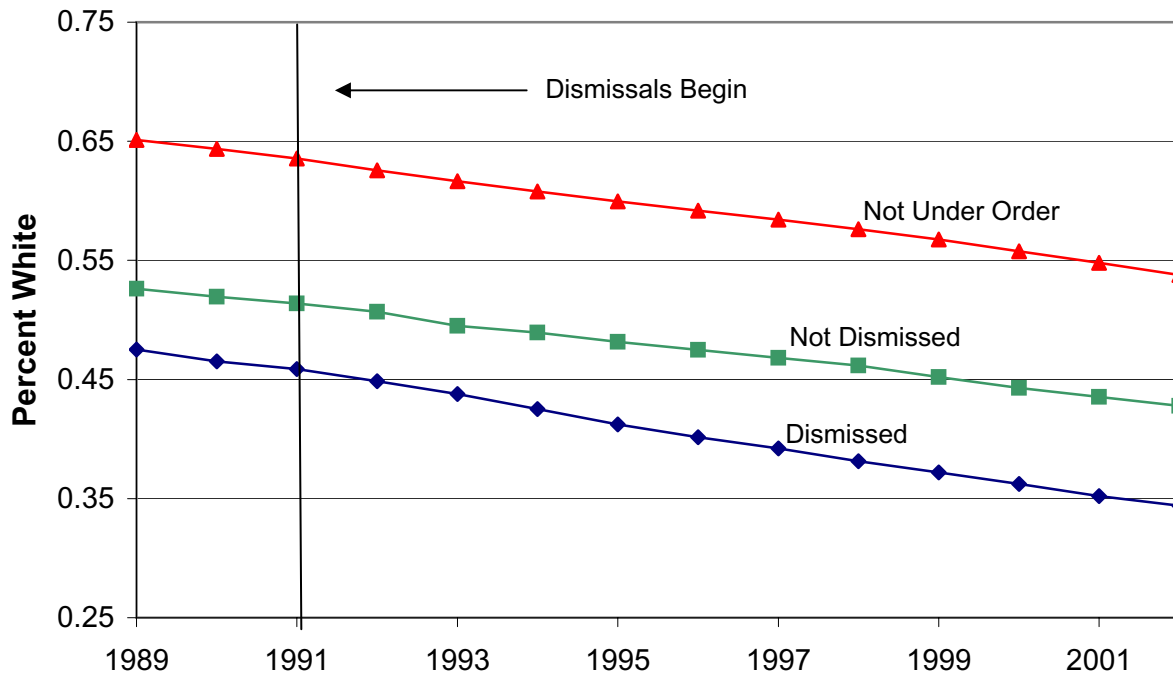
Note. The figure displays the number of dismissals of desegregation plans occurring among the set of school districts in the Rossell and Armor sample which were under a court-ordered desegregation plan at the start of 1991.

Figure 2: Segregation Trends

Panel A: Dissimilarity Index



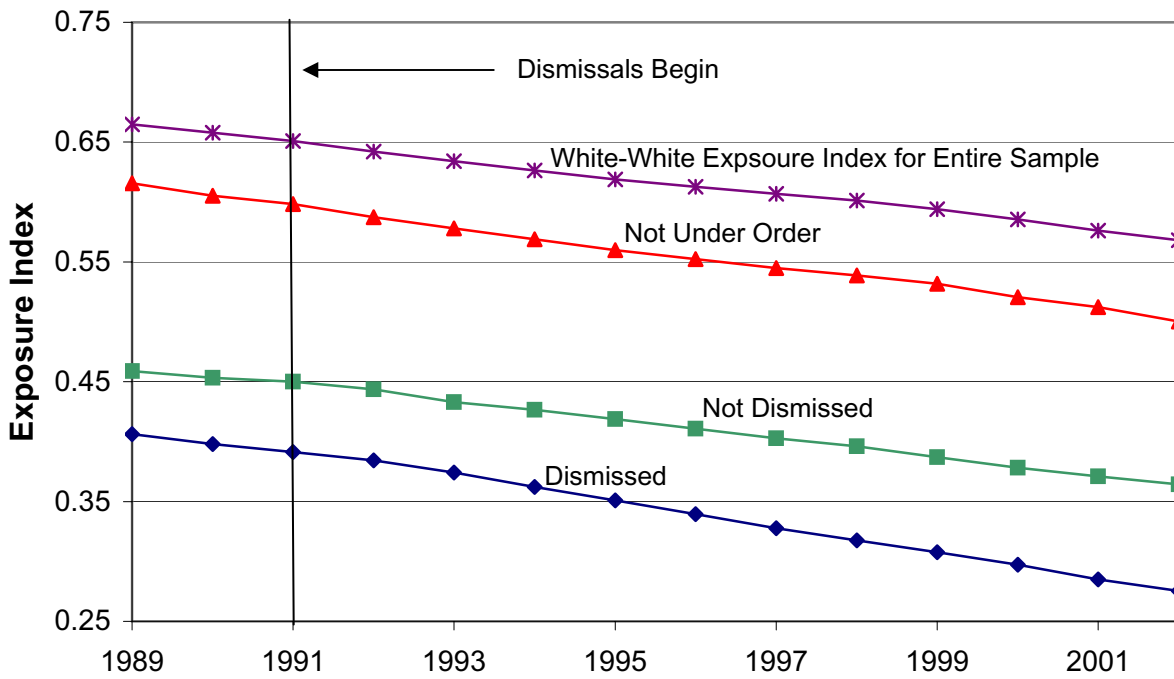
Panel B: Percent White



Note. The plots are means for the relevant groups. Dismissed refers to those districts under court-order in 1991 and dismissed from their desegregation plan between 1991 and 2002. These districts comprise the treatment group. Not dismissed refers to those districts under court-order in 1991 and not dismissed from their plans between 1991 and 2002. These districts comprise the control group. Not under order refers to those districts not under court order in 1991. The sample of districts is restricted to those in the Rossell and Armor sample which form a balanced panel from 1989 to 2002.

