

***Brown* Fades: The End of Court-Ordered School Desegregation  
and the Resegregation of American Public Schools**

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December, 2011

Forthcoming in *Journal of Policy Analysis and Management*

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**Abstract**

In this paper we investigate whether the school desegregation produced by court-ordered desegregation plans persists when school districts are released from court oversight. Over 200 medium-sized and large districts were released from desegregation court orders from 1991 to 2009. We find that racial school segregation in these districts increased gradually following release from court order, relative to the trends in segregation in districts remaining under court order. These increases are more pronounced in the South, in elementary grades, and in districts where pre-release school segregation levels were low. These results suggest that court-ordered desegregation plans are effective in reducing racial school segregation, but that their effects fade over time in the absence of continued court oversight.

Keywords: Court-Ordered Desegregation; School Segregation.

## Introduction

The Supreme Court's unanimous decision in the landmark *Brown v. Board of Education* case was intended to eliminate *de jure* school segregation. Although it took over a decade before the decision was enforced in Southern school districts, the Court's ruling in *Brown* (and its subsequent rulings in the *Green* and *Swann* cases)<sup>1</sup> ultimately led to substantial decreases in school segregation throughout the South (Cascio, Gordon, Lewis, & Reber, 2008; Guryan, 2004; Johnson, 2010; Lutz, 2011; Orfield, Glass, Reardon, & Schley, 1993; Reber, 2005b). In 1964, 99 percent of black students in the South attended all-black schools; by 1971, only about 20 percent attended such schools, and schools in the South were more integrated than elsewhere in the country (Cascio, et al., 2008; Orfield & Yun, 1999).

Since then, however, the Court has ruled that desegregation plans were never intended to be permanent,<sup>2</sup> and many of the school districts that were once under court order to desegregate have been released from court oversight. This trend raises the question of whether court-mandated desegregation plans create lasting patterns of school integration that persist even after school districts are released from them. On the one hand, if desegregation plans foster norms supporting school integration and integrated housing patterns, or if courts generally require, as a condition of awarding unitary status, that districts continue some set of desegregative practices after court oversight ends (see Clotfelter, Ladd, & Vigdor, 2006 for examples of this), then school districts may remain relatively integrated following the release from court order. On the other hand, if residential segregation remains high and school districts revert to neighborhood-based student assignment plans following the release from

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<sup>1</sup> *Brown v. Board of Education of Topeka*, (347 U.S. 483, 1954); *Green v. County School Board of New Kent* (391 U.S. 430, 1968); *Swann v. Charlotte-Mecklenburg Board of Education* (402 U.S. 1, 1971).

<sup>2</sup> *Board of Education v. Dowell* (498 U.S. 237, 1991).

court order, then school districts may rapidly resegregate back to levels commensurate with residential segregation. Understanding what happens when court orders end is central to understanding the legacy of *Brown*.

In this paper we examine trends in the use of court-ordered school desegregation plans in the U.S. during the last two decades and estimate the impact of being released from court-ordered desegregation on district enrollment patterns and segregation levels. Although prior research (most notably Lutz, 2011) has investigated these questions, we extend this prior work in several ways. First, while prior studies have been based on relatively small samples of districts (65 to 130 districts), ours is based on what we believe to be all of the 483 districts in the country that enrolled at least 2,000 students and were under court supervision as of 1990, enabling us to provide a comprehensive accounting of districts subject to court-ordered desegregation plans from 1990 to the present. Second, our data include 96 districts in non-Southern states that were under court order, enabling us to estimate the effects in the South and non-South separately. Third, we estimate the effects of release separately by grade level, allowing us to investigate if and how the impact of release varies across grades. And fourth, our district and state-by-grade-by-year fixed effects models provide a strong causal warrant, using information on the exact timing of dismissals, pre-dismissal trends in released districts, and non-released districts within the same state to construct average counterfactual trends for the released districts.

Our data show that over half of all districts ever under court-ordered desegregation have been released from court oversight, with most of the releases occurring in the last 20 years. Somewhat surprisingly, we find that the districts released from court order were very similar to those not released in terms of their racial composition and segregation levels, suggesting that the process of release is not tightly linked to the success of the court order in

producing integration. Moreover, after being released from court oversight, school districts become steadily more racially segregated. The rate of resegregation is much larger in elementary grades, in large districts and districts with larger black enrollments, and in districts where pre-dismissal school segregation levels were low or where residential segregation levels are high.

## **Background**

### *A Brief History of Court-Ordered Desegregation*

In 1954, the Supreme Court ruled in *Brown v. Board of Education of Topeka, Kansas* that *de jure* (mandated by law) racial segregation in public schools was unconstitutional (347 U.S. 483, 1954). Several subsequent Supreme Court decisions enforced the *Brown* decision, restricting the use of so-called “freedom of choice” student assignment plans, mandating strict racial balance quotas, and approving busing to achieve such balance (see *Green v. County School Board of New Kent*, 391 U.S. 430, 1968; *Swann v. Charlotte-Mecklenburg Board of Education*, 402 U.S. 1, 1971). These decisions also established that court supervision was not intended to be permanent.

In the early 1990s the Supreme Court handed down three decisions that made it easier for districts to be released from court oversight. In 1991, the Supreme Court ruled that desegregation orders were intended to be temporary and that a return to local control was preferable when a district had made a good faith effort to desegregate (*Board of Education v. Dowell* (498 U.S. 237, 1991). One year later, the Court ruled in *Freeman v. Pitts* (503 U.S. 467, 1992) that districts could be released from desegregation orders piecemeal. In *Pitts*, the court also placed particular emphasis on a district’s incremental approach to ending segregation. Three years later, the Supreme Court ruled in *Missouri v. Jenkins* (515 U.S. 79, 1995) that

district remedies need only bring the victims of past discriminatory acts to the point they would have occupied had the acts not occurred. Together, these three decisions relaxed the criteria required for release from court oversight.

In the 1990s, some scholars predicted that many districts would be released from court orders shortly after the *Dowell*, *Pitts*, and *Jenkins* decisions, and that these three decisions marked the end of school desegregation (Lutz, 2011; Orfield & Eaton, 1996; Parker, 2000). Orfield and Eaton highlighted the ease with which districts could now be dismissed and argued that many school systems had filed or were considering filing motions for unitary status. Nonetheless, evidence on the number of districts released from court oversight is incomplete.

In a review of 126 written court opinions, covering 90 school districts under court order in the South (specifically, in states under the jurisdiction of the Fifth and Eleventh Federal Circuit Courts) from 1983 through 1999, Parker (2000) found no evidence that districts were more likely to seek or to be granted unitary status after *Dowell*. In the eight years prior to *Dowell*, 10 of 17 districts (59 percent) seeking unitary status were successful; in the eight years following *Dowell*, only eight districts sought unitary status, four of which (50 percent) were successful. More recently, Lutz (2011) found that roughly half of a sample of 130 districts that were under order in 1991 had been released by 2007, with almost all of the dismissals occurring after 1996. Likewise, the United States Commission on Civil Rights (USCCR) reviewed the status of court orders in seven Southern states (Alabama, Georgia, Florida, Louisiana, Mississippi, North Carolina, and South Carolina) and found that 193 of 480 districts (40 percent) in those states that had been under court order had obtained unitary status by 2007, with the rate of release increasing since 2000 (U.S. Commission on Civil Rights, 2007). Both of these recent papers support the argument that many districts have been released since the early 1990s. However, none of the papers provide a complete description of

the current status of all districts ever under court order.

### *Procedures for Dismissal*

Once placed under court order, districts can achieve unitary status in a variety of ways, including legal appeals from districts, parent and community groups, judges, school boards, or the federal government (Holley-Walker, 2010; Lutz, 2011; Parker, 2003; U.S. Commission on Civil Rights, 2007). Historically, districts often initiated the judicial review of their existing court orders. In recent years, however, the federal government has taken a more proactive role in the process of dismissing districts from court supervision, conducting its own reviews, informing districts of satisfactory desegregation, and then filing joint motions for unitary status (Holley-Walker, 2010; U.S. Commission on Civil Rights, 2007).

Between 1999 and 2007, the Educational Opportunities Section of the Civil Rights Division of the Department of Justice (EOS) reviewed the status of over 265 active desegregation orders. The agency examined student assignment and race and ethnicity data, surveyed school and district officials, and analyzed relevant civil rights complaints in collaboration with national civil rights organizations. EOS has used the findings to inform unitary status proceedings; when deemed appropriate, it has joined defendant districts to request dismissal of the desegregation order. Between 2000 and 2007, this process yielded a net removal of over 180 cases from the federal docket (U.S. Commission on Civil Rights, 2007).

Regardless of the motivation for initiating dismissal procedures, Lutz (2011) argues, the timing of release is ultimately marked by “an element of randomness” (p. 134). Randomness stems from unequal caseloads across district courts, the varying and somewhat unpredictable duration of the release process, varying judicial approaches to desegregation, and the possibility of appeals from interested stakeholders, among other factors. We exploit this quasi-

randomness in the timing of release to estimate the effects of release from court oversight on school enrollment and segregation patterns.

### *Potential Impacts of the Release from Court Order on Segregation and Enrollment*

When a school district is released from court-ordered desegregation, school districts may return to neighborhood-based student assignment plans, which will lead to increased school segregation if residential segregation is high and the court order was effective at producing integrated schools. Even so, school segregation may not increase immediately. Parents may want their children to remain in the same schools they attended before the dismissal. Districts may phase in new assignment plans gradually, or only for students entering the system and at transitional grades. These processes would lead to gradual changes in segregation patterns, because in a given year, only a fraction of students in a district will have their school assignment affected by the end of a desegregation order.

On the other hand, districts may not adopt strict neighborhood-based student assignment plans when a desegregation order is lifted. In some cases, federal courts have required districts to adopt or maintain efforts to create integrated schools as a condition of release from court order (Clotfelter, et al., 2006). School districts also have at their disposal a number of legally-permissible voluntary methods of seeking to create racially diverse schools, including choice plans, magnet schools, and drawing attendance zones in ways that create integrated schools. If districts adopt (or continue) such plans following a declaration of unitary status—either to satisfy the court or because the districts and parents value integration—then a release from court oversight may not lead to resegregation, or may lead to less resegregation than would occur under a neighborhood-school assignment plan.

A release from court oversight may also affect the overall enrollment of a district. If



there are some families whose preference for neighborhood schools and/or racially homogeneous schools would, if there were a desegregation plan in place, lead them to live elsewhere or to enroll their child in private school, then the release from court oversight may increase the number of students from such families in a district. Because more white than non-white families prefer racially homogenous schools, and because middle-class families are more likely to have the resources to act on such preferences (Bobo & Zubrinsky, 1996; Crowder, 2000; Emerson, Yancy, & Chai, 2001; Krysan, 2002; Saporito, 2003; Saporito & Sohoni, 2006, 2007; Zubrinsky & Bobo, 1996), we might expect increases in white and middle-class student enrollment following release from a court order—a reversal of the “white flight” that some (e.g., Rossell & Armor, 1996) argue was caused by desegregation plans.

The impact of being released from a court order also might vary across grades or districts. First, any resegregative effects of the release from court order are likely to be stronger in elementary grades than in high school because elementary schools typically draw students from smaller, more homogeneous residential areas than high schools. Second, larger districts may experience a greater increase in segregation because they have more schools and so more opportunities for segregation. Third, because a return to neighborhood schools is likely to raise school segregation levels to match residential segregation levels, districts where pre-release school segregation was low relative to residential segregation—that is, districts where desegregation efforts were most effective at countering local residential segregation patterns—may experience the largest increases in segregation following release from court oversight.

#### *Prior Research on the Impact of Release from Court Order*

Several prior studies have explored the effect of release from court order on

segregation levels, albeit with small and non-representative samples of districts. In general these studies suggest that release from court order may result in increased segregation. Four studies provide estimates of the effect of release from court order on segregation levels. A report from the USCCR shows that districts that were no longer under order were slightly more segregated in 2004 than districts that remained under order, but this difference was not statistically significant (U.S. Commission on Civil Rights, 2007). However, because the USCCR report does not use information on the exact timing of dismissals, and does not control for pre-dismissal segregation levels, its results should not be interpreted causally.

Three other papers estimate the impact of release and do take into account the timing of dismissal, as well as some district-level control variables. An and Gamoran (2009) match 65 districts released from court order to similar non-released districts (i.e., districts that were either still under order or had never been under order) in the same state. They find that the white/black dissimilarity index in Southern districts increased modestly after declarations of unitary status. Clotfelter, Ladd, and Vigdor (2006) examined the relationship between legal decisions and resegregation in the 100 largest districts in the Southern and Border States and find that being released from a court order is associated with increased segregation, though they find that this effect lags three or more years behind the release. Lutz (2011) uses a sample of 130 school districts that were under court order as of 1991 and concludes that release from court oversight leads to increased segregation. His results, based on comparing trends in segregation levels in released districts to contemporaneous trends in districts in the same Census region that remained under order, suggest that being released from court order results in a steady increase in racial segregation that persists for at least 9 to 10 years following release. Because the Lutz study relies on comparing trends in segregation levels in released districts to contemporaneous trends in districts in the same Census region that remained

under order, it has a stronger causal warrant than the Clotfelter, Ladd, and Vigdor and An and Gamoran studies, which rely, in most models, on comparisons between districts that were released to districts that were never under order.

In our analyses we are able to extend and improve upon these studies in a variety of ways. First, while prior studies have been based on relatively small samples of districts (65 to 130 districts), ours is based on what we believe to be all of the 483 districts in the country that enrolled at least 2,000 students and were under court supervision as of 1990. Second, we include data through the 2009-10 school year, which enables us to investigate the effects of release from court order over a relatively long time period. Third, though the majority of the school districts ever under court supervision are located in the South, our sample includes 96 districts in non-Southern states that were under court order, enabling us to estimate the effects in the South and non-South separately. Fourth, we estimate the effects of release separately by grade level in order to test the hypothesis that the impact of release is greatest in the elementary grades. And fifth, we use models similar to Lutz's, which provide a stronger causal warrant than the other prior studies. In particular, our models use information on the exact timing of dismissals, control for pre-dismissal segregation levels and trends, use districts within the same state and still under order as a counterfactual, and include both district and state-by-grade-by-year fixed effects.

## **Data**

The data for our analyses come from two main sources. First, we created a database of school districts that were ever subject to a court-ordered desegregation plan, including whether and when they were released from order. We then merged these data with the Agency and School Universe components of the Common Core of Data (CCD) for school years from

1987-88 to 2009-10. The CCD contains information on all public schools and school districts in the United States and is compiled by the National Center for Education Statistics (Aboud, 2003).

### *District Court Order Status Data*

The number of U.S. school districts that were ever under court order to desegregate is not clear. Until relatively recently, no comprehensive accounting of districts under order existed. In 2004, Logan and Oakley compiled a list of more than 850 school districts that had been defendants in desegregation cases (Logan & Oakley, 2004). However, the Logan and Oakley list contains little or no information on whether and when each district was released from court oversight. Moreover, our own research also revealed a modest number of inaccuracies in the Logan and Oakley list.

In order to create a comprehensive database of districts that were ever under court order and the timing of their release, we drew on a number of secondary sources: 1) the list compiled by Logan and Oakley (2004);<sup>3</sup> 2) several lists provided to us by the Department of Justice;<sup>4</sup> 3) the list of districts used by Lutz (2011); 4) several lists compiled by the Harvard/UCLA Civil Rights Project (Coffee & Frankenburg, 2009; Ma, 2003); 5) the list of large Southern districts compiled by Clotfelter, Ladd, and Vigdor (2006); and 6) several reports of the US Commission on Civil Rights and its state-level advisory committees (Florida State Advisory Committee to the U.S. Commission on Civil Rights, 2007; Georgia Advisory Committee to the U.S. Commission on Civil Rights, 2007; Tennessee Advisory Committee to the U.S.

