The End of Court-Ordered Desegregation (forthcoming in *American Economic Journal: Economic Policy)*

By BYRON LUTZ*

In response to three Supreme Court rulings in the early 1990s numerous court-ordered desegregation plans have been terminated. Using a unique dataset and an event study research design, this paper explores the impact of these terminations. The results suggest that termination produces a moderate increase in racial segregation. Outside of the south, dismissal also increases the rate at which black students drop out of school and attend private school. In the south, in contrast, there is no change in the school attendance patterns of blacks. Finally, evidence is presented that whites re-enter dismissed districts in large numbers in the south.
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Court-ordered desegregation was one of the most ambitious and controversial government policies of the previous century. Its explicit aim – integrating public schools – was at least partially achieved (Finis Welch and Audrey Light 1987; Charles Clotfelter 2004; Sarah Reber 2005). More fundamentally, it appears to have validated the basic premise of the landmark *Brown v. the Board of Education* decision that integrated schools would improve the educational and life outcomes of black children; desegregation led to increased black educational attainment (Jonathan Guryan 2004; Reber *forthcoming*), increased incomes for black adults who had attended desegregated schools (Orley Ashelnfelter, William J. Collins and Albert Yoon 2006), and decreased rates of criminal offending by black youth (Dave Weiner, Byron F. Lutz and Jens Ludwig 2009).

Although *Brown* was issued in 1954, the courts desegregated few school districts before the late 1960s. 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 that 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

* Federal Reserve Board, 20th & C Sts., NW, Stop #83, Washington DC 20551; Byron.F.Lutz@frb.gov. This research was supported by grants from the American Educational Research Association and the National Science Foundation. The views expressed are those of the author and not necessarily those of the Federal Reserve Board or its staff. I owe thanks to several individuals for assistance with the data used in the paper. Daniel Feenberg generated the private school iterations of the 1990 School District Databook. Jacinta Ma of the Harvard Civil Rights Project provided data on the date of desegregation order dismissal. Christine Rossell and David Armor generously allowed me to use their survey data. Margo Schlanger provided the methodology used for the electronic legal searches. I thank Samuel Brown, Paul Eliason, Brian McGuire, Nirupama Rao, Shoshana Schwartz, and Daniel Stenberg for excellent research assistance. I thank the following individuals for useful comments: Daron Acemoglu, Josh Angrist, David Autor, Jane Dokko, Michael Greenstone, Jon Gruber, Jon Guryan, Chris Hansen, Bill Kerr, Ashley Lester, Adam Looney, Raven Molloy, Nancy Qian, Sarah Reber, Hui Shan, Dan Sichel, John Yun, several anonymous referees, and participants in the MIT Labor and Public Finance Lunches, the Spring 2005 NBER Children's Program Meeting, and the University of Chicago GSB Applied Economics Workshop.

desegregation cases. These decisions signaled the end of the era of court-ordered desegregation: in the post-1990 period numerous school districts have been declared unitary – a term indicating a district is no longer operating an illegal, racially dual school system – and had their court-mandated desegregation plans terminated.

Racial segregation increased in public schools over the same period – a development that has been termed resegregation (Gary Orfield and Susan Eaton 1996; John Boger 2002; Erica Frankenberg, Chungmei Lee and Orfield 2003; for a dissenting view, see David J. Armor and Christine H. Rossell 2002). Numerous observers have assumed an explicit link between the dismissal of desegregation plans and increasing segregation in public schools. A *New York Times* editorial states that "much of the blame [for resegregation] goes to the courts' increased hostility to desegregation suits" (*New York Times* 2003). Many scholarly articles have made similar assumptions (e.g. Boger 2002 pg. 3; Erwin Cherminsky 2002 pg. 5; Orfield 2001 pg. 15 – 16).

The effect of ending court-ordered desegregation, however, 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 2005). It is unclear whether desegregation plans still impose a significant constraint on racial segregation in the post-1990 period. Even if the termination of a plan were to cause an increase in segregation, the termination may or may not have adverse welfare consequences for black students. Some recent research concludes that school segregation has little influence on black student outcomes (e.g. David Card and Jesse Rothstein 2007). Furthermore, the phase-out of desegregation is occurring in a very different environment from which it was implemented. Residential segregation has decreased significantly (Edward L. Glaeser and Jacob L. Vigdor 2003), funding is more equalized across school districts (Sheila Murray, William Evans and Robert Schwab 1998, Caroline Hoxby 2001, Card and Abigail Payne 2002) and attitudes toward race have changed dramatically (Howard Schuman, Charles Steeh and Lawrence Bobo 1985; Lincoln Quillian 1996). Given the different environment, it is not clear that the dismissal of the plans will reverse the gains achieved by their implementation.

This paper examines the two questions raised above. First, does dismissal of a desegregation plan increase school segregation? Second, what are the welfare implications of the end of court-ordered desegregation?

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. Racial integration was the primary aim of court-ordered desegregation, and examining segregation levels therefore provides evidence on the efficacy of what has been called the most ambitious and idealistic social experiment in U.S. history (Richard Merelman 2002). Segregation is also of interest because of a possible link with educational outcomes. Peer effects potentially play an important role in human capital production, and the degree of segregation likely influences the level of educational resources provided to minority students.

The second question, which concerns welfare implications, is addressed by assessing the impact of dismissal on the value students and families place on public education. A dismissal potentially causes a complex transformation of the school environment. In addition to the peer and resources effects linked to changes in segregation, time-consuming bus rides may be replaced by neighborhood school attendance. School districts under a court-ordered desegregation plan

are monitored by the courts in regard to minority student performance. By removing the external monitoring, dismissal may reduce the effort expended on minority students. Finally, there is anecdotal evidence that dismissed school districts sometimes engage in capital investment in minority neighborhoods. These changes alter both the value and cost of education provided in a dismissed district.

It would be ideal to directly measure how these changes, on net, alter both the public and private return to education. Unfortunately these returns are inherently unobservable. It is possible, however, to infer the change in the value students and families place on the education provided by a dismissed district. If the net value of the educational services provided by a school district is decreased by dismissal, there is an expectation that students previously on the margin for exiting the school district – i.e. those for whom the benefits of attendance were only marginally greater than the costs of attendance – will exit after dismissal. Some students will be on the margin between attending the desegregated school district and other schooling options such as alternative public school districts or private schools; other students will be on the margin between continuing their education or dropping out. Dropout rates and rates of private school attendance are therefore examined in order to assess the net impact of dismissal on the value of public education to students and families.

The analysis uses a unique dataset compiled from multiple sources. An event study research design is used for the segregation outcomes: The evolution of segregation in the years before and after a dismissal is examined in order to determine if changes in segregation occur at the time of dismissal and hence are likely *caused* by dismissal. Alternatively, segregation may be changing before dismissal which would suggest dismissal and segregation are not causally connected. A difference-in-difference specification is used to analyze the dropout and private school outcomes as data constraints render the event study methodology infeasible.

The results suggest that dismissal causes a gradual, moderate increase in segregation levels. In independent contemporaneous work, Clotfelter, Helen Ladd and Vigdor (2006) 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 in the South – a finding broadly consistent with the results of this paper.[1] The results also suggest that dismissal induces a behavioral response from both white and black students and that the response varies by region. Outside of the South Census region, dismissal is associated with increases in the rate at which blacks dropout of school and the rate at which they attend private school. In the South Census region, dismissal does not affect black school attendance patterns, but it does increase

[1]There are many differences in approach between this paper and Clotfelter, Ladd and Vigdor (2006) (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 of medium and large school districts 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); 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, 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 Section III below), as well as a set of robustness checks, is useful in assessing whether or not the estimated increase in segregation reflects the causal impact of dismissal.

white enrollment in formerly desegregated districts, a phenomenon that can be termed "reverse white flight."

The regional variation in the impact of the end of court-ordered desegregation appears to be associated with two factors. First, by some measures, the increase in segregation following dismissal is more severe in the non-South. Second, there is some evidence that Southern school districts have taken remedial actions to blunt the impact of dismissal on black students, while there is no evidence that non-Southern districts have done so.

The paper proceeds as follows: Section I provides background information. Section II discusses the data and outcome measures. Section III presents the empirical model. Section IV presents the results of estimating the empirical model. Section V concludes.

I. Background Information

A. Court-Ordered Desegregation

Although *Brown v. Board of Education* was issued in 1954, little court-ordered desegregation occurred in the 1950s and early 1960s. Many small and medium size districts, particularly those located in the South, began to desegregate without court involvement following the passage of the Civil Rights Act of 1964, which banned racial discrimination in schools receiving federal aid (Elizabeth Cascio, Nora Gordon, Ethan Lewis and Sarah Reber 2008, 2010). The 1968 *Green* decision (*Green v. County School Board of New Kent County*, 391 U.S. 430), which stipulated that school desegregation must begin immediately in areas that had practiced *de jure* segregation, began large scale court-ordered desegregation in the South. The *Keyes* decision (*Keyes v. Denver School District*, 413 U.S. 189), issued in 1973, ruled that court-ordered desegregation could proceed in areas that had *de facto* segregation resulting from past state action. Desegregation became more 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 (Jeffrey A. Raffel 2002).

The legal environment for court-ordered desegregation changed radically with the 1991 *Board of Education of Oklahoma City v. Dowell* ruling (498 U.S. 237). This decision defines the requirements for a school district to be declared unitary and stipulates that once a district achieves unitary status, it must be permanently released from court control. The *Freeman v. Pitts* decision (503 U.S. 467; 1992) eases the burden placed on defendant school districts in desegregation suits. Finally, *Missouri v. Jenkins* (515 U.S. 70, 1995) 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 (Mark Tushnet 1996). A large number of school districts have been released from their desegregation plans and most observers have concluded

that the era of court-ordered desegregation is drawing to a close (Frankenburg, Lee and Orfield 2003, pg.20; Alfred Lindseth 2002, pg. 42).

B. The Dismissal Process

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. The process of dismissal, once initiated in the courts, typically takes several years, and virtually all districts are ultimately dismissed from court supervision. Every contested motion for unitary status post-1990 has resulted in a dismissal (NAACP 2000).

There is significant variety across districts in who initiates the dismissal process. 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 (NAACP 2000). Cleveland, Ohio, which had one of the least successful court-ordered desegregation plans, is another example of a defendant school board moving for dismissal (179 F.3d 453, 6th Cir, 1999). 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 (57 F.Supp.2d 228). Prince George's County, Maryland, a district where "white flight" undermined integration efforts, 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). In some cases, district judges have chosen to clear their dockets of desegregation cases at their own initiative (Wendy Parker 2000). 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.[2]

A final relevant piece of legal background involves desegregation plans operated by districts not under court-order. Recent federal and Supreme Court rulings have made it more difficult to legally operate voluntary, non-court-ordered plans.[3] As a result, school districts released from

[2]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.

[3]Among the more significant decisions are the following: *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 and *City of Richmond V. J.A. Croson Co.*, 488 U.S. 469. The June 2007 Supreme Court decision in *Meredith v. Jefferson Co. Board of Ed.* and *Parents Involved in the Community Schools v. Seattle School District No. 1* has made the operation of voluntary plans even more difficult. This ruling, however, does not impact the time period considered by this study, which ends in 2006. In addition, a substantive difference exists between districts in the federal Fourth Judicial Circuit and those elsewhere. Beginning in 1999, districts in that circuit knew that, once they were released from court oversight, they were banned from using race in any student assignments. Outside of the circuit, districts could continue to use race in this way. It is therefore quite possible that districts in and out of that circuit have had different post-dismissal experiences. Unfortunately the sample used in this paper is ill-suited to address this issue, because it contains only two dismissals from the Fourth Circuit. Clotfelter, Ladd and Vigdor (2006, 2008) explore the connection between the Fourth

court-ordered plans have limited ability to maintain desegregation efforts and most have returned to some form of neighborhood schooling (Lindseth 2002). In some 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).

II. Data

This section is divided into three subsections. The first discusses the data sample, the second discusses the outcome measures used by this study, and the third presents summary statistics.

A. Sample

This paper analyzes a nationally representative sample of mid and large-sized school districts. The primary source of school district data is the Common Core of Data (CCD) that contains basic descriptive data for public schools in the U.S. from 1987 through 2006. The School District Databook (SDDB), a school district-level tabulation of the U.S. Census, complements the CCD. It provides detailed demographic data for the geographic areas served by school districts, but is only available in 1990 and 2000.

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 court-ordered plans in 1991, the year of the *Oklahoma City* decision, and the dates of dismissal of these plans. The primary source for the presence of a court-ordered plan is a 1991 nationally representative survey of school districts conducted by Christine Rossell and David Armor (Laurie Steel, Roger Levine, Christine Rossell and David Armor 1993; Steel and Levine 1994; see section I.A of the Web Appendix for additional information). The primary source of information on dismissal of desegregation orders is an unpublished table produced by the Harvard Civil Rights Project (Jacinta Ma 2004). To supplement this table, which does not claim to be comprehensive, and the Rossell and Armor survey data, I collect information from eight additional sources. Although the collection of this data was a major undertaking, the details are left to section I.B of the Web Appendix.[4]

B. Outcome Measures

A primary aim of court-ordered desegregation is increasing the extent of contact between white and black students. The extent of this contact between the races can be changed via one of two

Circuit and segregation outcomes. Although in the first paper they fail to find evidence that the Fourth Circuit's ban on using race in student assignment has increased school segregation, in the second paper they document a sharp increase in segregation in the Fourth Circuit school district of Charlotte, NC following its dismissal from a court-ordered plan.

[4]Although the assembled data provide a relatively rich picture of the state of court-ordered school desegregation, particularly when viewed against the lack of national data on the subject, there are several pieces of information that would be informative but that are unavailable. It has proven infeasible to acquire additional legal information such as when the court action that leads to dismissal was initiated and who initiated the proceedings. It has also proven infeasible to obtain details of how individual school districts responded to the end of their desegregation plan – e.g. did districts phase out their plan gradually or terminate it discretely, what policy actions did individual districts take in response to the termination of their plans, etc.

primary mechanisms. First, holding the racial composition of the district fixed, students may be re-sorted among the schools that comprise a district. Court-ordered desegregation achieves racial integration primarily by this type of re-sorting. For example, busing produces integration by re-sorting students among schools within a district. The sorting of students within a district is measured using the dissimilarity index

$$D_t = \frac{1}{2} * \sum_{i=1}^{n} |\frac{b_{it}}{B_t} - \frac{w_{it}}{W_t}| \tag{1}$$

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 index ranges from 0 to 1, with 0 denoting perfect integration and 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 district's overall racial composition.

The second mechanism by which a dismissal may affect the extent of contact between blacks and whites is by altering the district wide demographic composition. It is well documented that whites responded to desegregation by moving to alternative public school districts or by 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 court-ordered desegregation plans. The termination of a plan may break this equilibrium and alter the racial composition of a district. 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 that I term "reverse white flight." 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

$$E_t = \frac{1}{B_t} \sum_{i=1}^{n} b_{it} * \frac{w_{it}}{t_{it}} \tag{2}$$

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. The dissimilarity index and exposure indices are commonly used metrics for racial segregation. For example, Rossell and Armor (1996), Rossell (2002), Guryan (2004) and Reber (2005) use these indices to study how desegregation plans affect racial segregation in public schools.

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 that promote integration, such as busing, 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, as policy makers have very little ability to influence the racial composition of a school district (although some policies, such as magnet schools, can exert limited influence over the racial makeup of a district). The exposure index remains of interest, because it summarizes the extent of contact between whites and blacks – a primary goal of court-ordered desegregation.

Dropout rates are measured using the status dropout rate S_{dt} – a measure of the *stock* of dropouts residing in a given school district.

$$S_{dt} = \frac{Drop_{dt}}{Tot_{dt}} \tag{3}$$

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 hold neither a high school degree nor its equivalent (e.g. a GED) and Tot_{dt} is the total number of 16 – 19 year old civilians.[5] The private school attendance rate is defined as the percent of enrolled students residing in the district who are enrolled in private school and therefore summarizes the percent of *potential* students each public school district has enrolled in private school.

C. Summary Statistics

There are 571 school districts in the Rossell and Armor survey data, 130 of which were under court-ordered desegregation plans in 1991, the year of the *Oklahoma City v. Dowell* decision. Of the 130 districts, 59, or approximately 45 percent, have been dismissed in the post-1990 period. Figure 1 graphs the timing of these dismissals, and Figure 2 maps their geographic distribution. While there are dismissals throughout the country, there is a concentration in the South Census region, reflecting the fact that a majority of court-ordered desegregation plans were in the South. Web Appendix Table A1 lists the districts under court-order in 1991 and the dates of their dismissal.

Table 1 presents summary statistics from 1990 for three sets of school districts – those districts under court-ordered desegregation plans in 1991 and subsequently dismissed within the sample range (i.e. in 2006 or before), 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. The dismissed group is further broken into those districts dismissed early in the sample period and those dismissed late in the sample period.

The table reveals that districts that lacked a court-ordered plan in 1991 (column (4)) differ in many ways from the districts that had a plan (columns (1) — (3)): they have smaller enrollment, a lower percentage of black students, a higher percentage of Hispanic students, fewer students receiving a free or reduced price lunch, and are less likely to be located in the South or in a central city. (The asterisks on Table 1 signify that the mean in column (1) - (3) is statistically distinguishable from the mean in column (4).) There is clear non-random selection into having a court-ordered plan in 1991. Consequently, the subsequent analysis will focus on a comparison of

[5]The status dropout rate is constructed using data from the 1990 and 2000 SDDB. The annual CCD dropout rate measure has numerous problems, including an extremely high incidence of missing values, and is therefore not used.

the dismissed and not dismissed districts. The dismissed districts form the treatment group, and the not dismissed districts form the control group. The not under court-order group is dropped from the sample.[6] The sample is then further restricted to districts with enrollment greater than 10,000 in the first year they are observed in most cases (this restriction is reflected on Table 1).[7] The remaining sample contains roughly 15 percent of total U.S. public school enrollment in 1997–the median year of the sample–and 33 percent of black public enrollment.

