Educational Evaluation and Policy Analysis
Month 201X, Vol. XX, No. X, pp. 1–26
DOI: 10.3102/0162373717725804

Ending to What End? The Impact of the Termination of Court-Desegregation Orders on Residential Segregation and School Dropout Rates

David D. Liebowitz
Organisation for Economic Co-Operation and Development

In the early 1990s, the Supreme Court established standards to facilitate the release of school districts from racial desegregation orders. Over the next two decades, federal courts declared almost half of all districts under court order in 1991 to be "unitary"—that is, to have met their obligations to eliminate dual systems of education. I leverage a comprehensive dataset of all districts that were under court order in 1991 to assess the national effects of the termination of desegregation orders on indices of residential-racial segregation and high-school dropout rates. I conclude that the release from court orders moderately increased the short-term rates of Hispanic–White residential segregation. Furthermore, the declaration of districts as unitary increased rates of 16- to 19-year-old school dropouts by around 1 percentage point for Blacks, particularly those residing outside the South, and 3 percentage points for Hispanics.

Keywords: *education policy, desegregation, residential segregation, educational attainment*

In a series of rulings between 1991 and 1995, the Supreme Court established standards to facilitate the release of local school districts from court-ordered racial desegregation plans. Prior to 1991, the Court relied on seven standards first outlined in *Green v. County School Board*, 391 U.S. 430 (1968) that required districts to demonstrate that the last vestiges of segregation had been eliminated "root and branch" before they could be released from court order. However in the early 1990s, the Supreme Court ruled in a series of three cases that district courts should apply more lenient standards to dismiss a desegregation order. In *Board of Ed. of Oklahoma City v. Dowell*, 498 U.S. 237 (1991), the Court found that if a school district had operated in good faith, demonstrated successful efforts to meet court mandates, and eliminated the last vestiges of discrimination, it would be declared "unitary," or no longer operating a dual system of education, and would be released from its court order.

One year after *Dowell*, Justice Kennedy argued in *Freeman v. Pitts*, 503 U.S. 495 (1992) that

> where resegregation is a product not of state action but of private choices, it does not have constitutional implications . . . Residential housing choices and their attendant effects on the racial composition of schools present an ever changing pattern, one difficult to address through judicial remedies.

If districts could demonstrate that they had made incremental efforts to resolve one or more of the seven criteria from *Green*, the supervising district court could release them from obligations related to that factor. Finally, in *Missouri v. Jenkins*, 515 U.S. 79 (1995), the Court ruled that districts need to only bring the non-White victims of past discrimination back to the status they would have held had the discrimination not occurred, not to full equality.

In this article, I extend prior research on the impact of the end of court-ordered desegregation

in a single school district, or within a limited dataset, to a comprehensive national sample of 480 districts under court order in 1991. Specifically, I investigate whether there is evidence of segregative private actions in response to court-mandated shifts in student-assignment policies in these districts. Furthermore, I estimate the causal impact these shifts in assignment policy have had on district-wide high-school dropout rates. I contend that whether and when school districts were released from court-desegregation orders was effectively exogenous, and consequently created a natural experiment upon which I can capitalize. I rely on a difference-in-differences approach to obtain an unbiased estimate of the causal effects of ending race-based student-assignment policies on residential segregation and high-school completion.

I do not find that the release of districts from court-desegregation orders caused an increase in Black residential segregation. In an innovation from prior research, I explore the impact of unitary-status declarations on another historically disadvantaged minority group who were a target of some initial desegregation plans: Hispanics.[1] I conclude that the end of court-desegregation orders resulted in an increase in Hispanic residential segregation for the first 3 years after districts' release, followed by a return to preunitary secular trends. Most importantly, the end of the desegregation plans increased the dropout rate for nonsouthern Black and Hispanic residents of these districts aged 16 to 19.

Context and Theory

Historical and Legal Context

In the landmark civil rights case, *Swann v. Charlotte-Mecklenburg Board of Ed.*, 401 U.S. 1 (1971), the U.S. Supreme Court ruled that federal courts could remedy racial segregation in schools by ordering school districts to take affirmative steps, such as rezoning attendance boundaries and transporting students across neighborhoods by bus, to eliminate all vestiges of segregation. The impact of these desegregation orders on the extent of school-level racial integration was substantial. Using data from a sample of 108 districts collected for a report of the U.S. Commission on Civil Rights, Reber (2005) found that measures of between-school segregation fell substantially in school districts under desegregation orders. Several studies have found positive effects of desegregation on Black students' high-school graduation rates (Guryan, 2004; Johnson, 2015), postsecondary labor-market outcomes (Crain & Strauss, 1985), adult earnings, incarceration and health outcomes (Johnson, 2015), homicide arrests and victimization (Weiner, Lutz, & Ludwig, 2011), and interracial prejudice (Pettigrew & Tropp, 2006).

Though *Keyes v. School District No. 1, Denver*, 412 U.S. 189 (1973) found an obligation to implement desegregation plans for Hispanics even if there was no evidence of de jure segregation, few student-assignment plans explicitly targeted Hispanics for remedy. However, by the nature of their residence within desegregated districts, they experienced some of the benefits of desegregation plans. Interestingly, as both my data and those of Reardon, Grewal, Kalogrides, and Greenberg (2012) demonstrate, the rates of residential segregation in 1990, 2000, and (in my article) 2010 were higher for Blacks than for Hispanics. However, Reardon et al. demonstrate that Hispanics were more racially isolated in schools during this time than Blacks. Thus, if intensive rates of school segregation are an important pathway for the hypothesized effects on school attainment, I posit that the effect of court release from desegregation orders on school-based outcomes may be more pronounced for Hispanics than for Blacks.

Despite the promising early successes of desegregation, these policies were economically costly and politically unpopular. Starting in 1991, the Supreme Court decided a series of cases that made it easier for lower courts to conclude that school districts had met their burden of eliminating two-track systems of schools and could be declared "unitary." In the subsequent 20 years, federal courts declared hundreds of school districts unitary—either as a result of school boards seeking release from court supervision, federal judges clearing their dockets of desegregation cases, or private parties filing suit to have the desegregation order lifted.

The 2002 release of the Charlotte-Mecklenburg Schools (CMS) from its desegregation order has yielded rich evidence on the causal impacts of a unitary-status declaration on a variety of outcomes in a single metropolitan region. Clotfelter, Ladd,

and Vigdor (2008) demonstrated that the unitary-status declaration in CMS increased racial segregation both between schools and between classrooms within a school. In addition, Mickelson, Smith, and Southworth (2009) found that the declaration caused an increase in school-level socioeconomic segregation and a decline in the overall academic performance of both White and Black students. Jackson (2009) documented increased sorting of more effective teachers to nonminority students; however, Vigdor (2011) observed no effect of the policy change on the gap between Black and White students in average test scores. Liebowitz and Page (2014) concluded that the declaration increased segregative residential moves among White families, and Billings, Deming, and Rockoff (2014) demonstrated that it increased criminal activity for poor, minority males, while increasing high-school graduation and college-matriculation rates for White students.

Although researchers have examined North Carolina extensively, the available literature is thinner outside of this state. Using data on the 100 largest districts in the South and Border states, Clotfelter, Ladd, and Vigdor (2006) found that segregation would have declined in some of these districts were it not for unitary-status declarations. In this study, I build substantially off the work of Lutz (2011) and Reardon et al. (2012) to explore the impact of the release of districts from court order at a national level. Using Common Core Data (CCD) from 1987 through 2006, Lutz showed that when federal courts released school districts from desegregation orders, indices of school segregation rose. He also used data from the 1990 and 2000 Censuses to conclude that the end of desegregation increased dropout rates for Black students outside the South census region. Lutz's study provides the initial motivation for this analysis of dropout rates, but his sample contained an incomplete group of 98 districts from among the 480 districts that were under court order in 1991. Furthermore, less than a third of all districts under court order in 1991 were released by the end of Lutz's window of analysis. Changes in the overall status of residential segregation and school dropout rates may take time to manifest, so I can capitalize on the release of 2010 Census data to estimate these long-term effects. Finally, Lutz focused exclusively on school-based outcomes for Black and White students, whereas I extend the analysis to Hispanics, the largest minority group attending U.S. schools.

Using a more comprehensive set of all districts under court order in 1991, Reardon et al. (2012) concluded that unitary declarations increased school segregation nationwide. Reardon and his coauthors also used levels of residential segregation in a subsample of 182 countywide school districts in 1990 as a covariate to assess whether districts with higher starting levels of residential segregation experienced higher rates of school segregation after release from court-desegregation orders. They did not, however, explore whether unitary declarations affected residential patterns or school completion rates over this period. Thus, no study has explored the impact of unitary-status declarations on either residential segregation or educational success for all racial/ethnic groups with a full sample of districts under court-desegregation order.

Dismissal Processes

Legal appeals from school districts, parent and community groups, judges, school boards, and the Department of Justice can all lead to a declaration of unitary status (Lutz, 2011; Reardon et al., 2012). In the 1990s, local actors often initiated the judicial review of their court orders. Since the 2000s, however, the Educational Opportunities Section (EOS) of the Civil Rights Division of the Department of Justice has begun several comprehensive analyses of desegregation-related data in districts under court order. When the EOS determines that a district has made satisfactory steps toward desegregation, it will partner with a local district and file a joint motion for unitary status (Reardon et al., 2012). Once districts are dismissed from court order, they typically move quickly to implement a new student-assignment system. In my sample, the mean difference between the year in which the district was released from court order and the first fall in which the district implemented a new student-assignment policy was 0.20 years for the 201 districts that were released from court order and for which information on the implementation of a new student-assignment policy was available. Of these districts, 169 changed their

assignment policy in the same year they were declared unitary. If the hypothesized effects of the declaration of unitary status are accurate, I am likely to find a discontinuous impact in the immediate aftermath of the release from court order.

Lindseth (2002) found that due to legal standards in place for the majority of my sample years, almost no districts used race as a factor in their postunitary student-assignment policies, and most localities implemented some form of neighborhood schooling. Some districts began a form of school choice, in particular magnet schools, but since 1999 unitary districts in the Fourth Circuit (10% of my sample) were explicitly precluded from using race in student assignment. Since 2007, most school districts have interpreted the *Parents Involved in Community Schools (PICS) v. Seattle School District No. 1*, 551 U.S. 701 (2007) decision to extend this prohibition nationwide.

Race-Based Student-Assignment Policies and Housing Preferences

There is strong evidence that the court-desegregation orders of the late 1960s and early 1970s contributed significantly to increases in residential segregation. Boustan (2012) compared housing prices immediately on either side of school-district boundaries and concluded that desegregation orders led to declines in demand for housing in urban school districts with high concentrations of minority students. Clotfelter (2004) presented descriptive evidence that White families with school-aged children moved out of jurisdictions with desegregated schools at a faster rate than White households without children. In addition, metropolitan regions consisting of smaller school districts were more likely to experience relocation of White families following desegregation orders, because smaller districts permitted families to sort themselves more easily based on race (Reber, 2005). Baum-Snow and Lutz (2011) decomposed trends in school segregation into two causes—migration to suburban districts and enrollment in private school—and concluded that increases in White migration and declines in Black migration to suburban communities were the primary drivers of these phenomena.