Figure 2: Segregation Trends

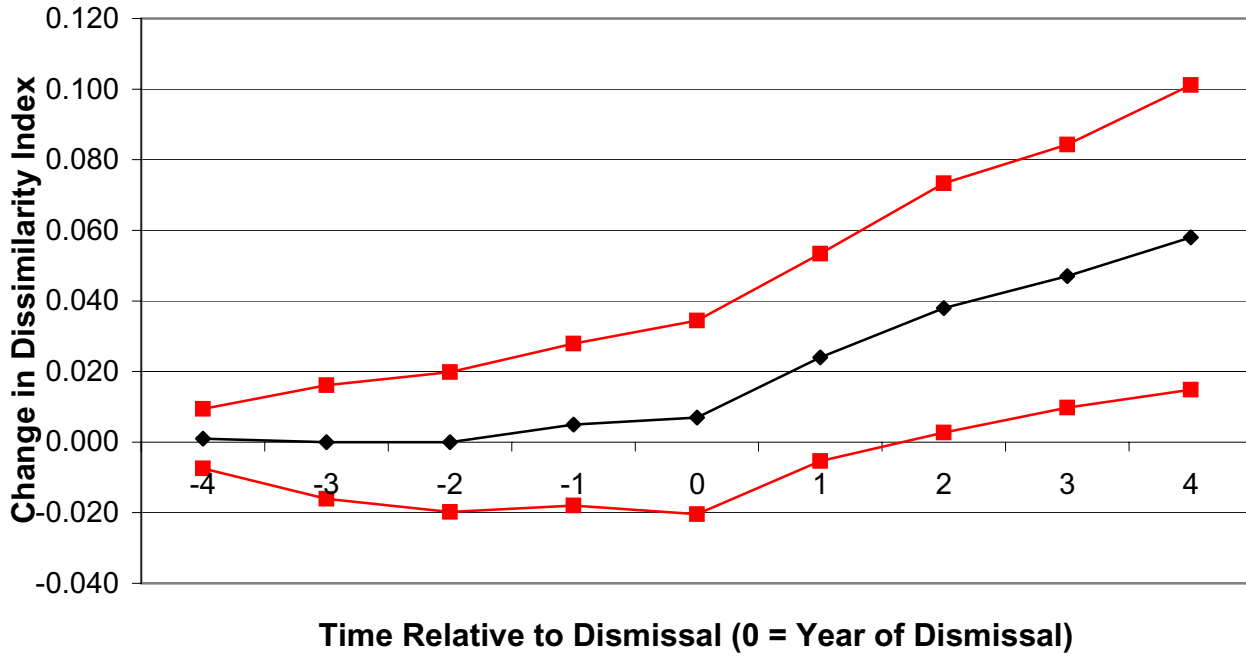
Panel C: Black-White Exposure Index



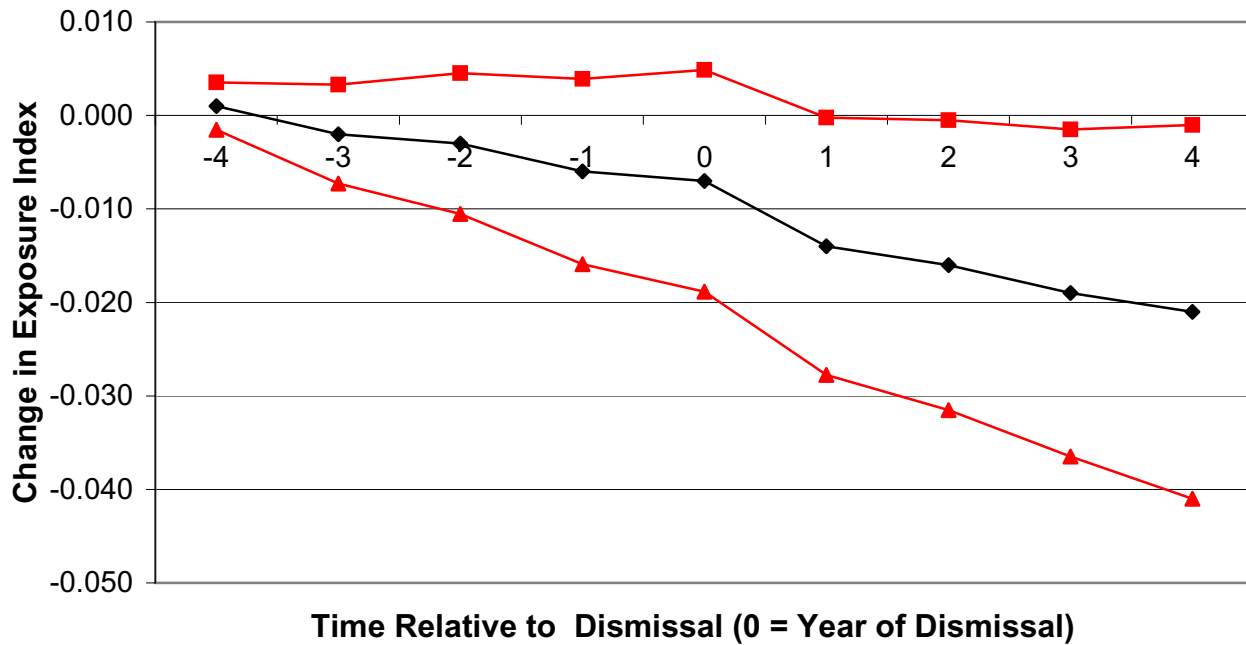
Note. The plots are means for the relevant groups. The dismissed, not dismissed and not under order group plots refer to the black-white exposure index. Dismissed refers to those districts under court-order in 1991 and dismissed from their desegregation plan between 1991 and 2002. These districts comprise the treatment group. Not dismissed refers to those districts under court-order in 1991 and not dismissed from their plans between 1991 and 2002. These districts comprise the control group. Not under order refers to those districts not under court order in 1991. The sample of districts is restricted to those in the Rossell and Armor sample which form a balanced panel from 1989 to 2002.

Figure 3: Effect of Dismissal on Segregation

Panel A: Dissimilarity Index



Panel B : Exposure Index

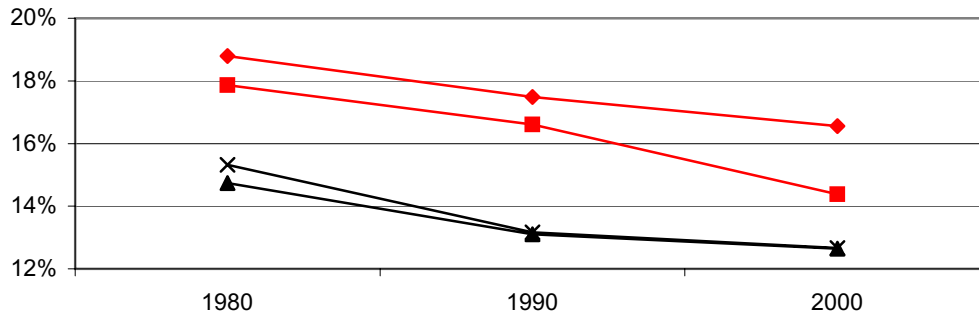


◆ Point Estimate ■ Upper 95% Confidence Interval ▲ Lower 95% Confidence Interval

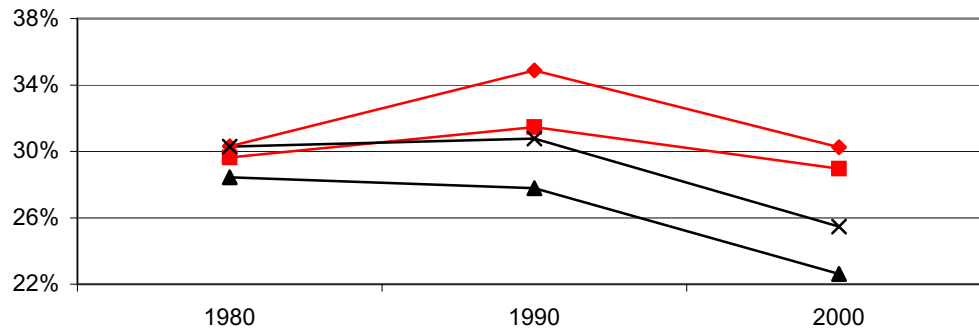
Note. Panel A plots the coefficients and their 90 percent confidence intervals from the specification presented in Column (3) of Table 2. Panel B plots the coefficients and their 90 percent confidence intervals from the specification presented in Column (3) of Table 3.