Table 1 also permits a partial assessment of the claim that the timing of dismissal is idiosyncratic and unrelated to observable characteristics. In addition to the school district characteristics in Panel A, Panel B contains characteristics of the black and white populations. The white characteristics are important, because it is possible that dismissal is associated with non-random selection on white attitudes toward integration, African-Americans, the equity of public school resources by race, and other issues (e.g. districts where whites do not favor integration may be more likely to be dismissed or more likely to be dismissed early in the sample period). Non-random selection of this type would potentially confound the interpretation of the analysis. Examination of Table 1 provides little support for this hypothesis. The undismissed group is generally quite similar to the dismissed group, both in terms of school district and population characteristics, suggesting that *if* a district is dismissed is unrelated to district characteristics. Similarly, districts dismissed early and late in the sample period are generally comparable, suggesting *when* a district is dismissed is also unrelated to observable characteristics. More formally, within rows none of the means are statistically distinguishable from each other across columns (1) - (3).

Figure 3 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 in the early 1990s, but by the mid-1990s the dismissed group experiences a more rapid increase. As shown in the figure, the relative increase in the dissimilarity index of the dismissed group appears associated with the cumulative number of dismissals.

Panel A also reveals that the dissimilarity index of the not dismissed group trends upward in the post-1990 period. This raises the *possibility* that the three Supreme Court decisions may be affecting the enforcement of desegregation plans even while districts remain under court-order. Both plaintiffs and those defendant school districts interested in maintaining their desegregations plans may be reluctant to engage in aggressive enforcement measures for fear that it will lead to dismissal. Judges may be less willing to aggressively enforce plans given the altered legal environment. This interpretation finds support in a recent legal analysis of court opinions that concludes that since 1992 judges presiding over desegregation cases have been disinclined to assert judicial authority (Parker 2006). The estimates presented below assess the impact of the dismissal of a desegregation plan in the post-1990 period. These estimates may not, however,

[6] In addition to being justified based on observables, the 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, such as the level of segregation, as a result of the changing legal status of voluntary plans, they will not form a valid counterfactual for the dismissed districts.

[7] The restriction eliminates a number of small districts, primarily from the control group, and increases the comparability of the treatment and control groups along observable dimensions. As a result, the analysis focuses on a set of medium and large-sized districts, mostly located in non-rural areas.

capture the total impact of the altered legal environment arising out of the Supreme Court decisions. Specifically, the results may be lower-bound estimates, because they fail to incorporate the reduction in desegregation plan efficacy associated with the changed legal environment.[8]

Panel B plots the percent of enrollment that 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.

Panel C plots the black-white exposure index. 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.

III. Empirical Model

The following event study specification is used to estimate the connection between dismissal of a court-ordered desegregation plan and the outcome variables

$$\Delta y_{it} = \sum_{g=-4}^{10} \beta_g \Delta D_{g,it} + \boldsymbol{\theta}_{jt} + \boldsymbol{\lambda}_t * \mathbf{X}_i + \Delta \epsilon_{it} \tag{4}$$

where y_{it} is the outcome variable for district i at time t, $\boldsymbol{\theta}_{jt}$ is a vector of Census region j - year t fixed-effects, $\mathbf{X}_i$ is a vector of district-specific characteristics measured as of the first year the district appears in the sample, $\boldsymbol{\lambda}_t$ is a vector of time-varying coefficients and $D_{g,it}$ is a dummy variable equaling one if district i at time t was released from its desegregation order g years ago, with $g = 1$ denoting the year of dismissal. $D_{10,it}$ equals one for all years t in which it has been 10 *or more* years since district i was released from its desegregation order and, similarly, $D_{-4,it}$ equals one for all years in which it is 4 *or more* years till dismissal. The year prior to dismissal ($g = 0$) is the omitted category.

The differencing controls for time-invariant district characteristics. The Census region-year fixed-effects, $\boldsymbol{\theta}_{jt}$, control for shocks common to districts at the region-year level such as demographic shifts. The $\mathbf{X}_i$ vector controls for trends in the outcome variable that are associated with the time-invariant base-period characteristics of the districts. For example, districts with higher than average levels of poverty may experience more rapid loss of white enrollment and thus have a negative trend in black-white exposure. If high poverty districts are also more likely to be dismissed than low poverty districts, the $\hat{\beta}_g$ estimates (from the specification where Δy_{it} is equal to the change in the exposure index) may spuriously reflect the influence of poverty on the exposure index. More formally, $\Delta D_{g,it}$ will be correlated with $\Delta \epsilon_{it}$, and $\hat{\beta}_g$ biased. A measure of the poverty rate from the pre-dismissal period – e.g. the number of students receiving free or reduced price lunch – entered into the model with a time-varying coefficient, $\boldsymbol{\lambda}_t$, controls for the presence

[8]The Supreme Court decisions can be seen as having induced two treatment effects. The first is the direct effect of plan dismissal – the focus of this paper. The second is the reduced efficacy of the plans which remain in place. The results presented in section IV do not capture this second treatment effect.

of such a trend in an extremely flexible manner.[9] Time-varying variables such as demographic information may be endogenous to the dismissals and therefore do not enter the model.[10]

The β vector is the parameter of interest. It 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 that replaced the $D_{g,it}$ variables with a single indicator variable for dismissal). Many of the dismissals explicitly stipulate a gradual elimination of the desegregation plan. Parents may wish their children to continue to attend the school in which they were enrolled before the dismissal, and shifts in attendance patterns resulting from changes in choice of residential location will evolve slowly.

The identifying assumption of the model is that, absent dismissal, the dismissed districts would have experienced outcomes similar to the control districts, conditional on the covariates. Underlying trends in the outcome variable correlated with dismissal are the most likely violation of this assumption. The pre-dismissal portion of the β vector provides a check against this possible violation. If dismissal is unassociated with underlying trends, there should be no trend in the β vector in pre-dismissal period.

The standard approach to estimating an event study model would be to use a fixed effect estimator (i.e. estimate by deviation from the mean). However, when the event study model is estimated by the fixed-effect estimator the empirical error terms exhibit severe serial correlation. As a result, the first-difference estimator yields considerable efficiency gains relative to the fixed-effect estimator (Jeffrey Wooldridge 2002, pg. 284-285). The interpretation of the β coefficients, though, is the same as it would be for the more standard fixed-effect approach – they trace out a series of intercept shifts in the level of the outcome variable.[11]

IV. Empirical Results

This section is divided into three subsections. Subsection A discusses the racial segregation results. Subsection B discusses the dropout and private school attendance results. Subsection C explores the potential mechanisms behind the dropout and private school results.

[9] The X_i vector contains the following eight variables: a central city indicator variable, percent of students who are white, percent of students who are hispanic, number of students enrolled, number of students enrolled squared, number of students enrolled cubbed, percent of students with free/reduced price lunch, and percent of students with a free/reduced price lunch squared.

[10] The effects of Hurricane Katrina are, however, controlled for by entering a vector of dummy variables for each of the five Southeastern Louisiana districts in the sample in 2005 and 2006. Hurricane Katrina is plausibly exogenous to desegregation, and none of these districts have been dismissed.

[11] For simplicity, ignore the right-hand side variables in equation (4) other than the treatment vector $D_{g,it}$. The model in levels is $y_{it} = \alpha + \sum_{g=-4}^{10} \beta_g D_{g,it} + \delta_i + \epsilon_{it}$ where δ_i is a school district fixed-effect. The standard approach would be to explicitly estimate the δ_i terms. Instead, the first-difference estimator is used and the δ_i terms are differenced away: $\Delta y_{it} = \sum_{g=-4}^{10} \beta_g \Delta D_{g,it} + \Delta \epsilon_{it}$. Note that the two equations contain the exact same β_g vector. Accordingly, the interpretation of the $\hat{\beta}_g$ vector in the same whether the fixed-effect or first-difference estimator is used.

A. Segregation Results

Estimation of the event study model provides strong evidence that dismissal increases racial segregation. Panel A of Figure 4 presents the results for the dissimilarity index. The β_g point estimates and standard errors are displayed graphically. The estimates to the right of the vertical line represent the post-dismissal period and will be referred to as the post-vector; the points on or to the left of the line represent the pre-dismissal period and will be referred to as the pre-vector.

The figure produces three conclusions: First, the dismissal of a court-ordered plan leads to an increase in the dissimilarity index, indicating that students are re-sorted within the dismissed school district in a manner that increases segregation. Second, the impact of dismissal unfolds in a gradual, linear manner with time from dismissal. Third, the flat pre-vector suggests the termination of a desegregation plan is causally associated with an increase in the dissimilarity index.

The estimated impact of dismissal grows from close to zero in the year of dismissal to 0.10 ten or more years after dismissal. The magnitude of this effect can be assessed in several ways. It is equal to 25 percent of the index sample mean and to 58 percent of the 1991 cross-sectional standard deviation of the index, comparisons suggesting a large effect. Another interpretation involves comparison to the change in the dissimilarity index produced by the implementation of court-ordered desegregation plans in the 1960s, 70s, and 80s. Unreported results that replicate the specification estimated in Reber (2005) on this paper's sample suggest that court-ordered desegregation reduces the dissimilarity index by approximately -0.17 and that this effect is quite persistent over time. (This is very similar to what Reber found.) Using this result as a metric, the dismissal of a desegregation plan reverses approximately 60 percent of the effect of the plan's implementation.

In Panel A, the individual coefficients are not all identified by the same set of districts. For example, districts dismissed in 2000 do not contribute to the identification of the 8 through 10 years post-dismissal coefficients because the sample ends in 2006. It is possible that the increase in the treatment effect with time from dismissal spuriously arises from the differing set of districts identifying the parameters. The specification presented in Panel B therefore uses a balanced panel of districts. In order to avoid excessive loss of sample size, both the pre and post vector are truncated to be one year shorter than in Panel A.[12] The sample is then restricted to districts that contribute an observation in each year of the sample and districts that are dismissed after 1998 are excluded because they would not contribute to the identification of all the post-vector coefficients. The estimates are quite similar to those in Panel A, although the magnitude of the effect is around 25 percent smaller, and the estimates are less precise (unsurprising given that the sample size is nearly 50% smaller).

Each district receives equal weight in Panel A and the coefficients therefore reflect the effect of dismissal for the typical, or mean, medium to large-sized school district. For many policy makers, such as a school district official interested in assessing the likely impact of being dismissed from a desegregation plan, this is likely the most relevant parameter. Alternatively, policy makers and others interested in assessing the overall, nationwide impact of the phase-out

[12] In addition, the sample period is truncated to 1989 - 2006 to avoid losing districts with missing values in 1987 and 1988 (these years have a relative high frequency of missing values).

of court-ordered desegregation, may be more interested in the effect on the mean student. Panel C therefore weights the observations by total student enrollment. The results are similar to those in panel A.

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 (e.g. ending busing plans). In contrast, there is no evidence at the national level of a behavioral response by black or white parents and students. Panels A and B of Figure 5 present the results of estimating the empirical model with the log of black and white enrollment, respectively, as the dependent variable. While the point estimates suggest that dismissal induces an outflow of black students and an inflow of white students, they are very imprecise. Results presented below, however, demonstrate that these national estimates obscure significant behavioral responses that vary by region.

The focus of the analysis is on black and white students, because court-ordered desegregation primarily aims to integrate black and white students. Nevertheless, dismissal may impact the behavior of Hispanics, a possibility explored in Panel C. No evidence is provided that Hispanics respond to dismissal.[13] Finally, weighted enrollment estimates for all three races are also quite imprecise (Web Appendix Figure A1).

Panel A of Figure 6 presents estimates suggesting that black exposure to whites is flat in the period prior to dismissal and then decreases afterwards. The decrease captures the net effect of the two behavioral responses explored above: changes in the sorting of students across schools and changes in the school district wide demographic composition. The balanced panel sample (Panel B) and weighted estimates (Panel C) produce similar conclusions.

Ten years after the termination of a desegregation plan, the exposure index declines by 3 to 5 percentage points, indicating the average black student is attending a school with 3 to 5 percent fewer white students – a modest decrease in exposure. By the year 2000, white students comprised, on average, only about $\frac{1}{3}$ of the enrollment of districts dismissed between 1991 and 2006 (see Panel B of Figure 3). As a result of this low percentage, the reshuffling of students within a district shown by the change in the dissimilarity index on Figure 4 produces only a moderate change in average interracial contact.

The magnitude of the exposure index response can be further interpreted in two ways. First, the effect ten years after dismissal is equal to 10 to 15 percent of the sample mean of the exposure index and approximately 15 to 25 percent of the 1991 cross-sectional standard deviation of the index. Second, the implementation of court-ordered desegregation initially increases the exposure index by about 0.10. The magnitude of this effect then gradually falls over time as whites leave desegregated districts: Fifteen years after implementation the effect is reduced to around 0.06 (based on unreported replication of the specification used in Reber (2005)). The exit of whites in response to desegregation therefore reverses about 40 percent of the initial impact and dismissal of the plan undoes another 30 to 50 percent.[14]

[13]The percent of sample enrollment comprised of Hispanic students grew rapidly over the period studied, expanding from 11 percent in 1990 to 20 percent in 2006. It is possible that inflows of Hispanics may influence white enrollment patterns – e.g. whites may exit districts experiencing a rapid inflow of Hispanics. Such a scenario would bias the results of this study only if the inflow of Hispanics is correlated with dismissal. The results in Panel C fail to provide strong evidence in favor of this hypothesis. Finally, Asians constituted a small and stable share of enrollment, equal to 3 percent in both 1990 and 2006. Unreported specifications find no evidence that Asian enrollment responds to dismissal.

[14]It is worth noting a limitation of the segregation results. The segregation indices measure segregation between

Extensive efforts were undertaken to explore potential sources of heterogeneity in the segregation response. Evidence of important regional heterogeneity is presented below as part of the interpretation of the dropout rate analysis. Little evidence was found to suggest a heterogeneous response along other dimensions.[15] Finally, section II of the Web Appendix presents and discusses both robustness checks for the segregation outcomes and estimates which define the dissimilarity and exposure indices in terms of nonwhite-white and Hispanic-white (as opposed to the black-white definition used here).

B. Dropout Rate and Private School Enrollment Results

If the end of a desegregation plan alters the value of publicly provided education, those students previously on the margin for either dropping out or attending private school will alter their behavior. This subsection therefore examines the response of dropout rates and private school attendance to dismissal.

Although dismissal potentially causes a complex, multi-dimensional change in the school environment (as discussed in the introduction), it likely alters the return to the educational services provided by a district primarily through two main channels – changes in peer group and changes in the quality/quantity of educational inputs. The exposure index analysis presented above documents that dismissal induces a change in peer group for black students. The quality of educational inputs may be changed by dismissal in two ways. First, the re-sorting of students potentially places black students in schools of lower average quality than they attended while their school district was under court-order.[16] Second, 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.[17] When a district is released from its plan, it no longer has an independent body (i.e. the courts) constantly monitoring its performance in regard to the educational outcomes of minority students. This may reduce the effort and resources expended on minority students independent of any re-sorting of students that occurs.

The dropout and private school analysis utilizes school district means pooled from the 1990 and 2000 Census data (referred to as the SDDB). Table 2 presents 1990 summary statistics. Means are presented for two groups, those districts dismissed between 1991 and 1999–the treatment

schools. Segregation may also occur within schools. The estimates cannot assess if the court dismissals have had an effect on within school segregation. Court-ordered desegregation focuses heavily on segregation between schools. As a result, it seems likely that the court-order dismissals have little impact on within school segregation. Alternatively, Clotfelter, Ladd and Vigdor (2003) present 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.

[15]The other factors explored include type of desegregation plan, residential segregation, countywide school district, number of alternative districts in the MSA, percent of enrollment in magnet schools, location in a central city, enrollment, per-pupil expenditures, percent of revenues from local sources, and percent of students receiving free lunch. These results are available from the author upon request.

[16]For instance, high minority enrollment schools tend to employ teachers with less experience than do low minority enrollment schools (Clotfelter, Ladd and Vigdor 2005).

[17]The 1977 *Milliken II* decision allows courts to mandate spending on compensatory educational programs for minority students (Orfield and Eaton 1996), and the *Freeman* decision allows courts to consider the "quality of education" in deciding whether or not to release districts from their desegregation plans (Lindseth 2002; Parker 2000).

group–and those under court-order but not dismissed within this time frame–the control group. These are slightly different treatment and control groups than used in section IV.A and displayed on Table 1.[18] The table displays the statistics separately for the full sample and the set of districts inside and outside the South Census region, because the subsequent analysis finds evidence of important regional heterogeneity in the response to dismissal.

The table reinforces the conclusions drawn from Table 1 – the dismissed and non-dismissed districts are similar along observable dimensions in 1990. This comparability also holds within region. One point of non-comparability bears mention, though. In the non-South, non-dismissed districts are much more likely than dismissed districts to have a large percentage of black students in highly segregated schools (those with less than 10% white enrollment).

Figure 7, Panel A, plots the trends in the black status dropout rate for four groups: the South dismissed and not dismissed groups and the non-South dismissed and not dismissed groups. The two Southern groups and the non-Southern not dismissed group all trend downward with a similar slope and have similar values, between 0.11 and 0.13, in 2000.[19] The non-South dismissed group, in contrast, is flat over this period. The difference in trends is suggestive evidence that dismissal of a court-ordered desegregation plan increases black dropout rates outside the South.