I theorize that families with school-aged children select their residence as a function of their personal characteristics, in combination with an assessment of the school-related amenities available to that residence and all other nonschool amenities to which that home entitles them, subject to their household-budget constraint. My difference-in-differences analytic strategy will seek to isolate the portion of a family's housing choice influenced by the race-based schooling factors postunitary declaration from both the starting values of other school and neighborhood characteristics and the secular trends of changing economic and social conditions over a 20-year period. A unitary declaration could affect within-district segregation levels in two ways: First, the release from court order and the accompanying new student-assignment policy could induce the re-sorting of households within a district. Second, the unitary declaration could induce between-district moves that affect segregation levels both between and within districts. Within-district segregation levels are the only outcome in my analysis, and so I am unable to distinguish these two processes. However, I test whether metropolitan areas that present more substantial geographic obstacles to between-district migration exhibit differential residential segregation responses to unitary-status declaration. I present a more complete formal housing preference model in Appendix A (available in the online version of the journal).

Race-Based Student-Assignment Policies and Educational Attainment

The preponderance of the evidence suggests that for Black and Hispanic children, there is an independent causal benefit to attending a school (cf. Billings et al., 2014; Guryan, 2004; Hanushek, Kain, & Rivkin, 2009; Johnson, 2015; Lutz, 2011; Pettigrew & Tropp, 2006; Vigdor & Ludwig, 2008; Weiner et al., 2011) and living in a neighborhood (cf. Ananat, 2007; Borjas, 1995; Card & Rothstein, 2007; Chetty, Hendren, & Katz, 2016; Cutler & Glaeser, 1997; Darden, Rahbar, Jezierski, Li, & Velie, 2009; Schwartz, 2010; Weinberg, 2000) with children of different racial backgrounds. Theorists and jurists have advanced various explanations for these benefits. The allocation of more resources to integrated

schools can increase opportunities to learn (Clotfelter, 2004); the creation of networks of high-social-capital peers can increase access to labor-market opportunities (cf. Bayer, Ross, & Topa, 2008); social contact among racial groups can decrease negative stereotypes (cf. Allport, 1954); exposure to students from multiple racial and cultural backgrounds prepares students for productive careers and citizenship in a pluralistic society (cf. *Grutter v. Bollinger*, 596 U.S. 306 (2003)); and peer effects can limit opportunities to learn in highly segregated environments (cf. Aizer, 2008; Angrist & Lang, 2004; Carrell & Hoekstra, 2010; Hoxby & Weingarth, 2005).

From this evidence, I theorize that ending student-assignment policies intended to generate diverse learning environments will lower average educational attainment for Black and Hispanic students affected by the policy. Based on prior research by Lutz (2011) and Reardon et al. (2012), I anticipate that the peer make-up of schools will change in a discontinuous way for students in school districts that are declared unitary, and that this will generate deterioration in the learning environment and worse school outcomes.

Research Questions

Boustan (2012), Reber (2005), and Baum-Snow and Lutz (2011) demonstrated that court-ordered desegregation of schools in the 1960s and 1970s led to White flight. Guryan (2004) and Johnson (2015) showed that the same orders increased the rate at which Blacks completed high school. I investigate what happened to these patterns when courts released school districts from long-standing desegregation requirements in the 1990s and 2000s. Scholars have attempted to examine these patterns in a subset of affected school districts, but none have yet done so with a complete national sample. Nor have they examined all affected populations of children. Thus, I seek to answer the following pair of linked research questions in my study:

Research Question 1: Did the end of court-ordered, race-based student-assignment policies increase levels of residential-racial segregation in affected school districts?

Research Question 2: Did the end of court-ordered, race-based student-assignment policies increase rates of high-school dropouts in affected school districts?

Research Design

To estimate the causal effect of the change in student-assignment policies on residential and school outcomes, I capitalize on the natural experiment induced by the policy disruption. Under this approach, I treat court declarations of unitary status as effectively exogenous disruptions in school districts' student-assignment policies, independent of any secular changes in residential segregation or high-school completion rates. I compare levels of residential-racial segregation and high-school status dropout rates in school districts that were released from court-desegregation order over a 20-year period to the levels of those same outcomes in school districts that were not released during the same time period. I analyze these data using difference-in-differences estimation, implemented in a regression framework.

To justify the claim that the change in student-assignment policy is the causal mechanism for changes in residential segregation or high-school completion, I provide evidence to justify my assumption that there are no unobserved differences between districts that were, and were not, declared unitary. Table 1 presents summary statistics from 1990 for three types of school districts that were under court order in 1991 in my sample: (a) districts that were released from court order between 1991 and 2000, (b) districts released between 2000 and 2010, and (c) districts never dismissed from court order. The table reveals that districts that were declared unitary differed in some ways in 1990 from those that were never declared unitary: They were more likely to be in the South census region, and they had a higher starting level of Hispanic residential segregation. The table permits some analysis of whether and when a district was declared unitary was, in fact, unrelated to observable differences, and therefore a truly exogenous shock. Although the difference-in-differences framework accounts for different starting values of all of these characteristics, it is important to assess whether certain characteristics of districts that were dismissed may have both led to differences in outcomes for these districts *and* made them more likely to be

TABLE 1

*School-District Characteristics in 1990, by Whether and When They Were Declared Unitary (*N *= 480)*

	Dismissed 1991–2000	Dismissed 2001–2010	Never dismissed
% White residents	0.703	0.699	0.621
	(0.027)	(0.023)	(0.033)
% Black residents	0.239	0.243	0.248
	(0.027)	(0.022)	(0.024)
% Hispanic residents	0.074*	0.123	0.165
	(0.017)	(0.060)	(0.042)
Gini median household value	0.254	0.213	0.244
	(0.008)	(0.012)	(0.014)
District-to-MSA area	0.238	0.483	0.256
	(0.079)	(0.200)	(0.028)
South	0.661*	0.796*	0.357
	(0.087)	(0.072)	(0.107)
Black–White dissimilarity index	0.677	0.622	0.678
	(0.017)	(0.021)	(0.031)
Hispanic–White dissimilarity index	0.361*	0.317*	0.500
	(0.019)	(0.020)	(0.031)
Black isolation correlation index	0.483	0.417	0.459
	(0.029)	(0.028)	(0.041)
Hispanic isolation correlation index	0.087*	0.110	0.211
	(0.022)	(0.045)	(0.040)
% dropout	0.145	0.130	0.145
	(0.007)	(0.006)	(0.009)
% dropout White	0.143	0.127	0.125
	(0.010)	(0.006)	(0.009)
% dropout Black	0.143	0.133	0.144
	(0.006)	(0.005)	(0.004)
% dropout Hispanic	0.209	0.176	0.207
	(0.016)	(0.016)	(0.015)
Number of observations	76	139	265

Note. Each cell is a 1990 school-district mean, weighted by total number of residents. Standard deviations are in parentheses. "*" signifies that the mean in column 1 or 2 is statistically distinguishable at the 95% confidence level from the mean in column 3. MSA = Metropolitan or Micropolitan Statistical Area.

dismissed. Table 1 provides no particular evidence that the districts not declared unitary are fundamentally unlike the unitary districts. The nonunitary districts are nearly statistically indistinguishable in terms of the starting proportion of White, Black, or Hispanic residents, their initial dropout rates, and other metropolitan characteristics. Neither are there meaningful differences between districts dismissed in the first or second 10 years under examination.

In addition to finding minimal differences in the 1990 characteristics of districts that were and were not declared unitary, I find no differential trends in residential segregation or dropout rates for districts that were declared unitary. In Appendix B: Table S1 (available in the online version of the journal), I present estimates of my outcomes of interest in the years before districts were declared unitary. Other than for the Black–White/Asian residential segregation rate, I find no evidence that districts that were to be declared unitary were experiencing increases in their segregation or dropout rates compared with nonunitary districts. In fact, the Hispanic dropout rate appeared to be declining compared with nonunitary districts prior to the end of the court order.

Other factors could both make some districts more likely to be released from court order, and to experience changes in their neighborhood and schooling outcomes. Reardon and coauthors (2012) showed that while Circuit court jurisdiction, size, and Northern racial composition are predictive of dismissal, demographic and segregation trends are not. While I use the same sample as Reardon et al., I have census, rather than school, data. I find signs that 1990 levels of residential segregation and school completion trends, particularly for Hispanics, are predictive of dismissal (Appendix B: Table S2, available in the online version of the journal). To address this concern, my identification strategy relies, in part, on differences in the timing when courts released school districts from desegregation orders. Lutz (2011) noted that the timing of release was marked by "an element of randomness" (p. 134). This randomness was a product of different caseloads across district courts that took some judges more time to clear from their dockets than others the uncertain nature of the release process, how individual judges approached desegregation, and importantly multiple appeals that added an element of unpredictability to when each district was declared unitary. Out of concern that there are unobserved differences in districts that are declared unitary, I perform checks on the robustness of my models by restricting the sample to the 215 districts that were declared unitary before 2010. This approach relies on the exogeneity of when the federal courts acted to limit bias in the results.

Despite these tests and robustness checks, my ability to make unbiased causal estimates of the effect of a unitary declaration is limited by the nature of my data which allows me to observe a district only once in a 10-year period. Thus, while I make substantial efforts to address omitted variable bias, my causal claims are tempered by the many events that could have occurred within these 10-year windows.

Dataset

This project would not be possible without Reardon et al.'s (2012) comprehensive collation of a starting dataset, containing information on 1,071 school districts, which documents each school district's status as under court-desegregation order, or not, and the timing of its release, if it occurred. These data were collected at the school-district level for all the years between 1964 and 2009. To address my first research question, I draw on the three most recent administrations of the short-form Decennial Census, in 1990, 2000, and 2010. These contain information on the race of all residents in the United States. The Census Bureau collects data from individuals and then aggregates this information to various levels of geography, including the census block, block group, and tract. I use information on the total population, and its racial and ethnic composition, aggregated to the block-group level, for each of those three census administrations.[2]

I merge these census-block group demographic data with geographic shapefiles—computer-generated geometric shapes that can be linked to a data source. Using geographic information in the shapefile, I assign each block group and its demographic information to a school district if the census-block group's geometric centroid falls within the school-district's boundary. I assign census-block-group data from each of the three administrations to a particular school district based on the 1990 school-district boundaries to ensure that changes in district geography are not endogenous to the policy shifts. Thus, my dataset is a *school-district-by-year* dataset containing three rows of aggregated data per district, representing each of the three census waves.