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<sup>3</sup> John Logan generously shared these data with us.

<sup>4</sup> The Civil Rights Division of the US Department of Justice (DOJ) maintains a list of school districts subject to active desegregation cases. Lists of districts that were still under court oversight in 2004 and 2006 were provided to us by Jeremiah Glassman at the DOJ.

Commission on Civil Rights, 2008; U.S. Commission on Civil Rights, 2007). Using these lists, we compiled a list of all districts ever under court order.

For some districts, these sources also included information on whether and when the district had been released from court oversight. In many cases, however, this information was omitted or the sources disagreed regarding the dismissal status or timing. In these cases, we attempted to contact administrators, lawyers, or staff in each of the districts in order to learn if and when the district had been released from court oversight or to reconcile the conflicting information from other sources. We also searched district websites and news and legal sources for information on court order release and examined the primary source of the information described in the secondary sources above, when available. See the Appendix for a detailed description of the construction of the final dataset.<sup>5</sup>

In this paper, we focus on districts that enroll at least 2,000 students. Districts smaller than this typically have only one or two schools serving students in a given grade, which means that between-school segregation is either impossible (if there is a single school per grade) or relatively insignificant. In addition, we exclude from our analysis and did not collect data on districts that were required to desegregate by the Department of Health, Education, and Welfare (HEW) as a condition of receiving federal funds and districts engaged in a voluntary desegregation plan independent of the courts. We do include four school districts that partook in desegregation consent decrees (i.e., agreements between districts and the court that resulted

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<sup>5</sup> All appendices are available at the end of this article as it appears in JPAM online. Go to the publisher's Web site and use the search engine to locate the article at <http://www3.interscience.wiley.com/cgi-bin/jhome/34787>.

from the threat of court order and that required court approval).<sup>6</sup>

We identified 755 school districts that were ever under court order to desegregate; of these, 153 enrolled fewer than 2,000 students (in 1990) and so are excluded from the remainder of our analyses, leaving 602 districts that were ever under court order and meet our criteria for inclusion (see Appendix Table 1).<sup>7</sup> Of these, 112 districts had been released from court oversight by 1990, 483 were still under order in 1990, and seven districts had unknown dismissal dates (and were therefore dropped from our analyses). Of the 483 districts that we identify as both under order and enrolling at least 2,000 students in 1990, 215 (45 percent) had been released from court oversight and 268 remained under court order as of the fall of 2009.

### *Enrollment and Segregation Data*

Most of the other data used in the analyses below come from the Common Core of Data (CCD), a publicly available data set compiled annually since 1987-88 by the National Center for Education Statistics. The CCD contains school and district-level data for all public schools in the U.S., including enrollment counts by grade, race and ethnicity (white, black, Hispanic, Asian, American Indian), and free and reduced-price lunch eligibility status. In order to compute grade-specific enrollment and segregation measures for each school district, we rely on the

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<sup>6</sup> For example, the Chapter 220 program enacted in Wisconsin in 1976 allows minority students from Milwaukee to attend suburban schools and non-minority suburban students to attend Milwaukee public schools. Because the program was started with a consent decree, we treat the school districts that participate in the Chapter 220 program as under court jurisdiction.

<sup>7</sup> We did not collect information on the dismissal status of districts with fewer than 2,000 students, so cannot report the dismissal status of these districts.

race-by-grade and free and reduced-price lunch eligibility enrollment counts in the CCD.

In the cases where race, free and reduced-price lunch, or enrollment data are missing at the school or grade level, we fill in missing data using the interpolation and imputation methods discussed in detail in the Appendix (and we do not impute if data are missing for an entire state).<sup>8</sup> Given the small proportion of schools missing race data (roughly 2 percent of schools in any given year), our racial segregation results are not sensitive to the imputation of missing data. Given the larger proportion of schools missing free and reduced-price lunch eligibility data (roughly 20 percent of schools in a given year, on average), our socioeconomic segregation results are somewhat less reliable.

In one set of analyses, we investigate the effects of dismissal from court order on private school enrollment patterns. For these analyses we use data from the Private School Survey (PSS), which provides enrollment counts by race for all private schools in the U.S. for every other school year from 1993-94 through 2007-08. In addition, the PSS includes a county identifier, which enables us to compute county-level private schools counts, by race. We link these data to the court-order data for the subset of Southern districts that are county-wide school districts. Because relatively few students cross county lines to attend private school, the PSS provides reasonably good proxies of the number of students residing within a county who attend private school.

### *Segregation Measures*

We compute several measures of within-district and within-district-by-grade

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segregation for use in our analyses. Starting with school-by-grade-level enrollment counts by race and free and reduced-price lunch eligibility status from the CCD, we compute measures of grade-specific within-district white/black, white/Hispanic, white/non-white, and poor/non-poor segregation for each school year from 1987-88 through 2009-10.<sup>9</sup> In analyses where we require a district-level (rather than district-by-grade-level) measure of segregation, we average the district-by-grade-level measures over grades K-12, capturing the average grade-specific segregation in a district.

We use three different measures of segregation—the information theory index ( $H$ ), the dissimilarity index ( $D$ ), and the exposure index ( $P^*$ ). The dissimilarity and information theory indices are measures of evenness, capturing the differential distribution of two groups (i.e., blacks and whites) among schools in a district (Massey & Denton, 1988). Both of the measures range from zero to one, with zero indicating that all schools have identical racial or economic compositions (no segregation) and one indicating that each school enrolls students of a single group; no student attends a school with any students of a different group (complete segregation).<sup>10</sup> The exposure index captures the degree of potential contact between members of two groups within schools in a district. For example, the white-black exposure index measures the fraction of white students at the typical black student’s school (Lieberman, 1981).

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<sup>9</sup> Throughout the paper, we use the term “white” to refer to non-Hispanic white students. We use the term “poor” to refer to students eligible for the Federal free- or reduced-price school lunch program.

<sup>10</sup> The dissimilarity and information theory indices are highly correlated; the dissimilarity index has been more commonly used (and is more readily interpretable), but the information theory index has more desirable mathematical properties (Massey & Denton, 1988; Reardon & Firebaugh, 2002).



Details on the computation of the segregation measures are in the Appendix.<sup>11</sup>

While these measures of exposure and evenness are correlated, they are conceptually distinct. Measures of exposure depend on the relative size of the minority and majority groups whereas measures of evenness do not. For example, blacks can be evenly distributed among schools in a district but have little exposure to whites if they make up a large proportion of a district. On the other hand, if blacks make up a small part of a district, they will have high levels of exposure to whites regardless of how evenly they are distributed across schools (Massey & Denton, 1988). As a result, the exposure measures will capture both changes in the composition of a district and changes in the distribution of students among schools, while evenness measures capture only changes in the relative distribution of students among schools. We focus our discussion of results on the evenness measures ( $H$  and  $D$ ) because they more closely align with the dimension of segregation likely to be directly affected by desegregation orders.

In some analyses, we also use measures of the residential segregation within a school district. For this, we compute the between-tract residential white/black segregation among school-age children using data from the 1990 Census. We compute these measures only for countywide school districts, as these are the districts for which we can easily identify which census tracts lie within district boundaries.

## **Analytic Methods**

Our analysis has two parts. First, we use discrete-time hazard models to examine the

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<sup>11</sup> All appendices are available at the end of this article as it appears in JPAM online. Go to the publisher's Web site and use the search engine to locate the article at <http://www3.interscience.wiley.com/cgi-bin/jhome/34787>.

associations between district characteristics and the timing of release from court-ordered desegregation plans. Second, we use comparative interrupted time-series models to estimate the effect of being released from court order on segregation levels and enrollment patterns.

### *Hazard Models*

Hazard models are appropriate when we wish to model the time until an event occurs but the data are censored (as is the case in our data, where not all districts' court orders are dismissed within the time frame we observe) (Allison, 1982; Singer & Willet, 2003). We use hazard models both to investigate the predictors of dismissal and to provide a partial test of our identifying assumption (that the timing of the release from court order is not associated with segregation levels, net of other covariates in the models). Because the timing of dismissal is measured in discrete years, we use a discrete-time hazard model of the form:

$$\eta_{it} = \ln\left(\frac{h_{it}}{1-h_{it}}\right) = \alpha_t + \mathbf{X}_{it}\mathbf{B} \quad (1)$$

where  $h_{it}$  is the probability that district  $i$  was released from court order in year  $t$ , given that it was still under order in year  $t - 1$ ; the  $\alpha_t, t \in 1991, \dots, 2009$ , are year-specific intercepts; and  $\mathbf{X}_{it}$  is a vector of time-varying characteristics of district  $i$  in year  $t$ . The unit of observation is the district-year, and the model is fit using all observations of a district from 1991 through 2009 or the year in which the district is released, whichever comes first. The vector  $\mathbf{X}_{it}$  includes measures of segregation, racial composition, total enrollment, proportion of students receiving free lunch, per pupil expenditures, urbanicity of district location, and, in some specifications, a set of dummy variables indicating in which state or federal circuit court the district is located. Because hazard models may yield biased estimates if the time-varying covariates are endogenous (i.e., if they are affected by the amount of time elapsed) (Barber, Murphy, & Verbitsky, 2004), we also fit these models using the covariate values from 1990 in

place of the time-varying values.

Several coefficients in the hazard models are of particular interest. First, the coefficient on school segregation allows us to test whether districts with low segregation are more likely to be released from court oversight. If districts with low segregation levels are more likely to be dismissed, this coefficient will be negative. Alternately, if those districts most resistant to the goal of desegregation are most likely to seek dismissal, we might observe a positive association between segregation levels and the likelihood of dismissal. And if the timing of dismissal is ignorable, we would expect to see no association between dismissal and segregation levels. In addition, we are interested in the coefficient on district size (enrollment). We hypothesize that this may be positive: larger districts may be more likely to be released because they may feel more burden from a court order and may have more resources to challenge it. Likewise, we expect districts with more resources relative to their needs (i.e., those with greater spending and fewer low-income students) to be more likely to be released, though it may be that poorer districts are more likely to seek dismissal in order to eliminate the expense of maintaining a desegregation program. Finally, we predict that racially homogeneous districts are more likely to be dismissed, because a desegregation plan may be unable to accomplish meaningful integration in such a district.

### *Changes to Segregation Levels Following Release*

In order to examine changes to segregation levels following release from court order we estimate a set of comparative interrupted time-series models using specifications similar to those used by Lutz (2011):

$$Y_{isgy} = \sum_{t=-21, t \neq 0}^{+17} \delta_t D_{iy}^t + \Gamma_i + \Lambda_{sgy} + \varepsilon_{isgy}. \quad (2)$$

In this model,  $i$  indexes districts,  $s$  indexes states,  $g$  indexes grades,  $y$  indexes school years, and  $D_{iy}^t$  is a dummy variable equal to one if district  $i$  was last subject to a court order in year  $y - t$  (thus,  $t = 1$  in the first year that a district is no longer subject to the order) and 0 otherwise (in particular,  $D_{iy}^t = 0$  for all  $t$  if the district had not been released by 2009). The omitted period is year 0 (the last year that the district was under order). The model includes both district fixed effects ( $\Gamma_i$ ) and state-by-grade-by-year fixed effects ( $\Lambda_{sgy}$ ). The vector of coefficients  $\delta_t$  indicates the average difference in the outcome variable  $Y$  in a given year  $y$  and grade  $g$  between districts that were released  $t$  years prior to  $y$  and districts in the same state that were under order but were not released by 2009, net of stable differences between the dismissed and not-dismissed districts. If the dismissal of a desegregation order causes increases in segregation levels, we expect that  $\delta_t > 0$  for all  $t > 0$ . The identifying assumption here is that districts that experienced dismissals from court-ordered desegregation would, in the absence of the dismissal, have had grade-specific trends in segregation levels similar to those in districts in the same state that were under order but not yet released. If valid, this assumption implies that  $\delta_t = 0$  for all  $t < 0$  (pre-dismissal trends in outcomes in dismissed districts should be the same as the contemporaneous trends in non-dismissed districts in the same state).

As an alternative specification, we also estimate models using a more parametric time specification and a difference-in-difference approach. For each observation, we define two continuous-time variables,  $T_{iy}$  and  $P_{iy}$ , where  $T_{iy}$  is year of observation, centered on the year of dismissal and  $P_{iy}$  is the number of years elapsed since dismissal (and is equal to 0 for all years prior to dismissal). Both  $T_{iy}$  and  $P_{iy}$  are set equal to 0 for districts not dismissed prior to 2009. We then fit the model

$$Y_{iy} = \gamma T_{iy} + \delta P_{iy} + \Gamma_i + \Lambda_{sgy} + \varepsilon_{isgy}. \quad (3)$$

As above, the state-by-grade-by-year fixed effects ( $\Lambda_{sgy}$ ) describe the average state- and grade-

specific trends in  $Y$  among districts still under court order through 2009. These trends are allowed to be fully nonparametric. The coefficient  $\gamma$  indicates the average pre-dismissal linear trend in the difference in  $Y$  between districts released from court order and those in the same state that were not released by 2009. Of primary interest is the coefficient  $\delta$ , which indicates the average change in this linear trend following dismissal. The identifying assumption of this model is that, absent dismissal, the average within-district grade-specific trend in the outcome would have changed by the same (linear) amount in dismissed districts as it did in non-dismissed districts in the same state, grade, and year.

We fit various versions of these models. In particular, we fit versions separately for grades K through 4, 5 through 8, and 9 through 12, in order to examine differences in the effects of dismissal at different grade levels. We also fit models with interactions between the linear trend variables and district characteristics (measured in 1990) in order to test whether the effects of dismissal vary across district types. In order to estimate model (2) and its variants, we include district-by-grade observations from all 483 districts that were under court order in 1990 and that enrolled at least 2,000 students. In all models we cluster the standard errors at the district level.

Finally, we fit a set of variations of these models designed to assess the sensitivity of our results to a range of model types and sample choices. We fit the models using a balanced panel of released districts (restricting the sample to only those districts with a fixed number of pre- and post-dismissal years of data) to ensure that our results are not affected by heterogeneity in the magnitude of the effects of release that is correlated with the number of years of available data. We also fit the model using only those districts that were released prior to 2009 (relying entirely on the pre-dismissal years of released districts to construct a counterfactual trend) in order to ensure that unobserved differences in released and never-released districts do not

bias our estimates. We also fit models in which 1990 district characteristics are interacted with a set of year dummy variables to flexibly control for any differences in segregation trends associated with observed district characteristics that may also be correlated with the timing of release from court order (we identify the relevant district characteristics from the hazard analyses described above). In addition, we fit models weighted by total district enrollment and total black enrollment to obtain person-average estimates rather than district-average estimates.

## Results

### *Descriptive Results*

Of the 483 districts enrolling at least 2,000 students and still under order in 1990, 215 (45 percent) were released before 2009 and 268 remained under court order. Roughly 80 percent of districts under order in 1990 were in the South,<sup>12</sup> and 75 percent of those released between 1990 and 2009 were in the South (see Appendix Table 1<sup>13</sup>). Figure 1 shows the timing of these dismissals. Prior to 2000, fewer than 10 districts were released per year with the exception of 1998, which includes 21 districts released as part of the 13 year phase out plan associated with *U.S. v. Board of School Commissioners of Indianapolis* in Indiana. There is a clear increase in the number of dismissals per year over time, at least through 2007.

Figure 1 here

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<sup>12</sup> We use the U.S. census definition of the South, which includes Alabama, Delaware, Florida, Georgia, Kentucky, Louisiana, Maryland, Mississippi, North Carolina, South Carolina, Tennessee, Texas, and Virginia.

<sup>13</sup> All appendices are available at the end of this article as it appears in JPAM online. Go to the publisher's Web site and use the search engine to locate the article at <http://www3.interscience.wiley.com/cgi-bin/jhome/34787>.