The two-period panel provided by the SDDB data renders the event study methodology infeasible as there is insufficient data to estimate the large vector of pre and post dismissal coefficients. The effect of dismissal on dropout rates and private school enrollment is therefore assessed using following more restrictive empirical model, henceforth referred to as the linear model

$$\Delta y_{it} = \beta \Delta L_{it} + \boldsymbol{\theta}_{jt} + \boldsymbol{\lambda}_t * \mathbf{X}_i + \Delta \epsilon_{it} \tag{5}$$

where L_{it} equals the number of years since dismissal for dismissal years 1 through 9, equals 10 for dismissal years 10 *or greater*, and equals zero in the years before a dismissal.

Relative to the event study model, which allows the treatment effect to be an arbitrary function of time since dismissal, the linear model imposes the assumption that the treatment effect is linear. This assumption is motivated by the results on Figures 4-6 which provide strong evidence that the treatment effect is linear. Estimating the linear model with the segregation outcome variables generates conclusions very similar to those displayed on Figures 4-6 and provides further support for the linearity assumption. These results are presented in Web Appendix section II (Table A2). Finally, given the use of pooled data, the observations are weighted by cell size on efficiency grounds when the linear model is estimated (see Lutz 2005 for a more detailed discussion).

The X_i vector includes both district-level and student-level covariates.[20] Of particular note,

[18]The 2000 Census data was collected in the spring of 2000 – during the 1999 - 2000 school year. Districts dismissed in 2000 - 2005, which are part of the treatment group in the CCD annual panel used in section IV.A, are part of the control group in this section.

[19]Nationally, the black status dropout rate held constant at approximately 12.5 percent over the course of the 1990s (NCES 2001). Thus, the three groups appear to be converging with the national black dropout rate during the 1990s.

[20]The district-level covariates, which are measured for all races and obtained from the CCD in the first year they are available, are a central city indicator variable, percent of enrollment which is white, percent of enrollment which is hispanic, number of students enrolled, number of students enrolled squared, number of students enrolled cubbed, percent of enrollment receiving a free or reduced price lunch, percent of enrollment receiving a free or reduced price lunch

it includes a measure of the percent of black students attending very segregated schools in 1990 (and the coefficient on the variable is allowed to vary by wether or not the district is located in the South). The highly segregated measure summary statistics on Table 2 suggest that judges may be reluctant to release districts with very segregated schools from court supervision. As a result, dismissal may be associated with the absence of such schools. If districts with and without highly segregated schools experienced differential trends in dropout rates during the 1990s, estimates of the impact of dismissal may, in turn, be biased. The inclusion of the highly segregated school measure in the X_i vector controls in a very flexible manner for this possibility. Its inclusion, though, has little impact on the results.

The identifying assumption of the model is that, absent dismissal, the dismissed districts would have experienced dropout rates similar to those of the non-dismissed districts, conditional on the covariates. The most likely violation of this assumption is district-specific trends in the outcome variable correlated with dismissal. To assess this possibility, it would be preferable to examine the trends in the black dropout rate for the treatment and control groups in the period before the dismissals began. Unfortunately, the 1980 school district tabulation of the Census does not permit calculating dropout rates by race. It does, however, permit calculating dropout rates for all races. Figure 7, Panel B, plots the trend in dropout rates for all races from 1980 to 2000. The plot reveals that from 1980 to 1990, the pre-dismissal period, the treatment and control groups in both the South and non-South trend in a very similar fashion. The similarity in the pre-trends provides supportive, although not conclusive, evidence in favor of the identifying assumption.

Estimation of the linear model provides evidence of an increase in black dropout rates in dismissed districts. Table 3 presents these results. Column (1) contains the standard specification (similar to the event study specifications on Panels A of Figures 4 and 6). Column (2) adds a vector of time-varying student level covariates to control for demographic shifts.[21] Column (3) conditions on the lagged change in the dropout rate for all races to control for persistence in the change in dropout propensity. Column (4) replaces the region-year effects with state-year effects (and drops the X_i vector given the specification is heavily saturated). Although this is a very demanding robustness check, it is potentially important because the 1990s/early 2000s was a period of substantial education policy reform. Many states introduced accountability measures, and some states underwent school finance reform. If these reforms both impact dropout behavior and the timing of their introduction is correlated with the timing of dismissal, the estimates on Table 4 may be biased.[22] Many of these reforms occurred at the state level and the state-year fixed-effects will control for their influence on dropout rates.

squared, percent of black students in the non-south in a school with less than 10% white enrollment, and percent of black students in the south in a school with less than 10% white enrollment. The student-level covariates, i.e. means calculated over the population of children of a given race, are obtained from the 1990 SDDB and include percent of mothers without a high school degree, percent of mothers with a four-year college degree, percent of children below the poverty line, the household income of households with children, percent of children with a parent who is foreign born, and percent of children born out of state.

[21] Time-varying covariates are not included in the segregation specifications because they may be endogenous to dismissal. Here, the time-varying covariates control for changes in population characteristics that may arise from migration endogenous to dismissal. Such migration potentially confounds the interpretation of the dismissal coefficient. See the discussion in section IV.C.

[22] There is no obvious reason, however, why the timing of reforms originating at the state level would be correlated with the timing of dismissal of Federal court orders affecting only individual single school districts.

The estimates in Panel A suggest that dismissal increases black dropout rates, but the estimates are not overly precise. Panel B allows the effect of dismissal to vary by region. The estimates in column (1) indicate that a non-Southern district experiences an increase of 0.0085 in the dropout rate each year following dismissal. To interpret this result, consider a district that was dismissed in 1997. Such a district, which is three years post-dismissal in 2000 (the average years since dismissal in the sample, conditional on being dismissed and located in the non-South, is 3.1), will experience a black dropout rate approximately 0.025 percentage points higher than if it had not been dismissed. The mean dropout rate for dismissed non-Southern districts in 1990 is 0.15, implying that dropout rates increase by around 15 percent. The non-South result is robust to the various specification permutations in columns (1) - (4): The estimates range from around 0.009 to 0.006 and are all quite precisely estimated. In contrast, the estimates for the South are small and imprecise, providing no evidence that dismissal influences dropout rates.

It is unlikely that dismissal causes a single discrete change in dropout rates. Desegregation plans are often phased out over time, and the re-sorting of students documented by the change in the dissimilarity index occurs only gradually. Nonetheless, the imposition of a linear relationship between years since dismissal and the dropout rate is a strong assumption. Column (5) tests the validity of this assumption by estimating specifications that include both the years since dismissal variable of equation (5) and an indicator variable equal to one if the district has been dismissed. These specifications allow the data to determine if an intercept shift model or a linear dismissal parameterization is the appropriate specification. The data appear to favor the years since dismissal parameterization as the intercept shift coefficient is imprecise while the linear coefficient is precise (although only at the 10 percent level).

Column (6) reports the results of a falsification test. The effect of dismissal is parameterized as an intercept shift. In addition to the dismissed indicator, the specification includes a placebo indicator equal to one if the district was dismissed after 1999. If the increase in the dropout rate documented in Panel B is the causal result of dismissal, the placebo districts should show no evidence of an increase in the dropout rate. The non-South placebo coefficient is $\frac{1}{5}$ the magnitude of the true dismissal coefficient.[23] Thus, the falsification test fails to provide evidence against a causal interpretation of the results in the preceding columns.[24]

There is little evidence that dismissal influences the dropout behavior of whites (columns (7) and (8)). The contrast between the white and black results is useful. It suggests the black non-south estimates are not merely capturing general, district-wide trends in dropout behavior in dismissed districts, the influence of education reforms, or other factors, such as deteriorating facilities. All these factors would reasonably be expected to influence both black and white dropout behavior.

The results on Table 4 indicate that dismissal increases private school attendance for black students in the non-South. Using the estimates in column (1) of panel B, a non-Southern district three years after dismissal experiences an increase of around 0.007 percentage points in the rate of private school attendance. Relative to the 1990 mean, this implies an increase of around 10

[23]The non-Southern placebo group is 60 percent as large as the non-Southern dismissed group.

[24]Numerous attempts (unreported) were made to find heterogeneity in the dropout rate response. For instance, the possibility that the type of desegregation plan might affect the response to dismissal was also explored. Other than the regional difference, no strong evidence of heterogeneity was found.

percent. The results for private school attendance are somewhat less robust than those for the dropout rate.[25] No evidence is found of a white private school response.

C. Candidate Mechanisms for the Dismissal Response

This subsection explores possible explanations for the rise in dropout rates and private school attendance associated with the end of court-ordered desegregation. Particular attention is paid to factors that might explain the sharp regional difference. The following possible mechanisms are explored: migration, regional variation in the response of school segregation to dismissal and regional differences in the response of school finances to dismissal.

MIGRATION. — Migration sparked by dismissal would alter the interpretation of the black dropout results. For example, dismissal may increase the dropout rate by inducing families who highly value education, and whose children have a low dropout propensity, to exit the school district. From both an economic and policy perspective, this is a very different interpretation than the one implicitly made in section IV.B: a hypothetical student resides in the district regardless of dismissal, but only finishes high school in the absence of a dismissal. Furthermore, migration potentially explains the divergent regional results, because school district switching is restrained in the South by the prevalence of large, county-wide districts.

There is no evidence, however, that dismissal induces net in or out migration for blacks (Web Appendix section III and Web Appendix Table A3, column (1)). The absence of net migration does not rule out the possibility of other forms of migration, though. Dismissal may induce neighborhood churn – migration that does not alter the number of residents but that does alter the demographic composition of the area – but there is no evidence to support this hypothesis (Web Appendix Table A3, columns (2) - (5)). Furthermore, controlling for demographic changes in the dropout rate and private school specifications (Tables 3 and 4) has little impact on the results.

To further assess the possibility that the dropout results are driven by migration, two alternative, census-based samples are constructed. The first uses the individual-level IPUMS (Integrated Public Use Microdata Series; Steven Ruggles, et. al. 2009) micro-data from 1990 and 2000. The geographic identifiers in these data are not sufficiently detailed to match individuals to their school district and the smallest area that is available, the PUMA (Public Use Microdata Area), changes boundaries between the 1990 and 2000 censuses in many cases. Faced with these data limitations, I utilize the procedure developed by Guryan (2004): I map 16 - 19 year-olds into school districts based on their PUMA of residence to form a geographic unit I refer to as a "PUMA group". A PUMA group is the smallest geographic area that can be identified in both

[25] First, the indicator treatment parameterization, presented in column (5) as part of the falsification exercise, produces no indication of an effect of dismissal. Second, while the dropout rate results are robust to estimation without weights (unreported), the private school results are less so. Finally, the private school estimates are sensitive to the exclusion of Cincinnati from the sample. Cincinnati is an outlier among the non-Southern dismissed districts in that its desegregation plan was dismissed in 1991, much earlier than other non-Southern districts (see Web Appendix Table A1). While the point estimates are generally robust to excluding Cincinnati, there is a significant loss of precision. Note, however, that Cincinnati belongs in the sample – there is no obvious justification for its exclusion. The dropout rate results, in contrast, are robust to excluding Cincinnati.

1990 and 2000 and contains the entirety of the school district. They are constructed using maps produced by the Census Bureau.

The PUMA group data has two advantages. First, it is possible to directly control, at the micro-level, for migration. Second, the PUMA groups encompass a significantly larger geographic area than the school districts. If the dropout results are due to migration across school district boundaries, but within PUMA groups, we would not expect any impact on dropout behavior when the analysis is conducted at the level of the PUMA group (Guryan (2004) contains a more extensive discussion of this point). Due to sample truncation that is significantly more severe in the South than in the non-South, and in the interest of brevity, the analysis is restricted to the non-South (when the South is analyzed, there is no evidence of a dismissal response). See section I.D of the Web Appendix for additional information.

Panel A of Table 5 displays the PUMA group analysis for black 16 - 19 year-olds. The first row presents results from a model with a discrete dependent variable indicating if the individual has moved from the PUMA where he resided five years ago.[26] No evidence is produced that dismissal induces migration. The second row of results uses an indicator for being a dropout as the dependent variable and produces evidence of a dropout effect. The effect is broadly similar in magnitude to that on Table 3 and is robust to various specification checks.[27] The third row presents dropout rate results that directly control for migration. Although migration is a marginally significant predictor of dropping out, its inclusion has no impact on the dismissal coefficient.

The second alternative data source uses the Census county aggregates (Summary Tape Files 1 - 4) maintained by the National Historical Geographic Information System (Minnesota Population Center 2004). These county-level data are then combined to form geographically consistent MSAs in 1980, 1990 and 2000 using PMSA definitions as of 1999. School districts are then mapped into MSAs based on their location. See section I.E of the Web Appendix for additional information.

The analysis of this data, which takes the logic of using a larger geographic unit of observation even further, is presented in Panel B of Table 5. The dismissal coefficient is again large and precise in columns (1) and (2). A drawback of the SDDB data used on Table 3 is the inability to control for district-specific trends due to the lack of race-specific dropout data in 1980. The MSA data, however, contain race-specific dropout rate information. Column (3) includes data from 1980 and controls for MSA-specific trends. The inclusion has little effect on the results. Thus, the analysis of the two alternative data sources provides no support for the hypothesis that endogenous migration is responsible for the dropout results.[28]

[26]The model is similar to equation (5), but the micro-data require estimation in levels. The model is $y_{it} = \alpha + \beta L_{it} + \delta_i + \theta_{jt} + \lambda_t * X_i + \epsilon_{it}$ where δ_i is a PUMA group fixed-effect.

[27]The magnitude of the dropout rate effect is somewhat smaller than in the comparable specifications in Panel B of Table 3 (the non-south estimates). As derived formally in Appendix D of Weiner, Lutz and Ludwig (2009), this is the expected result of expanding the geographic unit of observation, even in the absence of migration. On average, around 63 percent of black public school students in the PUMA group sample are actually enrolled in a dismissed district. The presence in the sample of the remaining 37 percent of students, who are not "treated" by the dismissal, attenuates the dismissal coefficient downward.

[28]The (unreported) black private school results for the PUMA group sample and the MSA sample are generally similar to those on Table 4, although the MSA sample estimates tend to be less precise. Results (unreported) which mimic Table 5, but focus on whites, find no evidence of a dismissal response.

REGIONAL HETEROGENEITY IN THE RESPONSE OF SEGREGATION TO DISMISSAL. — Table 6 returns to the annual segregation data in order to explore the possibility that regional variation in the response of segregation to dismissal explains the regional variation in the black attendance pattern results. Columns (1) and (2) present linear model (i.e. equation (5)) estimates for the non-South and South, respectively. Columns (3) and (4) restrict the data to the years prior to 2000 in order to make the results as comparable as possible to the dropout and private school attendance specifications (the 2000 census was taken in the spring of the 1999 school year). There is little evidence of regional heterogeneity in the segregation response as measured by the dissimilarity index (Panel A).[29] The exposure index (Panel B) produces somewhat ambiguous results as the south and non-south point estimates are similar in columns (3) and (4), but different in columns (1) and (2).

Panel C utilizes an alternative measure of interracial contact, the percent of black students in schools with less than 10% white enrollment. The exposure index is the *mean* of the distribution of the percent white in black students' schools. The 10% metric, alternatively, measures the mass in the far left of the distribution and can be interpreted as the percent of blacks attending extremely segregated schools. The 10% metric reveals starkly different responses by region. In the non-South, black students are moved in large numbers into extremely segregated schools. In these districts, six years after dismissal, the number of blacks attending extremely segregated schools increases by around 10 percentage points. The event study methodology–Panel A of Web Appendix Figure A2–suggests this increase is causal. In the South, in contrast, there is no evidence of an effect on this margin.

The difference along this margin potentially explains the regional heterogeneity in the dropout response for two reasons. First, the relationship between exposure to whites and dropout rates may be non-linear – moving black students into extremely segregated schools may have a particularly large impact on dropout rates. Stated more formally, the relationship between peer group ability/background and own outcome may be concave (see Changhui Kang 2007 for a discussion).

Second, the group of students being re-sorted into extremely segregated schools in the non-South likely contains an unusually high percentage of individuals on the margin for dropping out. The end of a desegregation plan shifts attendance patterns toward neighborhood schooling. Those individuals re-sorted into extremely segregated schools are therefore likely to reside in extremely segregated residential neighborhoods. There is a strong correlation between residential segregation and a host of negative social outcomes, including dropout rates (Douglas S. Massey 1990). The disproportionate impact on a group already predisposed to dropping out may intensify the impact of dismissal on dropout rates in the non-South, even if peer effects operate in a linear fashion.

There is evidence of regional heterogeneity in the response of public school enrollment to dismissal. In the non-South black students exit public schools in large numbers (column (1),

[29] An interesting interpretation can be made of the southern dissimilarity index results. Under *de jure* segregation, the dissimilarity index equaled 1. Thus, the cumulative effect of all state action between the 1954 *Brown* decision and 1990 is equal to around -0.6 (Table 2 suggests that the dissimilarity index in the south in 1990 was equal to about 0.4). Under this interpretation, 10 years after dismissal only about $\frac{1}{6}$ of the impact of all state actions on the dissimilarity index has been reversed. See Cascio, et. al. (2008, 2010) for information on non-judicial actions aimed at achieving racial integration in the South.

Panel D). Approximately one percent of the black student population exits the district each year following dismissal. Results from the event study methodology–Web Appendix Table A2, Panel B–are consistent with a causal interpretation.[30] The lack of evidence for the out migration of black youth (Table 5, Panel A and Web Appendix Table A3) suggests that the enrollment decline is the product of the increased black dropout rates and private school attendance.