Figure 4: Trends by Treatment and Control Group

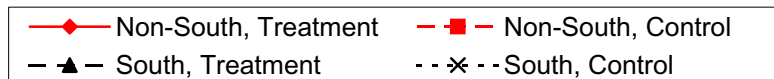
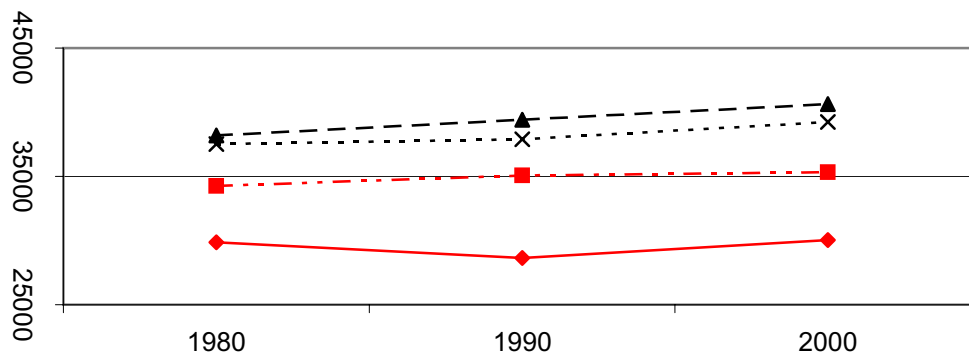
A. Status Dropout Rate for All Races



B. Percent of Blacks Beneath the Poverty Line



C. Median Household Income (\$1999)



Note. The charts display trends in mean school district demographic characteristics broken down by non-south and south and treatment and control group. The variables are obtained from school district tabulations of the U.S. Census. The treatment group is the set of districts dismissed from 1991 - 1999. The control group is the set of districts not dismissed in this period. The means are weighted by the number of 16 - 19 blacks residing in the district. These are the weights used in the regressions appearing on Table 7.

Table 1
Summary Statistics, 1987 - 2002 Panel

	Under Court-Order as of 1991		Not Dismissed as of 2002		Not Under Court-Order as of 1991	
	Dismissed 1991 - 2002 1990	2000	1990	2000	1990	2000
A. School District Characteristics (obtained from the CCD)						
Dissimilarity Index	0.33 (0.14)	0.41 (0.15)	0.34 (0.19)	0.37 (0.19)	0.32 (0.17)	0.31 (0.17)
Exposure Index	0.40 (0.16)	0.30 (0.17)	0.46 (0.22)	0.38 (0.22)	0.59 (0.29)	0.51 (0.30)
Enrollment	58528 (57055)	68904 (76046)	39445 (87106)	43213 (98670)	21178 (52573)	24365 (59984)
% black	0.39 (0.20)	0.42 (0.23)	0.35 (0.21)	0.38 (0.22)	0.16 (0.21)	0.18 (0.23)
% white	0.47 (0.17)	0.36 (0.18)	0.52 (0.23)	0.44 (0.24)	0.63 (0.29)	0.54 (0.31)
South Region	0.62 (0.49)	0.62 (0.49)	0.63 (0.49)	0.63 (0.49)	0.27 (0.45)	0.27 (0.45)
Serves a Central City	0.65 (0.49)	0.65 (0.49)	0.47 (0.50)	0.47 (0.50)	0.31 (0.46)	0.31 (0.46)
B. School District Community Characteristics (obtained from the SDDB)						
Black Status Dropout Rate	0.15 (0.04)	0.13 (0.04)	0.14 (0.05)	0.12 (0.05)	0.11 (0.12)	0.10 (0.10)
Median Household Income*	40895 (11192)	42052 (11542)	40847 (11727)	42026 (11644)	49108 (17064)	50697 (17661)
% Households Below Poverty Line	0.21 (0.09)	0.20 (0.08)	0.21 (0.10)	0.20 (0.08)	0.16 (0.11)	0.15 (0.09)
Number of Observations**	34		75		390	

Note. The cells are district means. Standard deviations are in parentheses. The sample is restricted to districts with non-missing values for the dissimilarity and exposure indices in 1990 and 2000. The construction of the status dropout rate is described in the text. Household refers to households with children. *Median household income is expressed in 1999 dollars. ** The sample displayed on the table is restricted to the set of districts comprising a balanced panel for the years displayed.

Table 2
Effect of Desegregation Order Dismissal on Dissimilarity Index

	(1)	(2)	(3)	(4)
A. Pre-Dismissal				
pre(-4)	0.002 (0.004)	0.002 (0.004)	0.001 (0.004)	0.001 (0.004)
pre(-3)	0.002 (0.008)	0.002 (0.007)	0.000 (0.008)	0.000 (0.009)
pre(-2)	0.005 (0.009)	0.004 (0.009)	0.000 (0.010)	0.001 (0.011)
pre(-1)	0.011 (0.011)	0.009 (0.011)	0.005 (0.012)	0.007 (0.013)
B. Post-Dismissal				
post(0)	0.014 (0.013)	0.012 (0.013)	0.007 (0.014)	0.009 (0.015)
post(1)	0.032 (0.014)	0.029 (0.014)	0.024 (0.015)	0.027 (0.016)
post(2)	0.043 (0.016)	0.041 (0.016)	0.038 (0.018)	0.040 (0.019)
post(3)	0.054 (0.017)	0.052 (0.017)	0.047 (0.019)	0.051 (0.019)
post(>=4)	0.064 (0.019)	0.063 (0.019)	0.058 (0.022)	0.061 (0.022)
Number of Observations	1712	1712	1712	1399
Dep. Var. Mean	0.363	0.363	0.363	0.368
Dep. Var. S.D.	0.180	0.180	0.180	0.183
Dep Var. 1991 Cross Section S.D.*	0.179	0.179	0.179	0.181
Index Components	Black-White	Black-White	Black-White	Black-White
School District Effects	X	X	X	X
Year Effects	X			
Year * Census Region Effects		X	X	X
Year * Base Period Demographics**			X	X

Note. Standard errors, clustered by district, are presented in parentheses. The sample is restricted to those districts under court-order in 1991. *The 1992 cross-sectional standard deviation is shown for column (4). **Base period demographic characteristics for columns (3) - (4) include a central city indicator variable, number of students enrolled, number of students enrolled squared, percent of students who are white and percent of students who are hispanic. Column (4) adds percent of households with children below the poverty line, median income for households with children and percent of households with children that speak a language other than English. Column (4) restricts the sample to 1991 - 2002.