Table 1 shows the average characteristics of all school districts in 1990 with at least 2,000 students and at least 5 percent black student enrollment, broken down by their court order status. Districts that were under a court order in 1990 were slightly more segregated than districts not under an order, had a higher percentage of black students, larger total enrollments, and lower per-pupil expenditures. There are, however, fewer statistically significant differences between districts that were released from court order between 1990 and 2009 and those that remained under order. Notably, those districts that were and were not released by 2009 had virtually identical average racial composition and average levels of racial segregation (both evenness and exposure) in 1990. Dismissed districts have fewer students receiving free and reduced-priced lunch than districts that were under order but not released. Also, disproportionately fewer dismissed districts are located in urban areas and in the South than are the non-dismissed districts.

Table 1 here

The descriptive analyses presented here suggest that there are large differences between districts under order and those never under order. However, districts that were under order and not released appear more similar to districts that were released from court oversight. Given the differences in observable characteristics between districts never under order and those under order, we, like Lutz (2011), use only districts that were under order but not released as a counterfactual in models evaluating the effect of release on segregation levels and changes to district demographics.

#### *Factors Associated With Release From Court Oversight*

Table 2 reports the estimated coefficients from a set of discrete-time hazard models predicting whether and when districts were dismissed. Model 1 indicates—as we would

expect from Table 1—that there is no bivariate association between white/black segregation levels and the probability of release from court oversight. In fact, white/black segregation as measured by either the dissimilarity index or the exposure index (models not shown) is never a statistically significant predictor of dismissal.

Table 2 here

Model 2, which includes a set of additional district-level covariates, including a quadratic term for the proportion black, indicates that the probability of release is lowest for districts with moderate proportions of black enrollment (conditional on other characteristics, the estimated probability of release is minimized when black enrollment is about 27 percent, which is the 66th percentile in the distribution of percent black in all districts) and higher for districts with very low or very high black proportions.<sup>14</sup> However, when state fixed effects are included in Model 4, a district's proportion black is no longer a statistically significant predictor of dismissal ( $p > 0.10$ ).

The models also indicate that larger and higher spending districts are more likely to be released from court oversight. In order to get a sense of the magnitude of these coefficients, we compute the predicted survival rate for a district of 2,000 students and a district of 20,000 students, setting the other covariates to their sample averages. By 2009, the small district would have a 35 percent chance of being dismissed and the large district, a 53 percent chance.

Models 3 and 4 include state and circuit court fixed effects to control for differences among states or in court jurisprudence. Model 3 indicates that there are significant differences

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<sup>14</sup> For parsimony, we do not include terms distinguishing the proportion Hispanic, Asian, and other; nor do we include a quadratic term for the proportion white, as these terms were never significant in our models and their inclusion did not change the substantive results. Likewise models that include categorical variables for different racial proportions show the same pattern of results as these more parsimonious models.



among the circuit courts in the probability of release ( $p < .001$ ); dismissal rates are highest in the Seventh and Tenth Circuits and lowest in the Second, Fifth, and Ninth Circuits.

In the last two models we include interactions of key covariates and a dummy variable for non-Southern states to test whether the predictors of dismissal are the same in Southern and non-Southern states. These models suggest that racial composition is not predictive of dismissal in Southern states, but is predictive in non-Southern states, where the hazard of dismissal is minimized for districts that are 25% black. Conversely, district enrollment is strongly (positively) associated with dismissal in the South, but not in non-Southern states.<sup>15</sup> We perform an additional check of the identifying assumption by predicting demographic and segregation trends (the difference between 1987 and 1995 demographics and segregation) based on whether a district is ever dismissed, excluding the thirteen districts dismissed before 1995, and find that dismissal status is not predictive of those initial trends (results not shown).

#### *Effect of Release on District Demographics and Segregation Levels*

We display the results from estimating equation (2) in figures 2 and 3 and in Appendix

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<sup>15</sup> In a set of models not shown, we include interactions between non-South and segregation levels; these models show that there is no association between segregation and dismissal in either the South or non-South. We also fit models that included white/Hispanic segregation, non-poor/poor segregation, and lagged segregation measures, as well as models that include white/Black exposure rather than white/Black dissimilarity; none of these factors were associated with the probability or timing of dismissal; nor did their inclusion substantially alter the other coefficient estimates or their statistical significance. Lastly, our results remain unchanged when we use covariates measured in 1990 rather than those that vary over time.

Figures 1 and 2.<sup>16</sup> These figures show average trends in the racial composition and white/black dissimilarity index following release from court order in Southern states, relative to contemporaneous trends in non-released districts in the same state. We show segregation results for changes to only the dissimilarity index for the sake of brevity, but the patterns of results are consistent across our other segregation measures as well. These figures plot the estimated coefficients (the  $\hat{\delta}_t$ s) on the variables indicating the number of years since dismissal from court order (the  $D_{i,y}^t$ s) and their 95 percent confidence intervals. Each of the figures shows estimates from both a balanced panel (using released districts only if they have eight years of pre- and six years of post- release data) and an unbalanced panel (using all districts). In general, the estimated trends from balanced and unbalanced panels are quite similar. In order to get a sense of the magnitude of these changes, note that the 1990 standard deviation of the dissimilarity indices for all districts under order is 0.19 for white/black dissimilarity, 0.26 for white/Hispanic dissimilarity, and 0.13 for non-poor/poor dissimilarity (see Table 1).

Figures 2-3 here

Figure 2 shows that the trends in white and black enrollment proportions were relatively similar in Southern dismissed and non-dismissed districts in the immediate pre- and post- dismissal years. A close examination of the data suggests that districts dismissed earlier (i.e. in the 1990s) had more rapid increases in their proportion black, relative to non-dismissed districts in their same states, than did districts released later. This pattern is responsible for the curvilinear shape of the trend in the unbalanced panel. Importantly, however, the balanced panel shows no change in the relative trends in racial composition before and after dismissal,

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<sup>16</sup> All appendices are available at the end of this article as it appears in JPAM online. Go to the publisher's Web site and use the search engine to locate the article at <http://www3.interscience.wiley.com/cgi-bin/jhome/34787>.

suggesting that there is no effect of dismissal on racial composition.

Figure 3 and Appendix Figure 1 show that the average pre-dismissal rate of change in white/black and white/Hispanic segregation levels was lower in dismissed districts than in comparison districts that were under order but not dismissed.<sup>17</sup><sup>18</sup> Following release from court oversight, however, segregation levels in dismissed districts grew steadily, relative to comparison non-dismissed districts and relative to their own pre-dismissal trend. The effect of being released from court order on white/black segregation reaches statistical significance three to four years after release. White/Hispanic segregation levels trend upward following dismissal, but the non-parametric estimates are generally not significantly different from zero. Ten years after release, the difference in the white/black dissimilarity index between dismissed and non-dismissed districts is an average of 0.05 greater than it was in the year of release, or about one-quarter of a standard deviation. Importantly, segregation levels do not rise sharply following release from court order, but rather rise gradually and steadily for 10 to 12 years after release. There appears to be no sizeable or statistically significant effect of

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<sup>17</sup> Note that Figure 3 shows the trend in *differences* in segregation levels in dismissed versus non-dismissed districts in the same state. The pre-dismissal trend of declining differences does not imply that segregation levels in dismissed districts actually declined, on average, prior to dismissal. In fact, the declining pre-dismissal trend difference is actually a result of the fact that segregation levels in dismissed districts were relatively stable in the years leading up to release, while contemporaneous segregation levels among districts in the same state that were still under order gradually increased, on average.

<sup>18</sup> All appendices are available at the end of this article as it appears in JPAM online. Go to the publisher's Web site and use the search engine to locate the article at <http://www3.interscience.wiley.com/cgi-bin/jhome/34787>.

release from court order on non-poor/poor segregation levels (Appendix Figure 2).<sup>19</sup>

Our results show some evidence of systematic, short-term effects of release from court order on segregation levels for districts outside the South (see Appendix Figures 3 through 5).<sup>20</sup> In a balanced panel of districts, white/black and white/Hispanic segregation appear to rise following dismissal, at least in the short term. This short-term effect is relatively rapid and is statistically significant in our parametric models (Appendix Table 2).<sup>21</sup> The unbalanced panel suggests a *decline* in segregation levels five or 10 years following release in the non-South, but this pattern is driven by the changing pool of districts in the sample over time.

Table 3 shows estimates from equation (3), which uses a linear parameterization of the pre- and post-dismissal trends to estimate the effects of dismissal more parsimoniously. Figures 2 and 3 above suggest that these linear trend models will capture most of the trends in racial composition and segregation differences both before and after dismissal. We estimate the models first for all districts and then separately for Southern districts (estimates for non-

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<sup>19</sup> All appendices are available at the end of this article as it appears in JPAM online. Go to the publisher's Web site and use the search engine to locate the article at <http://www3.interscience.wiley.com/cgi-bin/jhome/34787>.

<sup>20</sup> All appendices are available at the end of this article as it appears in JPAM online. Go to the publisher's Web site and use the search engine to locate the article at <http://www3.interscience.wiley.com/cgi-bin/jhome/34787>.

<sup>21</sup> All appendices are available at the end of this article as it appears in JPAM online. Go to the publisher's Web site and use the search engine to locate the article at <http://www3.interscience.wiley.com/cgi-bin/jhome/34787>.

Southern districts are shown in Appendix Table 2).<sup>22</sup>

Table 3 here

Like Figure 2, the top panel of Table 3 suggests that, rather than leading to an increase in white enrollments, dismissal from court order leads to a *decrease* in white (and Hispanic) enrollments, and an increase in black student enrollments. However, these increases are not large—the models indicate that, over a ten-year period following release, the average proportion of white students declines by 3 percentage points (from a 1990 average of 59 percent), and the proportion of black students increases by an equal amount (from a 1990 average of 34 percent). Moreover, these findings are not robust across alternative model specifications presented below (Table 4 and Appendix Table 2), particularly those relying on a balanced panel of dismissed districts (in which the coefficients have the opposite sign as in these models and are not significant).<sup>23</sup> In addition, the proportion of low-income students does not change following release from court oversight, suggesting that release from court order does not draw more middle-income families back into public school districts.

The next panel of Table 3 shows changes to private school enrollment following release from court order. Using only the set of Southern county-wide school districts (those whose boundaries are coterminous with county boundaries), we find no significant effects of release from court order on private school total enrollment and racial composition. This

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<sup>22</sup> All appendices are available at the end of this article as it appears in JPAM online. Go to the publisher's Web site and use the search engine to locate the article at <http://www3.interscience.wiley.com/cgi-bin/jhome/34787>.

<sup>23</sup> All appendices are available at the end of this article as it appears in JPAM online. Go to the publisher's Web site and use the search engine to locate the article at <http://www3.interscience.wiley.com/cgi-bin/jhome/34787>.

result, in combination with the generally null findings regarding the effect of dismissal on public school racial composition described above, suggests that dismissal from a court order does not lead to a sizeable or significant enrollment shift from private to public schools.

The remaining four panels of Table 3 report the estimated effects of release from court oversight on white/black, white/Hispanic, white/non-white, and non-poor/poor segregation levels, respectively. The estimates indicate that, in the South, dismissal from court-ordered desegregation led to increased levels of white/black, white/Hispanic, and white/non-white segregation grew (particularly as measured by the evenness indices). Segregation by poverty status, however, does not appear to have been affected by the release from court order.

The estimated effects, particularly on white/black segregation in the South, are relatively large. For example, the estimates suggest that the white/black dissimilarity index increases by 0.064 points (0.32 standard deviations) and the white/black information theory index increases by 0.056 points (0.47 standard deviations) on average over 10 years following release.<sup>24</sup> The estimated changes to white/Hispanic segregation are somewhat smaller in magnitude (0.14 standard deviations over 10 years as measured by the dissimilarity index and 0.36 standard deviations over 10 years as measured by the information theory index) and not consistently statistically significant. Likewise, the effects on segregation by poverty level are smaller still and are not statistically significant.

These results are robust to a variety of alternative model specifications shown in Table 4. In the interest of space, Table 4 presents only the post-dismissal coefficients from these

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<sup>24</sup> Standard deviation changes are computed by dividing the point estimate by the 1990 standard deviation of the segregation measure among districts under a court order (shown in Table 1).

models; the pre-dismissal coefficients are shown in Appendix Table 3.<sup>25</sup> We restrict the models to a balanced panel of districts—using districts with eight years of data before release and six years of data after release (column 2). In these models, all years of data are included for districts that were not released from their court orders, but only eight pre- and six post-dismissal years are included for dismissed districts, in order to keep the panel balanced. In these models, the estimates for changes to white/black segregation are 20 to 25 percent larger than those from our main set of models in Table 3. The following four specifications (columns 3 through 6) restrict the models to districts that were dismissed from court-orders by 2009 (column 3); include interactions between district characteristics (1990 total enrollment, proportion black, and per-pupil spending—the characteristics associated with the timing of dismissal in Table 2 above) and year indicators (column 4); weight by total enrollment (column 5); and weight by black enrollment (column 6). These sets of estimates are generally similar to those presented in Table 3, except that the estimated effects on district demographics, and all estimates of the effects of dismissal on white/black segregation, are statistically significant. Though the magnitude of the effects varies somewhat across the various specifications, our results (particularly for black-white segregation) are generally robust to alternative model and sample specifications.

Table 4 here

In Appendix Table 4 we present the same models shown in Table 3 but estimated

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<sup>25</sup> All appendices are available at the end of this article as it appears in JPAM online. Go to the publisher's Web site and use the search engine to locate the article at <http://www3.interscience.wiley.com/cgi-bin/jhome/34787>.

separately by grade level.<sup>26</sup> Consistent with our expectations, we find much larger effects of dismissal on changes to segregation levels in elementary school than in high school. Our estimates suggest that ten years after release the white/black dissimilarity index increases by about 0.10 points (0.49 standard deviations) in elementary schools, compared to a 0.05 point (0.25 standard deviations) increase in middle schools and a non-significant 0.03 point (0.15 standard deviation) increase in high schools; likewise, the white/black information theory index increases by about 0.77, 0.47, and 0.32 standard deviations in elementary, middle, and high schools, respectively, though in the case of the information theory index the effect is statistically significant at all grade levels.<sup>27</sup> The changes to white/Hispanic segregation levels following release from court order also are largest (and are statistically significant) in elementary schools.

One way of interpreting the magnitude of these estimates is to compare them to recent trends in segregation in districts that were not subject to court orders. To do so, we compute recent changes to segregation by taking the difference in segregation between 2009 and 1999 for districts never under court order using the Common Core of Data (Appendix Table 6).<sup>28</sup>

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<sup>26</sup> All appendices are available at the end of this article as it appears in JPAM online. Go to the publisher's Web site and use the search engine to locate the article at <http://www3.interscience.wiley.com/cgi-bin/jhome/34787>.

<sup>27</sup> The standard deviation of the white/black dissimilarity index is similar at the elementary, middle and high school levels at about .215 in 1990 among districts under a court order. The standard deviation of the white/black information theory index is 0.138, 0.126, 0.112 in elementary, middle, and high schools, respectively.

<sup>28</sup> All appendices are available at the end of this article as it appears in JPAM online. Go to the publisher's Web site and use the search engine to locate the article at <http://www3.interscience.wiley.com/cgi-bin/jhome/34787>.



There has been little average change in segregation levels in districts never under court order over the past decade, though the trends do vary among districts. Our treatment effects on the white/black dissimilarity index for Southern districts are equal to roughly 1.2 standard deviations of the change observed in these districts and are more than ten times as large as the average change. For the information theory index, the effects are even larger in comparison to trends in districts not under order.