In the South, there is a large increase in white enrollment associated with dismissal (Panel E). Ten years after dismissal, a Southern district experiences around a 10 percent increase in white enrollment. The event study methodology is again consistent with a causal interpretation–Web Appendix Table A2, Panel C. These results suggest dismissal increases the perceived value of public education for whites.

The phenomenon of "reverse white flight" has potentially significant consequences. White flight hindered school desegregation's ability to integrate schools. Depending on the racial composition of the schools whites return to, the phenomenon may blunt the negative impact of the end of court-ordered desegregation on racial integration. Even if whites return to largely segregated schools, they may increase the quality of the education provided blacks by increasing school tax bases and providing increased political support for public education. Finally, court-ordered desegregation is a factor in the population decentralization/suburbanization that has characterized the post-World War Two era (Nathaniel Baum-Snow and Lutz *forthcoming*, Leah Platt Boustan 2009). The return of whites to formerly desegregated school districts may slow or partially reverse this trend.

SCHOOL FINANCE. — Anecdotal evidence suggests some school districts engage in capital investment in minority neighborhoods around the time their desegregation plan is dismissed. The Pinellas County School Board invested $159 million in school facilities in African-American neighborhoods after being declared unitary (Elizabeth Delay-Pelot 2007). The Nashville, Tennessee school district pledged to build 11 new schools, many in predominately black neighborhoods, when it was released from its desegregation plan (Education Week 1998). The Lafayette Parish, Louisiana school board upgraded predominantly black schools in the period immediately before its desegregation plan was dismissed (NAACP 2006).

Table 7 presents more systematic evidence on the impact of dismissal on capital expenditures using annual school district finance data produced by the Census Bureau (expressed as thousands of 2001 dollars per pupil). The specification is a slightly truncated version of the event study model, equation (4). The pre-dismissal coefficients are potentially important, as capital investments take time to complete. It is possible that investments may be undertaken in the year(s) prior to dismissal in order to have the new or refurbished schools ready when the desegregation plan is terminated.

The results in columns (3) and (4) indicate that Southern districts significantly increase their capital investment in the year prior to dismissal (the $\Delta pre(0)$ coefficient) as well as in the year of

[30] Panel B of Web Appendix Figure A2 is also useful in reconciling the results in Panel D of Table 6. When the sample is truncated to years before 2000, the impact of dismissal on black enrollment becomes very imprecise (column (3)), whereas the estimate is large and precise when the full sample is used (column (1)). Panel B of Figure A2 suggests that the impact of dismissal on dropout rates and private school attendance takes a number of years to become apparent in the overall enrollment figures, a fact which may explain the imprecise estimate in the truncated sample. Most dismissed districts in the truncated sample have experienced only a couple years of dismissal.

dismissal (the $\Delta post(1)$ coefficient). The magnitude of the increase in per-pupil capital spending is large. Relative to the sample mean of 0.77, the effect is equal to around a 20 percent increase in the year prior to dismissal and around a 45 percent increase in the first year of dismissal. There is little evidence of a capital investment response in the non-South (columns (1) and (2)) and there is no evidence of an effect on current expenditures in either the South or the non-South (columns (5) and (6)).[31].

The anecdotal evidence presented above suggests that the capital investment in the South documented on Table 10 may be directed at minority neighborhoods. To the extent that these investments are in fact directed at minority neighborhoods (and this cannot be verified beyond the anecdotes), they can be seen as a compensatory actions intended by policy makers to reduce the negative consequences of the end of desegregation for black students. This compensatory action provides a potential explanation for the divergent dropout and private school results by region. Furthermore, the divergent capital expenditures may be a single manifestation of a broader phenomenon. Southern school districts may take additional, unobserved, compensatory actions to limit the negative impact of the termination of a desegregation plan (e.g. shifting financial resources toward schools with a high percentage of minority students), while policy makers in the non-South may not. This theory is, of course, highly speculative in nature.

V. Conclusion

The results of this paper suggest dismissal of a court-ordered desegregation plan produces a gradual, moderate increase in racial segregation. Dismissal also increases the exit of black students from public schools in non-Southern districts, both via dropping out and via entering private school.

The event study methodology suggests that the increase in racial segregation is the causal outcome of dismissal. Although credibly establishing causality is more challenging for the dropout and private school results given the limits inherent in a two-period panel, the analysis provides several pieces of evidence in support of a causal interpretation. First, a falsification check documents that districts which will be dismissed in the near future, but are not yet dismissed, do not experience increased dropout rate propensities – a result that suggests that it is the actual event of dismissal that generates the change in dropout behavior. Second, dismissal generates a gradual re-sorting of students across the schools of a district. Dropout rates and private school attendance also change in a linear fashion following dismissal. The similar evolution over time is suggestive evidence in favor of concluding that the re-sorting causes the change in attendance behavior. Third, white dropout rates do not respond to dismissal. If the black dropout estimates were spurious manifestations of district-wide trends in dropout behavior or were produced by educational reforms whose timing was correlated with dismissal, it is likely that whites would also display an increase in dropout propensity around the time of dismissal. Fourth, controlling for migration – either explicitly or by enlarging the unit of observation – produces no change in the results, suggesting migration does not drive the estimates. Finally, the results are robust to numerous permutations of the basic estimating equation, including the inclusion of state-year

[31]The student-teacher ratio in the average black child's school is another measure of the educational inputs provided to black students. Similar to the current expenditure results, estimates using this outcome (unreported) are imprecise.

fixed-effects.

Two primary channels exist through which dismissal may negatively impact the value of public education – peer effects and the quality/quantity of educational inputs. The sharply divergent regional experiences following dismissal provide an opportunity to assess the role of these mechanisms. The evidence, though not conclusive, suggests that both channels play a role.

It appears dismissal has a broadly similar effect across regions on segregation as measured by the dissimilarity and exposure indices. Dismissal does, however, appear to increase the prevalence of very segregated schools more outside of the South. If attending an extremely segregated school has a particularly strong influence on behavior – i.e. if peer effects are non-linear – this may explain the regional difference in the dropout and private school dismissal effect. Numerous studies have concluded that peer effects are non-linear (e.g. Vernon Henderson, Peter Mieszkowski and Yvon Sauvageau 1978; Anita Summers and Barbara Wolfe 1977; Ron W. Zimmer and Eugenia F. Toma 2000; David J. Zimmerman 2003; although the finding is not universal, e.g. Hoxby 2000).

The re-sorting of students apparent in the dissimilarity index results may place black children in schools with inferior educational inputs, such as less experienced teachers. In addition, the end of court oversight, with its emphasis on the outcomes of minority students, may reduce the effort and financial resources directed to black students independent of the re-sorting of students. The contrasting regional results in regards to compensatory capital investment could be interpreted as suggesting that court oversight of minority educational outcomes was more binding in the non-South than in the South. Furthermore, it is possible that the capital investment in the South is correlated with other compensatory actions that are unobservable, such as increasing, or simply maintaining, the average teacher quality of black students.[32]

The regional difference in the dropout rate estimates may also be explained by the difference in the group of students primarily affected by dismissal. The significant increase in extremely segregated schools in the non-South indicates that it is children in heavily segregated residential neighborhoods that are being most impacted by the re-sorting. Such neighborhoods are likely to contain an unusually large fraction of individuals on the margin for dropping out. Dismissal may produce a larger change in dropout rates in the non-South simply because there it impacts a group with a relatively high proportion of individuals on the margin for dropping out.

Finally, dismissal appears to pull white students back into districts previously under court-order in the South. This phenomenon may alter metro area demographic patterns and it may serve to blunt the negative impact of dismissal, both by moderating the increase in segregation and by increasing the resources available to formerly desegregated school districts.

VI. References

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[32]In at least one district in the South, however, this appears not to have been the case. Black students experienced decreased teacher quality following the end of court-ordered desegregation in Charlotte, NC (Clotfelter, Ladd and Vigdor (2008); Jackson (2009)).

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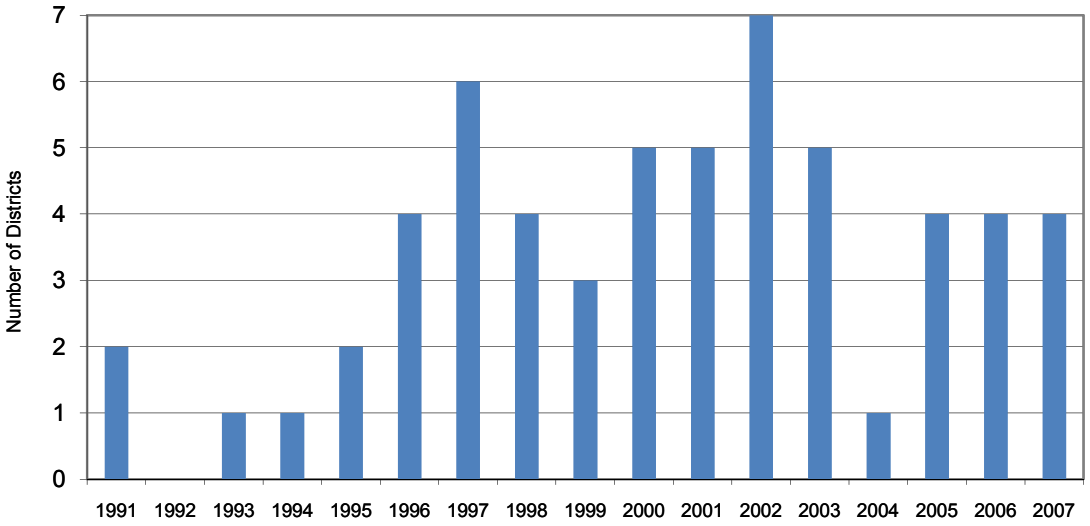
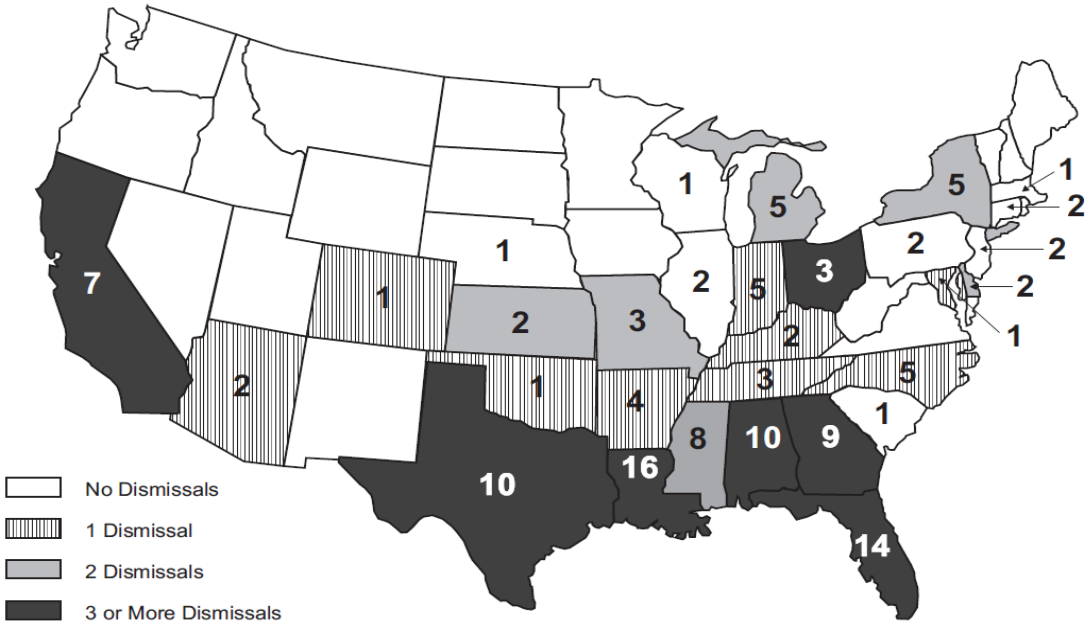
Figure 1: Desegregation Order Dismissals



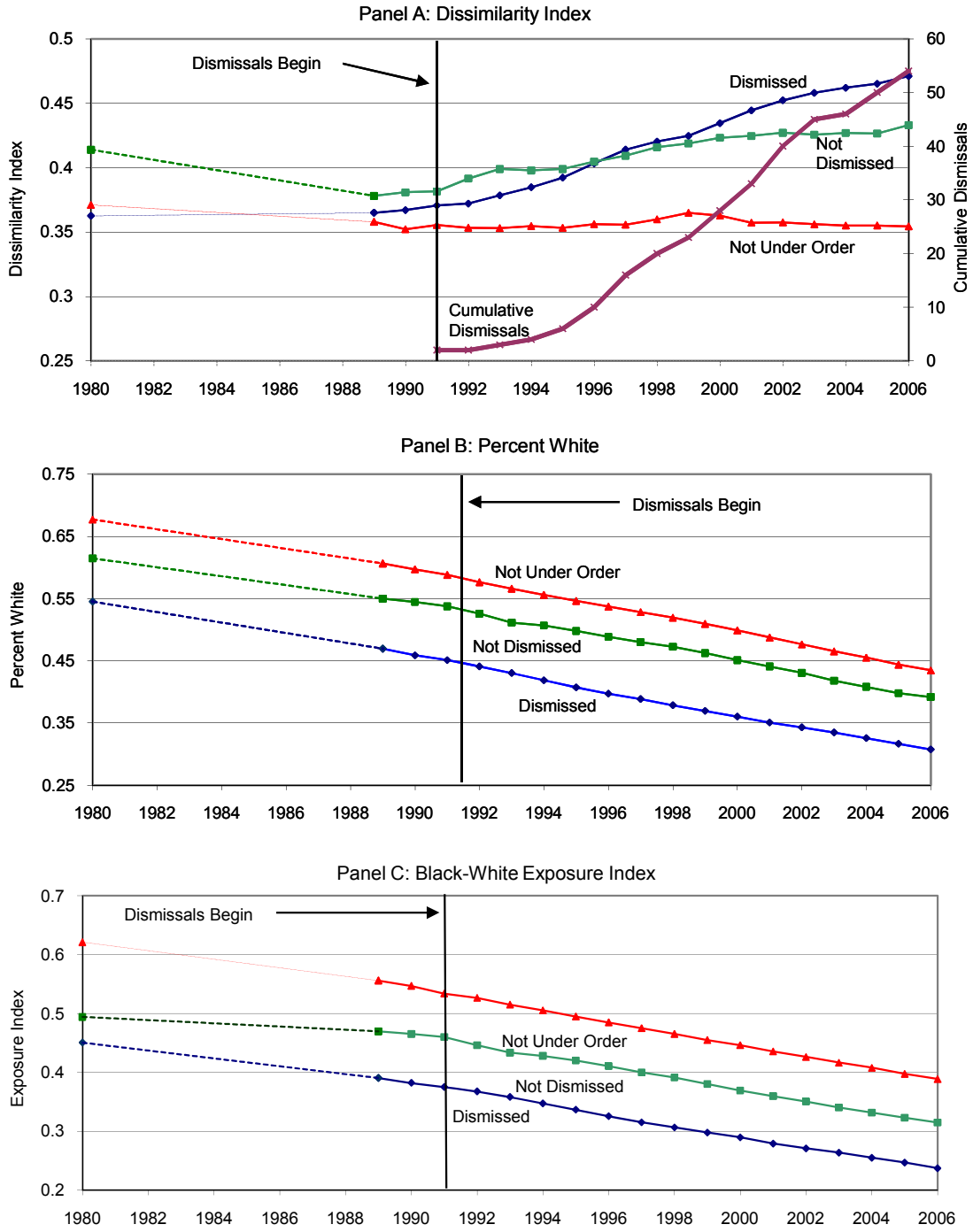
Note. The figure displays the number of dismissals of desegregation plans occurring among the set of school districts in the Rossell and Armor sample which were under a court-ordered desegregation plan at the start of 1991.