To answer my second research question, I combine data from the 1990 and 2000 administrations of the long-form Decennial Census and the American Community Survey (ACS) 2006–2010 five-year estimates. Following the same procedures as above, I assign aggregated census-block-group data on student-enrollment status by race for individuals aged 16 to 19 to my school-district-by-year dataset. As with the short-form Census, the ACS collects information from individuals and aggregates it up to various geographic levels. Unfortunately, while the Census Bureau collects information permitting analysis of educational enrollment by race, the public reporting of the variable "School Enrollment for the Population 16−19 Years of Age" is not disaggregated by race and ethnicity in the 2010 ACS and onward. By request, the ACS Office at the

Census Bureau provided me with a custom tabulation for School Enrollment for the Population 16 to 19 by Race and Hispanic Origin, aggregated at the school-district level. This provides me a unique opportunity to answer my research questions over a period of time during which these data were heretofore unavailable. The difference in geographic level of data collection (school district vs. census-block group) should not introduce any particular bias in my estimates since these should represent the simple sum of all census-block groups within the district. In fact, when I compare the 2010 total dropout rate (not disaggregated by race) at the school-district-reported level with the values I obtain by summing across census-block groups, they correlate nearly perfectly (0.998).

Sample

Following Reardon et al. (2012), I restrict my sample to school districts that were under court-desegregation order in 1991 and had a student enrollment greater than 2,000. School districts with fewer students than this generally only have one school per grade level, so the impacts of desegregation order (and release) are negligible. This restriction yields a sample of 480 school districts across 31 geographically diverse states (see Figure 1, Panel A) of which anywhere between 2 and 25 districts were released from court order in a given year (Figure 1, Panel B) for a total of 215 districts declared unitary by 2009.[3] These 480 school districts, though a small fraction of the more than 14,000 districts nationwide, are some of the largest in the United States, including a total of more than 67.5 million residents in 2010.

In Table 2, I report summary statistics for the population and demographic characteristics of my sample of school districts. In this, I include weighted averages and weighted medians for the 480 districts in my sample. The median statistic is particularly informative as it describes what the experience of a resident or student in a typically sized school district over this 20-year period would have been. As of 2000, the Census Bureau began collecting racial and ethnic information separately, so it is not possible to build a nonoverlapping race category that includes Hispanics. Therefore, I construct race categories of White, Black, and non-White, which includes all one-race categories other than White and all multirace individuals. Furthermore, I divide residents into two ethnicity groups of Hispanic and non-Hispanic.

The districts in my sample are emblematic of the broad demographic shifts the United States has experienced over the past 20 years. The proportion of White residents in these districts declined by 7 percentage points, whereas the proportion of Black residents remained steady. The overall proportion of non-White residents, including Asian and multiracial residents, grew 5 percentage points. Most dramatically, the proportion of Hispanic residents in these districts increased by 7 percentage points.

Measures

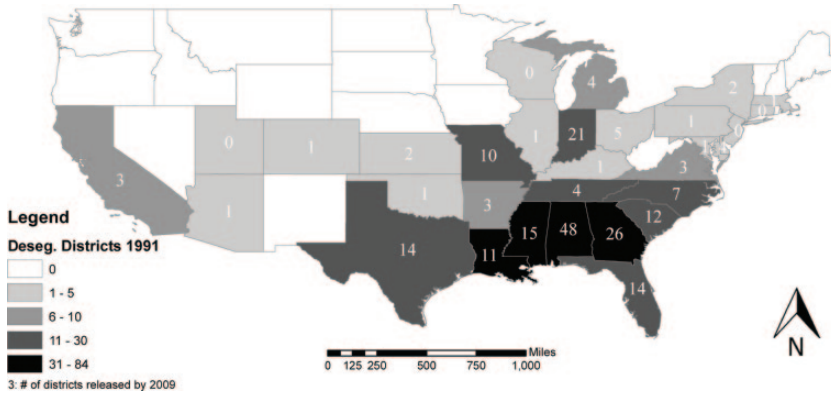
As discussed above, I organize my dataset so that it contains three observations on each school district, representing each year of census-data collection. Thus, the measures defined below are either time varying, and can take on different values in the different rows of my dataset, or they are time invariant, and have the same value across the multiple data waves.

To answer my first research question on residential segregation, I define my outcome at the school-district level and estimate its values using a standard racial- and ethnic-dissimilarity index. Panel C of Table 2 presents the average over the 20 years of study for two measures of residential segregation. I calculate as my primary outcome for this research question the value of the dissimilarity index (D) in school district j in time t as follows:

$$D_{jt} = \frac{1}{2}\sum_{i=1}^{n}\left|\frac{b_{it}}{B_{jt}} - \frac{w_{it}}{W_{jt}}\right|, \qquad (1)$$

where b_{it} is the number of Black or Hispanic residents in census-block group i, and w_{it} is the number of White and Asian-Pacific Islander residents in census-block group i.[4] I generate this measure using information aggregated by the Census Bureau at the block-group level and summing across block groups to create a school-district-level outcome, so the values of the index will be time varying. It is interpretable as the fraction of

Panel A. Districts under court order in 1991 (n = 480) and released (n = 215), by state



Panel B. Timing of release of districts from court order (n = 215), by year

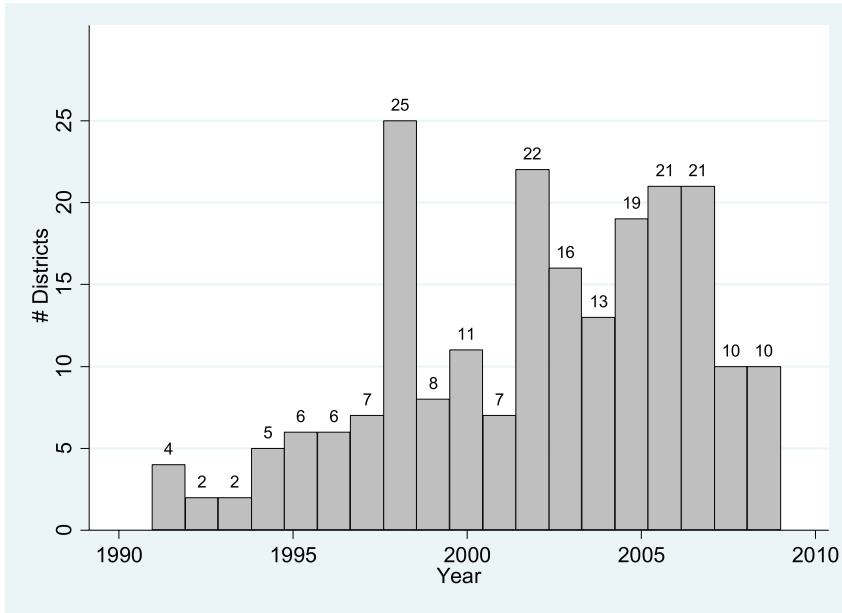


FIGURE 1. *School districts under court-desegregation order and declared unitary by 2010.*
Source. Reardon, Grewal, Kalogrides, and Greenberg (2012), with updates as described in Note 5.

Black or Hispanic individuals who would need to move to a different neighborhood for the school district's neighborhoods to be perfectly integrated, given the racial composition of the community. The values of this dissimilarity index range from 0 to 1, where a value of 0 indicates that the racial/ethnic composition of all census-block groups in the school district matches the overall residential-racial/ethnic composition of the school district, and a value of 1 indicates that no Whites and Asians lived in the same census-block group as Blacks or Hispanics. As opposed to the simple exposure index, the total numbers of Black or Hispanic (B_{jt}) and White/Asian (W_{jt}) residents appearing in the denominators in Equation 1 account for the sizes of the Black or Hispanic and White/Asian populations in the school district, and so the values of the dissimilarity index are adjusted for any changes in the overall racial and ethnic composition of the school district over time. In my sample, the values of the dissimilarity index have decreased, indicating that levels of residential integration in these districts have increased over these 20 years

TABLE 2

*Summary Statistics on Size, Racial/Ethnic Composition, Residential Segregation, and Dropout Rates of Sample of Districts (*N *= 480) Under Court Order in 1991*

	1990	2000	2010
Panel A: Total school-district residents			
Average district population	119,650	132,167	140,782
Median district population	[30,524]	[33,797]	[34,162]
% change 90 to 00		0.105	
		[0.092]	
% change 00 to 10			0.065
			[0.022]
% change 90 to 10			0.177
			[0.117]
Panel B: Racial and ethnic composition			
% White	0.657	0.603	0.588
	[0.644]	[0.602]	[0.583]
% Black	0.245	0.248	0.247
	[0.242]	[0.231]	[0.255]
% non-White	0.309	0.354	0.359
	[0.326]	[0.355]	[0.367]
% Hispanic	0.134	0.173	0.209
	[0.056]	[0.095]	[0.151]
Panel C: Outcome measures			
Black–White residential segregation			
Exposure	0.112	0.130	0.145
	[0.095]	[0.094]	[0.120]
Dissimilarity	0.666	0.611	0.557
	[0.698]	[0.629]	[0.544]
Isolation	0.456	0.377	0.314
	[0.484]	[0.378]	[0.295]
Hispanic–White residential segregation			
Exposure	0.121	0.160	0.197
	[0.059]	[0.110]	[0.150]
Dissimilarity	0.400	0.392	0.369
	[0.397]	[0.410]	[0.378]
Isolation	0.159	0.173	0.175
	[0.083]	[0.140]	[0.141]
Panel D: Dropout rates			
% dropout 16–19	0.142	0.124	0.078
	[0.135]	[0.116]	[0.073]
% dropout 16–19, White	0.130	0.111	0.070
	[0.123]	[0.101]	[0.060]
% dropout 16–19, Black	0.141	0.119	0.078
	[0.148]	[0.111]	[0.073]
% dropout 16–19, Hispanic	0.201	0.241	0.142
	[0.204]	[0.208]	[0.119]
Panel E: District court order status			
Unitary LEAs	0	76	215
Unitary block groups	0	11,719	22,331
Nonunitary	480	404	265
Nonunitary block groups	50,007	34,856	25,968

Note. Cells in Panels A–D contain means with medians displayed in brackets. All rows in Panels A–D (except for the first two rows in Panel A) are weighted by total residents within school-district boundaries. Cells in Panel E contain counts. LEAs = Local Education Agencies.

net of changing composition, though this reduction in residential segregation has been marginal for Hispanics.

As Massey and Denton (1988) highlighted, the literature on segregation measures is fraught with disagreement over the appropriateness of various measures of residential segregation. Therefore, I perform a check on the sensitivity of my findings to an alternate definition of segregation by testing the impact of the dismissal of the districts on the residential isolation correlation index.[5] The isolation correlation index, in parallel to the dissimilarity index, declined considerably for Blacks during the time period of my sample which mirrors results found by Reardon and Bischoff (2011) across all communities. Hispanics in my sample, however, became more spatially isolated from Whites and Asians over these 20 years.