A second way of interpreting the magnitude of these effects is to compare them to the difference between residential (between-tract) segregation and school segregation levels in 1990 (before any districts in our sample were released).<sup>29</sup> To do so, we restrict our analyses to county-wide school districts in the South, because residential segregation of school-age children can be readily computed for such school districts.<sup>30</sup> In 1990 the average white/black residential dissimilarity index was about 0.40 in Southern county-wide districts that were

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<sup>29</sup> Residential and school segregation levels are not exactly comparable because tracts are not perfect proxies for the school attendance zone catchment areas that would be used in the absence of a desegregation order. Nonetheless, because tracts typically contain roughly the same number of school-age children as a typical school (census tracts contain 2,500-8,000 residents—see [http://www.census.gov/geo/www/cen\\_tract.html](http://www.census.gov/geo/www/cen_tract.html)—implying a school-age population of roughly 33-100 students per grade level living in each census tract, comparable to the enrollment of most elementary and middle schools), residential segregation measured between census tracts provides a reasonable (albeit crude) approximation to the level of school segregation that might occur if student assignment were based solely on where students live.

<sup>30</sup> We show differences between county-wide and non-county-wide districts in Appendix Table 5. Approximately 50 percent of the districts in our sample are county-wide districts. County-wide districts are somewhat larger in terms of enrollment and tend to have more schools than non-county-wide districts. They also have higher levels of segregation, more black students and slightly more students in poverty compared to non-county-wide districts.

under court order (Appendix Table 7).<sup>31</sup> School segregation in these counties was about 20 percent lower than residential segregation, with an average dissimilarity index of 0.33 (see also Reardon & Yun, 2003). This suggests that district policies were somewhat successful in overcoming residential segregation, at least to the extent that attendance zones align with Census tracts (which is likely to be the case for elementary schools though perhaps not for middle or high schools). The effect of release on the white/black dissimilarity index ten years after dismissal is about 0.09 in these districts (based on a model like that in Table 3, but restricted to county-wide districts; results not shown). This is the same order of magnitude as the 1990 difference between residential and school segregation in these districts, suggesting that the average change to school segregation following dismissal in these districts is large enough to raise school segregation to levels comparable with between-tract residential segregation.

Our final analysis investigates whether the effect of release from court order is associated with district characteristics (measured in 1990).<sup>32</sup> In particular, we examine variation in the effects of release by district enrollment, proportion black, 1990 white/black school segregation levels, and 1990 white/black county residential segregation levels (among county-wide districts). The district characteristics are centered on their 1990 averages for dismissed Southern districts before computing the interaction terms. In Southern districts, we

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<sup>31</sup> All appendices are available at the end of this article as it appears in JPAM online. Go to the publisher's Web site and use the search engine to locate the article at <http://www3.interscience.wiley.com/cgi-bin/jhome/34787>.

<sup>32</sup> If a district does not have values for a particular covariate in 1990, we use their observed value from 1991 to 1993. If a district does not have values for a particular covariate from 1990 to 1993, then they are dropped from this analysis.

find that the effect of release on white/black segregation levels was greater in larger districts, districts with a higher percentage of black students, and districts with lower pre-release levels of white/black segregation (Appendix Table 8, Model 2).<sup>33</sup> For example, a ten-fold difference in district enrollment is associated with a 0.09 difference in the size of the effect of release on the black-white dissimilarity index ten years after dismissal. A 10 percentage point difference in districts' proportion black is associated with a 0.015 difference in the effect of release on segregation levels after ten years. Finally, the effects of release are smaller for districts that were more segregated prior to release. A difference of 0.10 the 1990 black-white dissimilarity index is associated with a 0.019 smaller effect of release on the black-white dissimilarity theory index ten years after release. This latter finding suggests that the effects of release are largest in districts whose desegregation plans were most effective in integrating schools.

We next restrict the analyses to 200 Southern county-wide districts, in order to examine whether the effects of release are related to the level of residential segregation (we restrict these analyses to county-wide districts because we only have measures of residential segregation in the county-wide districts). In these models, we find evidence that residential segregation is positively associated with the magnitude of the dismissal effect (dismissal leads to a greater increase in school segregation in districts with higher levels of residential segregation; see Appendix Table 8, Model 4), but this association is smaller and no longer

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<sup>33</sup> All appendices are available at the end of this article as it appears in JPAM online. Go to the publisher's Web site and use the search engine to locate the article at <http://www3.interscience.wiley.com/cgi-bin/jhome/34787>.

statistically significant once we control for additional district characteristics.<sup>34</sup> Moreover, the district characteristics associated with the magnitude of the dismissal effect (size, racial composition, pre-dismissal school segregation level) in the full sample are no longer statistically associated with the dismissal effect in this restricted sample. It is unclear if this is due to the imprecision of the estimates based on the county-wide district sample or whether the associations between district characteristics and the effects of release are in fact different in the larger county-wide districts than in the sample as a whole. We conclude from model 2 that the effect of release does vary with district size, racial composition, and segregation levels among our full sample of districts, but that we cannot reject the null hypothesis that the effect of release does not vary with residential segregation levels, at least among the sample of county-wide districts.

## Discussion

Our data indicate that almost half of the school districts that were under court order to desegregate as of 1990 were released from court oversight in the last two decades. Moreover, the rate at which districts have been released has increased over time: more than twice as many districts were released in the 2000s as in the 1990s. If these trends continue, the era of federal court supervision of school desegregation—the era that began with the *Brown* decision—will soon be at an end.

But what legacy do these court-ordered desegregation plans leave? Do their desegregative effects persist beyond the conclusion of the court's involvement? Our analyses

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here indicate that their effects fade over time, at least in the South, where most of the districts under court order are located. Following the release from court order, white/black desegregation levels begin to rise within a few years of release and continue to grow steadily for at least 10 years. Within 10 years of release, the white/black dissimilarity index grows by an average of 0.064 in Southern districts, a sizeable amount (roughly a third of the 1990 standard deviation of segregation levels for districts under order; see Table 1). This is not to say that segregation patterns revert to those of *de jure* segregation present in the South prior to the *Brown*, *Green*, and *Swann* decisions (when the dissimilarity index was one), but segregation does increase substantially relative to levels attained under the court orders.

Though we do not know the actual policies adopted by districts, the pattern of results is consistent with what we would expect if most districts adopt neighborhood-based school assignment policies following the release from court order. Segregation levels grow fastest among elementary schools, which typically draw from smaller (and therefore more homogeneous) catchment areas under neighborhood-based assignment plans, and slowest among high schools. In addition, segregation grows faster in districts where the pre-release school segregation levels were lowest. Because low pre-release school segregation suggests that a desegregation plan was particularly effective, this suggests that segregation rises most quickly following release in those districts where court-ordered desegregation was most effective at reducing segregation.

Moreover, the effects of release from court oversight emerge gradually, consistent with what we would expect if changes to the assignment policies are phased in slowly or only in transitional grades, or if parents prefer to have their students remain in the same school following the end of the court order. The fact that resegregation does not occur immediately suggests that any preference for neighborhood schools is not so strong that it overwhelms the

preferences of districts or parents to maintain present (desegregated) school assignments when possible, even if not required to do so by the courts. Nonetheless, despite the gradual nature of resegregation trends, the degree of resegregation is substantial.

One additional set of findings is worth noting. Although there may be some reason to think that dismissal from court order would lead to some “reverse white/middle-class flight,” we find no evidence of such an effect. Specifically, we find no effects of the release from court order on the size or composition of school district enrollments. In Southern districts, we find no change in total enrollments, no change in the proportion of students who are eligible for free or reduced-price lunch, and no significant change in racial composition following release from court order. Additionally our analyses show no impact of the end of court oversight on the number or racial composition of students enrolled in private schools within the district boundaries. Together, these findings suggest that, between 1991 and 2009, desegregation court orders had little or no impact on most families’ decisions about where to live or whether to enroll in private or public school.

Importantly, we have not considered here the impact of release from court order (and the resulting increase in school segregation) on educational outcomes such as academic achievement and graduation. A growing body of evidence indicates that the desegregation that took place in the 1960s and 1970s improved black students’ educational attainment but had little or no effect on white students’ attainment (Ashenfelter, Collins, & Yoon, 2005; Card & Krueger, 1992; Guryan, 2004; Johnson, 2010; Reber, 2010). Prior research finds that the desegregation plans of the 1960s and 1970s were associated with declines in the black-white dissimilarity index of around 0.20 and increases in the black-white exposure index of about 0.15 (Guryan, 2004; Johnson, 2010; Reber, 2005a), and that blacks’ high school dropout rates declined between three and six percent during that time (Guryan, 2004; Johnson, 2010).

The effect of these early desegregation efforts on academic achievement is less clear. Some argue that the narrowing of the black-white achievement gap was due to school policies like desegregation (Cook & Evans, 2000; Grissmer, Flanagan, & Williamson, 1998) while others posit alternative explanations (Chay, Guryan, & Mazumder, 2009). Evidence on the effects of school or neighborhood context on academic achievement is rare and mixed. One recent random assignment study does show that school segregation—specifically, socioeconomic segregation—may negatively impact the educational achievement of students from historically disadvantaged backgrounds (Schwartz, 2010). Conversely, results from the Moving to Opportunity Study (MTO) show no impact of moving to lower poverty neighborhoods on children’s test scores (Sanbonmatsu, Kling, Duncan, & Brooks-Gunn, 2006). A recent meta-analysis of results from MTO and other recent studies, however, concludes that neighborhood context does affect children’s test scores, though the mechanisms and factors producing such effects are unclear (Burdick-Will et al., 2011). We have even less high-quality recent evidence regarding the effects of school context on achievement than we have on neighborhood context. As a result, it remains unclear if, and to what extent, school racial segregation affects student achievement.

Despite the evidence that desegregation in the 1960s and 1970s improved black students’ educational attainment (and possibly their achievement), it is not clear that resegregation in the 1990s and 2000s will have the opposite effect. The changes in segregation levels that occurred in the 1960s and 1970s were substantially larger than our estimated changes following release from court order. Moreover, the desegregation of public schools in the 1960s and 1970s was highly visible (and contentious, in many cases), led to significant changes in the quality of schooling available to black students, and occurred in an era when racial equality improved in many other domains as well. As a result of these differences in the

magnitude and context of changes in segregation levels, it is unclear whether the effects of desegregation in the 1960s and 1970s can be generalized to estimate the effects of resegregation in the 1990s and 2000s. If the effects are similar to the effects of desegregation in the 1960s and 1970s, we would expect an increase in black dropout rates of about one to two percent following release from court-order, given our estimated increase of 0.06 in the black-white dissimilarity index in Southern districts ten years after dismissal from court-order. However, Lutz (2011) finds similar levels of resegregation post-dismissal to ours and does not find an associated increase in black dropout-rates, suggesting that the effects of resegregation in the 1990s and 2000s may not be as dramatic.

In sum, then, we have shown that the long-term desegregative effect of the *Brown* decision is diminished by the end of court oversight in Southern school districts. Once dismissed, school districts do not maintain the levels of integration they achieved under court order. We do not know, however, whether the increases in school segregation induced by the end of court-ordered desegregation plans lead to declines in educational attainment and achievement, or to increases in racial educational disparities. This is an important topic for future work.



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## **Acknowledgements**

This research was supported by a grant from the U.S. Department of Education, Institute of Education Sciences (Grant # R305A070377). We received excellent research assistance from Noli Brazil, Max Friedmann, Nipun Kant, Lori Rhodes, and Karma Wilson. We thank John Logan, Nora Gordon, Jacinta Ma, Roslyn Mickelson, Erica Frankenberg, and Jeremiah Glassman for providing us with data or assisting us with obtaining it. Finally, we are indebted to the many school superintendents, secretaries, lawyers, and other school officials who answered our many questions about the status and timing of desegregation efforts and court actions in the hundreds of school districts described in this paper. Despite all their help, we have undoubtedly made some errors, for which we alone are responsible.

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## **Appendix: Data Appendix**

### *Description of Data Collection*

In order to catalogue all school districts ever under court order, we began with the data set compiled by Logan and Oakley (Logan & Oakley, 2004) (hereafter the LO dataset), which was provided to us by John Logan. The LO dataset contains information on the case name, state, National Center for Education Statistics (NCES) district identification number, year of the initial case, current status of the plan, and dismissal date when available for 1094 districts. Logan and Oakley created the data set by compiling information from case dockets and bibliographies for desegregation court orders from the Department of Justice, NAACP Legal Defense and Educational Fund, and the U.S. Department of Education, as well as a set of published sources (see also Logan, Oakley, & Stowell, 2008). They then checked each case against legal databases, including Westlaw. The total case inventory includes 358 court cases, which resulted in desegregation plans involving 850 school districts as defendants, plus 207 HEW actions involving 207 school districts since 1978.

The LO dataset often does not include information on whether and when a district was released from court oversight, except in a few cases. In order to determine this, we examined a number of other sources, including other lists of districts under or released from desegregation order (including Clotfelter, et al., 2006; Coffee & Frankenburg, 2009; Florida State Advisory Committee to the U.S. Commission on Civil Rights, 2007; Georgia Advisory Committee to the U.S. Commission on Civil Rights, 2007; Lutz, 2005; Ma, 2003; Tennessee Advisory Committee to the U.S. Commission on Civil Rights, 2008; U.S. Commission on Civil Rights, 2007, as well as several lists provided to us by the US Department of Justice). In addition, we conducted online searches for information on districts under court order, using Google ([www.google.com](http://www.google.com)) and LexisNexis ([www.lexisnexis.com](http://www.lexisnexis.com)). When clear and definitive evidence of district status or

timing of release was found in an online newspaper article or court document, we used that information for the timing of release. If no such evidence was found through online searches (as was often the case), we called a district's administrative office. We first asked for the superintendent and asked whether the district was still under a court order for desegregation. If the answer was no, then we asked when the district had been released from court order and declared unitary. If the superintendent was not available or did not know the status of the district, then we contacted the legal office or attorney affiliated with the district. In cases where the attorney did not have information or contact was not made, we called the district and asked who might have the information on whether or not the district was under court order.

In our search for information on the current status and timing of release of desegregation orders, we found that some districts listed in the LO dataset had not, in fact, ever been under order; we also found a small number of districts that had been under order but that were not included in the LO dataset. In cases where our information conflicted with that in the LO dataset or where two or more sources disagreed about the status or timing of a district's court order, a second research assistant contacted the district for clarification and conducted additional online searches. In most cases we were able to resolve these discrepancies through such additional investigation. In a small number of cases (seven of the 483 districts under order in 1990), we either could not find information on the status of a district's court order, or could not reconcile conflicting sources of information. These districts were dropped from subsequent analyses. Although it is likely that some errors persist in our dataset, we believe that such errors are relatively few.

### *Missing Data and Data Imputation*

Enrollment data from the CCD are incomplete in some cases. These counts are missing for some schools in some years for three reasons: state non-response, school non-response, and changes in the CCD survey format. In the early years of the CCD, particularly, a number of states did not provide race and ethnic and free and reduced-price lunch eligibility enrollment counts by school. Since the 1993-94 school year, however, all states have reported school-level racial enrollment data, except for Idaho (which did not report race data until 2000-01) and Tennessee (which did not report race data after 1998-99), and most states reported free and reduced-price lunch eligibility data. Also in each year, a few schools (roughly two percent of schools in any given year) are missing racial enrollment data, despite the fact that their state reported CCD data that year. The number of schools missing free and reduced-price lunch data averages around 20 percent across years.

Prior to 1998-99, the CCD includes school enrollment counts by grade and by race, but not grade-by-race counts. Similarly, free and reduced-price lunch counts are never reported separately by grade in the CCD. In the years prior to 1998-99, we impute race-by-grade counts by assuming that racial proportions within each grade are constant across grades within the same school. To get the approximate number of students receiving free and reduced-price lunches in each grade, we multiply the percentage receiving free and reduced lunch at the school by the enrollment count in a grade.