Figure 2: Geographic Distribution of Sample and Dismissals



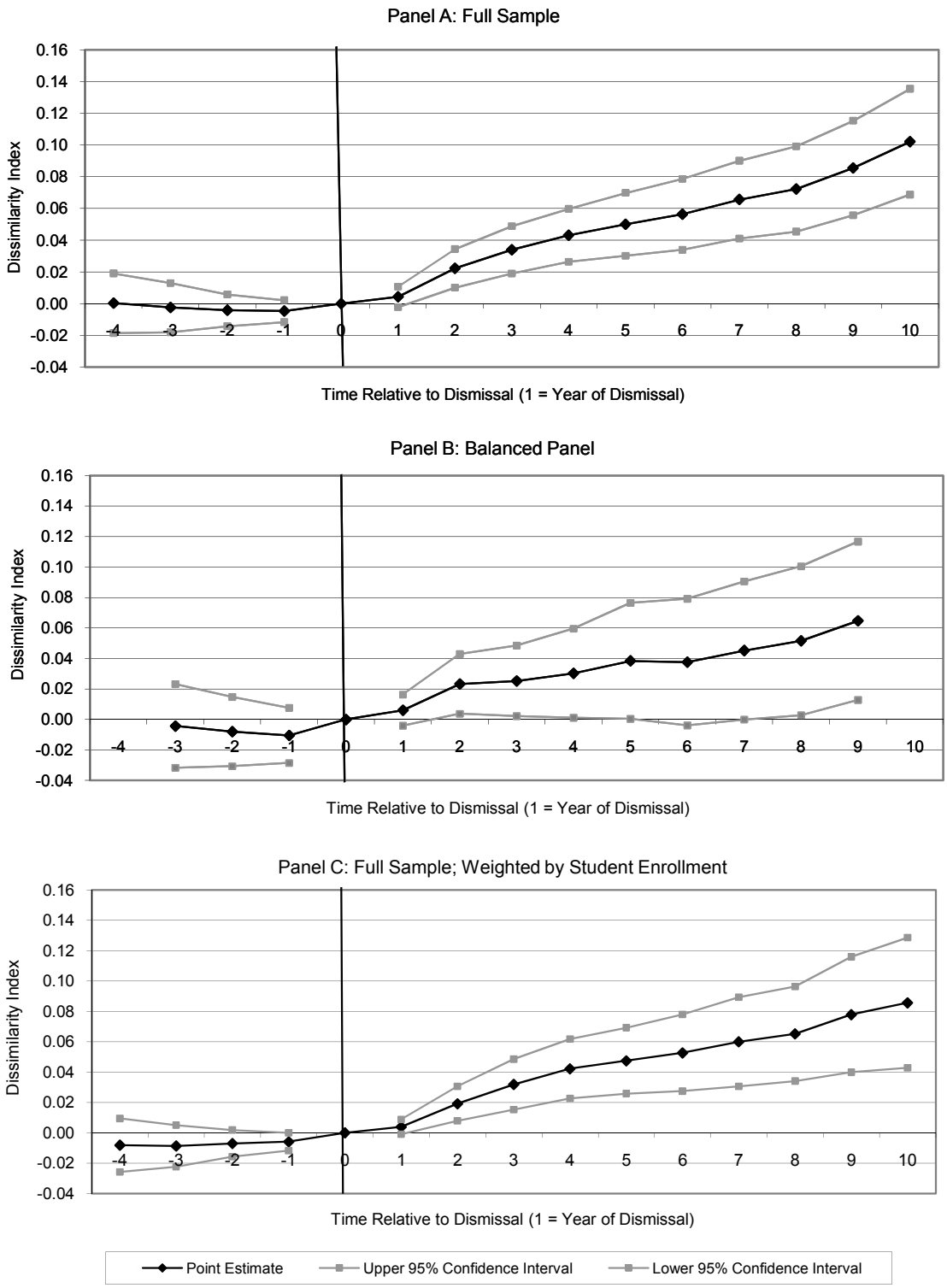
Note. The figure displays the number of dismissals of court-ordered desegregation plans by state from 1991 to 2007. The numbers within the states are the number of school districts in the state that appear in the Rossell and Armor survey data and were under a court-ordered desegregation plan in 1991.

Figure 3: Segregation Trends



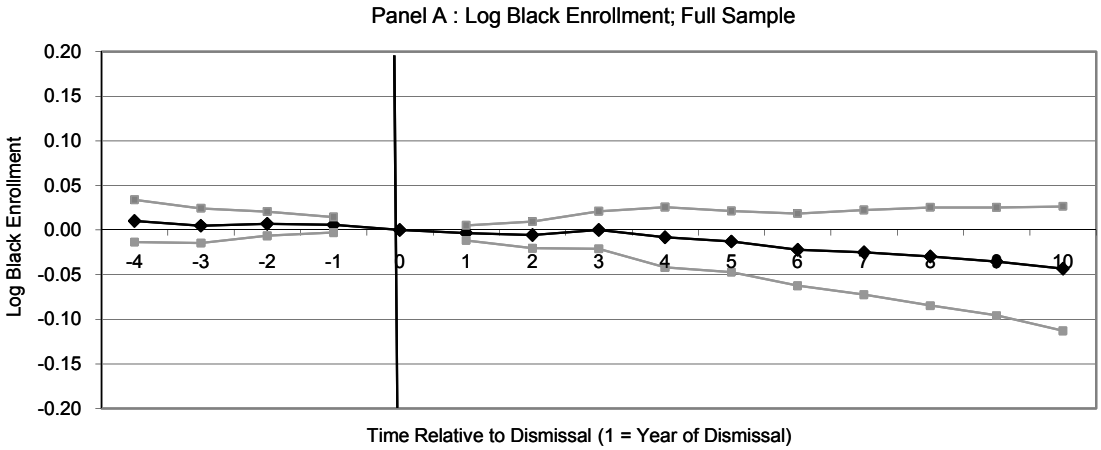
Note. The plots are means for the relevant groups. The data is from the CCD annual panel. Dismissed refers to those districts under court-order in 1991 and dismissed from their desegregation plan between 1991 and 2006. Not dismissed refers to those districts under court-order in 1991 and not dismissed from their plans between 1991 and 2006. 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 and which had enrollment greater than 10,000 in the first year they were observed in the CCD. The number of school districts in each group, with the number excluded due to the balance panel requirement in parentheses, is: dismissed 37 (11), not dismissed 43 (9), and not under order 167 (49).

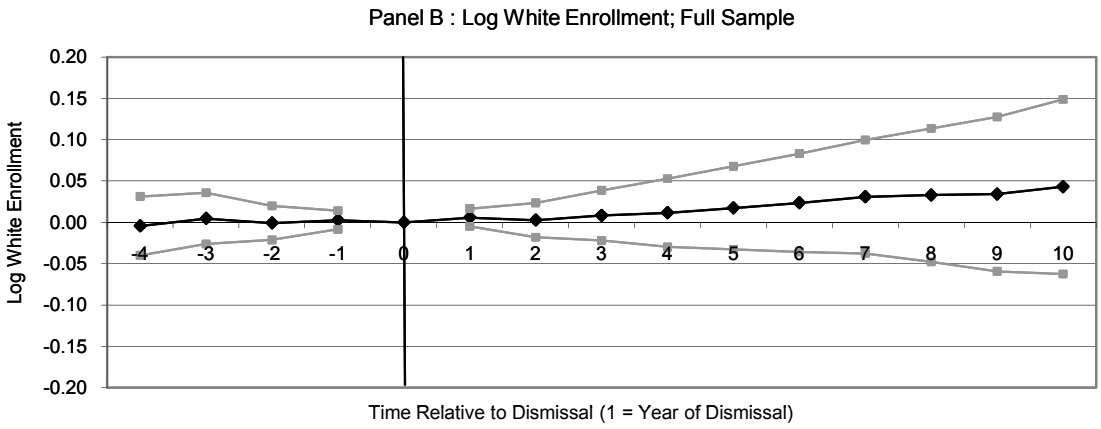
Figure 4: Effect of Dismissal on Dissimilarity Index

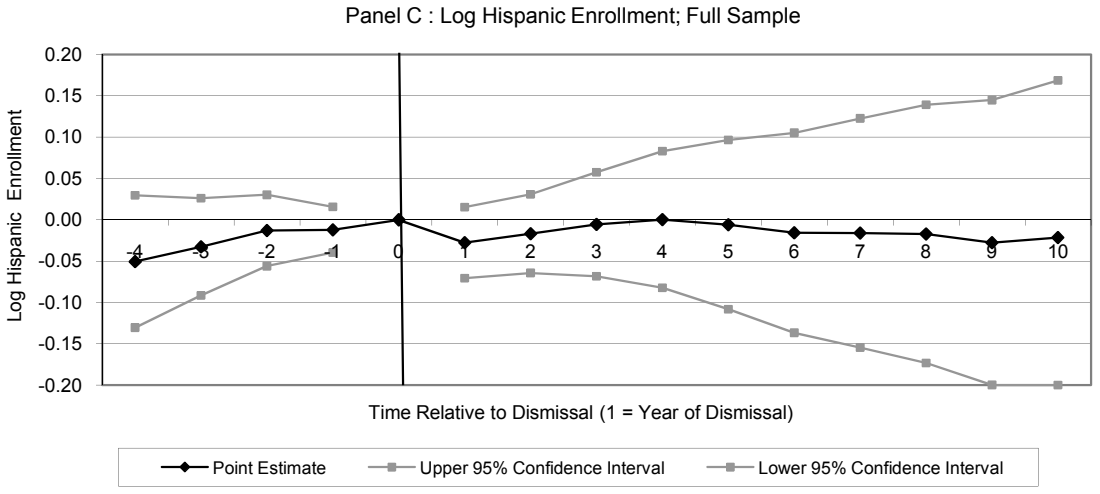


Note. The figures display the β vector coefficient estimates and associated confidence intervals from equation (4). The confidence intervals are constructed using standard errors clustered by school district. The dependent variable is the dissimilarity index (obtained from the annual CCD panel). The estimation sample is given in the panel title. The sample is restricted to districts with enrollment greater than 10,000 in the first year the district is observed in the sample in all panels. The sample size is 1754 in panels A and C and 944 in panel B. Coefficient estimates for the complete set of covariates available from the author upon request.

Figure 5: Log Enrollment

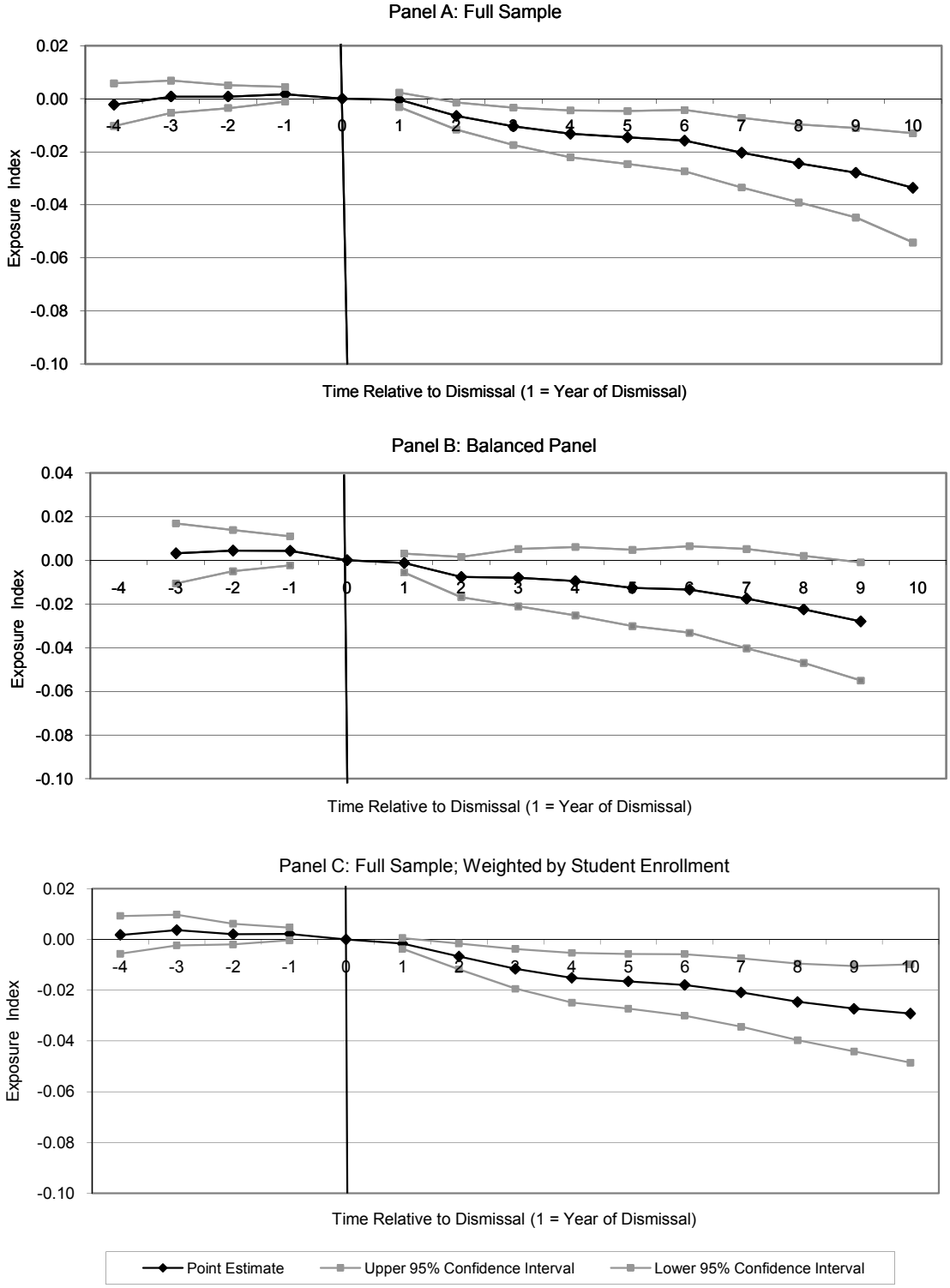






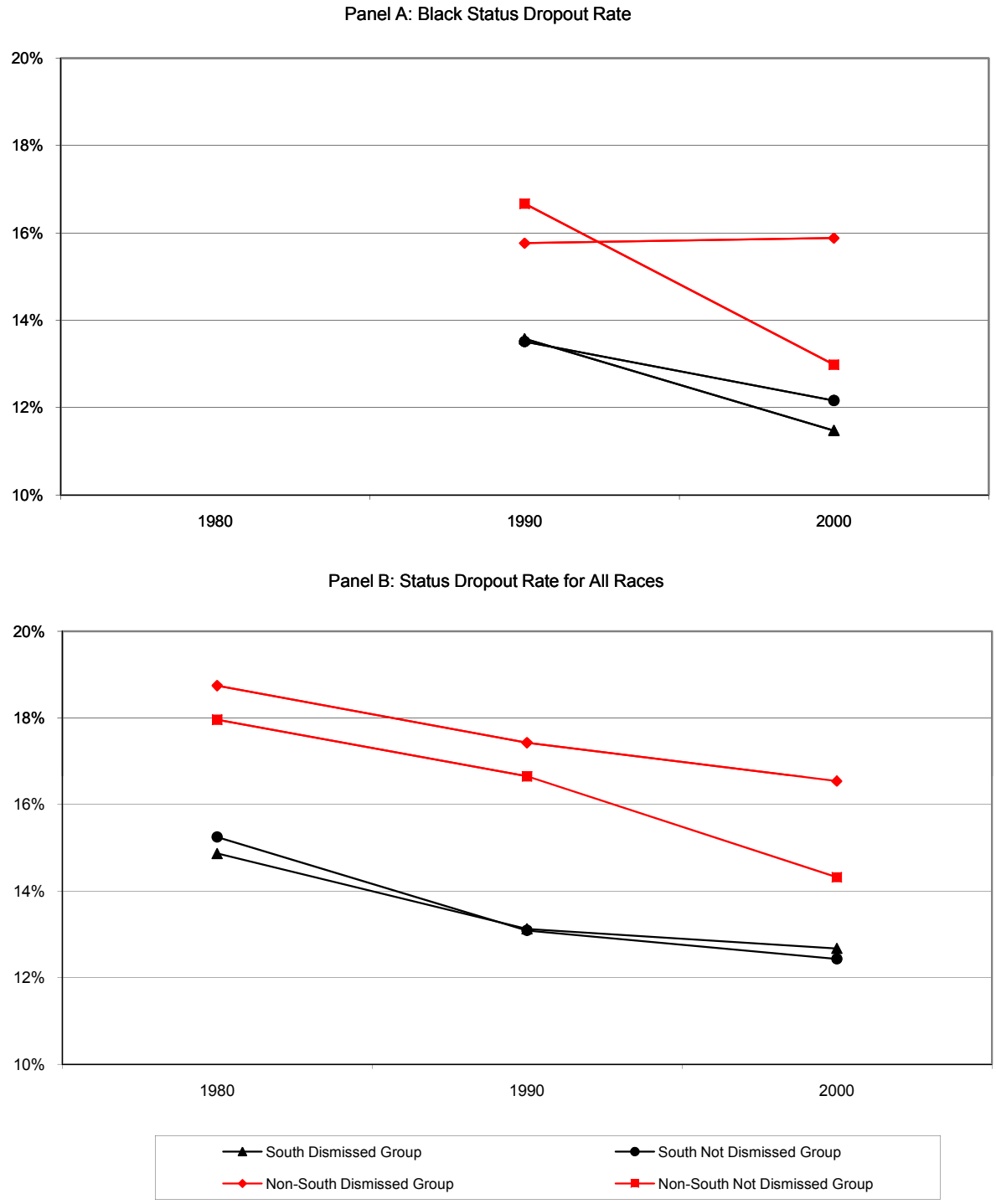
Note. The figures display the β vector coefficient estimates and associated confidence intervals from equation (4). The confidence intervals are constructed using standard errors clustered by school district. The dependent variable is the log enrollment of the race given in the panel title (obtained from the annual CCD panel). The sample is restricted to districts with enrollment greater than 10,000 in the first year the district is observed in the sample in all panels. The sample size is 1754 in panels A and B and 1749 in Panel C. Coefficient estimates for the complete set of covariates available from the author upon request.

Figure 6: Effect of Dismissal on Exposure Index



Note. The figures display the β vector coefficient estimates and associated confidence intervals from equation (4). The confidence intervals are constructed using standard errors clustered by school district. The dependent variable is the exposure index (obtained from the annual CCD panel). The estimation sample is given in the panel title. The sample is restricted to districts with enrollment greater than 10,000 in the first year the district is observed in the sample in all panels. The sample size is 1754 in panels A and C and 944 in panel B. Coefficient estimates for the complete set of covariates available from the author upon request.

Figure 7: Trends in Status Dropout Rate



Note. The chart displays the mean school district status dropout rate by treatment and control group for the South and non-South. 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. In both panels the sample is restricted to the set of districts under court-order in 1991 for which enrollment exceeded 10,000 in the first year the district is observed in the CCD. In Panel A, the sample is restricted to the set of districts for which non-missing observations exist for the two years displayed. In Panel B, the sample is restricted to the set of districts for which non-missing observations exist for the three years displayed. The means are weighted by the number of 16 - 19 year-old blacks residing in the district. These are the weights used in the black status dropout rate regressions appearing on Table 3.