To answer my second research question, I use a school-district-level outcome variable. *SD_DROPOUT* is a time-varying measure that the Census Bureau collects on persons aged 16 to 19, by race, in the long-form Census and the ACS that describes whether they self-report as being "not enrolled in school" and are "not a high-school graduate." Thus, this number represents the proportion of individuals in this age range at a given moment in time who are high-school dropouts. In accordance with standard practice (Murnane, 2013), I refer to this as the "status dropout rate." I aggregate the corresponding block-group level averages to the school-district level for 1990 and 2000 and use the school-district-level outcome for 2010. To examine whether the impacts of the unitary declarations on my outcome differ by race, I analyze the impact on three distinct time-varying outcomes: the sample proportions of (a) White, (b) Black, and (c) Hispanic 16- to 19-year-olds who are dropouts and reside in a census block with its centroid within the school-district boundaries.

These outcomes are not ideal. First, they rely on self-report. Second, as Murnane (2013) noted, they conflate general education diploma (GED) recipients with traditional graduates. This is problematic because labor-market outcomes of GED recipients are closer to those of dropouts than to traditional high-school graduates, and because the number of GED recipients has increased rapidly in recent years, especially among Blacks and Hispanics. Fortunately, the growth in GED recipients will be largely addressed by the second difference in the difference-in-differences estimation strategy I present in the next section. As long as residents in districts declared unitary are no more or less likely to pursue the GED than residents in districts that are not (or not yet) declared unitary, the secular changes in trends of GED receipt will be accounted for in my identification strategy. It is possible that the second difference will not fully account for changes in GED-earning patterns if the declaration of unitary status reduces the quality of education for Black or Hispanic students in unitary districts, resulting in more Black and Hispanic students opting for the GED. However, to the extent that this may have occurred, my results will be downward-biased estimates of the impact of unitary status on high-school completion for Black and Hispanic students. The third problem with these outcomes is that they assign a student to a school district even if he or she moves to a census block within the school district *after* having dropped out—a particular concern given the high mobility rates among young Americans and an even graver concern for young immigrants who may not be attending school but have not dropped out of a U.S. school system. If there has been selective migration of low-income and limited-education Hispanics to neighborhoods in districts declared as unitary, but not to districts that were not declared unitary, this would represent a threat to the validity of my findings. As with the GED issue, however, any variation in the arrival of young immigrants by year that is uniform in districts that were and were not released from court order will also be addressed by the second difference.

Panel D of Table 2 reports the weighted averages and medians for school dropout rates in my sample. Consistent with national trends over this 20-year period, the proportion of residents aged 16 to 19 years in my sample who had dropped out of school declined precipitously from 14.2% in 1990 to 12.4% in 2000, and to 7.8% in 2010, representing nearly a halving of the dropout rate. The trends for White and Black residents of the school districts mirror the overall pattern of consistent declines in the dropout rate for the entire

sample. The average and median dropout rate for Hispanics, however, increased in 2000 from 1990 before declining substantially in 2010. This is consistent with national trends in the Hispanic status rate. There is also some potential noise in the 1990 and 2000 data due to small numbers of Hispanics residing in some of the census-block groups, resulting in nonreports due to privacy concerns.

My central question predictor in the analyses to address both research questions is the time-varying dichotomous predictor, *UNITARY*, coded 0 if the school district has not been declared unitary in the 10 years prior to that row of census-data collection, and 1 if the district has been declared unitary in the previous 10 years. Once a district is declared unitary, I code *UNITARY* as 1 in all subsequent years of census-data collection. As Panel E of Table 2 indicates, there were a total of 76 districts, representing more than 11,000 block groups released from court order by 2000, and a total of 215 districts, representing 22,000 block groups by 2010.

I also use parameterized and nonparameterized sets of predictors to capture the short- and midterm effects on my outcomes of being released from a court order. The time-varying continuous predictor *YRS_UNITARY* interacts *UNITARY* with a continuous count of the number of years it has been since the district was declared unitary in the current year's census-data collection. For example, the courts declared the Denver Public Schools (DPS) unitary in 1995. In the 2000 row of data collection, I code *YRS_UNITARY* equal to 5 because DPS had been unitary for 5 years at that data collection point. In 2010, *YRS_UNITARY* equals 15 for DPS.

I have no preexisting theoretical model to describe the appropriate functional form for the relationship between the length of time that a district has been declared unitary and its rate of segregation or high-school dropout. Thus, I also rely on the nonparametric approach of creating a vector of dichotomous predictors, $UNITARY\_PLUS_t$, where t runs from −19 to 19 that indicate how many years a district has been free from court order at the time of that wave of census-data collection. In the case of DPS, in 2000 $UNITARY\_PLUS_5$ is set equal to 1, and all other indicators are set equal to 0.

There are two key covariates necessary for implementing my difference-in-differences strategy in a regression framework. They are as follows: (a) a vector of time-invariant school-district indicators (Γ_j) and (b) a vector of time-varying year indicators (Φ_t). The school-district indicators are a series of 480 dichotomous variables, coded 1 in each respective district, and 0 otherwise. The values are identical in each of the three rows of the district-year dataset, for each school district. The year indicators are a set of three dummies, coded 1 if the observation corresponds to the respective year.

I also include in my analysis three covariates that, when interacted with my question predictor *UNITARY*, may highlight interesting heterogeneity in treatment effects. The first of these is an interaction of a time-invariant variable *GINI_HOUSEVALUE*, containing the values of Gini coefficients measuring the median home value estimated at the school-district level in 1990, with my *UNITARY* indicator. This variable allows for the treatment effect to vary by the starting differences in housing affordability across census-block groups—differences that may influence residents' ability to change residences before and after the assignment-policy change. The second covariate is *SOUTH*, a dichotomous time-invariant indicator of whether the school district is in the southern census region (1 = situated in the South; 0 otherwise). The final covariate is the continuous time-invariant *AREA*, which records the district's geographic area as a proportion of the Metropolitan or Micropolitan Statistical Area (MSA). This variable will serve as a proxy for residents' ability to move to different school districts to escape the effects of segregation. In making this decision, I hypothesize that school districts that cover larger proportions of their metropolitan areas may have been more likely to have experienced increased residential segregation in the aftermath of the unitary declaration because a move within the district could yield a more racially homogeneous school than such a move would have when the district was under court order.

Statistical Model

To address my first research question, I fit the following weighted-least-squares regression

model in my school-district-year dataset to implement my proposed difference-in-differences strategy for estimating the causal effects of the declaration of unitary status on the dissimilarity index (D_{jt}) in district j in year t:

$$D_{jt} = \Gamma_j + \beta_1 UNITARY_{jt} + \gamma\left(\mathbf{X}_{jt} \cdot \mathbf{\Phi_t}\right) + \varepsilon_j, \qquad (2)$$

where **X** is the vector of district-level time-invariant covariates defined above. The district-level error term (ε_j) will be heteroscedastic because my estimates of the dissimilarity index will be known with greater precision in districts with a larger population and a greater number of census-block groups. Therefore, I weight each school-district-year observation by the total number of residents in that district for that year. By weighting observations by district size, it ensures that my findings are representative of the average or typical students' experience in a district that is declared unitary. This strategy makes the estimates most representative of the population to which I am generalizing.

The key identifying mechanism that ensures my estimates can be interpreted causally is my assumption that the sudden, court-mandated change in student-assignment policy (recorded in the values of question predictor, *UNITARY*) is exogenous. Implicitly, in fitting the model, I estimate as my first difference the average difference in outcome (residential segregation of school districts) before and after they were declared unitary. This difference corresponds either to the period between 1990 and 2000 or 2000 and 2010, depending on when federal courts released the district from their desegregation order. Also, implicitly within the same model fit, I estimate and subtract a second difference in the average outcome, representing any secular trend that may have affected the entire system over the same time period, using only school districts that were under court-desegregation order in 1991, but were either not released by 2010 or were not released until after 2000.

For instance, the courts declared the DPS unitary in 1995. Thus, the difference in this district's dissimilarity index between 1990 and 2000 contributes to the estimated first difference. The courts declared the Little Rock School District unitary in 2007. Thus, the difference in this district's dissimilarity index between 1990 and 2000 contributes to the estimated value of the second difference, whereas the difference in the district's index between 2000 and 2010 contributes to the estimation of the first difference. In the model, β_1 is the key parameter of interest, representing the causal effect of being released from a court-mandated desegregation order on residential segregation. It will be positive and statistically significant if the declaration of unitary status increased levels of residential-racial sorting. Given the structure of my data, a key assumption I make is that the specific timing of the dismissal of the desegregation order is exogenous since I only observe each district once every 10 years. In my nonparametric models, I compare districts that have been unitary for 7 years at the time of the census-data collection with others that have been unitary for 3 years, but I am never able to observe the same district at these two time periods.

To address my second research question, I rely on the same difference-in-differences framework as in Equation 2, except my outcomes are district-level dropout rates (*SD_DROPOUT*) for the entire population aged 16 to 19, and disaggregated for White, Black, and Hispanic residents living within the school-district boundaries aged 16 to 19. My weights in these estimates are the number of youth aged 16 to 19 of that race or ethnicity in the school district.

Residential Segregation Results

The difference-in-differences analyses provide minimal evidence that the declaration of unitary status increased the rate of residential segregation for Blacks. It provides some evidence that it did so for Hispanics, particularly in the short term and in districts that were declared unitary by 2010, and had less initial variation in their housing prices.

Table 3 reports a taxonomy of fitted regression models from Equation 2 with the Black–White/Asian and the Hispanic–White/Asian dissimilarity indices as the outcomes. Model 1 in Panel A is the most basic model and is interpretable as a declaration of unitary status causes a district to experience a decline in the Black–White/Asian dissimilarity index by 0.017, but the t statistic on this parameter is small, so the impact of dismissal from court order is indistinguishable from 0 in the population. Model 2 and all subsequent models

TABLE 3

Linear Regression Models Estimating the Effect of Declaration of Unitary Status on the Black–White/Asian and the Hispanic/White/Asian Residential Dissimilarity Index (1990–2010)

	1	2	3	4	5
Panel A: Black–White/Asian dissimilarity index					
Unitary	−0.017	−0.012†		0.000	
	(0.011)	(0.006)		(0.005)	
Unitary + 5 years			−0.017*		
			(0.007)		
Falsification test					−0.012
					(0.009)
LEAs	480	480	480	215	480
R^2	0.952	0.960	0.960	0.945	0.960
Panel B: Hispanic–White/Asian dissimilarity index					
Unitary	0.030**	0.013		0.007	
	(0.011)	(0.008)		(0.008)	
Unitary + 5 years			−0.001		
			(0.008)		
Falsification test					0.015
					(0.011)
LEAs	480	480	480	215	480
R^2	0.881	0.901	0.900	0.867	0.901
District fixed effects	X	X	X	X	X
Year fixed effects	X				
% Black/Hispanic residents (1990) × Year fixed effects		X	X	X	X
Census region × Year fixed effects		X	X	X	X

Note. The table displays coefficients from Equation 2. Standard errors (in parentheses) are adjusted to account for the serial intercorrelation caused by the clustering of observations within districts. The dependent variable is the dissimilarity index defined in the text. LEAs = Local Education Agencies.
†Significant at 10%. *Significant at 5%. **Significant at 1%.

include controls for the starting (1990) demographic composition and the census region of the district. In Model 2, I find marginally significant evidence that the declaration of unitary status caused these districts to experience a decline in the Black–White/Asian dissimilarity index. The lag measure for 5 years after the release of districts from court order (Model 3) returns the same result. Model 4 restricts the sample to those districts that were ever declared unitary during this time period, and I find no effects of being declared unitary on districts' patterns of Black–White/Asian residential segregation. In Model 5, I conduct a falsification test, assigning each district that was ever declared unitary a year of court-ordered release 7 years prior to when it was actually declared unitary. If my identification strategy is accurate, I should have no reason to see an increase in residential segregation 7 years prior to when a district is declared unitary. In this specification, I observe a nonsignificant (though same-signed) result. Although not presented in the table, the point estimates for results using the isolation correlation index as the outcome are similarly signed, though smaller in magnitude, and insignificant. If anything, the preponderance of evidence suggests that when districts are declared unitary, they experience a small decrease in Black–White segregation.