Table 3
Effect of Desegregation Order Dismissal on Enrollment by Race

	Log Enrollment		
	Black (1)	Non-White (2)	White (3)
A. Pre-Dismissal			
lag(-4)	0.002 (0.007)	0.005 (0.007)	0.008 (0.012)
lag(-3)	0.002 (0.009)	0.006 (0.011)	-0.016 (0.016)
lag(-2)	0.002 (0.011)	0.011 (0.014)	-0.024 (0.022)
lag(-1)	-0.002 0.014	0.009 0.018	-0.039 (0.029)
B. Post-Dismissal			
post(0)	-0.003 (0.017)	0.008 (0.022)	-0.038 (0.034)
post(1)	-0.001 (0.019)	0.010 (0.024)	-0.044 (0.041)
post(2)	0.008 (0.023)	0.020 (0.028)	-0.030 (0.044)
post(3)	0.005 (0.024)	0.017 (0.030)	-0.035 (0.050)
post(>=4)	-0.003 (0.028)	0.010 (0.034)	-0.041 (0.059)
N	1712	1712	1712
Dep. Var. Mean	8.948	9.382	9.131
Dep. Var. S.D.	1.314	1.327	1.288
Dep Var. 1991 Cross Section S.D	1.321	1.316	1.282

Note. Standard errors, clustered by district, are presented in parentheses. The sample is restricted to those districts under court-order in 1991. The dependent variable is the log of enrollment for the given race. The specification, similar to that on column (3) of Table 2, includes district fixed effects, year-census region effects and base period characteristics interacted with year indicator variables. Base period demographic characteristics include a central city indicator variable, number of students enrolled and number of students enrolled squared.

Table 4
Effect of Desegregation Order Dismissal on the Exposure Index

	(1)	(2)	(3)	(4)
A. Pre-Dismissal				
pre(-4)	0.000 (0.001)	0.000 (0.001)	0.001 (0.001)	0.001 (0.002)
pre(-3)	-0.003 (0.002)	-0.003 (0.003)	-0.002 (0.003)	-0.002 (0.003)
pre(-2)	-0.005 (0.003)	-0.004 (0.004)	-0.003 (0.004)	-0.002 (0.004)
pre(-1)	-0.009 (0.005)	-0.008 (0.005)	-0.006 (0.005)	-0.005 (0.006)
B. Post-Dismissal				
post(0)	-0.009 (0.006)	-0.008 (0.006)	-0.007 (0.006)	-0.006 (0.007)
post(1)	-0.017 (0.007)	-0.016 (0.007)	-0.014 (0.007)	-0.013 (0.007)
post(2)	-0.019 (0.007)	-0.018 (0.007)	-0.016 (0.008)	-0.014 (0.008)
post(3)	-0.022 (0.008)	-0.021 (0.008)	-0.019 (0.009)	-0.018 (0.010)
post(>=4)	-0.025 (0.009)	-0.025 (0.009)	-0.021 (0.010)	-0.020 (0.011)
Number of Observations	1712	1712	1712	1399
Dep. Var. Mean	0.391	0.391	0.391	0.379
Dep. Var. S.D.	0.215	0.215	0.215	0.211
Dep Var. 1991 Cross Section S.D.*	0.210	0.210	0.210	0.206
Index Components	Black-White	Black-White	Black-White	Black-White
School District Effects	X	X	X	X
Year Effects	X			
Year * Census Region Effects		X	X	X
Year * Base Period Demographics**			X	X

Note. Standard errors, clustered by district, are presented in parentheses. The sample is restricted to those districts under court-order in 1991. *The 1992 cross-sectional standard deviation is shown for column (4). **Base period demographic characteristics for columns (3) - (4) include a central city indicator variable, number of students enrolled, number of students enrolled squared, percent of students who are white and percent of students who are hispanic. Column (4) adds percent of households with children below the poverty line, median income for households with children and percent of households with children that speak a language other than English. Columns (4) restricts the sample to 1991 - 2002.

Table 5
Effect of Desegregation Order Dismissal on School District Expenditures and Revenues

	Log Expenditures			Log Revenues
	Total (1)	Capital Expenditures (2)	Construction Expenditures (3)	Revenues from State (4)
A. Pre-Dismissal				
lag(-4)	-0.002 (0.014)	0.037 (0.115)	0.204 (0.223)	0.009 (0.027)
lag(-3)	0.004 (0.020)	0.090 (0.156)	0.255 (0.273)	0.012 (0.030)
lag(-2)	-0.005 (0.017)	0.065 (0.152)	0.284 (0.258)	0.019 (0.033)
lag(-1)	0.000 0.020	0.130 0.166	0.269 0.292	0.054 0.038
B. Post-Dismissal				
post(0)	-0.007 (0.023)	0.094 (0.193)	0.257 (0.353)	0.011 (0.042)
post(1)	-0.005 (0.029)	0.167 (0.200)	0.724 (0.509)	0.018 (0.041)
post(2)	-0.001 (0.031)	0.265 (0.223)	0.893 (0.554)	0.032 (0.047)
post(3)	-0.007 (0.039)	0.319 (0.304)	1.072 (0.665)	-0.022 (0.056)
post(>=4)	-0.013 (0.043)	0.324 (0.316)	0.771 (0.672)	-0.010 (0.057)
N	1190	1190	1094	1190
Dep. Var. Mean	12.114	9.391	8.755	11.425
Dep. Var. S.D.	1.199	1.586	2.104	1.183

Note. Standard errors, clustered by district, are presented in parentheses. The sample is restricted to those districts under court-order in 1991. The dependent variable is the log of the given expenditure or revenue item measured in thousand of constant 2001 dollars. The specification, the same as column (3) of Table 2, includes district fixed effects, year- census region effects and base period characteristics interacted with year indicator variables. Base period demographic characteristics include a central city indicator variable, number of students enrolled, number of students enrolled squared, percent of students who are hispanic and percent of students who are white.

Table 6
Summary Statistics for the 1990 and 2000 Panel

	Black Students				White Students			
	Dismissed 1991 - 1999		Not Dismissed*		Dismissed 1991 - 1999		Not Dismissed*	
	1990	2000	1990	2000	1990	2000	1990	2000
Status Dropout Rate	0.15 (0.04)	0.13 (0.03)	0.15 (0.05)	0.13 (0.05)	0.14 (0.06)	0.10 (0.05)	0.12 (0.05)	0.097 (0.04)
Private School Atten.	0.06 (0.02)	0.07 (0.03)	0.05 (0.04)	0.06 (0.03)	0.28 (0.11)	0.31 (0.11)	0.24 (0.14)	0.27 (0.14)
Average Housing Value**	125623 (82870)	139046 (97113)	112863 (69791)	119821 (59859)	*	*	*	*
South**	0.55 (0.51)	0.55 (0.82)	0.67 (0.47)	0.67 (0.47)	*	*	*	*
% Total HHs in Poverty**	0.06 (0.02)	0.05 (0.03)	0.10 (0.06)	0.04 (0.03)	*	*	*	*
HHs Unemploy. Rate**	0.06 (0.02)	0.05 (0.01)	0.07 (0.02)	0.04 (0.03)	*	*	*	*
Mother Not High Sch. Grad.	0.25 (0.10)	0.19 (0.07)	0.30 (0.13)	0.20 (0.09)	0.14 (0.07)	0.11 (0.06)	0.15 (0.07)	0.12 (0.06)
Mother College Grad.	0.13 (0.06)	0.15 (0.06)	0.11 (0.07)	0.14 (0.09)	0.31 (0.13)	0.36 (0.15)	0.28 (0.14)	0.31 (0.17)
Household Income***	33896 (7415)	39682 (9446)	33111 (10255)	39456 (118080)	61662 (14913)	70647 (20500)	60860 (19652)	68560 (25009)
Number of Observations			22	99			22	99

Note. The cells are district means. Standard deviations are in parentheses. The sample is restricted to districts under court-order in 1991. The construction of the variables is described in the text and Data Appendix. * Includes districts dismissed after 1999. ** Denotes a variable which does not vary by race. ***Household income is expressed in 1999 dollars and refers to household with children.