Table 1
1990 School District Characteristics

	Under Court-Order as of 1991			Not Under Court-Order as of 1991
	Dismissed as of 2006			
	Dismissed 1991 - 2000	Dismissed 2001-2006	Not Dismissed as of 2006	
	(1)	(2)	(3)	(4)
	A. School District Characteristics			
Dissimilarity Index	0.35	0.37	0.38	0.36
	(0.13)	(0.14)	(0.20)	(0.16)
25% percentile	0.23	0.28	0.22	0.23
50% percentile	0.36	0.35	0.33	0.33
75% percentile	0.45	0.43	0.50	0.44
Exposure Index	0.42	0.38*	0.46	0.53
	(0.15)	(0.18)	(0.23)	(0.28)
25% percentile	0.30	0.21	0.33	0.30
50% percentile	0.39	0.36	0.49	0.56
75% percentile	0.59	0.50	0.64	0.74
Enrollment	52409	68932*	56634	35845
	(37090)	(62843)	(104708)	(71038)
% Black	0.34*	0.36*	0.34*	0.17
	(0.18)	(0.18)	(0.21)	(0.20)
% Hispanic	0.12	0.13	0.010*	0.18
	(0.17)	(0.16)	(0.15)	(0.23)
% Receiving Free Lunch	0.43*	0.40*	0.40*	0.31
	(0.13)	(0.14)	(0.17)	(0.19)
South Region	0.57	0.71*	0.63*	0.36
	(0.51)	(0.46)	(0.49)	(0.48)
Serves a Central City	0.71*	0.57	0.58	0.45
	(0.46)	(0.51)	(0.50)	(0.50)
	B. School District Community Characteristics			
Average Black Household Income	33264*	34163*	33472*	43144
	(6414)	(8237)	(7900)	(15604)
% Black Children Beneath Poverty Line	0.43*	0.41*	0.41*	0.32
	(0.08)	(0.11)	(0.14)	(0.16)
Average White Household Income	60329	63462	60656	64936
	(14018)	(13793)	(14507)	(17947)
% White Children Beneath Poverty Line	0.13	0.12	0.12	0.11
	(0.07)	(0.07)	(0.05)	(0.07)
% White Mothers w/o High School Degree	0.15	0.14	0.14	0.13
	(0.07)	(0.05)	(0.06)	(0.10)
% White Mothers w/ College Degree	0.29	0.29	0.30	0.32
	(0.12)	(0.11)	(0.13)	0.14
Number of Observations	21	21	48	195

Note. The cells are 1990 school district means unless otherwise stated. Standard deviations are in parentheses. Household refers to households with children. All columns restrict the sample to districts with non-missing values for the dissimilarity and exposure indices in 1990. The sample is restricted to those school districts with enrollment greater than 10,000 in the first year they are observed in the CCD (the same restriction applied for the empirical specifications which include the control group — i.e. those districts under court-order in 1991 and not dismissed as of 2006). "*" signifies that the mean in column (1) - (3) is statistically distinguishable from the mean in column (4). None of the means within columns (1) - (3) are statistically distinguishable from one another.

Table 2
1990 School District Community Characteristics of Black Students

	Full Sample		Non-Southern Districts		Southern Districts	
	Dismissed 1991 - 1999	Not Dismissed[a]	Dismissed 1991 - 1999	Not Dismissed[a]	Dismissed 1991 - 1999	Not Dismissed[a]
	(1)	(2)	(3)	(4)	(5)	(6)
			A. Outcome Variables			
Black Status Dropout Rate	0.15	0.14	0.15	0.17	0.14	0.13
	(0.04)	(0.05)	(0.04)	(0.06)	(0.04)	(0.04)
25% percentile	0.12	0.11	0.12	0.13	0.11	0.10
50% percentile	0.14	0.14	0.15	0.15	0.13	0.13
75% percentile	0.16	0.17	0.18	0.18	0.16	0.16
Black Private School Attendance	0.06	0.06	0.07	0.07	0.06	0.05
	(0.02)	(0.03)	(0.01)	(0.03)	(0.02)	(0.03)
25% percentile	0.05	0.04	0.06	0.05	0.05	0.03
50% percentile	0.07	0.05	0.07	0.07	0.06	0.05
75% percentile	0.07	0.07	0.08	0.09	0.07	0.06
			B. Other Variables			
Exposure Index	0.37	0.45	0.36	0.39	0.38	0.47
	(0.14)	(0.21)	(0.12)	(0.22)	(0.16)	(0.20)
25% percentile	0.29	0.29	0.30	0.21	0.29	0.32
50% percentile	0.34	0.47	0.34	0.37	0.33	0.49
75% percentile	0.45	0.62	0.44	0.56	0.55	0.65
% Black in Schools < 10% White	0.13	0.16	0.06	0.20	0.19	0.13
	(0.16)	(0.25)	(0.07)	(0.27)	(0.2)	(0.23)
Dissimilarity Index	0.37	0.37	0.35	0.35	0.39	0.38
	(0.14)	(0.17)	(0.13)	(0.19)	(0.16)	(0.16)
Percent White	0.45	0.53	0.40	0.44	0.48	0.57
	(0.15)	(0.22)	(0.14)	(0.22)	(0.15)	(0.21)
Percent Receiving Free Lunch	0.45	0.39	0.51	0.46	0.41	0.36
	(0.14)	(0.16)	(0.14)	(0.16)	(0.13)	(0.15)
% Total HHs in Poverty[b]	0.09	0.09	0.10	0.09	0.08	0.09
	(0.04)	(0.04)	(0.04)	(0.03)	(0.04)	(0.05)
HHs Unemploy. Rate[b]	0.06	0.07	0.07	0.07	0.06	0.07
	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)
Mother Not High Sch. Grad.	0.25	0.26	0.23	0.25	0.26	0.27
	(0.10)	(0.10)	(0.09)	(0.10)	(0.11)	(0.10)
Mother College Grad.	0.13	0.13	0.12	0.13	0.13	0.12
	(0.06)	(0.06)	(0.06)	(0.07)	(0.06)	(0.06)
Black Household Income[c]	34371	34013	33108	36065	35423	33007
	(6837)	(8924)	(6316)	(8979)	(7345)	(8810)
Number of Observations	22	76	10	25	12	51

Note. The cells are 1990 school district community means for black students from the SDDB except for the first five variables in Panel B which are school district-level variables from the first year that are observed in the CCD. Standard deviations are in parentheses. The sample is restricted to districts under court-order in 1991 with enrollment greater than 10,000 in the first year they are observed in the CCD. The construction of the variables is described in the text and section 1 of the Web Appendix. [a] Includes districts dismissed after 1999. [b] Denotes a district level variable - i.e. it does not vary by race. [c] Household income is expressed in 2001 dollars and refers to households with children.

Table 3
Effect of Desegregation Order Dismissal on Status Dropout Rate

	Black						White	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	A. No Heterogeneity by Region							
Δ Years Since Dismissal	0.0034	0.0037	0.0032	0.0005	*	*	-0.0013	-0.0009
	(0.0019)*	(0.0021)*	(0.0021)	(0.0028)	*	*	(0.0013)	(0.0012)
	B. Heterogeneity by Region							
Δ Years Since Dismissal * Non-South	0.0085	0.0087	0.0081	0.0064	0.0049		0.0010	0.0010
	(0.0015)***	(0.0016)***	(0.0016)***	(0.0022)***	(0.0027)*		(0.0013)	(0.0015)
Δ Years Since Dismissal * South	-0.0006	-0.0001	-0.0003	-0.0040	0.0017		-0.0026	-0.0020
	(0.0019)	(0.0021)	(0.0021)	(0.0025)	(0.0032)		(0.0012)**	(0.0013)
Δ Indicator Dismissal * Non-South					0.0281	0.0498		
					(0.0177)	(0.0175)***		
Δ Indicator Dismissal * South					-0.0118	-0.0046		
					(0.0179)	(0.0128)		
Δ Placebo Ind. Dismissal * Non-South						0.0108		
						(0.0180)		
Δ Placebo Ind. Dismissal * South						-0.0014		
						(0.0113)		
Observations	98	98	97	98	98	98	98	98
Restricted to Enrollment > 10,000	X	X	X	X	X	X	X	X
Region * 2000	X	X	X		X	X	X	X
State * 2000				X				
Base Demographics[a] * 2000	X	X	X		X	X	X	X
Time-Varying Covariates[b]		X	X					X
Lagged Δ Dropout Rate for All Races			X					
Restricted to Districts Dismissed Post 1990								

Note. The table displays coefficient estimates from equation (5). Standard errors clustered by district in parentheses. All columns are weighted by cell size. The dependent variable is the change in mean status dropout rate for 16 - 19 year-olds of the race given in the column heading (obtained from the two-period SDDB panel). South refers to the South Census region. [B] Base demographics, which are time-invariant, include both student and district level variables. The district level covariates, which are measured for all races and obtained from the CCD in the first year they are available, are a central city indicator variable, percent of enrollment which is white, percent of enrollment which is hispanic, number of students enrolled, number of students enrolled squared, number of students enrolled cubed, percent of enrollment receiving a free or reduced price lunch, percent of enrollment receiving a free or reduced price lunch squared, percent of black students in the non-south in a school with less than 10% white enrollment, and percent of black students in the south in a school with less than 10% white enrollment. The student level covariates, i.e. means calculated over the population of children of the race given in the panel heading, are obtained from the 1990 SDDB and include percent of mothers' without a high school degree, percent of mothers with a four-year college degree, percent of children below the poverty line, the household income of households with children, percent of children with a parent who is foreign born, and percent of children born out of state. [b] Time-varying covariates are the same as the student-level base demographics listed above. Coefficient estimates for the complete set of covariates available from the author upon request. * significant at 10%; ** significant at 5%; *** significant at 1%

Table 4
Effect of Desegregation Order Dismissal on Private School Attendance Rate

	Black					White	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
A. No Heterogeneity by Region							
Δ Years Since Dismissal	0.0012	0.0010	0.0019	*	*	0.0002	-0.0005
	(0.0010)	(0.0011)	(0.0011)*	*	*	(0.0016)	(0.0016)
B. Heterogeneity by Region							
Δ Years Since Dismissal * Non-South	0.0024	0.0023	0.0034	0.0040		0.0011	-0.0014
	(0.0011)**	(0.0012)*	(0.0015)**	(0.0013)***		(0.0027)	(0.0029)
Δ Years Since Dismissal * South	0.0002	-0.0001	0.0008	0.0010		-0.0002	-0.0001
	(0.0013)	(0.0014)	(0.0013)	(0.0028)		(0.0019)	(0.0019)
Δ Indicator Dismissal * Non-South				-0.0122	-0.0025		
				(0.0107)	(0.0082)		
Δ Indicator Dismissal * South				-0.0046	-0.0036		
				(0.0118)	(0.0065)		
Δ Placebo Ind. Dismissal * Non-South					-0.0152		
					(0.0144)		
Δ Placebo Ind. Dismissal * South					-0.0039		
					(0.0056)		
Observations	98	98	97	98	98	98	98
Restricted to Enrollment > 10,000	X	X	X	X	X	X	X
Region * 2000	X	X		X	X	X	X
State * 2000			X				
Base Demographics[a] * 2000	X	X		X	X	X	X
Time-Varying Covariates[b]		X					X
Restricted to Districts Dismissed Post 1990							

Note. The table displays coefficient estimates from equation (5). Standard errors clustered by district in parentheses. All columns are weighted by cell size. The dependent variable is the change in the private school attendance rate of the race given in the column heading. (obtained from the two-period SDDB panel). South refers to the South Census region. [a] Base demographics, which are time-invariant, include both student and district level variables. The district level covariates, which are measured for all races and obtained from the CCD in the first year they are available, are a central city indicator variable, percent of enrollment which is white, percent of enrollment which is hispanic, number of students enrolled, number of students enrolled squared, number of students enrolled cubed , percent of enrollment receiving a free or reduced price lunch, percent of enrollment receiving a free or reduced price lunch squared, percent of black students in the non-south in a school with less than 10% white enrollment, and percent of black students in the south in a school with less than 10% white enrollment . The student level covariates, i.e. means calculated over the population of children of the race given in the panel heading, are obtained from the 1990 SDDB and include percent of mothers' without a high school degree, percent of mothers' with a four-year college degree, percent of children below the poverty line, the household income of households with children, percent of children with a parent who is foreign born and percent of children born out of state. [b] Time-varying covariates are the same as the student-level base demographics listed above. Coefficient estimates for the complete set of covariates available from the author upon request. * significant at 10%; ** significant at 5%; *** significant at 1%

Table 5
Effect of Desegregation Order Dismissal on Black Migration and Black Status Dropout Rate

	(1)	(2)	(3)
	A. Census Micro Data (PUMA Group): Non-South		
	Residing in Different PUMA Than 5 years Ago		
Years Since Dismissal	0.0028	0.0031	*
	(0.0035)	(0.0030)	*
	Dropout Status		
Years Since Dismissal	0.0059	0.0051	*
	(0.0016)***	(0.0016)***	*
	Dropout Status		
Years Since Dismissal	0.0059	0.0051	*
	(0.0016)***	(0.0016)***	*
Different PUMA than 5 Years Ago	0.0137	0.0149	*
	(0.0074)*	(0.0079)*	*
Observations	33,121	31,126	*
PUMA Groups	30	30	*
	B. Census Data Aggregated to MSA		
	Δ Status Dropout Rate		
Δ Years Since Dismissal * Non-South	0.0059	0.0060	0.0062
	(0.0013)***	(0.0013)***	(0.0020)***
Δ Years Since Dismissal * South	0.0007	-0.0001	0.0006
	(0.0014)	(0.0017)	(0.0025)
Observations	80	80	160
Restricted to Enrollment > 10,000	X	X	X
Region * 2000	X	X	X
Base Demographics[a] * 2000	X	X	
Time-Varying Covariates[b]		X	
MSA Linear Trends			X

Note. The table displays coefficient estimates from equation (5). Standard errors clustered by PUMA group (Panel A) or MSA (Panel B) in parentheses. The dependent variable is given in the panel subheadings (e.g. "Dropout Status"). The dependent variables are obtained from the decennial census. The data is a two-period panel in all cases expect for column (3) of Panel B where it is a three-period panel. All columns are weighted by cell size in Panel B. The unit of observation is the individual in Panel A and the MSA in Panel B. In Panel A, the sample is restricted to black 16-19 year-olds located outside of the South Census region. In Panel B the black status dropout is calculated over the population of black 16-19 year-olds. [a] Base demographics, which are time-invariant, include district level variables in Panel A and both district and student level variables in Panel B. The district level covariates, which are measured for all races and obtained from the CCD in the first year they are available, are a central city indicator variable, percent of enrollment which is white, percent of enrollment which is hispanic, number of students enrolled, number of students enrolled squared, number of students enrolled cubbed, percent of enrollment receiving a free or reduced price lunch, percent of enrollment receiving a free or reduced price lunch squared, percent of black students in a school with less than 10% white enrollment in the south, and percent of black students in a school with less than 10% white enrollment in the non-south. The student level covariates in Panel B are measured at the level of the MSA, pertain to the black population of the MSA and include percent of those over age 25 without a high school degree, percent of those over age 25 with a four-year college degree, percent of families with children below the poverty line, the household income of households with children, percent of population born out of state, and percent of population foreign born. [b] Time-varying covariates are measured at the level of the individual in Panel A and include indicators for the mother not having a high school degree, mother holding a four-year college degree, being below the poverty line, being born out of state and a continuous variable for the individual's household income. Time-varying covariates are the same as the student-level base demographics in Panel B. Coefficient estimates for the complete set of covariates available from the author upon request. * significant at 10%; ** significant at 5%; *** significant at 1%

Table 6
Effect of Desegregation Order Dismissal on Racial Segregation by South and non-South

	Full Sample		Restricted to Years <=1999	
	Non-South	South	Non-South	South
	(1)	(2)	(3)	(4)
	A. Δ Dissimilarity Index			
Δ Years Since Dismissal	0.0085	0.0093	0.0076	0.0086
	(0.0030)***	(0.0018)***	(0.0049)	(0.0027)***
	B. Δ Exposure Index			
Δ Years Since Dismissal	-0.0007	-0.0039	-0.0031	-0.0028
	(0.0015)	(0.0012)***	(0.0020)	(0.0014)**
	C. Δ Percent Black Students in School with < 10% White Enrollment			
Δ Years Since Dismissal	0.0159	0.0031	0.0228	0.0001
	(0.0070)**	(0.0040)	(0.0114)**	(0.0072)
	D. Δ Log of Black Enrollment			
Δ Years Since Dismissal	-0.0090	0.0003	0.0034	0.0040
	(0.0037)**	(0.0038)	(0.0056)	(0.0033)
	E. Δ Log of White Enrollment			
Δ Years Since Dismissal	-0.0010	0.0112	-0.0008	0.0115
	(0.0065)	(0.0056)**	(0.0089)	(0.0082)
Observations	646	1108	405	680
Number of School Districts	36	64	36	64
Region-Year Effects	X	X	X	X
Restricted to Enrollment > 10,000	X	X	X	X
Base Demographics * Year Effects	X	X	X	X

Note. The table displays coefficient estimates from equation (5). The dependent variable is given in the panel headings (all are obtained from the annual CCD panel). Each cell presents the results of a separate regression. Standard errors clustered by district in parentheses. Base demographic characteristics, which are time-invariant, include a central city indicator variable, percent of enrollment which is white, percent of enrollment which is hispanic, number of students enrolled, number of students enrolled squared, number of students enrolled cubbed, percent of enrollment receiving a free or reduced price lunch and percent of enrollment receiving a free or reduced price lunch squared. * significant at 10%; ** significant at 5%; *** significant at 1%

Table 7
Effect of Desegregation Order Dismissal on School Finances

	Δ Per-Pupil Capital Expenditures				Δ Per-Pupil Current Expenditures	
	Non-South		South		Non-South	South
	(1)	(2)	(3)	(4)	(5)	(6)
Δ pre(-2)	0.08		-0.06		0.01	-0.06
	(0.16)		(0.08)		(0.14)	(0.06)
Δ pre(-1)	0.06		0.00		0.11	-0.09
	(0.16)		(0.10)		(0.20)	(0.06)
Δ pre(0)	-0.03	-0.09	0.17	0.17	0.13	-0.03
	(0.18)	(0.11)	(0.11)	(0.07)***	(0.29)	(0.06)
Δ post(1)	0.17	0.11	0.34	0.34	0.11	-0.01
	(0.16)	(0.17)	(0.14)**	(0.14)**	(0.27)	(0.08)
Δ post(2)	0.40	0.33	0.26	0.26	0.40	0.00
	(0.20)*	(0.27)	(0.21)	(0.20)	(0.31)	(0.11)
Δ post(3)	0.31	0.24	0.21	0.21	0.40	-0.08
	(0.24)	(0.33)	(0.18)	(0.17)	(0.33)	(0.12)
Δ post(>=4)	0.32	0.25	0.17	0.17	0.50	-0.09
	(0.29)	(0.38)	(0.21)	(0.19)	(0.41)	(0.13)
Observations	524	524	916	916	524	916
Number of School Districts	36	36	61	61	36	61
Dep. Var. Mean	0.86	0.86	0.77	0.77	8.41	6.40
Dep. Var. S.D.	0.72	0.72	0.61	0.61	1.90	1.35
Region-Year Effects	X	X	X	X	X	X
Base Demographics[a] * 2000	X	X	X	X	X	X
Restricted to Enrollment > 10,000	X	X	X	X	X	X

Note. The table displays coefficient estimates from a truncated version of equation (4). Standard errors clustered by district are presented in parentheses. The dependent variable is identified in the column headings and is expressed in thousands of 2001 dollars. The depenent variables are obtained from the *Public Elementary-Secondary Education Finance Data* produced by the Census Bureau. The date range of the sample is 1992 to 2007. [a] See the note on Table 3. * significant at 10%; ** significant at 5%; *** significant at 1%

Web Appendix to "The End of Court-Ordered Desegregation"

By BYRON LUTZ*

This document contains the unpublished web appendix to "The End of Court-Ordered Desegregation."