We handle missing data differently across our two sets of analyses. The data we use for our hazard models predicting release from court orders have one observation for each district in each year. We use multiple imputation to fill in missing data for this analysis following the methods implemented in Stata by Royston (Royston, 2004). We impute five data sets, estimate the models separately for each data set, and combine the estimates. Since observations from

this panel data are not independent, we reshape the data set (so that each district has a single line of data with 17 variables—one for each year from 1990-2009—for each variable of interest) before conducting the imputation. This allows us to use non-missing information about a district in one year to predict missing information about the district in a subsequent or prior year.

Filling in missing data for our analysis of the effects of court order release proved more computationally challenging. The strategy used above will not work because the number of variables needed exceeds the number of districts. These data have one observation for each of 13 grades (pre-K through 12) in each district and in each of 22 years from 1987 through 2009. However, these observations are not independent; observations of a given variable within a district are correlated across time and across grades. Each of 20 variables of interest is observed up to 286 times (13 grades times 20 years), yielding a total of over 5,000 variables to impute if we treat each variable-grade-year combination as a variable to be imputed. In the absence of available multilevel multiple imputation software, we could not impute the data this way (because there are only 483 districts, far fewer than the number of variables to impute). Given these difficulties, in lieu of multiple imputation we fill in missing data for these analysis by interpolating race counts from adjacent years from the same grade and district, when available. Data were left as missing when information was not available from adjacent years. Our results were substantively insensitive to whether we imputed data or not.

### *Measuring Segregation*

The dissimilarity index measures departure from evenness by taking the average absolute difference of each school's minority proportion from the district's minority proportion, weighted by the enrollment of each school and scaled to range from a minimum of

zero to a maximum of one. That is, if  $j$  indexes schools and  $t_j$  and  $p_j$  denote the total enrollment and proportion of group A, respectively, in school  $j$ , and  $T$  and  $P$  denote the total enrollment proportion of group A in a district, then the dissimilarity index is

$$D = \sum_{j=1}^J \frac{t_j |p_j - P|}{TP(1 - P)}. \tag{A1}$$

The dissimilarity index may range from 0 to 1 and can be interpreted as the proportion of minority students that would have to change schools to be evenly distributed across the district (James & Taeuber, 1985).

The information theory index, originally proposed by Theil (1972), measures departures from evenness by computing the average deviation of each student's school racial diversity from the district-wide racial diversity, where the district diversity ("entropy") is computed as

$$E = P \cdot \ln(P) + (1 - P) \ln(1 - P)$$

and the diversity of school  $j$  is

$$E_j = p_j \cdot \ln(p_j) + (1 - p_j) \ln(1 - p_j).$$

The information theory index is then

$$H = \sum_{j=1}^J \frac{t_j (E - E_j)}{TE} \tag{A2}$$

Like the dissimilarity index, the information theory index has a value of 0 when there is no segregation and a value of 1 when there is complete segregation. The information theory index satisfies the "principle of transfers," which means that a transfer of a student from a school in which her group is underrepresented to a school in which her group is overrepresented will

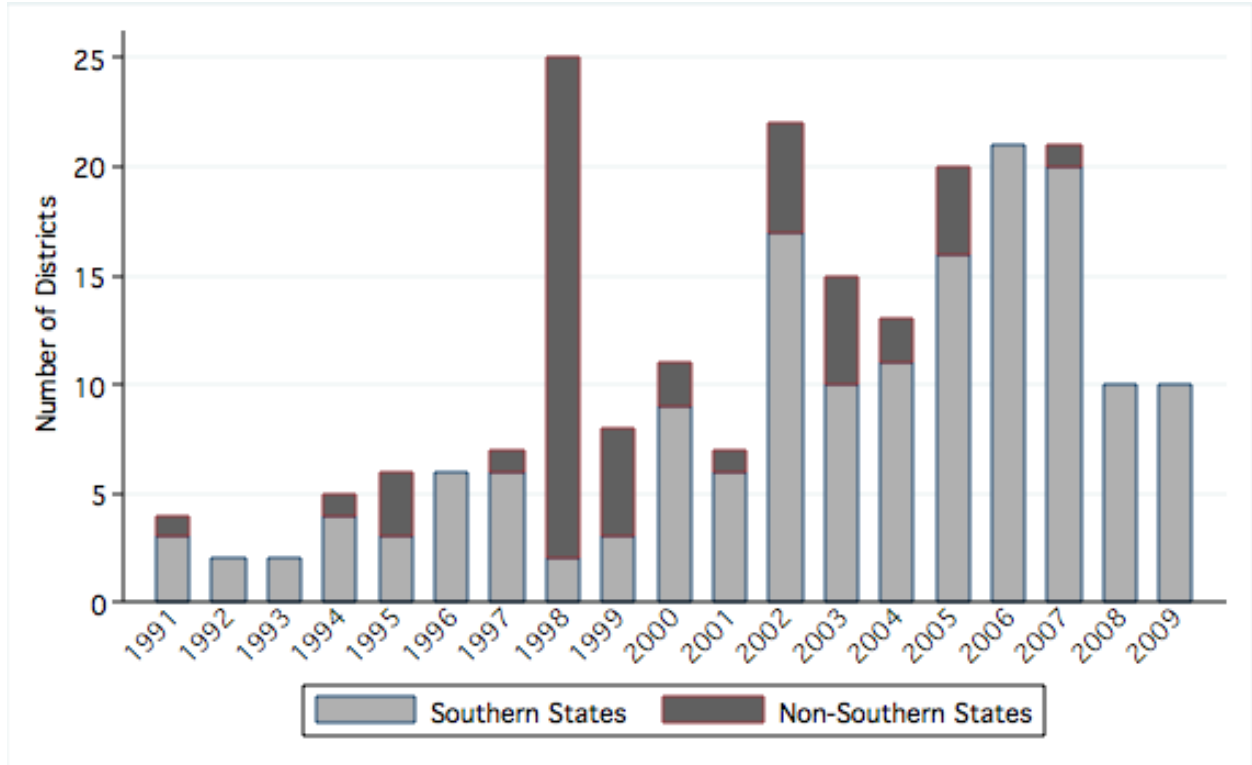
always register as an increase in measured segregation (James & Taeuber, 1985). The dissimilarity index does not satisfy this property, making it less satisfactory as a measure of segregation. For more detail on the similarities and differences between the two, see James and Taeuber (1985) and Reardon and Firebaugh (2002).

The exposure index of one group to another is defined as the average proportion of members of group  $a$  in the schools of members of group  $b$ . It is computed as

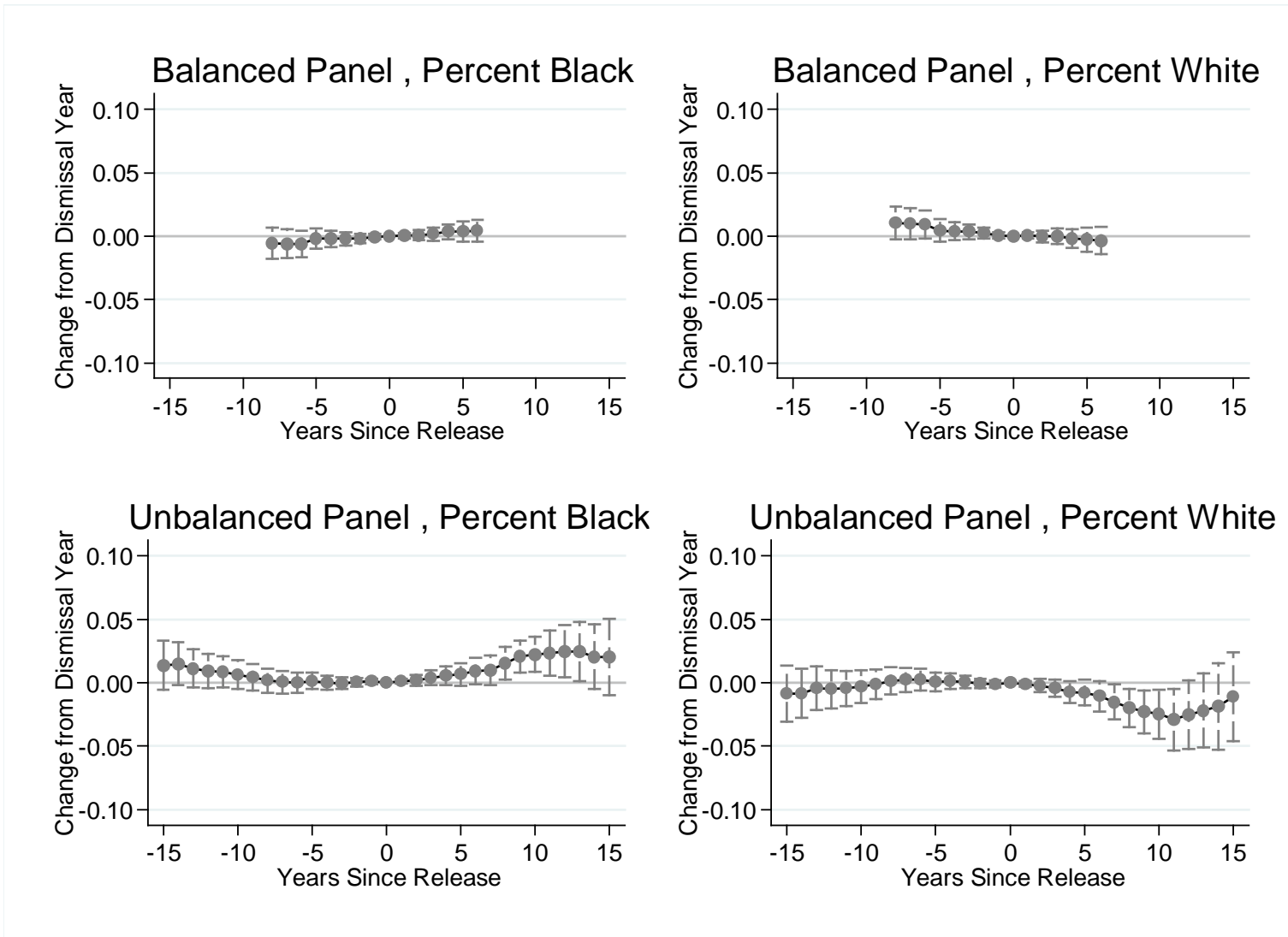
$${}_aP_b^* = \sum_{j=1}^J \frac{p_{aj}t_j}{P_aT} p_{bj}.$$

(A3)

**Figure 1: Number of Districts Dismissed from Court Order, by Year and Region**

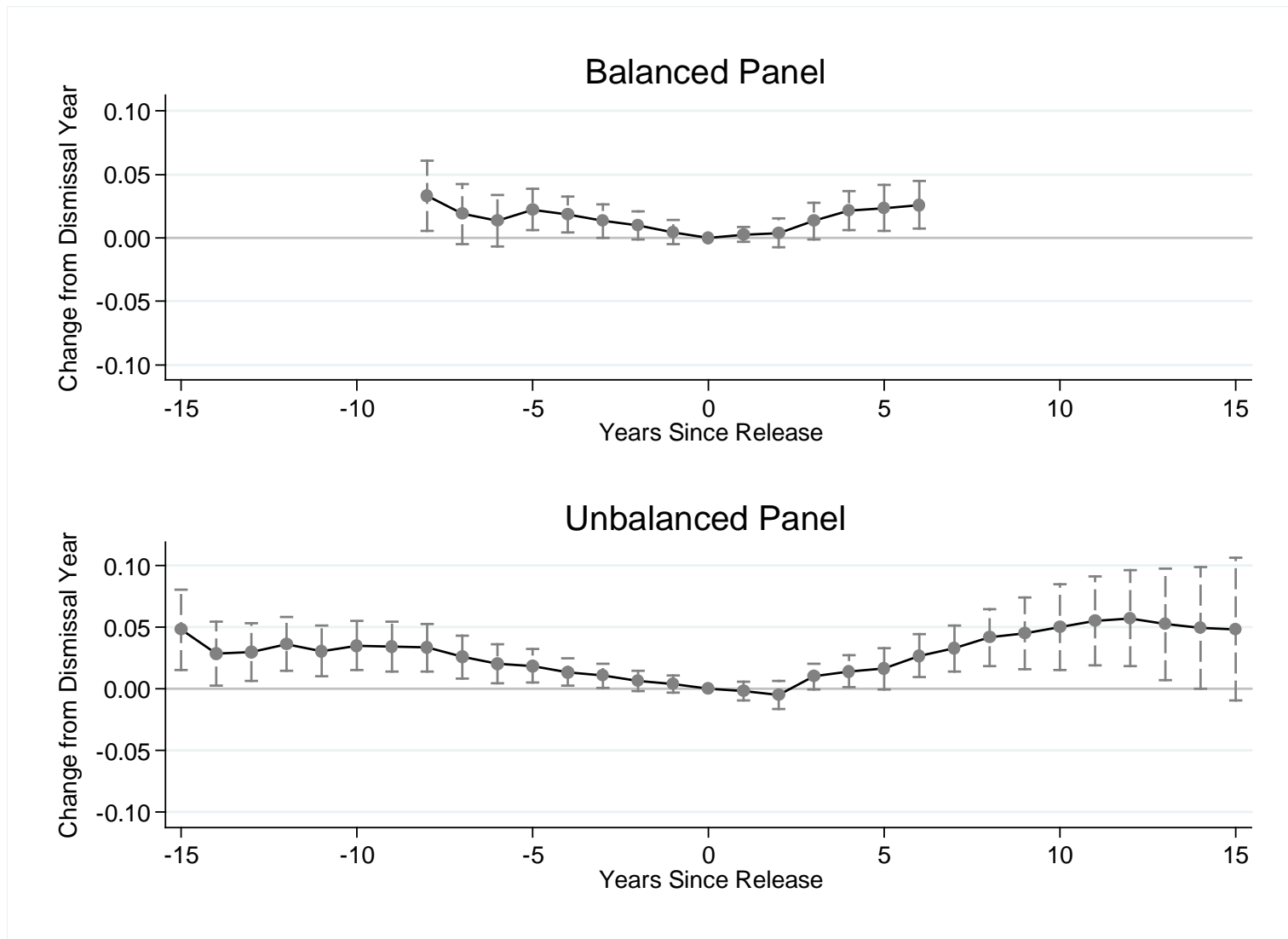


**Figure 2: Non-Parametric Estimates of the Effect of Release from Court Ordered Desegregation on District Racial Composition, Southern Districts Released After 1990.**





**Figure 3: Non-Parametric Estimates of the Effect of Release from Court Ordered Desegregation on White/Black Dissimilarity Index, Southern Districts Released from Court Order After 1990**