Table 7
Effect of Desegregation Order Dismissal on Black Status Dropout Rate

	Fixed Effect					Lagged Dependent Variable				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Dismissed * 2000	0.0038 (0.002)	0.0029 (0.0025)	0.0044 (0.0020)**	0.0039 (0.0023)*	0.0141 (0.0096)	0.0024 (0.003)	0.0015 (0.0020)	0.0033 (0.0019)*	0.0026 (0.002)	0.0084 (0.0078)
Placebo Dismissed * 2000			0.004 (0.010)		0.004 (0.010)					0.001 (0.006)
2000	-0.0238 (0.0047)***	-0.0360 (0.0164)**	-0.0358 (0.0134)***	-0.0149 (0.025)	-0.0120 (0.02530)					
Dropout Rate(t-1)						0.465 (0.1312)***	0.398 (0.1167)***	0.222 (0.0878)**	0.180 (0.0884)**	0.174 (0.0896)*
B. Heterogeneity by Southern vs. Non-Southern Districts										
Dismissed * 2000	0.0107 (0.0016)***	0.0099 (0.0017)***	0.0103 (0.0012)***	0.0107 (0.0016)***	0.0406 (0.0136)***	0.0088 (0.0014)***	0.0074 (0.0010)***	0.0087 (0.0012)***	0.0074 (0.0011)***	0.037 (0.0136)***
Dismissed * 2000 * South	-0.0133 (0.0028)***	-0.0127 (0.0026)***	-0.0112 (0.0022)***	-0.0123 (0.0024)***	-0.0484 (0.0179)***	-0.0116 (0.0021)***	-0.0101 (0.0014)***	-0.0092 (0.0020)***	-0.0085 (0.0019)***	-0.0455 (0.0163)***
Placebo Dismissed * 2000					0.011 (0.015)					0.011 (0.013)
Placebo Dismissed * 2000 * South					-0.018 (0.018)					-0.018 (0.017)
2000	-0.0359 (0.0081)***	-0.0427 (0.0145)***	-0.0406 (0.0117)***	-0.0285 (0.024)	-0.0179 (0.02250)					
South * 2000^A	0.0230 (0.0092)**	0.0384 (0.0122)***	0.0215 (0.0064)***	0.0202 (0.017)	0.0148 (0.01670)	0.007 (0.008)	0.024 (0.0085)***	0.014 (0.0077)*	0.013 (0.012)	0.008 (0.01200)
Dropout Rate(t-1)						0.485 (0.1277)***	0.440 (0.1168)***	0.294 (0.0885)***	0.237 (0.0939)**	0.235 (0.1026)**
Observations	242	242	242	242	242	121	121	121	121	121
Linear Dismissal Variable	X	X	X	X	X	X	X	X	X	X
Indicator Dismissal Variable					X					X
(Region, Cent. City, 1990 Enroll) * 2000^B		X		X	X		X		X	X
Time-Varying Micro Covariates^B			X	X	X		X		X	X
Time-Varying Macro Covariates^B				X	X		X		X	X

Note. Standard errors clustered by district in parentheses. All columns are weighted by cell size. The dependent variable is the black status dropout rate for 16 - 19 year olds. In columns (1) - (4) and (6) - (9) the dismissed variable equals the number of years since dismissal relative to 2000. In columns (5) and (10) the dismissal variables equals one if the district was dismissed between 1991 and 1999. * significant at 10%, ** significant at 5%, *** significant at 1%. ^A For columns (6) - (10) the row labeled 2000 * South could also be labeled simply South - the lagged dependent variable specifications utilize a single cross-section of data from 2000. ^B See text for complete listing of covariates. Coefficient estimates for the complete set of covariates available from the author upon request.

Table 8
Effect of Desegregation Order Dismissal on Black Private School Attendance

	Fixed Effect				Lagged Dependent Variable					
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Dismissed * 2000	0.0019 (0.0009)**	0.0012 (0.0007)*	0.0015 (0.0010)	0.0006 (0.0007)	-0.0039 (0.0046)	0.0016 (0.0009)*	0.0012 (0.0007)*	0.0016 (0.0010)	0.0012 (0.0007)*	0.0002 (0.0043)
Placebo Dismissed * 2000					-0.002 (0.005)					-0.001 (0.0048)
2000	0.0021 (0.0031)	-0.0011 (0.0059)	-0.0151 (0.0070)**	0.0003 (0.0108)	0.0108 (0.0117)					
Dropout Rate(t-1)						0.854 (0.0886)**	1.000 (0.1028)**	0.834 (0.0840)**	0.847 (0.1017)**	0.863 (0.1035)**
B. Heterogeneity by Southern vs. Non-Southern Districts										
Dismissed * 2000	0.0037 (0.0011)**	0.0027 (0.0009)**	0.0030 (0.0013)**	0.0016 (0.0011)	-0.0061 (0.0078)	0.0032 (0.0013)**	0.0026 (0.0009)**	0.0025 (0.0014)*	0.0020 (0.0008)**	-0.0003 (0.0072)
Dismissed * 2000 * South	-0.0036 (0.0015)**	-0.0026 (0.0012)**	-0.0028 (0.0020)	-0.0017 (0.0017)	0.0036 (0.0093)	-0.0028 (0.0020)	-0.0025 (0.0014)*	-0.0019 (0.0017)	-0.0014 (0.0013)	-0.0004 (0.0093)
Placebo Dismissed * 2000					-0.021 (0.0126)					-0.013 (0.0143)
Placebo Dismissed * 2000 * South					0.024 (0.0140)*					0.016 (0.0161)
2000	-0.0051 (0.0049)	-0.0011 (0.0059)	-0.0178 (0.0059)**	0.0003 (0.0108)	0.0108 (0.0117)					
South * 2000^A	0.0133 (0.0057)**	0.0175 (0.0046)**	0.0109 (0.0047)**	0.0184 (0.0072)**	0.0118 (0.0073)	0.010 (0.0086)	0.019 (0.0075)**	-0.002 (0.0078)	-0.006 (0.0075)	-0.008 (0.0089)
Dropout Rate(t-1)						0.922 (0.1139)**	1.014 (0.0992)**	0.805 (0.0990)**	0.857 (0.1019)**	0.865 (0.1020)**
Observations	242	242	242	242	242	121	121	121	121	121
Linear Dismissal Variable	X	X	X	X		X	X	X	X	
Indicator Dismissal Variable					X					X
(Region, Cent. City, 1990 Enroll) * 2000^B					X		X		X	X
Time-Varying Micro Covariates^B			X	X	X			X	X	X
Time-Varying Macro Covariates^B				X	X				X	X

Note. Standard errors clustered by district in parentheses. All columns are weighted by cell size. The dependent variable is the rate of black private school attendance. In columns (1) - (4) and (6) - (9) the dismissed variable equals the number of years since dismissal relative to 2000. In columns (5) and (10) the dismissal variables equals one if the district was dismissed between 1991 and 1999. * significant at 10%; ** significant at 5%; *** significant at 1%. ^A For columns (6) - (10) the row labeled 2000 * South could also be labeled simply South - the lagged dependent variable specifications utilize a single cross-section of data from 2000. ^B See text for complete listing of covariates. Coefficient estimates for the complete set of covariates available from the author upon request.