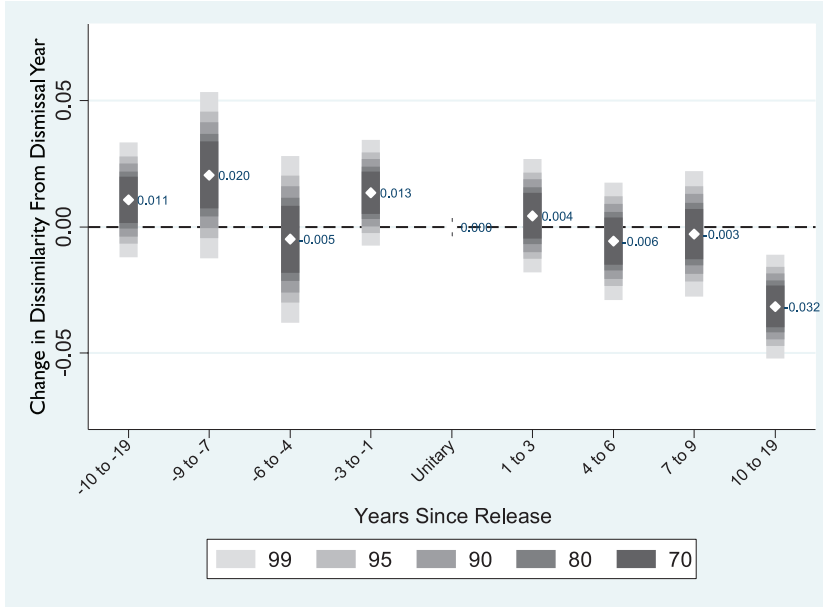
Panel B in Table 3 reports the results of being declared unitary on the Hispanic–White/Asian

dissimilarity index. In contrast with the results for Black residents, I observe more consistent, positively signed impacts of being declared unitary on a district's rate of Hispanic residential segregation. In the simplest form (Model 1), the impact of a declaration of unitary status results in a 0.030 increase in the dissimilarity index, interpretable as a further 3.0 percentage point increase in the number of Hispanics who would need to move to new census-block groups for the district to be perfectly integrated, on top of the 36.9% average across all districts in 2010. The inclusion of controls for a district's starting racial composition and census region attenuates the impact of being declared unitary on residential segregation, but there is still a positively signed, though imprecisely estimated, value on *UNITARY* in Model 2. The lagged indicator for 5 years after the unitary declaration (Model 3) is indistinguishable from 0 as are results when restricting the sample to districts that were ever declared unitary (Model 4). I do not find any trends in my falsification test in Model 5 toward an increase in the dissimilarity index prior to being declared unitary.

Given the conflicting nature of the evidence, particularly with respect to the Hispanic–White/Asian index, it is instructive to look at the trends over time in districts that have been declared unitary. While the linear time-trend (*YRS_UNITARY*) results reveal no new information, Figure 2 plots the nonparametric results of the pooled average of single-year dummies for how many years until or since a district has been released from court order. I compare the effects over time of being released from court order on the dissimilarity index relative to trends in non- or not-yet released districts. I report each estimate of the impact on the dissimilarity index relative to the year a district is first released from court order. I construct a series of three 3-year bins on either side of the year of release from court order with a 10- to 19-year bin before and after unitary declaration. I use year bins to improve interpretability, avoid a small number of districts driving a single year's results, and increase the precision of the estimates. Results are robust to alternative binning specifications and available on request. Panel A of Figure 2 plots the impact of the unitary declaration on Black–White/Asian dissimilarity rates. Prior to unitary declaration, there is substantial noise in the data with no clear trend toward increased or decreased levels of residential segregation. After districts were released from court order, there was a distinct downward trajectory in their levels of Black–White/Asian residential segregation, consistent with earlier categorical results shown in Table 3. Panel B shows meaningfully different patterns for the Hispanic–White/Asian dissimilarity index. Prior to unitary declaration, segregative trends were declining in a stable and consistent way. In the year of unitary-status declaration and in the first 3 years following release from court order, districts experienced a jump in the dissimilarity index, but then experienced a return to predismissal trends over the subsequent 15 years. There is a significant difference between the linear combination of the pre-court release coefficients and the first 3 years after dismissal ($z = 2.02$, $p = 0.043$), but this increase in the dissimilarity index in unitary districts does not persist after 3 years. It, thus, appears that there were short-term increases in Hispanic–White/Asian segregative moves following releases from court orders, but then these districts returned to their secular patterns.

In addition to the main effect of *UNITARY*, I examine in Table 4 whether variation in home affordability in 1990, a district's physical size relative to its MSA, and its census region influence the impact of a release from court order on rates of residential segregation. In parallel with the results in Table 3, the main effect of *UNITARY* on the Black–White dissimilarity index (Panel A) remains negative and insignificant in Models 1 through 3 which include these additional treatment conditions. The estimates on the interaction of *UNITARY* with variations in housing affordability in 1990 and size relative to the metro- or micropolitan area are all insignificant. My inability to reject the null hypothesis as it relates to the impact of a unitary declaration on within-district residential segregation for districts that represent a larger proportion of their metropolitan area means that I am unable to provide insight as to whether changes in within-district segregation rates stem from within- or between-district migration. Since the inclusion of *AREA* reduces my sample because not all districts are in an MSA, I exclude it from further estimates. I do find marginally significant evidence in Model 3 that within southern districts that were declared

Panel A. Black–White/Asian dissimilarity index



Panel B. Hispanic–White/Asian dissimilarity index

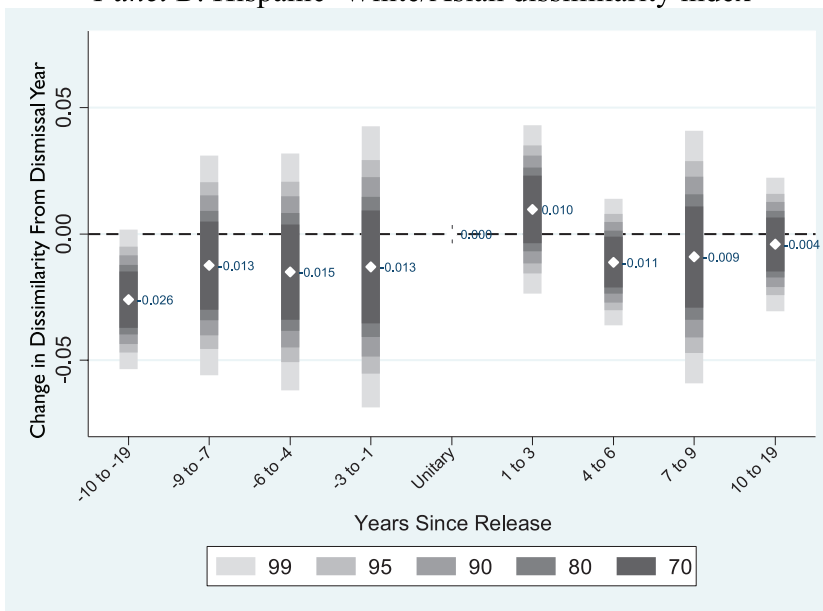


FIGURE 2. *Nonparametric regression estimates of the effect of unitary-status declaration on the dissimilarity index, by race/ethnicity.*

Note. The figure generated from point estimates replacing the categorical variable *UNITARY* in Table 3, Model 2 in Panels A and B with a series of 20-year dummies (single-year indicators for −9 to +9 years since release and then 10-year span indicators for 10−19 years before unitary and 10−19 years after, year of court release is omitted). Three-year bins created through a linear average of the coefficients on the year indicators. Full tables for these estimates as well as the nonsignificant linear time trend available from author.

unitary by 2010, when they were declared unitary, they experienced greater rates of Black–White/Asian residential segregation after their release from court order than before. There is no

TABLE 4

Linear Regression Models Estimating the Effect of Declaration of Unitary Status on the Black–White/Asian and Hispanic–White/Asian Residential Dissimilarity Index, Allowing for Variation in the Effect by Housing Affordability, Census Region, and Relative Size (1990–2010)

	A. Black–White/Asian dissimilarity				B. Hispanic–White/Asian dissimilarity			
	1	2	3	4	1	2	3	4
Unitary	−0.031	−0.033	−0.029		−0.021	−0.027	0.066*	
	(0.025)	(0.026)	(0.019)		(0.041)	(0.042)	(0.032)	
Gini house value × Unitary	0.090	0.095	0.073		0.092	0.123	−0.307*	
	(0.097)	(0.103)	(0.078)		(0.174)	(0.185)	(0.142)	
South × Unitary	−0.001	−0.007	0.016†		0.017	0.018	0.011	
	(0.015)	(0.017)	(0.009)		(0.016)	(0.019)	(0.012)	
Area × Unitary		0.018				−0.016		
		(0.015)				(0.021)		
Falsification test				−0.009				−0.035
				(0.040)				(0.060)
Gini housevalue × Unitary_false				0.050				0.236
				(0.151)				(0.257)
South × Unitary_false				−0.016				0.006
				(0.021)				(0.022)
District fixed effects	X	X	X	X	X	X	X	X
% Black/Hispanic residents (1990) × Year fixed effects	X	X	X	X	X	X	X	X
Census region × Year fixed effects	X	X	X	X	X	X	X	X
1990 Housing value × Year fixed effects	X	X	X	X	X	X	X	X
South × Year fixed effects	X	X	X	X	X	X	X	X
Area × Year fixed effects		X				X		
LEAs	480	360	215	215	480	360	215	215
R^2	0.962	0.963	0.947	0.947	0.901	0.913	0.871	0.873

Note. The table displays coefficients from Equation 2. Standard errors (in parentheses) are adjusted to account for the serial intercorrelation caused by the clustering of observations within districts. The dependent variable is the dissimilarity index defined in the text. LEAs = Local Education Agencies.
†Significant at 10%. *Significant at 5%. **Significant at 1%.

evidence from the falsification test in Model 4 that this is an artifact of secular trends; however, I am skeptical of making much of this singular result. The inclusion of the 5-year lag indicator for unitary declaration adds no additional insight, so I do not present it here.