I. Data Appendix

A. 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 that 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. District-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 percent 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 did not resume until 2005; all Tennessee districts therefore drop out of the sample from

* Federal Reserve Board, 20th & C Sts., NW, Stop #83, Washington DC 20551; Byron.F.Lutz@frb.gov. This research was supported by grants from the American Educational Research Association and the National Science Foundation. The views expressed are those of the author and not necessarily those of the Federal Reserve Board or its staff. I owe thanks to several individuals for assistance with the data used in the paper. Daniel Feenberg generated the private school iterations of the 1990 School District Databook. Jacinta Ma of the Harvard Civil Rights Project provided data on the date of desegregation order dismissal. Christine Rossell and David Armor generously allowed me to use their survey data. Margo Schlanger provided the methodology used for the electronic legal searches. I thank Samuel Brown, Paul Eliason, Brian McGuire, Nirupama Rao, Shoshana Schwartz, and Daniel Stenberg for excellent research assistance. I thank the following individuals for useful comments: Daron Acemoglu, Josh Angrist, David Autor, Jane Dokko, Michael Greenstone, Jon Gruber, Jon Guryan, Chris Hansen, Bill Kerr, Ashley Lester, Adam Looney, Raven Molloy, Nancy Qian, Sarah Reber, Hui Shan, Dan Sichel, John Yun, several anonymous referees, and participants in the MIT Labor and Public Finance Lunches, the Spring 2005 NBER Children's Program Meeting, and the University of Chicago GSB Applied Economics Workshop.

1999 through 2004. The sample partially overlaps with the sample produced by Welch and Light (1987) that has been widely used in school desegregation research (e.g. Guryan 2004, Reber 2005, Weiner, Lutz and Ludwig 2009). Of the 571 districts in the Rossell and Armor sample, 106 also appear in the Welch and Light sample. There is a greater degree of overlap for the 130 districts in the Rossell and Armor sample that were under a court-ordered desegregation plan as of 1991 (the districts used in estimation and listed on Web Appendix Table A1). 55, or about 40%, of these districts also appear in the Welch and Light sample.

B. Legal Variables

I construct two school district level variables based on the legal status of the school district in relation to court-ordered desegregation. 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. 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 (2004), a spreadsheet produced by the Harvard Civil Rights Project titled "List of School Districts Previously Under Desegregation Orders Dismissed between 1990 – 2004," 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 (2004) 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, issued by Federal District and Appeals Courts, and available via Lexis-Nexis and Westlaw, often contain extensive information on desegregation cases. In addition to being used for determining the date of dismissal, these opinions were examined

for any mention of trends in minority educational outcomes such as dropout rates. To the extent that such trends are mentioned, improvement in the outcome is typically cited. Thus, the opinions provide no evidence that judges tend to dismiss districts experiencing worsening outcomes for blacks. Furthermore, the opinions contain no mention of future or expected trends in segregation or educational outcomes.

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. 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 potentially 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 that 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. (Schlanger (2003) provides an example of using PACER for legal research.) 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.

7) A report published by the United States Commission on Civil Rights (2007) contains a data appendix with information on school desegregation for all school districts in Alabama, Florida, Georgia, Louisiana, Mississippi, North Carolina, and South Carolina. The appendix includes information on whether districts are currently under a court-order, whether they were ever under a court-order, the date they were placed under court-order, and the date they were declared unitary.

8) The Florida and Tennessee Advisory Committees to the United States Commission on Civil Rights both issued reports in 2008 on the state of court-ordered desegregation for all school districts in their states. Information includes whether or not districts were ever subject to

desegregation, the date of their desegregation, whether or not they have obtained unitary status, and the date of unitary status.

9) 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.

10) Personal communication with school district officials were used in cases when all of the above sources failed to provide sufficient information.

C. School District Data Book

The School District Data Book (SDDB) is a public school district level tabulation of the U.S. Census that focuses on children (the 2000 version is referred to as the School District Tabulation – STP2). A child is included in a district's 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. 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. The following covariates from the SDDB are averaged over the set of parents with children: mothers' 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 IV.A, and the SDDB data, used in section IV.B, bears mention. The CCD maintains Hispanic as a separate racial category along with white, black, Asian and native American. The SDDB, however, treats Hispanic background as an aspect of ethnicity. An individual of a given race, for instance an individual whose racial category is white, can indicate that she is, or is not, ethnically Hispanic. For the purposes of section IV.B, 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.

D. Census Micro Data (PUMA Groups)

Individual-level micro census data for 1990 and 2000 are obtained from the IPUMS (Integrated Public Use Microdata Series; Ruggles, et. al 2009) and then mapped into "PUMA groups" as described in the text. A PUMA group is the smallest geographic area that can be identified in both 1990 and 2000 and contains the entirety of the school district. The analysis of this data is restricted to areas located outside of the South Census region and to those districts in the Rossell

and Armor sample under court-order in 1991 and with a minimum of 10,000 students. Five non-Southern school districts in the sample could not be mapped into PUMA groups because the geographic area required for longitudinally consistency is too large (in most of these cases the PUMA group would have to cover the entire state in order to achieve geographic consistency across the two censuses). This problem is significantly more severe for Southern districts and is one reason (brevity being the other) that the analysis on Panel A of Table 5 is restricted to the non-South. Five of the constructed PUMA groups contain two school districts in the sample and a single PUMA group contains three school districts. In these cases the PUMA group is assigned the earliest dismissal date of the two (three) school districts and the base period school district characteristics are constructed as weighted averages, with the weights based on 1990 enrollment. On average, the black enrollment of the sample school district(s) is equal to 63.1 percent of the size of the black population enrolled in public school within the PUMA group in 1990. As in the SDDB data, black refers to both Hispanic and non-Hispanic blacks.

E. Census Data Aggregated to MSA

Census data aggregated to the level of the county (Summary Tape Files 1 - 4) are obtained from the National Historical Geographic Information System (Minnesota Population Center 2004). These data are then combined to form geographically consistent MSAs in 1980, 1990 and 2000 using PMSA definitions as of 1999. School districts are then mapped into MSAs based on their location. New England MSAs cross county borders. This causes a problem in the case of two school districts in Connecticut (Bridgeport and Waterbury). These districts are located in distinct MSAs, but both MSAs contain portions of New Haven County. The two MSAs are combined into a single, new MSA. The analysis of this data is restricted to those districts in the Rossell and Armor sample under court-order in 1991 and with a minimum of 10,000 students. The sample contains 80 MSA, 32 of them in the non-South. For the sample as a whole, 13 MSAs contains 2 school districts in the sample, and 2 MSAs contain 3 districts. In the non-South, 4 MSAs contain 2 school districts. In these cases the MSA is assigned the earliest dismissal date of the school districts within the MSA. On average, the black enrollment of the sample school district(s) is equal to 69.5 percent of the size of the black population enrolled in public school within the entire MSA in 1990. In the non-South, the comparable figure is 62.4 percent. As in the SDDB data, black refers to both Hispanic and non-Hispanic blacks.

II. Segregation Results from the Linear Model

This section presents the results of estimating the linear model–equation (5)–with the segregation measures as the outcome variable. The linear model has three primary advantages relative to the more flexible event study model–equation (4)–used to produce the primary segregation outcome results reported in section IV.A. First the estimates produced are directly comparable to the dropout and private school attendance results in section IV.B because they are produced using the same empirical model. Second, the imposition of linearity yields significantly more precise estimates. Third, collapsing the treatment effect to a single point estimate permits the concise presentation of a number of robustness checks.

Web Appendix Table A2, Panel A, Column (1), presents the linear specification results for the dissimilarity index. β is estimated with considerable precision and indicates that each year after dismissal produces an increase in the dissimilarity index of 0.01. Ten years after dismissal, a district will have experienced an increase in the index of around 0.1 – the same result produced by the event study specification. The linear specification also produces results that are similar in magnitude to, but more precise than, those produced by the event study specification for the exposure index (column (1) of Panel C).

The standard specification in column (1) uses the set of districts under a court-ordered desegregation plan in 1991 and still under the plan as of 2006 as the control group. The specification is identified under the assumption that, conditional on the covariates, both *if* and *when* a district is dismissed is unrelated to trends in the outcome variable. The specification presented in column (2) relaxes this assumption by restricting the sample to only those districts dismissed after 1990 (i.e. the control group is dropped). The identifying assumption for this specification requires only that *when* a district is dismissed is unrelated to trends in the outcome variable. In addition, the X_i vector is dropped–the only control variables included are the region-year effects–and the sample is not restricted to those districts with enrollment greater than 10,000.[1] The results are extremely similar to those in Column (1).

Columns (3) - (6) present additional robustness checks (all of which, unlike column (2), include the control group). Column (3) excludes the vector of base period characteristics, X_i, column (4) includes districts with enrollment less than 10,000, column (5) includes district-specific linear time trends, column (6) conducts a very demanding robustness check by replacing the region-year effects with state-year effects. The results are generally robust to these permutations, although the exposure index results suffer a loss of precision in some cases and are smaller in magnitude when the Xi vector is dropped (column (3)) and when state-year effects are included (column (6)).

Columns (7) and (8) replace the black-white indices with equivalent nonwhite-white indices and Hispanic-white indices, respectively. The nonwhite-white results are similar to the black-white results. The Hispanic-white dissimilarity results in Panel A suggest that Hispanics experience an increase in segregation from whites, but the magnitude of the increase is equal to only about $\frac{1}{3}$ of the increase experienced by blacks. There is no evidence that Hispanics' exposure to whites is impacted by dismissal (Panel C).

Finally, Panels B and D present the results of weighting the observations by total enrollment (similar to the specifications shown in Panel C of Figures 4 and 6). In general, these results are similar to their unweighted counterparts, although the magnitude of the weighted dissimilarity index coefficients are somewhat less than the unweighted coefficients.

III. Migration Results

Column (1) of Web Appendix Table A3 examines the effect of dismissal on the log of 16 - 19 year-olds blacks residing in the district. There is no evidence that dismissal induces net in

[1]The enrollment restriction is motivated by a desire to produce a treatment and control group with similar observables. There is no control group when the "only dismissed districts" sample is used and correspondingly no need for the restriction.

or out migration.[2] The absence of net migration does not rule out the possibility of other forms of migration, however. Dismissal may induce neighborhood churn – migration that does not alter the number of residents but that does alter the demographic composition of the area. The remaining columns examine this possibility, but fail to find evidence to support it. Although there is some indication that the percent of black mothers with a college degree increases outside of the South, the estimate is only marginally precise. Furthermore, with regards to the possibility of migration explaining the dropout rate results, it is unlikely that an increase in parental education would induce an increase in the dropout rate.

IV. References Not Appearing in Reference Section of Published Paper

Florida Advisory Committee to the United States Commission on Civil Rights, 2008. "Desegregation of Public School Districts in Florida".

Rossell, Christine H., May 2003. “The Desegregation Efficiency of Magnet Schools,” Urban Affairs Review, vol. 38.

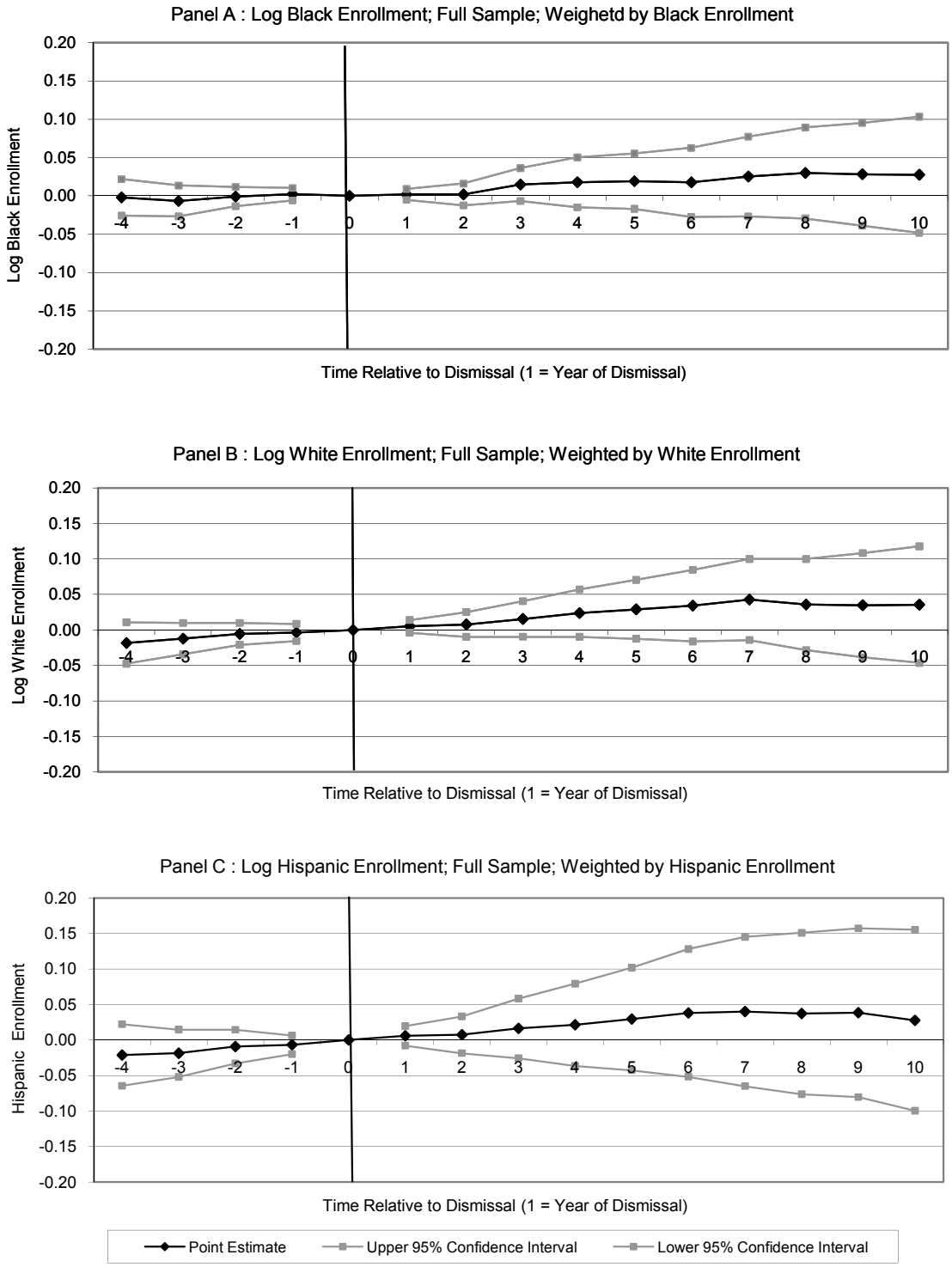
Schlanger, Margo, April 2003.“Inmate Litigation,” Harvard Law Review, vol. 116.

Tennessee Advisory Committee to the United States Commission on Civil Rights, 2008. "School Desegregation in Tennessee".

United States Commission on Civil Rights, September 2007. "Becoming Less Separate?" School Desegregation, Justice Department Enforcement and the Pursuit of Unitary Status".