**Table 1. District Characteristics, 1990, by Court Order Status**

	<b>Districts Under Order in 1990</b>			Districts Not Under Order in 1990	Dismissed vs Not-Dismissed ( <i>p</i> -value)	Under Order vs Not Under Order ( <i>p</i> -value)
	All	Dismissed by 2009	Not Dismissed by 2009			
White/Black Dissimilarity Index	0.255 (0.194)	0.252 (0.210)	0.257 (0.205)	0.201 (0.167)	0.867	0.000
White/Hispanic Dissimilarity Index	0.338 (0.257)	0.327 (0.236)	0.348 (0.294)	0.245 (0.198)	0.146	0.000
Poor/Non-Poor Dissimilarity Index	0.173 (0.134)	0.168 (0.153)	0.178 (0.146)	0.159 (0.109)	0.905	0.021
Percent Black	0.347 (0.255)	0.339 (0.290)	0.353 (0.264)	0.241 (0.221)	0.850	0.000
Percent White	0.581 (0.286)	0.587 (0.284)	0.576 (0.260)	0.641 (0.249)	0.743	0.000
Percent Free Lunch	0.335 (0.211)	0.311 (0.220)	0.354 (0.215)	0.302 (0.179)	0.001	0.000
Per Pupil Expenditures (in \$1,000s)	4.308 (1.785)	4.421 (1.232)	4.217 (2.125)	4.897 (2.147)	0.208	0.000
<b>Enrollment</b>						
25th Percentile (in 1000s)	3.283	3.384	3.192	2.928		
50th Percentile (in 1000s)	5.252	6.022	4.852	4.506		
75th Percentile (in 1000s)	12.287	16.237	10.817	8.440		
Log (Base 10) District Enrollment	3.860	3.920	3.813	3.743	0.014	0.000
<b>Urbanicity</b>						
Urban	39.8%	28.4%	48.9%	25.2%	0.000	0.000
Suburban	37.9%	44.2%	32.8%	40.5%		
Rural	22.4%	27.4%	18.3%	34.3%		
<b>Region</b>						
South	80.1%	74.9%	84.3%	25.6%	0.002	0.000
Midwest	3.1%	1.9%	4.1%	25.3%		
Northeast	14.3%	20.9%	9.0%	32.2%		
West	2.5%	2.3%	2.6%	16.9%		
<b>Circuit Court</b>						
Circuit Court 1 (MA)	0.4%	0.5%	0.4%	1.9%	0.000	0.000
Circuit Court 2 (NY, CT)	1.0%	0.9%	1.1%	11.4%		
Circuit Court 3 (PA, NJ, DE)	2.5%	2.3%	2.6%	12.4%		
Circuit Court 4 (VA, MD, DC, NC, SC)	10.6%	10.7%	10.4%	10.4%		
Circuit Court 5 (TX, LA, MS)	28.8%	18.6%	36.9%	9.4%		
Circuit Court 6 (MI, OH, KY, TN)	5.2%	6.5%	4.1%	22.5%		
Circuit Court 7 (WI, IL, IN)	5.8%	10.2%	2.2%	5.2%		
Circuit Court 8 (NE, MN, MO, AR)	5.8%	5.6%	6.0%	7.7%		
Circuit Court 9 (CA, NV, AZ)	2.1%	1.9%	2.2%	14.1%		
Circuit Court 10 (UT, CO, KS, OK)	1.0%	1.9%	0.4%	1.9%		
Circuit Court 11 (AL, GA, FL)	36.9%	40.9%	33.6%	5.0%		
<b>N</b>	<b>483</b>	<b>215</b>	<b>268</b>	<b>4491</b>		

Notes: Standard deviations in parentheses. Significance tests are for t-tests or F-tests of the null hypothesis that there is no difference in means between dismissed and non-dismissed districts or between districts under order and those not under order in 1990. The sample includes all districts had at least 2,000 students in 1990 and had at least 5% black student enrollment.

**Table 2: Hazard Model Estimates of the Associations Between District Characteristics and Release From Court Order**

	Model 1	Model 2	Model 3	Model 4	Model 5	Model 6
White/Black Dissimilarity <sup>a</sup>	-0.029 (0.352)	-0.871 (0.461)	-0.810 (0.479)	-0.515 (0.527)	-0.678 (0.500)	-0.918 (0.547)
Percent Black		-1.496 (1.021)	-1.540 (1.156)	0.343 (1.368)	-0.077 (1.249)	2.485 (1.520)
Percent Black <sup>2</sup>		2.735 ** (1.019)	2.460 * (1.064)	0.886 (1.145)	0.300 (1.219)	-0.921 (1.271)
Percent White		0.990 (0.570)	0.221 (0.662)	0.211 (0.862)	-0.621 (0.665)	0.640 (0.993)
Log Enrollment (Base 10)		0.563 ** (0.212)	0.515 * (0.219)	0.568 * (0.255)	0.781 *** (0.228)	1.214 *** (0.303)
Rural		0.611 * (0.245)	0.457 (0.252)	0.057 (0.284)	0.461 (0.249)	0.102 (0.274)
Suburban		0.467 * (0.197)	0.533 ** (0.197)	0.335 (0.206)	0.453 * (0.203)	0.180 (0.215)
Per Pupil Expenditure (\$1,000s)		0.025 (0.014)	0.028 * (0.014)	0.035 * (0.015)	0.029 * (0.014)	0.038 * (0.015)
Percent Free-Lunch		0.108 (0.565)	0.026 (0.589)	0.070 (0.653)	0.465 (0.633)	0.826 (0.741)
Non-South		0.260 (0.195)				
Non-South x Percent Black					-3.646 (2.787)	-10.116 ** (3.496)
Non-South x Percent Black <sup>2</sup>					7.120 ** (2.625)	9.245 ** (2.931)
Non-South x Percent White					3.714 *** (1.073)	-1.797 (1.932)
Non-South x Log Enrollment					-0.494 (0.275)	-1.522 ** (0.503)
Circuit Court 1 (MA)			-0.312 (1.090)		0.682 (1.283)	
Circuit Court 2 (NY, CT)			-0.599 (0.776)		0.453 (1.011)	
Circuit Court 3 (PA, NJ, DE)			-0.373 (0.502)		-0.080 (0.720)	
Circuit Court 4 (VA, MD, DC, NC, SC)			-0.199 (0.249)		-0.314 (0.251)	
Circuit Court 5 (TX, LA, MS)			-0.595 ** (0.205)		-0.745 *** (0.210)	
Circuit Court 6 (MI, OH, KY, TN)			0.225 (0.307)		0.163 (0.350)	
Circuit Court 7 (WI, IL, IN)			0.971 *** (0.291)		0.764 (0.698)	
Circuit Court 8 (NE, MN, MO, AR)			-0.447 (0.328)		-0.844 (0.672)	
Circuit Court 9 (CA, NV, AZ)			-0.612 (0.630)		0.253 (0.991)	
Circuit Court 10 (UT, CO, KS, OK)			1.864 ** (0.611)		2.069 * (0.848)	
Year Dummies	X	X	X	X	X	X
State Fixed Effects				X		X
N	7700	7700	7700	7700	7700	7700
p(F-test) Black		0.006	0.038	0.265	0.929	0.177
p(F-test) BlackxNon-South					0.001	0.006
p(F-test) Circuit Courts			0.000		0.000	

<sup>a</sup> Average white/black dissimilarity of previous two years.

Notes: + p<0.10; \* p<0.05; \*\* p<0.01; \*\*\* p<0.001. Standard errors in parenthesis. The unit of observation is the district-year, and the model is fit using all observations of a district from 1991 through 2009 or the year in which the district is released, whichever comes first.

**Table 3. Estimated Effects of Release From Court Order on District Enrollment and Segregation**

	<b>All Districts</b>		<b>Southern Districts</b>	
	Pre-Dismissal Trend	Post-Dismissal Effect	Pre-Dismissal Trend	Post-Dismissal Effect
<b>District Demographics</b>				
Total Enrollment	48.331 (50.278)	122.880 (135.865)	64.338 (54.413)	106.477 (157.484)
Number of Schools	-0.085 (0.320)	1.236 (0.783)	0.248 (0.273)	1.877 * (0.861)
Percent White	0.005 (0.007)	-0.028 * (0.012)	0.006 (0.007)	-0.024 + (0.013)
Percent Black	-0.007 (0.006)	0.032 ** (0.010)	-0.009 (0.006)	0.028 ** (0.010)
Percent Hispanic	0.002 (0.004)	-0.004 (0.007)	0.002 (0.004)	-0.004 (0.008)
Percent F/R Lunch	0.004 (0.008)	-0.004 (0.013)	0.003 (0.008)	-0.001 (0.014)
N (Districts/District Years)	483/10000	483/10000	387/8000	387/8000
<b>Private School Enrollment</b>				
Total Enrollment			20.280 (232.260)	944.644 (709.192)
Percent White			0.014 (0.022)	-0.034 (0.032)
Percent Black			0.006 (0.016)	-0.003 (0.026)
Percent Hispanic			-0.019 (0.015)	0.027 (0.020)
N (Districts/District Years)			192/1352	192/1352

(cont. next page)

**Table 3, Continued**

<b>White/Black Segregation</b>				
Dissimilarity Index	-0.025 ** (0.009)	0.051 ** (0.017)	-0.027 ** (0.009)	0.064 *** (0.019)
Information Theory Index	-0.020 *** (0.006)	0.046 *** (0.012)	-0.022 *** (0.006)	0.056 *** (0.014)
Black-White Exposure	0.012 (0.007)	-0.036 ** (0.014)	0.013 + (0.008)	-0.031 * (0.015)
N (Districts/District Years)	483/9900	483/9900	387/8032	387/8032
<b>White/Hispanic Segregation</b>				
Dissimilarity Index	-0.003 (0.011)	0.033 (0.021)	-0.005 (0.012)	0.036 (0.024)
Information Theory Index	-0.006 (0.006)	0.034 * (0.013)	-0.008 (0.006)	0.037 * (0.015)
Hispanic-White Exposure	0.003 (0.009)	-0.031 * (0.015)	0.003 (0.010)	-0.023 (0.017)
N (Districts/District Years)	483/9580	483/9580	387/7690	387/7690
<b>White/Non-White Segregation</b>				
Dissimilarity Index	-0.022 * (0.008)	0.044 ** (0.016)	-0.024 ** (0.009)	0.056 ** (0.017)
Information Theory Index	-0.018 ** (0.005)	0.036 *** (0.011)	-0.020 *** (0.006)	0.044 *** (0.012)
Non-White-White Exposure	0.012 + (0.007)	-0.030 * (0.013)	0.014 + (0.008)	-0.027 + (0.014)
N (Districts/District Years)	483/10000	483/10000	387/8042	387/8042
<b>Poor/Non-Poor Segregation</b>				
Dissimilarity Index	-0.008 (0.008)	0.007 (0.012)	-0.006 (0.008)	0.013 (0.013)
Information Theory Index	-0.004 (0.004)	0.006 (0.006)	-0.004 (0.004)	0.007 (0.007)
Poor-Non-Poor Exposure	-0.004 (0.008)	0.001 (0.012)	-0.005 (0.009)	0.004 (0.012)
Black Exposure to Poverty	-0.002 (0.008)	0.005 (0.014)	-0.003 (0.009)	0.010 (0.015)
White Exposure to Poverty	0.011 (0.008)	-0.024 + (0.014)	0.009 (0.009)	-0.023 (0.015)
N (Districts/District Years)	483/7332	483/7332	387/5985	387/5985

Notes: +  $p < 0.05$ ; \*  $p < 0.05$ ; \*\*  $p < 0.01$ ; \*\*\*  $p < 0.001$ . Standard errors in parentheses and are clustered at the district level. All models include district fixed effects and state-by-grade-by-year fixed effects. Models include grade-by-year observations for 483 districts that were under court order as of 1990. 215 of these districts were released by 2009. 387 of the districts under order in 1990 are located in the South and 96 are located outside of the South. 161 Southern and 54 non-Southern districts were released by 2009. There are an average of 14 years of pre-dismissal data for each model and 5 years of post dismissal data for each model for the districts that were released. All coefficients are multiplied by 10, so they indicate the average change in a given outcome over 10 years. The models based on all districts include between 95000 and 130000 district-by-grade-by-year observations and the models based on southern districts include between 77000 and 105000 district-by-grade-by-year observations. The private school models are restricted to southern county-wide districts.

**Table 4. Alternative Model Specifications, Southern Districts**

	Base Model <b>1</b>	Balanced Panel (8 Pre- and 6 Post-Dismissal Years) <b>2</b>	Restricted to Dismissed Districts <b>3</b>	With Year by District Characteristic Interactions <b>4</b>	Weighted by Total Enrollment <b>5</b>	Weighted by Black Enrollment <b>6</b>
<b>District Demographics</b>						
Percent White	-0.024 + (0.013)	0.008 (0.009)	-0.028 + (0.016)	0.007 (0.015)	-0.049 *** (0.013)	-0.045 ** (0.015)
Percent Black	0.028 ** (0.010)	0.000 (0.008)	0.033 ** (0.012)	0.006 (0.012)	0.023 * (0.011)	0.016 (0.013)
Percent F/R Lunch	-0.001 (0.014)	-0.015 (0.015)	0.005 (0.015)	-0.020 (0.014)	-0.006 (0.015)	-0.020 (0.018)
N Districts/District Years	387/8000	245/6187	159/3367	387/8000	387/8000	387/8000
<b>White/Black Segregation</b>						
Dissimilarity Index	0.064 *** (0.019)	0.079 ** (0.027)	0.066 ** (0.022)	0.051 * (0.022)	0.077 *** (0.018)	0.076 *** (0.019)
Information Theory Index	0.056 *** (0.014)	0.067 *** (0.016)	0.057 *** (0.016)	0.045 ** (0.015)	0.063 *** (0.013)	0.060 *** (0.014)
Black-White Exposure	-0.031 * (0.015)	-0.032 * (0.015)	-0.030 + (0.017)	-0.006 (0.017)	-0.057 *** (0.015)	-0.053 *** (0.016)
N Districts/District Years	387/8032	245/6134	159/3356	387/8032	387/8032	387/8000
<b>White/Hispanic Segregation</b>						
Dissimilarity Index	0.036 (0.024)	0.056 + (0.032)	0.035 (0.028)	0.023 (0.029)	0.054 ** (0.019)	0.067 ** (0.022)
N Districts/District Years	387/7690	245/5651	159/3243	387/7690	387/7690	387/7690
<b>White/Non-White Segregation</b>						
Dissimilarity Index	0.056 ** (0.017)	0.057 * (0.027)	0.057 ** (0.020)	0.042 * (0.020)	0.063 *** (0.016)	0.063 *** (0.018)
N Districts/District Years	387/8042	245/6157	159/3366	387/8042	387/8042	387/8000
<b>Poor/Non-Poor Segregation</b>						
Dissimilarity Index	0.013 (0.013)	0.033 + (0.020)	0.005 (0.013)	0.010 (0.017)	0.013 (0.015)	0.011 (0.018)
N Districts/District Years	387/5985	245/4668	159/2430	387/5985	387/5985	387/5985

Notes: +p<.10; \* p<0.05; \*\* p<0.01; \*\*\* p<0.001. Standard errors are in parentheses and clustered at the district level. All models include district fixed effects, state-by-year-by-grade fixed effects, and a linear pre-treatment time term. Model 2 includes 69 dismissed districts while the other models include 159 dismissed districts.

**Appendix Table 1: Number of U.S. School Districts Under Desegregation Orders**

	South	Non-South	Total
Total identified as under order	732	339	1071
HEW plans/actions	53	121	174
Never under order	68	74	142
Total ever under order	611	144	755
Small (<2,000 students)	127	26	153
Total ever under order (>2,000)	484	118	602
Unsure status/date	4	3	7
Dismissed by 1990	93	19	112
Districts (>2,000) still under order in 1990	387	96	483
Dismissed 1991-2009	161	54	215
Districts (>2,000) still under order in Fall 2009	226	42	268

Note: top row of table includes all districts that any of our primary or secondary sources listed as ever having been subject to a court order or HEW action to desegregate. The 142 districts listed as "Never under order" are districts that our research indicates were not ever subject to a court order, despite the fact that they were listed in at least one secondary source as having been under order.