In Panel B, I examine the effect of unitary declarations on the Hispanic–White/Asian dissimilarity index. Although estimates on the full sample in Models 1 and 2 allowing outcomes to vary by housing affordability, district size, and census region reveal no new insight, the restricted sample of districts that were ever declared unitary does. In Model 3, the estimate on *UNITARY* is positive and significant, but there is a strong countervailing weight to the starting unevenness in home values. This suggests that in districts that were declared unitary and had a fairly fluid housing market, the declaration of unitary status increased residential segregation for Hispanics. In districts with more restrictive housing markets in which some residents might have substantial difficulty in relocating to a community with a different racial makeup due to variability in

housing costs, there was no correlated increase in segregation. Model 4's falsification test provides no evidence that prior trends drive this outcome.

The unit of analysis in my study is the school district. As a further robustness check, I fit the same models without weighting the districts by their population to verify that my results are not driven by a few large districts. In Appendix B: Table S3 (available in the online version of the journal), I present results that are broadly similar to the main results, with evidence that unitary declarations increased the Hispanic–White/Asian index. There is also evidence that the court release decreased Black–White/Asian segregation, but my falsification test finds that these patterns preceded the unitary declaration.

In sum, in both categorical and time-trend models, there is evidence that the declaration of unitary status increased the Hispanic dissimilarity index, though this appears to be primarily true in the short run and in districts that were declared unitary by 2010 and had fluid housing markets. There is no corresponding evidence that it increased Black–White residential segregation.

High-School Dropout Rates

I find evidence that the declaration of unitary status caused an increase in the Hispanic status dropout rate across specifications. I find similar outcomes for Blacks; these results are driven by Blacks residing outside the South census region. Alternative specifications used to test the robustness of the impact of unitary declarations on the Black dropout rate are consistently positive in sign and similar in magnitude, though often statistically insignificant due to imprecision of the estimates.

As Panel A in Table 5 indicates, there is no significant impact of unitary declaration on the White dropout rate for individuals aged 16 to 19. As Lutz (2011) noted, the contrast between Black and White results (with the addition of Hispanics in my study) is informative because it suggests that these race-specific estimates are not capturing "district-wide trends in dropout behavior in dismissed districts, the influence of education reforms, or other factors, such as deteriorating facilities" (p. 157). The discrepancy indicates that any increases in Black and Hispanic dropout patterns are unlikely to be simply an artifact of secular trends or similarly timed events to the declaration of unitary status, but rather a causal outcome of the lifting of the court order.

Panel B reports the causal effect of the declaration of unitary status on Black status dropout rates. Model 1 indicates that releasing a district from a court-desegregation order causes a 1.3 percentage point increase in the status dropout rate for Blacks aged 16 to 19. To interpret this in concrete terms, consider the average Black status dropout rate in 1990: 14.1%. Consider a district dismissed between 1991 and 2000. I estimate that, on average, all districts would experience a decline in the Black dropout rate of 2.3 percentage points during this time period; however, I estimate this released district to experience an additional increase in its dropout rate of 1.3 percentage points. Thus, compared with a district still under court order which I estimate would have an 11.8% Black dropout rate, this dismissed district would have a 13.1% dropout rate. The inclusion of additional controls in Model 2 reduces the point estimate, as does an examination of dropout rates 5 years after unitary declaration (Model 3) and restricting my sample to only those districts that had ever been declared unitary (Model 4). Although these alternate specifications are not statistically significant, they are consistently positive in sign. The falsification test in Model 5 finds no significant effect on the Black dropout rate of being 7 years away from being declared unitary.[6]

The results in Panel C show a stronger causal impact of unitary status on Hispanic status dropout rates. Model 1 indicates that districts receiving a unitary designation experienced a 3.5 percentage point increase in the Hispanic dropout rate over districts that were not released from court order—the estimate falls fractionally short of the 95% confidence threshold. The estimated Hispanic dropout rate in 1990 was 24.2%. The prototypical district saw its estimated dropout rate decline by 13.3 percentage points between 1990 and 2010, for an estimated dropout rate of 10.9%. However, if the district was declared unitary during this time, I estimate a countervailing causal effect of a 3.5 percentage point increase to 14.4%. When I include controls for initial demographic makeup and census region in my preferred specification of Model 2, the magnitude of the estimate persists and

TABLE 5

Linear Regression Models Estimating the Effect of Declaration of Unitary Status on the White, Black, and Hispanic Dropout Rates (1990–2010)

	1	2	3	4	5	6
Panel A: White dropout rate						
Unitary	0.002	0.002		0.010		
	(0.005)	(0.004)		(0.007)		
Unitary + 5 years			0.003			
			(0.005)			
Falsification test					−0.006	
					(0.004)	
LEAs	480	480	480	215	480	
R^2	0.817	0.826	0.826	0.850	0.827	
Panel B: Black dropout rate						
Unitary	0.013**	0.006		0.006		
	(0.005)	(0.004)		(0.006)		
Unitary + 5 years			0.004			
			(0.004)			
Falsification test					0.006	
					(0.005)	
LEAs	476	476	476	211	476	
R^2	0.709	0.725	0.725	0.714	0.725	
Panel C: Hispanic dropout rate						
Unitary	0.035†	0.029*		0.032†		0.031*
	(0.018)	(0.012)		(0.017)		(0.012)
Unitary + 5 years			0.027*			
			(0.012)			
Falsification test					0.012	
					(0.011)	
LEAs	474	474	474	211	474	394
R^2	0.805	0.846	0.846	0.847	0.843	0.881
District fixed effects	X	X	X	X	X	X
Year fixed effects	X					
% Black/Hispanic residents (1990) × Year fixed effects		X	X	X	X	X
Census region × Year fixed effects		X	X	X	X	X

Note. The table displays coefficients from Equation 2. Standard errors (in parentheses) are adjusted to account for the serial intercorrelation caused by the clustering of observations within districts. The dependent variable is the status dropout rate defined in the text. LEAs = Local Education Agencies.
†Significant at 10%. *Significant at 5%. **Significant at 1%.

becomes significant at traditional confidence levels. When I examine outcomes 5 years after the declaration of unitary status (Model 3), I find the same impact of being declared unitary: a 2.7 percentage point increase in the Hispanic dropout rate, estimated with precision at the 95% confidence level. I conduct the same robustness check, looking only at districts that were ever declared unitary (Model 4), and find that the magnitude of the point estimate on *UNITARY* persists. I employ a falsification check in Model 5, and attempt to predict whether a district would have seen an increase in the Hispanic dropout rate 7 years before being

declared unitary. Although the result is still positive, it is much smaller in magnitude and insignificant. I take this as evidence that my results are not driven by secular trends in the Hispanic dropout rate. Finally, a sizable number of school districts reported no census-block-group counted Hispanic dropouts aged 16 to 19 years in 1990 and 2000. As a robustness check, Model 6 excludes districts that reported no Hispanic dropouts. Similar to the previous models, I find a 3.1 percentage point increase in the status dropout rate postunitary declaration for these districts.

To explore further the impact that the declarations of unitary status have over time, I estimate parametric and nonparametric models of the impact of these rulings on the Black and Hispanic high-school status dropout rates. Although the linear and polynomial terms I include in my specifications yield trivial results, the nonparametric analysis proves interesting. Figure 3 presents the nonparametric results of the pooled average of single-year dummies for how many years until or since a district has been released from court order with confidence intervals. Panel A of Figure 3 highlights a downward trend in the Black status dropout rate in the 3 years prior to a district being declared unitary, followed by a 1.3 percentage point increase in the 3 years following their release from court order.

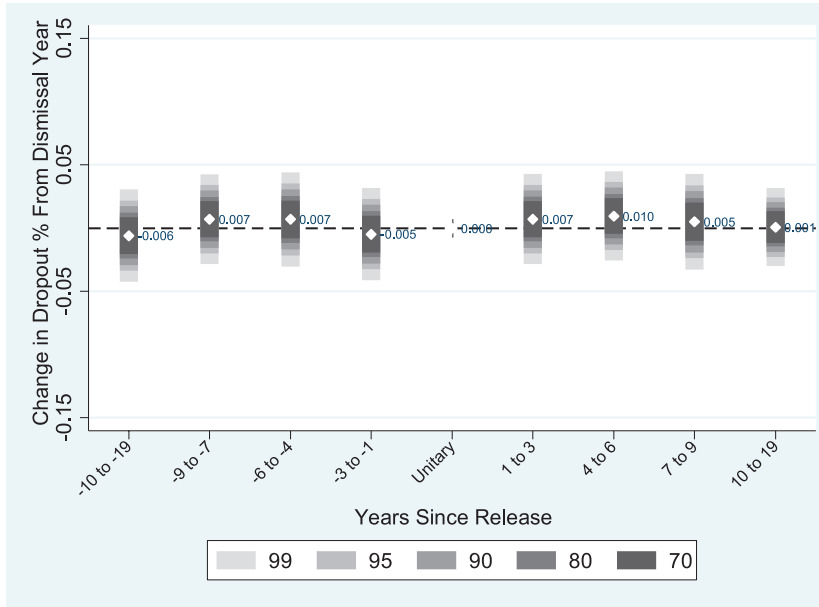
The Hispanic dropout rate increased more sharply once districts were declared unitary. As Panel B of Figure 3 indicates, the status dropout rate in the years leading up to a district being declared unitary was consistently declining. However, there was a large and significant increase in the dropout rate in the years following unitary declaration. A balanced panel comparison of the 6 years leading up to and after unitary-status declaration reveals a significant difference ($z = 2.24$, $p = 0.025$) in the Hispanic dropout rate as a consequence of release from court order. The coefficients in Figure 3 suggest that the impact on dropout rates was concentrated in Years 1 through 6 after the declaration of unitary status, before returning to the declining secular trend.

In Table 6, I examine whether variation in a district's home affordability in 1990 and its census region influences the impact of a release from court order on dropout rates. In Panel A, I find that within the sample of districts that were declared unitary by 2010 (Model 2), White students in the South do appear to experience a 3.1 percentage point increase in their rate of dropping out. However, my falsification test (Model 3) finds that 7 years prior to unitary declaration districts outside the South were experiencing reduced rates of White dropouts, while those in the South were experiencing increases in White dropouts. The magnitudes of these changes are approximately the same pre- and postunitary declaration. I take this as evidence that the results on the White dropout rates in Model 2 are largely an artifact of secular trends.