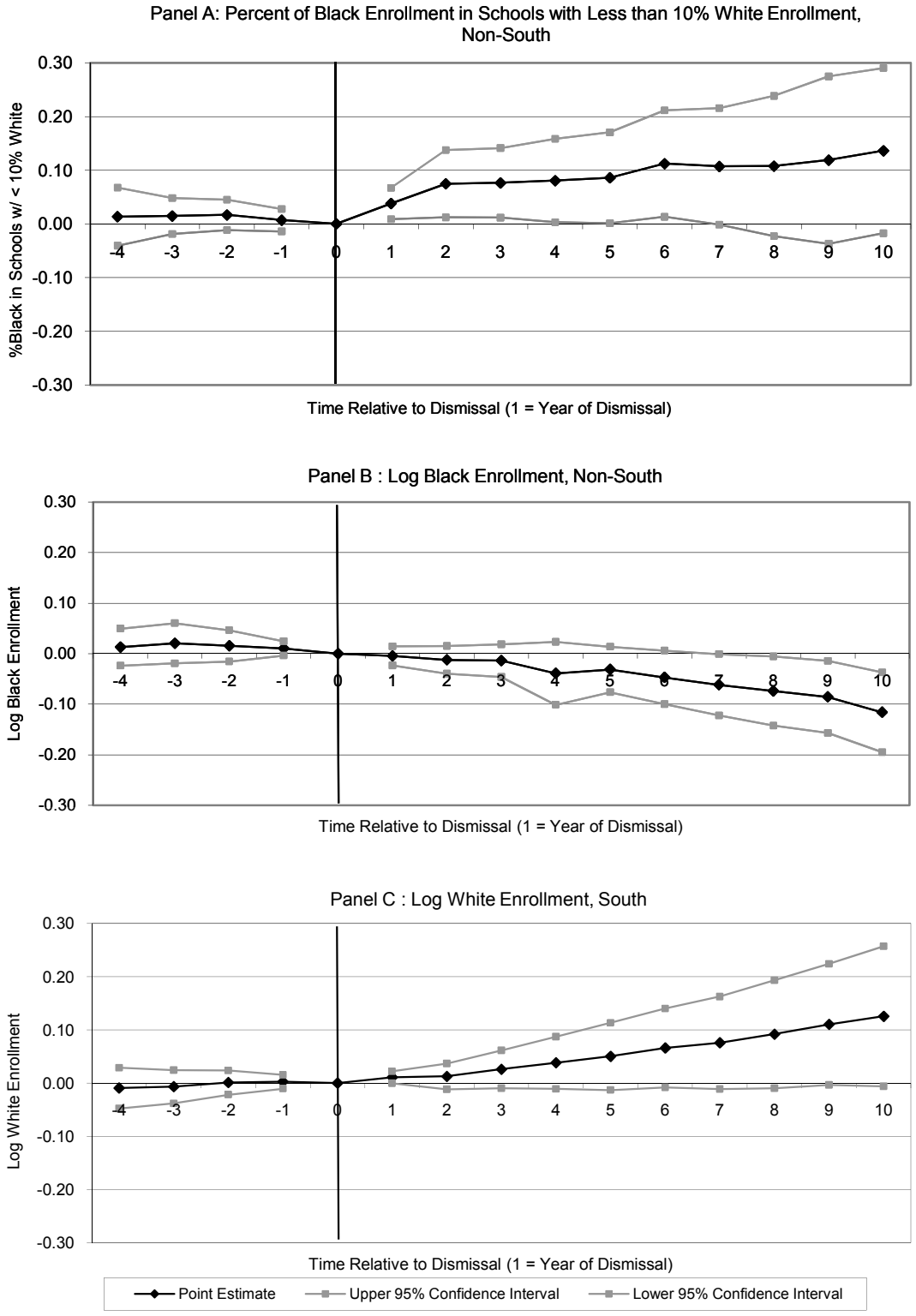
[2]The estimate in column (1) differs from those using the CCD school enrollment data, presented on Figures 5, Web Appendix Figures A1 and A2 and Table 6, because they focus on all 16 - 19 year-olds (the group over which the dropout rates are calculated) *residing* in the school district, while the CCD estimates focuses on students of all ages *enrolled* in the public school system. The CCD results capture the net effect of migration, changes in the dropout rate, and changes in the private school attendance rate. The results here focus only on net migration.

Figure A1: Weighted Log Enrollment



Note. The figures display the β vector coefficient estimates and associated confidence intervals from equation (4). The confidence intervals are constructed using standard errors clustered by school district. The dependent variable is the log enrollment of the race given in the panel title (obtained from the annual CCD panel). All panels are weighted by the enrollment of the race given in the panel title. The sample is restricted to districts with enrollment greater than 10,000 in the first year the district is observed in the sample in all panels. The sample size is 1754 in panels A and B and 1749 in Panel C. Coefficient estimates for the complete set of covariates available from the author upon request.

Figure A2: Event Study Estimates by South and Non-South



Note. The figures display the β vector coefficient estimates and associated confidence intervals from equation (4). The confidence intervals are constructed using standard errors clustered by school district. The dependent variable is the dissimilarity index (obtained from the annual CCD panel). The estimation sample is given in the panel title. The sample is restricted to districts with enrollment greater than 10,000 in the first year the district is observed in the sample in all panels. The sample size is 646 in panels A and C and 1108 in panel B. Coefficient estimates for the complete set of covariates available from the author upon request.

Appendix Table A1
Districts in Rossell and Armor Sample Under a Court-Ordered Desegregation Plan in 1991

District Name	State	Dismissal Date	Base Period Enrollment
AUTAUGA COUNTY SCH DIST	AL	2005	6,920
BIBB COUNTY SCH DIST	AL	2006	3,571
CALHOUN COUNTY SCH DIST	AL		11,105
DOTHAN CITY SCH DIST	AL		10,028
HUNTSVILLE CITY SCH DIST	AL		24,987
JACKSON COUNTY SCH DIST	AL		6,720
JEFFERSON COUNTY SCH DIST	AL		41,143
MOBILE COUNTY SCH DIST	AL	1997	67,841
MONTGOMERY COUNTY SCH DIST	AL	1993	36,010
SAINT CLAIR COUNTY SCH DIST	AL	2000	5,638
PHOENIX UNION HIGH SCHOOL DISTRICT	AZ	2005	21,117
TUCSON UNIFIED DISTRICT	AZ		58,917
FORREST CITY	AR		5,621
LITTLE ROCK	AR	2002	26,854
N. LITTLE ROCK	AR		9,725
PULASKI CO. SPECIAL.	AR		22,280
LOS ANGELES UNIFIED	CA		589,311
OAKLAND UNIFIED	CA		51,298
SAN BERNARDINO CITY UNIFIED	CA		35,033
SAN DIEGO CITY UNIFIED	CA	1998	116,557
SAN FRANCISCO UNIFIED	CA	2005	63,881
SAN JOSE UNIFIED	CA	1998	29,333
STOCKTON CITY UNIFIED	CA	2005	31,051
DENVER COUNTY 1	CO	1995	59,439
BRIDGEPORT SCHOOL DISTRICT	CT		19,416
WATERBURY SCHOOL DISTRICT	CT		13,298
CHRISTINA SCHOOL DISTRICT	DE	1996	16,438
RED CLAY CONSOLIDATED SCHOOL DISTRICT	DE	1996	14,189
BAY COUNTY SCHOOL DISTRICT	FL		21,541
BROWARD COUNTY SCHOOL DISTRICT	FL	1996	137,366
DADE COUNTY SCHOOL DISTRICT	FL	2001	253,323
DUVAL COUNTY SCHOOL DISTRICT	FL	2001	105,049
ESCAMBIA COUNTY SCHOOL DISTRICT	FL	2004	42,066
HILLSBOROUGH COUNTY SCHOOL DISTRICT	FL	2001	118,031
JACKSON COUNTY SCHOOL DISTRICT	FL		7,565
LEE COUNTY SCHOOL DISTRICT	FL	2003	37,708
MARION COUNTY SCHOOL DISTRICT	FL	2007	26,433
ORANGE COUNTY SCHOOL DISTRICT	FL		88,878
PINELLAS COUNTY SCHOOL DISTRICT	FL	2001	88,866
POLK COUNTY SCHOOL DISTRICT	FL	2000	61,244
SEMINOLE COUNTY SCHOOL DISTRICT	FL	2006	43,511
ST. LUCIE COUNTY SCHOOL DISTRICT	FL	1997	18,260
BIBB COUNTY	GA	2007	25,158
CHATHAM COUNTY	GA	1994	35,358
DECATUR COUNTY	GA		5,810
DEKALB COUNTY	GA	1996	81,468
DOUGHERTY COUNTY	GA		18,760
FULTON COUNTY	GA	2003	50,190
LOWNDES COUNTY	GA		7,982
MUSCOGEE COUNTY	GA	1997	31,984
RICHMOND COUNTY	GA		35,422
CITY OF CHICAGO SCHOOL DIST 299	IL		419,537
JOLIET PUBLIC SCH DIST 86	IL		8,823
FORT WAYNE COMMUNITY SCHOOLS	IN		32,405

INDIANAPOLIS PUBLIC SCHOOLS	IN	1998	50,496
M S D DECATUR TOWNSHIP	IN		5,146
M S D WAYNE TOWNSHIP	IN		12,066
SCHOOL CITY OF HAMMOND	IN		13,737
KANSAS CITY	KS	1997	22,897
TOPEKA PUBLIC SCHOOLS	KS	1999	14,783
FAYETTE CO	KY		31,191
JEFFERSON CO	KY	2000	93,198
CADDO PARISH SCHOOL BOARD	LA		52,309
CALCASIEU PARISH SCHOOL BOARD	LA		32,726
CITY OF MONROE SCHOOL BOARD	LA		10,922
EAST BATON ROUGE PARISH SCHOOL BOARD	LA	2003	60,279
EVANGELINE PARISH SCHOOL BOARD	LA		6,907
JEFFERSON PARISH SCHOOL BOARD	LA		57,663
LAFAYETTE PARISH SCHOOL BOARD	LA	2006	28,392
ORLEANS PARISH SCHOOL BOARD	LA		84,428
OUACHITA PARISH SCHOOL BOARD	LA		17,523
POINTE COUPEE PARISH SCHOOL BOARD	LA		3,868
RAPIDES PARISH SCHOOL BOARD	LA	2006	24,404
SAINT LANDRY PARISH SCHOOL BOARD	LA		17,379
SAINT TAMMANY PARISH SCHOOL BOARD	LA		28,055
TANGIPAHOA PARISH SCHOOL BOARD	LA		17,266
WASHINGTON PARISH SCHOOL BOARD	LA		5,554
WEST FELICIANA PARISH SCHOOL BOARD	LA	2007	2,050
PRINCE GEORGES COUNTY PUB SCHS	MD	2002	104,661
HOLYOKE	MA		6,732
BENTON HARBOR AREA SCHOOLS	MI	2002	7,129
FLINT CITY SCHOOL DISTRICT	MI	2002	30,202
GRAND RAPIDS PUBLIC SCHOOLS	MI		25,225
KALAMAZOO PUBLIC SCHOOL DISTRICT	MI		12,810
LANSING PUBLIC SCHOOL DISTRICT	MI		22,477
CARROLL COUNTY SCHOOL DIST	MS		1,218
CLEVELAND SCHOOL DIST	MS		4,726
HATTIESBURG PUBLIC SCHOOL DIST	MS	1997	5,789
HOLMES CO SCHOOL DIST	MS		4,362
JACKSON PUBLIC SCHOOL DIST	MS		32,920
NATCHEZ-ADAMS SCHOOL DIST	MS	2003	6,841
RANKIN CO SCHOOL DIST	MS		12,126
VICKSBURG WARREN SCHOOL DIST	MS		10,380
KANSAS CITY 33	MO	2003	35,227
ROCKWOOD R-VI	MO		16,484
ST. LOUIS CITY	MO	1999	42,088
OMAHA PUBLIC SCHOOLS	NE		41,416
MONTCLAIR TOWN	NJ		5,141
UNION TWP	NJ		5,971
BUFFALO CITY SD	NY	1995	46,251
NEW ROCHELLE CITY SD	NY		7,633
SYRACUSE CITY SD	NY		20,972
UTICA CITY SD	NY		8,317
YONKERS CITY SD	NY	2002	17,744
CHARLOTTE-MECKLENBURG SCHOOLS	NC	2001	74,149
CUMBERLAND COUNTY SCHOOLS	NC		44,222
DURHAM PUBLIC SCHOOLS	NC		17,483
FORSYTH COUNTY SCHOOLS	NC		38,311
HALIFAX COUNTY SCHOOLS	NC		6,608
CINCINNATI CITY SD	OH	1991	51,819
CLEVELAND MUNICIPAL SD	OH	1999	71,743
DAYTON CITY SD	OH	2002	28,768

OKLAHOMA CITY	OK	1991	39,149
ERIE CITY SD	PA		12,485
PHILADELPHIA CITY SD	PA		194,698
SUMTER COUNTY SCHOOL DISTRICT 02	SC		8,661
MEMPHIS CITY SCHOOL DISTRICT	TN		105,856
NASHVILLE-DAVIDSON COUNTY SD	TN	1998	66,973
SHELBY COUNTY SCHOOL DISTRICT	TN		33,683
ALDINE ISD	TX	2002	37,657
CORPUS CHRISTI ISD	TX	1997	41,850
CROSBY ISD	TX		3,246
DALLAS ISD	TX	2003	130,885
ECTOR COUNTY ISD	TX		25,770
GALENA PARK ISD	TX	2007	13,938
GARLAND ISD	TX		34,603
RICHARDSON ISD	TX		32,080
TEMPLE ISD	TX	2000	8,110
WICHITA FALLS ISD	TX	2000	15,055
MILWAUKEE	WI		91,648

Note. Base period enrollment is total student enrollment in the first year the district appears in the sample.

Table A2
Effect of Desegregation Order Dismissal on Racial Segregation

	Black-White						Nonwhite-White	Hispanic-White
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
				A. Δ Dissimilarity Index				
Δ Years Since Dismissal	0.0098	0.0093	0.0089	0.0080	0.0100	0.0061	0.0077	0.0036
	(0.0016)***	(0.0018)***	(0.0016)***	(0.0019)***	(0.0022)***	(0.0021)***	(0.0017)***	(0.0018)**
				B. Δ Dissimilarity Index Weighted by Enrollment				
Δ Years Since Dismissal	0.0082	0.0065	0.0068	0.0079	0.0080	0.0046	0.0061	0.0014
	(0.0020)***	(0.0023)***	(0.0019)***	(0.0019)***	(0.0023)***	(0.0024)*	(0.0022)***	(0.0016)
				C. Δ Exposure Index				
Δ Years Since Dismissal	-0.0032	-0.0040	-0.0018	-0.0030	-0.0033	-0.0014	-0.0022	0.0007
	(0.0009)***	(0.0011)***	(0.0011)*	(0.0010)***	(0.0013)**	(0.0012)	(0.0008)***	(0.0013)
				D. Δ Exposure Index Weighted by Enrollment				
Δ Years Since Dismissal	-0.0029	-0.0028	-0.0021	-0.0028	-0.0032	-0.0023	-0.0018	0.0008
	(0.0008)***	(0.0008)***	(0.0011)**	(0.0008)***	(0.0012)***	(0.0013)*	(0.0007)**	(0.0010)
Observations	1754	1039	1754	2283	1754	1754	1754	1751
Number of School Districts	100	59	100	130	100	100	100	100
Region-Year Effects	X	X	X	X	X		X	X
Base Demographics * Year Effects	X			X	X	X	X	X
Restricted to Enrollment > 10,000	X		X		X	X	X	X
Restricted to Districts Dismissed Post 1990		X						
School District Specific Linear Trends					X			
State-Year Effects						X		
Weighted by Enrollment								

Note. The table displays coefficient estimates from equation (5). Standard errors clustered by district in parentheses. In columns (1) - (6) the dependent variable is the black-white index identified in the panel heading. In column (7) the dependent variable is the nonwhite-white index identified in the panel headings. In column (8) the dependent variable is the hispanic-white index identified in the panel headings. All dependent variables are obtained from the annual CCD panel. Base demographic characteristics, which are time-invariant, include a central city indicator variable, percent of enrollment which is white, percent of enrollment which is hispanic, number of students enrolled, number of students enrolled squared, number of students enrolled cubbed, percent of enrollment receiving a free or reduced price lunch, and percent of enrollment receiving a free or reduced price lunch squared. Coefficient estimates for the complete set of covariates available from the author upon request.* significant at 10%; ** significant at 5%; *** significant at 1%

Table A3
Effect of Desegregation Order Dismissal on Black Migration

	Log Black 16-19 Year Olds	Mean Black Household Income	% Black Mothers w/o High School Degree * 100	% Black Mothers w/ College Degree * 100	% Black Children Below the Poverty Line * 100
	(1)	(2)	(3)	(4)	(5)
Non-South Linear Dis. * 2000	0.0023	0.0034	0.0014	0.0017	-0.0022
	(0.0071)	(0.0029)	(0.0019)	(0.0010)*	(0.0026)
South Linear Dis. * 2000	0.0217	-0.0059	0.0002	0.0013	0.0042
	(0.0136)	(0.0045)	(0.0025)	(0.0014)	(0.0031)
Observations	98	98	98	98	98
Region * 2000	X	X	X	X	X
Base Demographics[a] * 2000	X	X	X	X	X

Note. The table displays coefficient estimates from equation (5). Standard errors clustered by district in parentheses. All columns are weighted by the number of black 16 - 19 year-olds. The dependent variable is given in the column header (obtained from the two-period SDDB panel). Mean black household income refers to black households with children. [a] The base demographics include only district level variables. The district level covariates, which are measured for all races and obtained from the CCD in the first year they are available, are a central city indicator variable, percent of enrollment which is white, percent of enrollment which is hispanic, number of students enrolled, number of students enrolled squared, number of students enrolled cubbed, percent of enrollment receiving a free or reduced price lunch, percent of enrollment receiving a free or reduced price lunch squared, percent of black students in the non-south in a school with less than 10% white enrollment, and percent of black students in the south in a school with less than 10% white enrollment . * significant at 10%; ** significant at 5%; *** significant at 1%