Table 9
Effect of Desegregation Order Dismissal on White and Non-White Status Dropout Rate
and Private School Attendance, 1990 and 2000 Panel
Fixed Effects Model

	(1)	(2)	(3)	(4)	(5)
A. White Status Dropout Rate					
Dismissed * 2000	-0.001 (0.001)	-0.001 (0.001)	-0.0007 (0.0013)	-0.001 (0.001)	-0.007 (0.007)
Placebo Dismissed * 2000					-0.0007 (0.005)
B. White Status Dropout Rate - Heterogeneity by Southern vs. Non-Southern School District					
Dismissed * 2000	0.0011 (0.001)	0.0009 (0.0013)	0.0006 (0.002)	0.0000 (0.001)	-0.0164 (0.0118)
Dismissed * 2000 * South	-0.0035 (0.0020)*	-0.0032 (0.0019)*	-0.0023 (0.002)	-0.0011 (0.002)	0.0155 (0.0139)
Placebo Dismissed * 2000					0.022 (0.014)
Placebo Dismissed * 2000 * South					-0.025 (0.015)
C. White Private School Attendance					
Dismissed * 2000	0.0017 (0.002)	0.0010 (0.003)	-0.0008 (0.002)	-0.0010 (0.003)	-0.0042 (0.0123)
Placebo Dismissed * 2000					0.012 (0.010)
D. White Private School Attendance - Heterogeneity by Southern vs. Non-Southern School District					
Dismissed * 2000	0.0068 (0.0018)***	0.0051 (0.0016)***	0.0033 (0.003)	0.0027 (0.002)	-0.0042 (0.0198)
Dismissed * 2000 * South	-0.0078 (0.0035)**	-0.0064 (0.0035)*	-0.0055 (0.004)	-0.0060 (0.004)	-0.0002 (0.0252)
Placebo Dismissed * 2000					0.019 (0.013)
Placebo Dismissed * 2000 * South					-0.009 (0.019)
Observations	242	242	242	242	242
Linear Dismissal Variable	X	X	X	X	
Indicator Dismissal Variables					X
(Region, Cent. City, 1990 Enroll) * 2000^B		X		X	X
Time-Varying Micro Covariates^B			X	X	X
Time-Varying Macro Covariates^B				X	X

Note. Standard errors clustered by district in parentheses. All columns are weighted by cell size. The dependent variable is as labeled in the panel headings. In columns (1) - (4) and (6) - (9) the dismissed variable equals the number of years since dismissal relative to 2000. In columns (5) and (10) the dismissal variables equals one if the district was dismissed between 1991 and 1999. * significant at 10%; ** significant at 5%; *** significant at 1%. ^B See text for complete listing of covariates. All columns include an indicator for the year 2000 and panels B and D include a south * 2000 interaction term. Coefficient estimates for the complete set of covariates available from the author upon request.

Table 10
Effect of Desegregation Order Dismissal on Property Values

	(1)	(2)	(3)	(4)
Dismissed * 2000	0.069 (0.062)	0.001 (0.036)	-0.028 (0.035)	-0.013 (0.034)
Placebo Dismissed * 2000	0.054 (0.056)	-0.003 (0.032)	-0.001 (0.029)	0.019 (0.029)
2000	0.045 (0.048)	-0.146 (0.049)	0.155 (0.115)	0.196 (0.191)
1990 HH Unemploy. Rate * 2000				1.536 (1.408)
1990 % HHs in Poverty * 2000				-0.566 (0.614)
1990 % HHs Nonenglish * 2000				-0.405 (0.467)
1990 % Black Head HH * 2000				-0.199 (0.194)
1990 % Hispanic Head HH * 2000				-0.042 (0.473)
Observations	242	242	242	242
District Fixed Effect	X	X	X	X
(Region, Cent. City, 1990 Enroll.) * 2000		X	X	X
1990 Housing Stock Age Vector			X	X

Note. Standard errors clustered by district in parentheses.

Table A1
Effect of Desegregation Order Dismissal on Dissimilarity and Exposure Indices; Robustness Checks

	(1)	(2)	(3)	(4)	(5)
A. Dissimilarity Index					
Pre-Dismissal					
pre(-4)	0.001 (0.005)	0.002 (0.004)	0.000 (0.004)	-0.009 (0.005)	0.000 (0.004)
pre(-3)	0.002 (0.009)	0.006 (0.009)	-0.001 (0.008)	-0.003 (0.008)	-0.001 (0.007)
pre(-2)	0.002 (0.011)	0.007 (0.011)	-0.002 (0.011)	-0.003 (0.010)	-0.001 (0.009)
pre(-1)	0.008 (0.012)	0.014 (0.012)	0.003 (0.013)	0.005 (0.012)	0.004 (0.011)
Post-Dismissal					
post(0)	0.012 (0.015)	0.019 (0.014)	0.004 (0.016)	0.015 (0.013)	0.005 (0.014)
post(1)	0.030 (0.015)	0.035 (0.015)	0.023 (0.017)	0.029 (0.015)	0.018 (0.015)
post(2)	0.036 (0.017)	0.042 (0.016)	0.038 (0.021)	0.047 (0.024)	0.033 (0.018)
post(3)	0.046 (0.018)	0.049 (0.017)	0.047 (0.022)	0.057 (0.025)	0.040 (0.018)
post(>=4)	0.053 (0.020)	0.056 (0.019)	0.057 (0.025)	0.071 (0.029)	0.049 (0.021)
Number of Observations	1296	1712	1712	982	1712
Dep. Var. Mean	0.392	0.363	0.363	0.362	0.328
Dep. Var. S.D.	0.169	0.180	0.180	0.184	0.164
Dep Var. 1991 Cross Section S.D.*	0.165	0.179	0.179	0.184	0.165
Index Components	Black-White	Black-White	Black-White	Black-White	Nonwhite-White
School District Effects	X	X	X	X	X
Year Effects					
Year * Census Region Effects	X	X	X	X	X
Year * Base Period Demographics**	X	X	X	X	X
Enrollment >= 10,000 in base period	X				
Weighted by Base Period Enrollment		X			
District Specific Trends			X		
Balanced Panel				X	

Note. Standard errors, clustered by district, are presented in parentheses. The sample is restricted to those districts under court-order in 1991. **Base period demographic characteristics include a central city indicator variable, number of students enrolled, number of students enrolled squared, percent of students who are white and percent of students who are hispanic.