Panel B provides no evidence that there were significant variations in the impact of a unitary-status declaration on the Black dropout rate given a district's initial housing market or its census region. However, it is important to note that the positive coefficients on the main effect of *UNITARY* persist. In addition, though insignificant, the negative coefficients on the interaction between *SOUTH* and *UNITARY* corroborate Lutz's (2011) findings that the effects of release from court order on Black's graduation rates were greatest in nonsouthern districts. I explore this phenomenon further in Table 7, and in alignment with Lutz, as evident in Model 1, unitary-status declarations increased Black dropout rates by 1.9 percentage points, estimated at a significant level, though these effects are counterbalanced by a decline in dropout rates for districts located in the southern census region. When I subset my sample to estimate my results on only nonsouthern districts (Model 2), I find similar results, significant at the 90% confidence level. Thus, I find strong and reliable evidence that aligns with prior research findings that unitary-status declarations result in increases in dropout rates for Blacks outside the South Census region. Results for Hispanics are consistent in sign and magnitude with my other results, though imprecisely estimated.

As Lutz (2011) noted, nonsouthern Blacks were more likely to be reassigned to extremely segregated schools due to the higher overall rates of between-district segregation outside the South. It may also be that reassigned Blacks outside the South were more frequently on the brink of dropping out since they started in more racially

Panel A. Black status dropout rate, ages 16–19



Panel B. Hispanic status dropout rate, ages 16–19

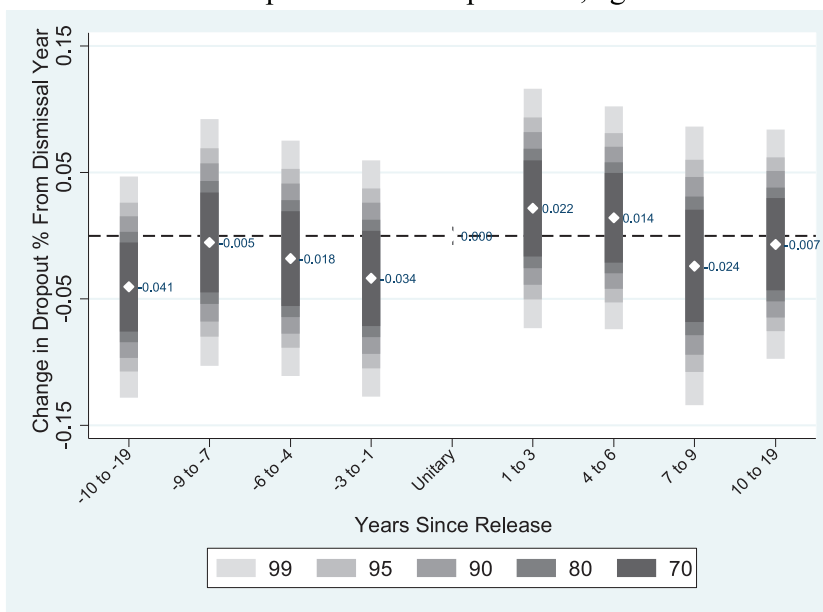


FIGURE 3. *Nonparametric regression estimates of the effect of unitary-status declaration on the dropout rate, by race/ethnicity.*

Note. The figure generated from point estimates replacing the categorical variable *UNITARY* in Table 5, Model 2 in Panels A and B with a series of 20-year dummies (single-year indicators for −9 to +9 years since release, and then 10-year span indicators for 10 to 19 years before unitary and 10 to 19 years after, year of court release is omitted). Three-year bins created through a linear average of the coefficients on the year indicators. Full tables for these estimates as well as the nonsignificant linear time trend available from author.

isolated schools. Finally, Lutz presents anecdotal evidence that southern districts invested in school capital construction projects in minority communities postunitary declarations. These theories on

TABLE 6

Linear Regression Models Estimating the Effect of Declaration of Unitary Status on the White, Black, and Hispanic Dropout Rates, Allowing for Heterogeneity by Housing Affordability and Census Region (1990–2010)

	A. White dropout rate			B. Black dropout rate			C. Hispanic dropout rate			
	1	2	3	1	2	3	1	2	3	4
Unitary	−0.013	−0.038		0.007	0.021		0.061	0.055		0.075
	(0.016)	(0.033)		(0.019)	(0.027)		(0.048)	(0.078)		(0.050)
Gini house value × Unitary	0.049	0.086		0.038	−0.059		−0.113	−0.226		−0.159
	(0.061)	(0.147)		(0.066)	(0.114)		(0.156)	(0.331)		(0.160)
South × Unitary	0.005	0.031**		−0.012	−0.002		−0.005	0.044		−0.007
	(0.010)	(0.010)		(0.011)	(0.015)		(0.029)	(0.036)		(0.029)
Falsification test			−0.057*			0.002			−0.052	
			(0.025)			(0.022)			(0.059)	
Gini housevalue × Unitary_false			0.111			0.034			0.284	
			(0.084)			(0.080)			(0.206)	
South × Unitary_false			0.023			−0.003			−0.025	
			(0.015)			(0.012)			(0.024)	
District fixed effects	X	X	X	X	X	X	X	X	X	X
% Black/Hispanic residents (1990) × Year fixed effects	X	X	X	X	X	X	X	X	X	X
Census region × Year fixed effects	X	X	X	X	X	X	X	X	X	X
1990 Housing value × Year fixed effects	X	X	X	X	X	X	X	X	X	X
South × Year fixed effects	X	X	X	X	X	X	X	X	X	X
LEAs	480	215	215	476	211	476	474	211	474	394
R^2	0.830	0.858	0.858	0.730	0.715	0.728	0.852	0.854	0.849	0.886

Note. The table displays coefficients from Equation 2. Standard errors (in parentheses) are adjusted to account for the serial intercorrelation caused by the clustering of observations within districts. The dependent variable is the status dropout rate defined in the text. LEAs = Local Education Agencies.
†Significant at 10%. *Significant at 5%. **Significant at 1%.

mechanisms for regional heterogeneity are largely speculative.

Panel C of Table 6 provides continuing evidence that the declaration of unitary status increased the Hispanic dropout rate, though the models are imprecisely estimated. Model 1 indicates that outside the South and in districts with a lower spread of housing affordability, there was up to a 6 percentage point increase in the Hispanic dropout rates when districts were declared unitary, though the standard errors are large. The magnitude of these effects persists when restricting the sample to only those districts ever declared unitary (Model 2) and those districts that reported at least some Hispanic dropouts (Model 4). Importantly, the falsification test in Model 3 indicates that when districts were 7 years away from being declared unitary, they had a lower rate of Hispanic dropouts. I take this again as evidence that the main results as well as those indicating variation by starting qualities of a district are not artifacts of secular trends.

As with the residential segregation results, I conduct an unweighted robustness check on the dropout outcome. In Appendix B: Table S4 (available in the online version of the journal), I find similarly small magnitude impacts on the Black dropout rate and larger (but imprecisely estimated) increases in the Hispanic dropout rate.

TABLE 7

Linear Regression Models Estimating the Effect of Declaration of Unitary Status on Dropout Rate by Race in Nonsouthern Census Regions, Controlling for 1990 Demographics and Census Region (1990–2010)

	Black		Hispanic	
	1	2	1	2
Unitary	0.019*	0.017†	0.040	0.008
	(0.009)	(0.010)	(0.025)	(0.025)
South × Unitary	−0.018†		−0.018	
	(0.010)		(0.030)	
District fixed effects	X	X	X	X
% Black/Hispanic residents (1990) × Year fixed effects	X	X	X	X
Census region × Year fixed effects	X	X	X	X
LEAs	476	92	474	94
R^2	0.727	0.849	0.846	0.913

Note. The table displays coefficients from Equation 2. Standard errors (in parentheses) are adjusted to account for the serial intercorrelation caused by the clustering of observations within districts. The dependent variable is the status dropout rate defined in the text. Model 1 is the full sample. Model 2 is nonsouthern districts only. LEAs = Local Education Agencies.
†Significant at 10%. *Significant at 5%. **Significant at 1%.

Discussion

In this study, I provide nationwide evidence on the impact of the end of court-mandated desegregation orders on a complete sample of districts subject to these decrees. Although previous studies have found similar effects within a single district, within a limited sample of districts, or within a restricted time period, this study includes the most comprehensive list of districts available and extends the period of analysis through a time in which a substantial number of districts were subject to the unitary declarations. Furthermore, in contrast to other studies on this subject, I analyze the impact of these shifts on the largest minority group in American schools—Hispanics. I conclude that barring districts from using race-conscious mechanisms for assigning students to schools increased short-term rates of Hispanic residential segregation, modestly increased dropout rates for Blacks, particularly those residing in districts outside the southern census region, and substantially increased Hispanic dropout rates.

Unfortunately, the difference-in-differences empirical approach is ill suited to tease out the causal mechanisms that might explain why these patterns occur. One reasonable hypothesis could be that since the unitary declarations increased residential isolation, and the literature cited above suggests that residential segregation negatively affects school outcomes, the first outcome of interest (residential segregation) caused the second (school dropout). However, the evidence does not support this hypothesis (see Appendix B: Table S5 and discussion, available in the online version of the journal).

Despite these limitations, this study advances the policy and legal discussion on the impact of the end of race-based student assignment. My findings indicate that state action that does not explicitly take into account race in assigning students to schools increases the rate at which some Black and Hispanic students dropout of high school. This has broad implications for jurists considering current challenges to race-based affirmative action in higher education as well as disparate impact claims. If legal doctrine shifts to prohibit consideration of race in the development of policy or on the impact that a policy will have, it may lead to other similar negative outcomes. Furthermore, local school boards and superintendents should heed closely the ways in which the Office of Civil Rights and Department of Education continue to permit the use of race in assigning students to schools. If the evidence indicates that when school districts cease to use this information in their student-assignment

policies, schools not only become less racially and ethnically diverse, so too does the composition of the graduating high school class, it is in local officials' best interests to design plans which seek to limit racial isolation.

Author's Note

The views expressed in this article are my own and do not necessarily represent those of the Organisation for Economic Co-Operation and Development (OECD). Errors and omissions are my own.

Acknowledgment

I am grateful to Richard Murnane, John Willett, Marty West, Matt Kraft, and participants at the Fall 2015 Association for Public Policy Analysis and Management (APPAM) Research Conference for their feedback on earlier versions of this article.

Declaration of Conflicting Interests

The author(s) declared no potential conflicts of interest with respect to the research, authorship, and/or publication of this article.

Funding

The author(s) received no financial support for the research, authorship, and/or publication of this article.

Notes

1. Throughout this article, I use the term "Hispanic" to refer to the current Office of Management and Budget (OMB) definition of "Hispanic or Latino" for the sake of nomenclature consistency. The 1990 Census uses the term "Hispanic" exclusively. The current OMB standards specify "that race and Hispanic origin (also known as ethnicity) are two separate and distinct concepts. These standards generally reflect a social definition of race and ethnicity recognized in this country, and they do not conform to any biological, anthropological, or genetic criteria."