**Appendix Table 2. Alternative Model Specifications, Non-Southern Districts**

	Base Model <b>1</b>	Balanced Panel (8 Pre- and 6 Post-Dismissal Years) <b>2</b>	Restricted to Dismissed Districts <b>3</b>	With Year by District Characteristic Interactions <b>4</b>	Weighted by Total Enrollment <b>5</b>	Weighted by Black Enrollment <b>6</b>
<b>District Demographics</b>						
Percent White	-0.020 0.026	0.014 (0.024)	-0.052 0.054	-0.064 + 0.033	-0.016 0.021	-0.002 0.015
Percent Black	0.021 0.026	-0.017 (0.027)	0.030 0.053	0.061 + 0.032	0.001 0.020	-0.016 0.024
Percent F/R Lunch	0.013 0.033	0.005 (0.036)	-0.041 0.039	-0.009 0.026	0.041 0.051	0.025 0.059
N Districts/District Years	86/1892	70/1483	44/989	86/1892	86/1892	86/1892
<b>White/Black Segregation</b>						
Dissimilarity Index	0.007 0.043	0.110 ** (0.041)	-0.077 0.059	-0.033 0.039	0.057 0.046	0.038 0.066
Information Theory Index	0.017 0.027	0.084 ** (0.031)	-0.043 0.041	-0.020 0.027	0.047 0.030	0.036 0.047
Black-White Exposure	-0.059 * 0.025	-0.018 (0.032)	-0.050 0.052	-0.057 + 0.030	-0.024 0.019	-0.004 0.022
N Districts/District Years	86/1829	70/1435	44/935	86/1829	86/1829	86/1829
<b>White/Hispanic Segregation</b>						
Dissimilarity Index	-0.017 0.038	0.092 * (0.046)	0.035 0.031	0.027 0.041	0.049 0.043	0.067 0.044
N Districts/District Years	86/1842	70/1445	44/948	86/1842	86/1842	86/1842
<b>White/Non-White Segregation</b>						
Dissimilarity Index	-0.003 0.037	0.088 * (0.036)	-0.053 0.056	-0.038 0.034	0.045 0.045	0.028 0.063
N Districts/District Years	86/1890	70/1482	44/987	86/1890	86/1890	86/1890
<b>Poor/Non-Poor Segregation</b>						
Dissimilarity Index	-0.026 0.022	0.065 + (0.038)	0.045 + 0.025	-0.015 0.024	-0.041 0.033	-0.061 0.048
N Districts/District Years	86/1328	70/1083	44/728	86/1328	86/1328	86/1328

Notes: +p<.10; \* p<0.05; \*\* p<0.01; \*\*\* p<0.001. Standard errors are in parentheses and clustered at the district level. All models include district fixed effects, state-by-year-by-grade fixed effects, and a linear pre-treatment time term. Model 2 includes 39 dismissed districts while the other models include 44 dismissed districts.



**Appendix Table 3. Alternative Model Specifications, Southern Districts, Pre-Dismissal Trends**

	Base Model <b>1</b>	Balanced Panel (8 Pre- and 6 Post-Dismissal Years) <b>2</b>	Restricted to Dismissed Districts <b>3</b>	With Year by District Characteristic Interactions <b>4</b>	Weighted by Total Enrollment <b>5</b>	Weighted by Black Enrollment <b>6</b>
<b>District Demographics</b>						
Percent White	0.006 (0.007)	-0.013 (0.009)	-0.028 (0.021)	0.001 (0.008)	0.007 (0.008)	0.001 (0.008)
Percent Black	-0.009 (0.006)	0.007 (0.008)	-0.032 * (0.015)	-0.004 (0.007)	-0.011 + (0.006)	-0.007 (0.007)
Percent F/R Lunch	0.003 (0.008)	0.012 (0.011)	0.042 + (0.022)	0.008 (0.009)	0.019 * (0.009)	0.030 ** (0.011)
N Districts/District Years	387/8000	245/6187	159/3367	387/8000	387/8000	387/8000
<b>White/Black Segregation</b>						
Dissimilarity Index	-0.027 ** (0.009)	-0.035 * (0.016)	0.098 *** (0.026)	-0.023 * (0.010)	-0.028 ** (0.009)	-0.025 * (0.010)
Information Theory Index	-0.022 *** (0.006)	-0.027 ** (0.010)	0.090 *** (0.023)	-0.018 ** (0.007)	-0.023 *** (0.006)	-0.022 (0.007)
Black-White Exposure	0.013 + (0.008)	0.009 (0.011)	-0.073 ** (0.027)	0.006 (0.008)	0.015 + (0.008)	0.010 (0.009)
N Districts/District Years	387/8032	245/6134	159/3356	387/8032	387/8032	387/8000
<b>White/Hispanic Segregation</b>						
Dissimilarity Index	-0.005 (0.012)	-0.031 (0.021)	0.182 *** (0.040)	-0.002 (0.013)	-0.003 (0.011)	-0.003 (0.012)
N Districts/District Years	387/7690	245/5651	159/3243	387/7690	387/7690	387/7690
<b>White/Non-White Segregation</b>						
Dissimilarity Index	-0.024 ** (0.009)	-0.031 * (0.016)	0.093 *** (0.027)	-0.019 + (0.010)	-0.025 ** (0.008)	-0.024 * (0.009)
N Districts/District Years	387/8042	245/6157	159/3366	387/8042	387/8042	387/8000
<b>Poor/Non-Poor Segregation</b>						
Dissimilarity Index	-0.006 (0.008)	-0.027 * (0.012)	0.108 *** (0.023)	-0.004 (0.009)	-0.013 (0.008)	-0.011 (0.010)
N Districts/District Years	387/5985	245/4668	159/2430	387/5985	387/5985	387/5985

Notes: \*p<.10; \* p<0.05; \*\* p<0.01; \*\*\* p<0.001. Standard errors are in parentheses and clustered at the district level. All models include district fixed effects, state-by-year-by-grade fixed effects, and a linear pre-treatment time term. Model 2 includes 69 dismissed districts while the other models include 159 dismissed districts.

**Appendix Table 4. Estimated Effects of Release From Court Order on District Enrollment and Segregation, by Grade Level, Southern School Districts**

	All Grades	Elementary	Middle School	High School
<b>District Demographics</b>				
Total Enrollment	106.477 (157.484)	-0.092 (127.284)	88.904 (191.566)	280.449 (188.507)
Number of Schools	1.877 * (0.861)	2.957 * (1.283)	0.609 (1.155)	1.217 ** (0.449)
Percent White	-0.024 + (0.013)	-0.022 (0.013)	-0.024 + (0.014)	-0.028 + (0.015)
Percent Black	0.028 ** (0.010)	0.025 * (0.010)	0.029 ** (0.011)	0.032 ** (0.012)
Percent Hispanic	-0.004 (0.008)	-0.004 (0.009)	-0.005 (0.008)	-0.004 (0.007)
Percent F/R Lunch	-0.001 (0.014)	-0.004 (0.014)	-0.001 (0.014)	0.005 (0.015)
N Districts/District Years	387/8043	387/8043	387/8043	387/8043
<b>White/Black Segregation</b>				
Dissimilarity Index	0.064 *** (0.019)	0.098 *** (0.024)	0.049 * (0.021)	0.030 (0.023)
Information Theory Index	0.056 *** (0.014)	0.077 *** (0.018)	0.047 *** (0.013)	0.032 * (0.014)
Black-White Exposure	-0.031 * (0.015)	-0.037 * (0.015)	-0.028 + (0.014)	-0.024 (0.017)
N Districts/District Years	387/8032	387/8032	387/8032	387/8032
<b>White/Hispanic Segregation</b>				
Dissimilarity Index	0.036 (0.024)	0.059 * (0.030)	-0.009 (0.031)	0.020 (0.023)
Information Theory Index	0.037 * (0.015)	0.061 ** (0.020)	0.015 (0.016)	0.012 (0.011)
Hispanic-White Exposure	-0.023 (0.017)	-0.030 (0.019)	-0.019 (0.018)	-0.023 (0.018)
N Districts/District Years	387/7690	387/7690	387/7690	387/7690
<b>White/Non-White Segregation</b>				
Dissimilarity Index	0.056 ** (0.017)	0.086 *** (0.024)	0.039 * (0.019)	0.026 (0.021)
Information Theory Index	0.044 *** (0.012)	0.061 *** (0.016)	0.036 ** (0.012)	0.026 * (0.011)
Non-White-White Exposure	-0.027 + (0.014)	-0.029 + (0.015)	-0.024 + (0.014)	-0.025 + (0.015)
N Districts/District Years	387/8042	387/8042	387/8042	387/8042
<b>Poor/Non-Poor Segregation</b>				
Dissimilarity Index	0.013 (0.013)	0.027 (0.018)	-0.007 (0.017)	0.003 (0.015)
Information Theory Index	0.007 (0.007)	0.012 (0.009)	0.002 (0.008)	0.004 (0.008)
Poor-Non-Poor Exposure	0.004 (0.012)	0.005 (0.013)	0.009 (0.013)	-0.001 (0.015)
Black Exposure to Poverty	0.010 (0.015)	0.015 (0.016)	0.007 (0.015)	0.007 (0.016)
White Exposure to Poverty	-0.023 (0.015)	-0.034 * (0.015)	-0.017 (0.016)	-0.009 (0.016)
N Districts/District Years	387/5985	387/5985	387/5985	387/5985

Notes: + p<0.05; \* p<0.05; \*\* p<0.01; \*\*\* p<0.001. Standard errors in parentheses and are clustered at the district level. All models include district fixed effects and state-by-grade-by-year fixed effects. Models include grade-by-year observations for 483 districts that were under court order as of 1990. 215 of these districts were released by 2009. 387 of the districts under order in 1990 are located in the South and 96 are located outside of the South. 161 Southern and 54 non-Southern districts were released by 2009. There are an average of 14 years of pre-dismissal data for each model and 5 years of post dismissal data for the districts that were released. All coefficients are multiplied by 10, so they indicate the average change in a given outcome over 10 years. The models based on all districts include between 95000 and 130000 district-by-grade-by-year observations and the models based on southern districts include between 77000 and 105000 district-by-grade-by-year observations.

**Appendix Table 5. Comparison of 1990 Values for Southern County-Wide and Non-County-Wide Districts**

	<u>Non County-Wide Districts</u>		<u>County-Wide Districts</u>	
	<u>Mean</u>	<u>SD</u>	<u>Mean</u>	<u>SD</u>
White/Black Dissimilarity Index	0.243	0.237	0.306	0.185
White/Hispanic Dissimilarity Index	0.371	0.277	0.425	0.240
Poor/Non-Poor Dissimilarity Index	0.197	0.152	0.226	0.139
Percent Black	0.363	0.265	0.421	0.235
Percent White	0.587	0.260	0.553	0.226
Percent Free Lunch	0.393	0.234	0.426	0.170
Per Pupil Expenditures (in \$1,000s)	3,740	863	4,142	975
Total Enrollment	10,739	17,833	17,889	34,640
Number of Schools	7.09	13.33	8.65	16.51
N (Total)	200	200	181	181
N (Released)	76	76	82	82

**Appendix Table 6. Ten Year Change in School Segregation Among Districts Never Under Court Order, 1999-2009**

	<u>South</u>		<u>Non-South</u>		<u>All</u>	
	<b>Mean</b>	<b>SD</b>	<b>Mean</b>	<b>SD</b>	<b>Mean</b>	<b>SD</b>
Black-White Dissimilarity	0.006	0.053	-0.002	0.071	-0.001	0.076
Black-White Information Theory	0.003	0.030	-0.002	0.039	0.001	0.041
Black-White Exposure	-0.083	0.072	-0.095	0.079	-0.099	0.085
Hispanic-White Dissimilarity	-0.030	0.086	-0.015	0.095	-0.022	0.094
Hispanic-White Information Theory	-0.008	0.038	-0.003	0.045	-0.005	0.044
Hispanic-White Exposure	-0.103	0.097	-0.110	0.085	-0.112	0.093
Non White-Black Dissimilarity	0.000	0.025	-0.002	0.034	0.000	0.034
Non White-Black Dissimilarity	0.004	0.049	0.002	0.070	0.004	0.069
Non White-White Exposure	-0.082	0.072	-0.097	0.079	-0.099	0.085
Poor-Non Poor Dissimilarity	0.002	0.026	-0.002	0.039	0.001	0.035
Poor-Non Poor Information Theory	0.008	0.065	-0.006	0.072	0.001	0.072
Poor-Non Poor Exposure	-0.126	0.081	-0.120	0.099	-0.122	0.094

Includes U.S. districts never under court order with 2000 or more students and 5% or larger black enrollment in 2009. Segregation values are averaged over grades.

**Appendix Table 7: School and Residential Segregation, Southern County-Wide School Districts, 1990-2000**

	1990	2000
<b>School Segregation</b>		
White/Black Dissimilarity Index	0.33	0.35
White/Black Information Theory Index	0.14	0.15
White/Hispanic Dissimilarity Index	0.44	0.43
White/Hispanic Information Theory Index	0.12	0.15
White/Non-White Dissimilarity Index	0.31	0.33
White/Non-White Information Theory Index	0.13	0.14
N (District-Grades)	2366	2366
N (Districts)	182	182
<b>Residential Segregation</b>		
White/Black Dissimilarity Index	0.40	0.40
White/Black Information Theory Index	0.19	0.19
White/Hispanic Dissimilarity Index	0.28	0.28
White/Hispanic Information Theory Index	0.06	0.07
White/Non-White Dissimilarity Index	0.38	0.35
White/Non-White Information Theory Index	0.17	0.15
N (Districts)	182	182

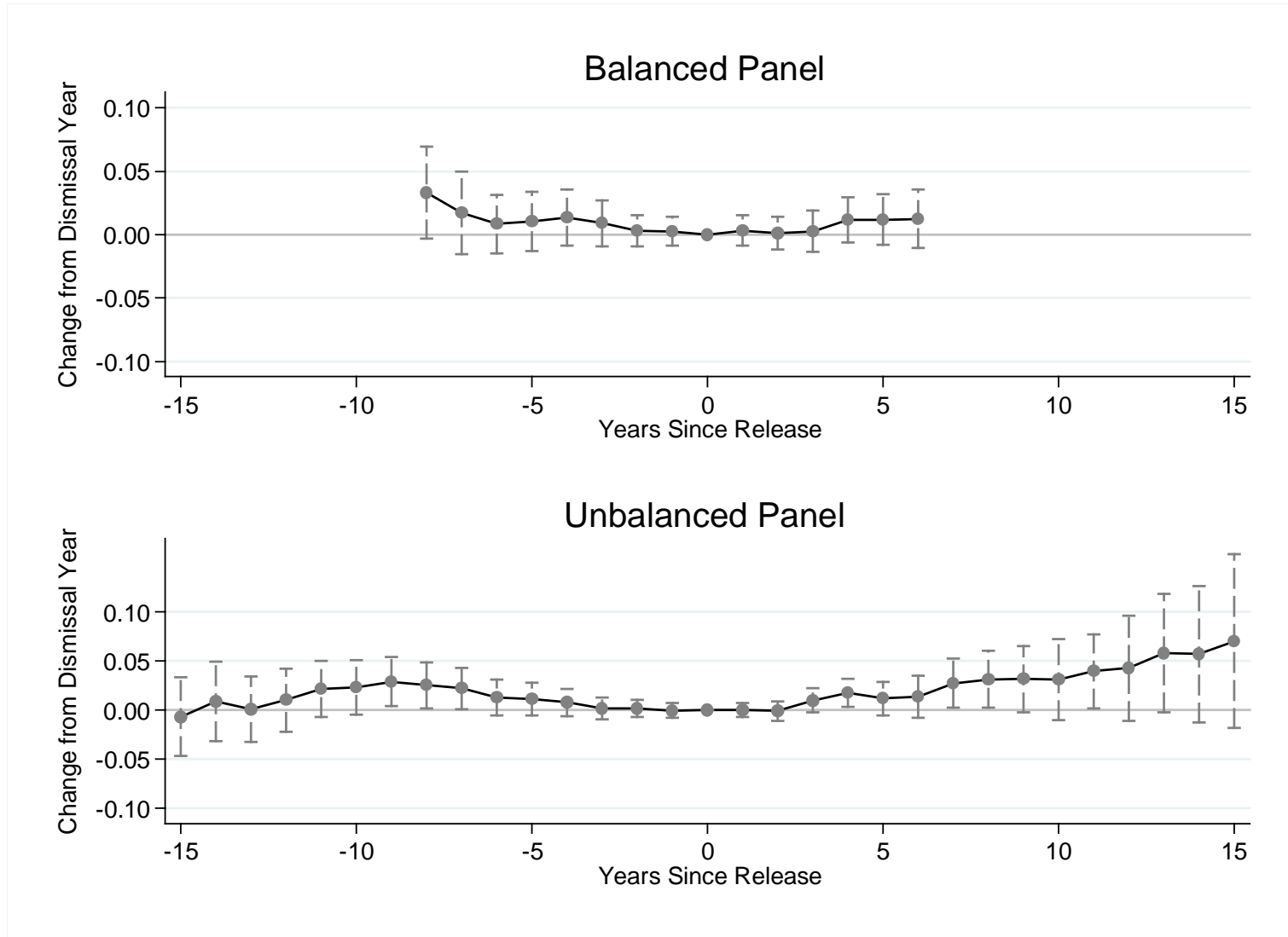
Notes: School segregation is computed as the average between-school segregation across grades within a district. Residential segregation is between-tract segregation of school-age children (ages 5-17). Table includes county-wide districts in the South that were under a court-ordered desegregation plan in 1990, except districts where all grades had only one school in 1990.