**Table A1 (cont.)
Effect of Desegregation Order Dismissal on Dissimilarity and Exposure Indices; Robustness Checks**

	(1)	(2)	(3)	(4)	(5)
B. Exposure Index					
Pre-Dismissal					
pre(-4)	0.002 (0.001)	0.001 (0.001)	0.002 (0.001)	0.003 (0.003)	0.001 (0.001)
pre(-3)	-0.001 (0.003)	-0.003 (0.002)	0.000 (0.003)	-0.003 (0.005)	-0.002 (0.002)
pre(-2)	0.000 (0.004)	-0.002 (0.003)	0.000 (0.003)	-0.004 (0.007)	-0.003 (0.004)
pre(-1)	-0.002 (0.005)	-0.006 (0.004)	-0.002 (0.004)	-0.008 (0.010)	-0.005 (0.005)
Post-Dismissal					
post(0)	-0.002 (0.006)	-0.008 (0.005)	-0.002 (0.005)	-0.013 (0.011)	-0.006 (0.006)
post(1)	-0.008 (0.006)	-0.012 (0.006)	-0.008 (0.006)	-0.019 (0.013)	-0.011 (0.007)
post(2)	-0.008 (0.007)	-0.014 (0.007)	-0.010 (0.007)	-0.019 (0.015)	-0.013 (0.007)
post(3)	-0.011 (0.008)	-0.016 (0.008)	-0.012 (0.008)	-0.023 (0.016)	-0.015 (0.008)
post(>=4)	-0.012 (0.009)	-0.017 (0.008)	-0.013 (0.009)	-0.027 (0.017)	-0.017 (0.010)
Number of Observations	1296	1712	1712	982	1712
Dep. Var. Mean	0.381	0.391	0.391	0.397	0.399
Dep. Var. S.D.	0.208	0.215	0.215	0.221	0.215
Dep Var. 1991 Cross Section S.D.*	0.204	0.210	0.210	0.214	0.209
Index Components	Black-White	Black-White	Black-White	Black-White	Nonwhite-White
School District Effects	X	X	X	X	X
Year Effects					
Year * Census Region Effects	X	X	X	X	X
Year * Base Period Demographics**	X	X	X	X	X
Enrollment >= 10,000 in base period	X				
Weighted by Base Period Enrollment		X			
District Specific Trends			X		
Balanced Panel				X	

Note. Standard errors, clustered by district, are presented in parentheses. The sample is restricted to those districts under court-order in 1991. **Base period demographic characteristics include a central city indicator variable, number of students enrolled, number of students enrolled squared, percent of students who are white and percent of students who are hispanic.

Table A2
Effect of Desegregation Order Dismissal on Black Status Dropout Rate
Models Including Both the Indicator and Linear Dismissal Parameterizations; Fixed Effect Model

	(1)	(2)	(3)	(4)
Linear Dismissed * 2000	0.009 (0.0030)***	0.009 (0.0035)***	0.009 (0.0031)***	0.010 (0.0031)***
Linear Dismissed * 2000 * South	-0.010 (0.0039)***	-0.011 (0.0045)**	-0.010 (0.0053)*	-0.012 (0.0043)***
Indicator Dismissed * 2000	0.010 -(0.023)	0.004 -(0.023)	0.008 -(0.022)	0.009 -(0.020)
Indicator Dismissed * 2000 * South	-0.016 -(0.028)	-0.009 -(0.028)	-0.008 -(0.031)	-0.002 -(0.026)
2000	-0.037 (0.0088)***	-0.043 (0.0146)***	-0.041 (0.0118)***	-0.028 -(0.024)
South * 2000	0.024 (0.0099)**	0.039 (0.0123)***	0.023 (0.0066)***	0.019 -(0.017)
Observations	242	242	242	242
(Region, Cent. City, 1990 Enroll) * 2000^B		X		X
Time-Varying Micro Covariates^B			X	X
Time-Varying Macro Covariates^B				X

Note. Standard errors clustered by district in parentheses. The dependent variable is the black status dropout rate for 16 - 19 year olds. The linear dismissed variable equals the number of years since dismissal. The indicator dismissal variable is a dummy variable equal to one if the district has been dismissed. * significant at 10%; ** significant at 5%; *** significant at 1%.

^B See text for complete listing of covariates. Coefficient estimates for the complete set of covariates available from the author upon request.

Legal Appendix: Table L1

Districts in Rossell and Armor Sample and Under a Court-Ordered Desegregation Plan in 1991

District Name	State	Dismissal Date	Base Period Enrollment
AUTAUGA COUNTY SCH DIST	AL		6920
BIBB COUNTY SCH DIST	AL		3571
CALHOUN COUNTY SCH DIST	AL		11105
DOTHAN CITY SCH DIST	AL		10028
HUNTSVILLE CITY SCH DIST	AL		24987
JACKSON COUNTY SCH DIST	AL		6720
JEFFERSON COUNTY SCH DIST	AL		41143
MOBILE COUNTY SCH DIST	AL	97	67841
MONTGOMERY COUNTY SCH DIST	AL		36010
SAINT CLAIR COUNTY SCH DIST	AL		5638
WILCOX COUNTY SCH DIST	AL		2939
PHOENIX UNION HIGH SCHOOL DISTRICT	AZ		21117
FORREST CITY	AR		5621
LITTLE ROCK	AR	102	26854
N LITTLE ROCK	AR		9725
PULASKI CO SPECIAL	AR		22280
LOS ANGELES UNIFIED	CA		589311
OAKLAND UNIFIED	CA		51298
SAN BERNARDINO CITY UNIFIED	CA		35033
SAN DIEGO CITY UNIFIED	CA	98	116557
SAN FRANCISCO UNIFIED	CA		63881
SAN JOSE UNIFIED	CA	98	29333
STOCKTON CITY UNIFIED	CA		31051
DENVER COUNTY 1	CO	95	59439
BRIDGEPORT SCHOOL DISTRICT	CT		19416
WATERBURY SCHOOL DISTRICT	CT		13298
CHRISTINA SCHOOL DISTRICT	DE	96	16438
RED CLAY CONSOLIDATED SCHOOL DISTRICT	DE	96	14189
BAY COUNTY SCHOOL DISTRICT	FL		21541
BROWARD COUNTY SCHOOL DISTRICT	FL	96	137366
DADE COUNTY SCHOOL DISTRICT	FL	101	253323
DUVAL COUNTY SCHOOL DISTRICT	FL	101	105049
ESCAMBIA COUNTY SCHOOL DISTRICT	FL		42066
HILLSBOROUGH COUNTY SCHOOL DISTRICT	FL	101	118031
JACKSON COUNTY SCHOOL DISTRICT	FL		7565
LEE COUNTY SCHOOL DISTRICT	FL	103	37708
MARION COUNTY SCHOOL DISTRICT	FL		26433
ORANGE COUNTY SCHOOL DISTRICT	FL		88878
PINELLAS COUNTY SCHOOL DISTRICT	FL	101	88866
POLK COUNTY SCHOOL DISTRICT	FL	100	61244
SEMINOLE COUNTY SCHOOL DISTRICT	FL		43511
ST LUCIE COUNTY SCHOOL DISTRICT	FL	97	18260
BIBB COUNTY	GA		25158
CHATHAM COUNTY	GA	94	35358
DECATUR COUNTY	GA		5810
DEKALB COUNTY	GA	96	81468
DOUGHERTY COUNTY	GA		18760
FULTON COUNTY	GA	103	50190