2. Census-block groups are statistical subdivisions of census tracts and aggregations of census blocks. They contain between 600 and 3,000 people, with an average of about 1,500 (U.S. Census Bureau, 2010). I select the census-block group geography as my unit of analysis because as a smaller unit than the census tract it permits more fine-grained examination of patterns of residential segregation. Census blocks are such small units of geography with very few residents that the demographic characteristics of the population residing within them are imprecisely estimated and public reports censor much of the data.

3. For all my analyses, I use the year of release from court order as the unitary shock, defined by Reardon, Grewal, Kalogrides, and Greenberg (2012) as inpaper2==1. Results are robust to using the first fall in which a new student-assignment plan was implemented, defined by Reardon et al. as inpaper1==1. Also, the number of districts in my sample is three fewer than Reardon et al. I exclude the Alabama districts of Leeds and Trussville and the Missouri district of St. Louis Special, since these districts did not exist in 1990. They were created after the start of the observations, which is potentially endogenous to the declaration of unitary status. Finally, I recode several metropolitan Kansas City districts' (Ft. Osage, Grandview, and Raytown) year of dismissal to 2003. These districts are all included in a metropolitan-wide desegregation order that was resolved simultaneously and were inaccurately assigned different years in the original Reardon sample.

4. I use White and Asian residents as the contrast group as these populations have, on average, higher incomes and school attainment rate than other racial and ethnic groups. In the dissimilarity results for Hispanics, I test the results for both Hispanics–White/Asians and Hispanics–non-Hispanics. This is motivated by a desire to investigate whether Hispanics became more physically isolated from historically privileged racial groups. The Hispanic–non-Hispanic measure would be a downward-biased estimate since the reference group would also include Blacks. An additional complicating factor is that, in the 2010 Census collection, White and Hispanic are potentially overlapping groups. Ultimately, my results are insensitive to this choice, and I report the Hispanic–White/Asian dissimilarity results because they most closely address the question above. However, it means that the measure will be underreported in 2010 when the White racial group also includes residents of Hispanic ethnicity. However, there is no reason to believe that this will affect the answer to my research question, since the rate of Hispanic–White overlap should not vary based on whether or when a district was declared unitary.

5. The isolation correlation index is defined as

$$IC = \frac{I_{jt} - P_{jt}}{1 - P_{jt}},$$

where $I_{jt} = \sum_{i=1}^{n} [(b_{it} / B_{jt})(b_{it} / t_{it})]$ and $P = B_{jt}/T_{jt}$. The notation in my study is similar to that above, but I examine the extent to which one racial group, Blacks (b) or Hispanics (h), lives in isolation compared with the total population (t). The isolation correlation index measures "the extent to which minority members are exposed only to one another" (Massey & Denton, 1988, p. 288). The number estimates the probability

that a member of the minority will share a housing unit area with another member of that minority.

6. The *N* for LEAs for the dropout rate results is lower than 480 in Panels B and C of Table 5 because some districts had no census-reported 16- to 19-year-old Blacks or Hispanics. This does not necessarily mean that there were no Black or Hispanic individuals of that age living in these districts, but because the Census does not report on estimates below a threshold of 5, if there were no census-block groups with greater than four Black or Hispanic 16- to 19-year-olds, a district would have no observations for this outcome. Similarly, the American Community Survey (ACS) does not report school-district estimates if there are fewer than 50 unweighted individuals aged 3 to 19.

References

Aizer, A. (2008). *Peer effects and human capital accumulation: The externalities of ADD* (NBER Working Paper No. 14354). Cambridge, MA: National Bureau of Economic Research.

Allport, G. (1954). *The nature of prejudice*. Reading, MA: Addison Wesley.

Ananat, E. O. (2007). *The wrong side(s) of the tracks: Estimating the causal effects of racial segregation on city outcomes* (NBER Working Paper No. 13343). Cambridge, MA: National Bureau of Economic Research.

Angrist, J. D., & Lang, K. (2004). Does school integration generate peer effects? Evidence from Boston's METCO program. *The American Economic Review*, *94*, 1613–1634.

Baum-Snow, N., & Lutz, B. (2011). School desegregation, school choice and changes in residential location patterns by race. *American Economic Review*, *101*, 3019–3046.

Bayer, P., Ross, S., & Topa, G. (2008). Place of work and place of residence: Informal hiring networks and labor market outcomes. *Journal of Political Economy*, *116*, 1150–1196.

Billings, S., Deming, D. J., & Rockoff, J. (2014). School resegregation, educational attainment and crime: Evidence from the end of busing in Charlotte-Mecklenburg. *Quarterly Journal of Economics*, *129*, 435–476.

Borjas, G. J. (1995). Ethnicity, neighborhoods, and human-capital externalities. *American Economic Review*, *85*, 365–390.

Boustan, L. P. (2012). School desegregation and urban change: Evidence from city boundaries. *American Economic Journal: Applied Economics*, *4*, 85–108.

Card, D., & Rothstein, J. (2007). Racial segregation and the Black–White test score gap. *Journal of Public Economics*, *91*, 2158–2184.

Carrell, S., & Hoekstra, M. (2010). Externalities in the classroom: How children exposed to domestic violence affect everyone's kids. *American Economic Journal: Applied Economics*, *2*, 211–228.

Chetty, R., Hendren, N., & Katz, L. (2016). The effects of exposure to better neighborhoods on children: New evidence from the Moving to Opportunity experiment. *American Economic Review*, *106*, 855–902.

Clotfelter, C. H. (2004). *After Brown: The rise and retreat of school desegregation*. Princeton, NJ: Princeton University Press.

Clotfelter, C. H., Ladd, H., & Vigdor, J. (2008). School segregation under color-blind jurisprudence: The case of North Carolina. *Virginia Journal of Social Policy and Law*, *16*(1), 46–86.

Clotfelter, C. T., Ladd, H., & Vigdor, J. (2006). Federal oversight, local control and specter of resegregation in Southern schools. *American Law and Economics Review*, *8*(Summer), 1–43.

Crain, R. L., & Strauss, J. (1985). *School desegregation and Black occupational attainments: Results from a long-term experiment* (Report No. 359). Baltimore, MD: Center for the Social Organization of Schools.

Cutler, D. M., & Glaeser, E. L. (1997). Are ghettos good or bad? *Quarterly Journal of Economics*, *112*, 827–872.

Darden, J., Rahbar, M., Jezierski, L., Li, M., & Velie, E. (2009). The measurement of neighborhood socioeconomic characteristics and Black and White residential segregation in metropolitan Detroit: Implications for the study of social disparities in health. *Annals of the Association of American Geographers*, *100*, 137–158.

Guryan, J. (2004). Desegregation and Black dropout rates. *The American Economic Review*, *94*, 919–943.

Hanushek, E., Kain, J., & Rivkin, S. (2009). New evidence about Brown V. Board of Education: The complex effects of school racial composition on achievement. *Journal of Labor Economics*, *27*, 349–383.

Hoxby, C. M., & Weingarth, G. (2005). *Taking race out of the equation: School reassignment and the structure of peer effects*. Unpublished manuscript.

Jackson, C. K. (2009). Student demographics, teacher sorting, and teacher quality: Evidence from the end of school desegregation. *Journal of Labor Economics*, *27*, 213–256.

Johnson, R. C. (2015). *Long-run impacts of school desegregation & school quality on adult attainments* (NBER Working Paper No. 16664). Cambridge, MA: National Bureau of Economic Research.

Liebowitz, D., & Page, L. (2014). Does school policy affect housing choices? Evidence from the end of desegregation in Charlotte-Mecklenburg. *American Educational Research Journal*, *51*, 671–703.

Lindseth, A. A. (2002). Legal issues relating to school funding/desegregation. In C. H. Rossell, D. J. Armor, & H. J. Walberg (Eds.), *School desegregation in the 21st century* (pp. 41–66). Westport, CT: Praeger.

Lutz, B. F. (2011). The end of court-ordered desegregation. *American Economic Journal: Economic Policy*, *3*, 130–168.

Massey, D., & Denton, N. (1988). The dimensions of residential segregation. *Social Forces*, *67*, 281–315.

Mickelson, R. A., Smith, S. S., & Southworth, S. (2009). Resegregation, achievement, and the chimera of choice in post-unitary Charlotte-Mecklenburg schools. In C. E. Smrekar & E. B. Goldring (Eds.), *From the courtroom to the classroom: The shifting landscape of school desegregation* (pp. 129–156). Cambridge, MA: Harvard Education Press.

Murnane, R. J. (2013). U.S. high school graduation rates: Patterns and explanations. *Journal of Economic Literature*, *51*, 370–422.

Pettigrew, T. F., & Tropp, L. R. (2006). A meta-analytic test of intergroup contact theory. *Journal of Personality and Social Psychology*, *90*, 751–783.

Reardon, S. F., & Bischoff, K. (2011). Income inequality and income segregation. *American Journal of Sociology*, *116*, 1092–1153.

Reardon, S. F., Grewal, E., Kalogrides, D., & Greenberg, E. (2012). Brown fades: The end of court ordered school desegregation and the resegregation of American public schools. *Journal of Policy Analysis and Management*, *31*, 876–904.

Reber, S. (2005). Court-ordered desegregation: Successes and failures in integrating American schools since *Brown*. *Journal of Human Resources*, *40*, 559–590.

Schwartz, H. (2010). *Housing policy is school policy: Economically integrative housing promotes academic success in Montgomery County, Maryland*. Washington, DC: The Century Foundation.

Tiebout, C. M. (1956). A pure theory of local expenditures. *The Journal of Political Economy*, *64*, 416–424.

U.S. Census Bureau. (2010). *2010 census redistricting data (Public Law 94-171) summary file*. Washington, DC: Author.

Vigdor, J. L. (2011). School desegregation and the black-white test score gap. In G. J. Duncan & R. J. Murnane (Eds.), *Whither opportunity? Rising inequality, schools, and children's life chances* (pp. 443–464). New York, NY: Russell Sage Foundation.

Vigdor, J. L., & Ludwig, J. (2008). Segregation and the test score gap. In K. Magnuson & J. Waldfogel (Eds.), *Steady gains and stalled progress: Inequality and the Black-White test score gap* (pp. 181–211). New York, NY: Russell Sage Foundation.

Weinberg, B. A. (2000). Black residential centralization and the spatial mismatch hypothesis. *Journal of Urban Economics*, *48*, 1110–1134.

Weiner, D. A., Lutz, B. F., & Ludwig, J. (2011). *The effects of school desegregation on crime* (NBER Working Paper No. 15380). Cambridge, MA: National Bureau of Economic Research.

Author

DAVID D. LIEBOWITZ is a policy analyst at the Organisation for Economic Co-Operation and Development (OECD). His research interests include student-assignment policies, the interaction of education policy with other social policies, and the evaluation of school resource allocation.

Manuscript received June 28, 2016
First revision received December 28, 2016
Second revision received April 19, 2017
Third revision received May 30, 2017
Fourth revision received July 20, 2017
Accepted July 20, 2017