**Appendix Table 8: Estimated Variation in the Effect of Release from Court Order on the Black-White Dissimilarity Index, Southern Districts**

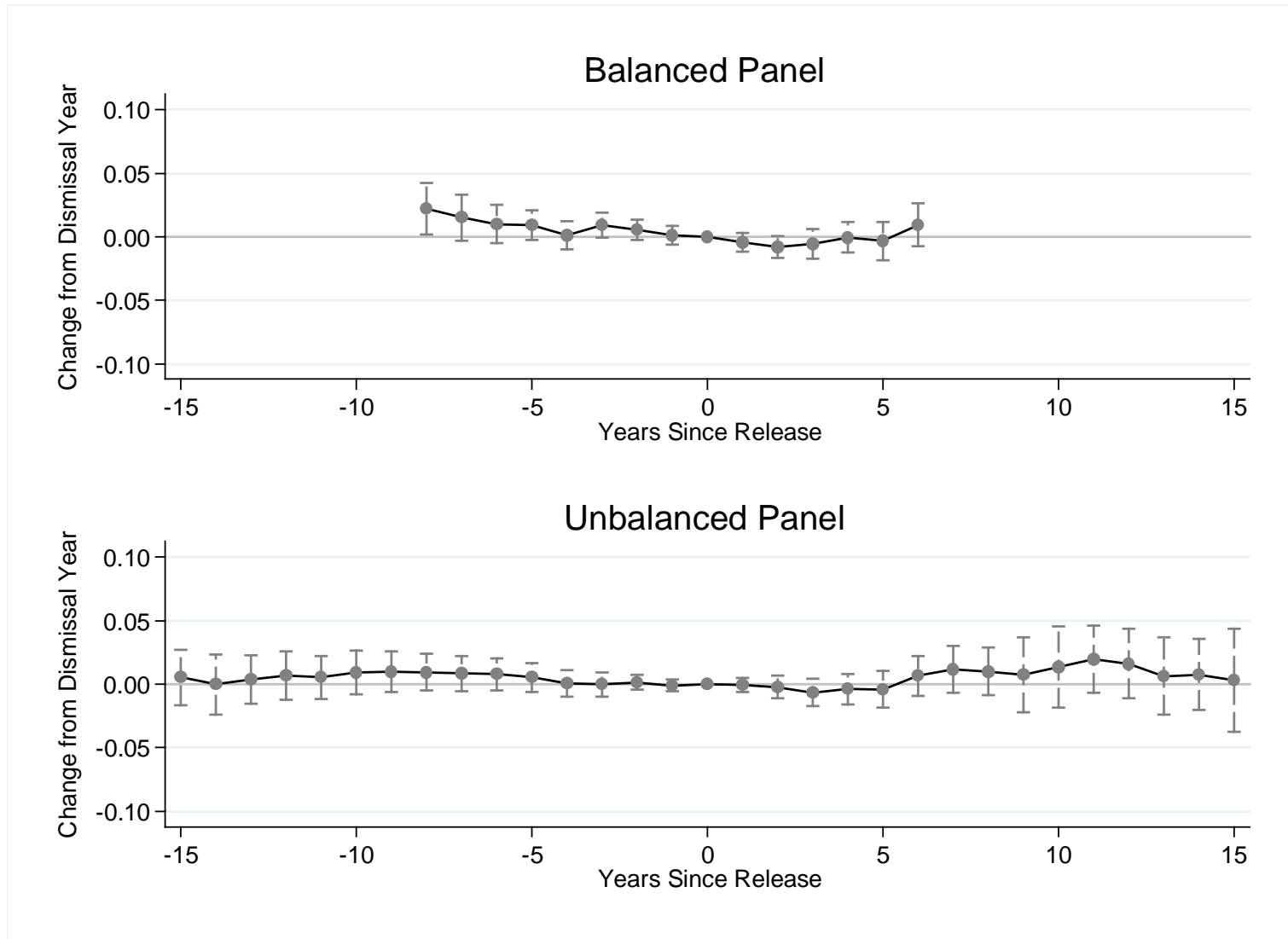
	<u>All Southern Districts</u>		<u>Southern County-Wide Districts</u>				
	1	2	3	4	5	6	7
Pre-Dismissal Trend	-0.031 ** (0.010)	-0.021 * (0.009)	-0.033 * (0.015)	-0.030 + (0.016)	-0.030 + (0.015)	-0.025 + (0.014)	-0.025 + (0.014)
Post-Dismissal Effect	0.072 *** (0.018)	0.022 (0.017)	0.091 *** (0.021)	0.065 ** (0.022)	0.065 ** (0.023)	0.031 (0.026)	0.027 (0.026)
Pre-Dismissal Trend*Log Enrollment		0.045 * (0.018)				0.045 (0.030)	-0.165 + (0.094)
Post-Dismissal Trend*Log Enrollment		0.090 * (0.037)				0.076 (0.048)	-0.105 (0.145)
Pre-Dismissal Trend*Proportion Black		0.001 (0.034)				0.045 (0.075)	-0.036 (0.072)
Post-Dismissal Trend*Proportion Black		0.149 * (0.071)				0.089 (0.112)	0.171 (0.150)
Pre-Dismissal Trend*School Dissimilarity Index	-0.125 *** (0.036)	-0.147 *** (0.044)	-0.134 * (0.066)		-0.147 + (0.078)	-0.177 + (0.094)	0.053 (0.033)
Post-Dismissal Effect*School Dissimilarity Index	0.060 (0.073)	-0.192 * (0.096)	0.122 (0.102)		0.016 (0.126)	-0.081 (0.152)	0.035 (0.060)
Pre-Dismissal Trend*Residential Dissimilarity Index				-0.075 (0.060)	0.017 (0.070)		0.046 (0.074)
Post-Dismissal Effect*Residential Dissimilarity Index				0.251 * (0.102)	0.208 + (0.119)		0.089 (0.106)
N (Districts/District Years)	387/8032	387/8032	200/3800	200/3800	200/3800	200/3800	200/3800

Notes: + p<.10; \* p<0.05; \*\* p<0.01; \*\*\* p<0.001. Standard errors are in parentheses and are clustered at the district level. All models include district fixed effects and state-by-grade-by-year fixed effects. District characteristics are centered on the 1990 grade-specific mean for dismissed Southern districts before computing the interaction terms.

**Appendix Figure 1: : Non-parametric Estimates of the Effect of Release from Court Ordered Desegregation on White/Hispanic Dissimilarity Index, Southern Districts Released from Court Order After 1990**

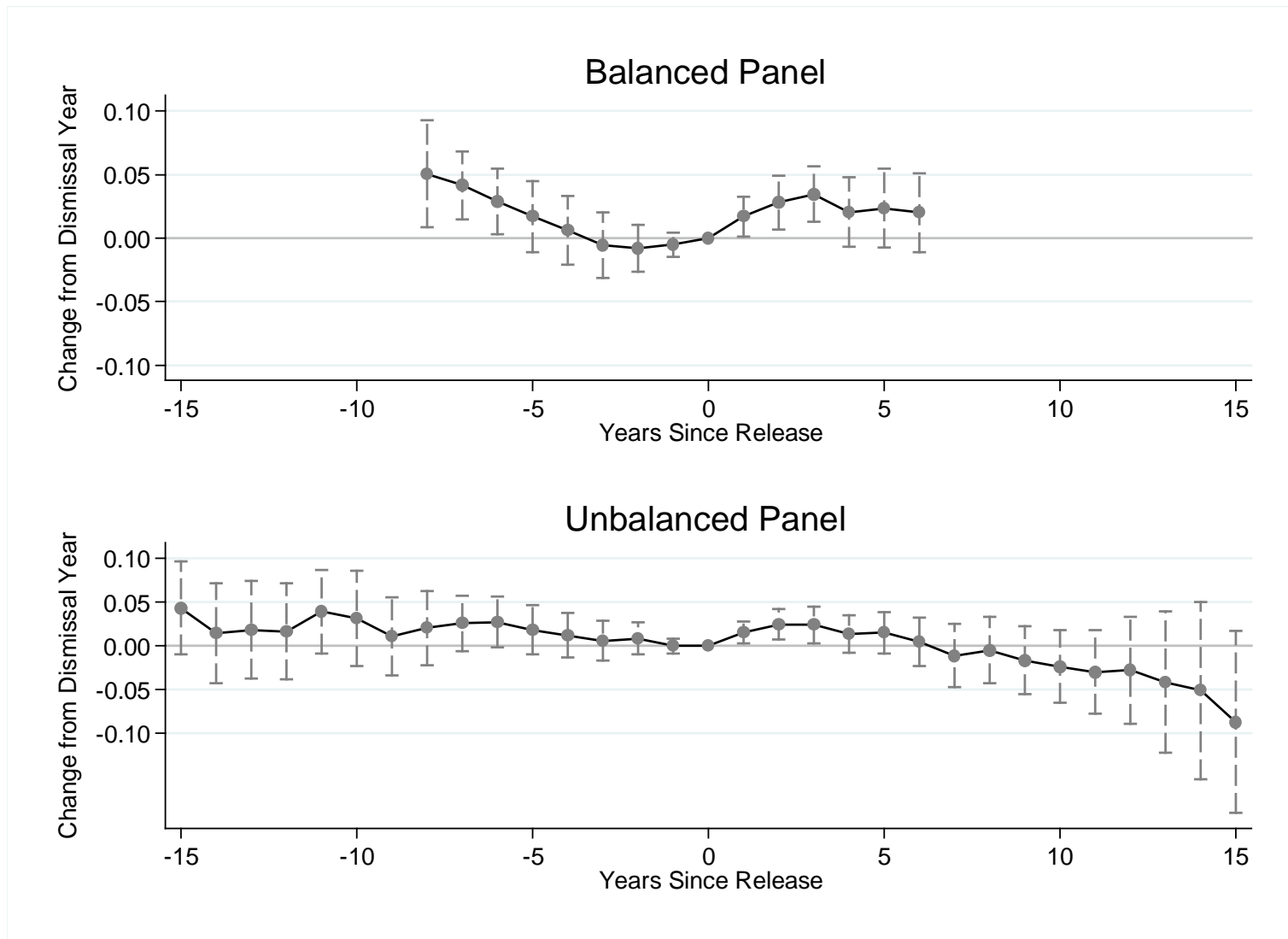


**Appendix Figure 2: Non-Parametric Estimates of the Effect of Release from Court Ordered Desegregation on Non-Poor/Poor Dissimilarity Index, Southern Districts Released from Court Order After 1990**

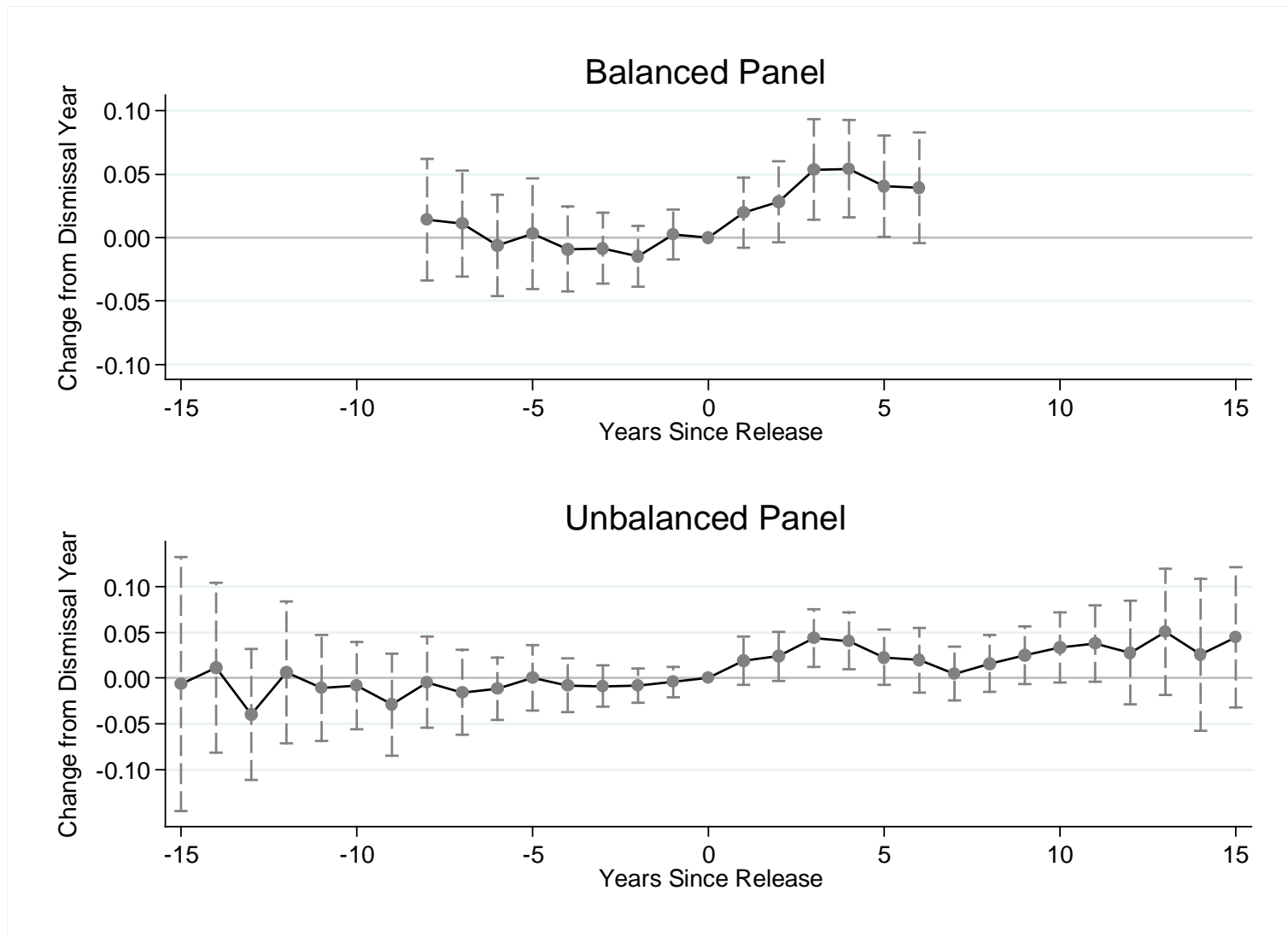




**Appendix Figure 3: Non-Parametric Estimates of the Effect of Release from Court Ordered Desegregation on White/Black Dissimilarity Index, Non-Southern Districts Released from Court Order After 1990**



**Appendix Figure 4: Non-Parametric Estimates of the Effect of Release from Court Ordered Desegregation on White/Hispanic Dissimilarity Index, Non-Southern Districts Released from Court Order After 1990**



**Appendix Figure 5: Non-Parametric Estimates of the Effect of Release from Court Ordered Desegregation on Non-Poor/Poor Dissimilarity Index, Non-Southern Districts Released from Court Order After 1990**