LOWNDES COUNTY	GA		7982
MUSCOGEE COUNTY	GA	97	31984
CITY OF CHICAGO SCHOOL DIST 299	IL		419537
JOLIET PUBLIC SCH DIST 86	IL		8823
FORT WAYNE COMMUNITY SCHOOLS	IN		32405
INDIANAPOLIS PUBLIC SCHOOLS	IN	98	50496
M S D DECATUR TOWNSHIP	IN		5146
M S D WAYNE TOWNSHIP	IN		12066
SCHOOL CITY OF HAMMOND	IN		13737
KANSAS CITY	KS	97	22897
TOPEKA PUBLIC SCHOOLS	KS	99	14783
FAYETTE CO	KY		31191
JEFFERSON CO	KY	100	93198
CADDO PARISH SCHOOL BOARD	LA		52309
CITY OF MONROE SCHOOL BOARD	LA		10922
EAST BATON ROUGE PARISH SCHOOL BOARD	LA	102	60279
EVANGELINE PARISH SCHOOL BOARD	LA		6907
JEFFERSON PARISH SCHOOL BOARD	LA		57663
LAFAYETTE PARISH SCHOOL BOARD	LA		28392
OUACHITA PARISH SCHOOL BOARD	LA		17523
POINTE COUPEE PARISH SCHOOL BOARD	LA		3868
RAPIDES PARISH SCHOOL BOARD	LA	102	24404
SAINT LANDRY PARISH SCHOOL BOARD	LA		17379
SAINT TAMMANY PARISH SCHOOL BOARD	LA		28055
WEST FELICIANA PARISH SCHOOL BOARD	LA		2050
PRINCE GEORGES COUNTY PUB SCHS	MD	102	104661
HOLYOKE	MA		6732
BENTON HARBOR AREA SCHOOLS	MI	102	7129
FLINT CITY SCHOOL DISTRICT	MI	102	30202
GRAND RAPIDS PUBLIC SCHOOLS	MI		25225
KALAMAZOO PUBLIC SCHOOL DISTRICT	MI		12810
LANSING PUBLIC SCHOOL DISTRICT	MI		22477
CARROLL COUNTY SCHOOL DIST	MS		1218
CLEVELAND SCHOOL DIST	MS		4726
HATTIESBURG PUBLIC SCHOOL DIST	MS	97	5789
NATCHEZ-ADAMS SCHOOL DIST	MS		6841
RANKIN CO SCHOOL DIST	MS		12126
VICKSBURG WARREN SCHOOL DIST	MS		10380
KANSAS CITY 33	MO	103	35227
ROCKWOOD R-VI	MO		16484
ST LOUIS CITY	MO	99	42088
OMAHA PUBLIC SCHOOLS	NE		41416
MONTCLAIR TOWN	NJ		5141
UNION TWP	NJ		5971
BUFFALO CITY SD	NY	95	46251
NEW ROCHELLE CITY SD	NY		7633
SYRACUSE CITY SD	NY		20972
UTICA CITY SD	NY		8317
YONKERS CITY SD	NY	102	17744
HIGH POINT CITY	NC		8160
CHARLOTTE-MECKLENBURG SCHOOLS	NC	101	74149
FORSYTH COUNTY SCHOOLS	NC		38311

HALIFAX COUNTY SCHOOLS	NC		6608
VANCE COUNTY SCHOOLS	NC		7561
CINCINNATI CITY SD	OH	91	51819
CLEVELAND MUNICIPAL SD	OH	99	71743
DAYTON CITY SD	OH	102	28768
LORAIN CITY SD	OH		12212
OKLAHOMA CITY	OK	91	39149
ERIE CITY SD	PA		12485
PHILADELPHIA CITY SD	PA		194698
SUMTER COUNTY SCHOOL DISTRICT 02	SC		8661
CHATTANOOGA CITY SCHOOLS	TN		22872
MEMPHIS CITY SCHOOL DISTRICT	TN		105856
NASHVILLE-DAVIDSON COUNTY SD	TN	98	66973
SHELBY COUNTY SCHOOL DISTRICT	TN		33683
ALDINE ISD	TX	102	37657
CORPUS CHRISTI ISD	TX	97	41850
CROSBY ISD	TX		3246
DALLAS ISD	TX	103	130885
ECTOR COUNTY ISD	TX		25770
GALENA PARK ISD	TX		13938
GARLAND ISD	TX		34603
RICHARDSON ISD	TX		32080
TEMPLE ISD	TX	100	8110
WICHITA FALLS ISD	TX	100	15055
MILWAUKEE	WI		91648
Note. Base period enrollment is total student enrollment in the first year the district appears in the sample.			
See Appendix B.			

Legal Appendix: Table L2

Districts with Desegregation Order Dismissal Post 1990 (in Armor and Rossell Sample)

Year of Dismissal	District Name	State
91	CINCINNATI CITY SD	OH
91	OKLAHOMA CITY	OK
94	CHATHAM COUNTY	GA
95	DENVER COUNTY 1	CO
95	BUFFALO CITY SD	NY
96	CHRISTINA SCHOOL DISTRICT	DE
96	RED CLAY CONSOLIDATED SCHOOL DISTRICT	DE
96	BROWARD COUNTY SCHOOL DISTRICT	FL
96	DEKALB COUNTY	GA
97	MOBILE COUNTY SCH DIST	AL
97	ST. LUCIE COUNTY SCHOOL DISTRICT	FL
97	MUSCOGEE COUNTY	GA
97	KANSAS CITY	KS
97	HATTIESBURG PUBLIC SCHOOL DIST	MS
97	CORPUS CHRISTI ISD	TX
98	SAN DIEGO CITY UNIFIED	CA
98	SAN JOSE UNIFIED	CA
98	INDIANAPOLIS PUBLIC SCHOOLS	IN
98	NASHVILLE-DAVIDSON COUNTY SD	TN
99	TOPEKA PUBLIC SCHOOLS	KS
99	ST. LOUIS CITY	MO
99	CLEVELAND MUNICIPAL SD	OH
100	POLK COUNTY SCHOOL DISTRICT	FL
100	JEFFERSON CO	KY
100	TEMPLE ISD	TX
100	WICHITA FALLS ISD	TX
101	DADE COUNTY SCHOOL DISTRICT	FL
101	DUVAL COUNTY SCHOOL DISTRICT	FL
101	HILLSBOROUGH COUNTY SCHOOL DISTRICT	FL
101	PINELLAS COUNTY SCHOOL DISTRICT	FL
101	CHARLOTTE-MECKLENBURG SCHOOLS	NC
102	LITTLE ROCK	AR
102	RAPIDES PARISH SCHOOL BOARD	LA
102	PRINCE GEORGES COUNTY PUB SCHS	MD
102	BENTON HARBOR AREA SCHOOLS	MI
102	FLINT CITY SCHOOL DISTRICT	MI
102	YONKERS CITY SD	NY
102	DAYTON CITY SD	OH
102	ALDINE ISD	TX
103	LEE COUNTY SCHOOL DISTRICT	FL
103	FULTON COUNTY	GA
103	EAST BATON ROUGE PARISH SCHOOL BOARD	LA
103	KANSAS CITY 33	MO
103	DALLAS ISD	TX

Source: See Appendix B.