Ending to What End? The Impact of the Termination of Court Desegregation Orders on Patterns of Residential Choice and High-School Completion

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THE IMPACT OF THE TERMINATION OF COURT DESEGREGATION ORDERS ON PATTERNS OF RESIDENTIAL CHOICE AND HIGH-SCHOOL COMPLETION

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Acknowledgments

I dedicate this work to the children of Chelsea, Massachusetts. I wish we lived in a society where you went to school with some kids who were rich and some who were poor, with some kids who were white, some who were brown, and some who were black. Until then, let's work together to make sure you learn a lot and change the world when it's your turn.

I would like to thank the Center for Education Policy Research at Harvard University and the Charlotte-Mecklenburg Schools who supplied Lindsay and me with the data for the first paper. The second paper in this thesis is possible only as a result of the tireless work of Sean Reardon, Elena Grewal, Demetra Kalogrides, and Erica Greenberg to compile the comprehensive list of school districts under court desegregation orders, an invaluable resource to researchers everywhere.

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A Note on Authorship

I have walked with many others on the path towards a more equitable and excellent system of American education This thesis reflects a small portion of the support I have received along the way. I had the good fortune to work with Lindsay Page on the first essay in this thesis. Though I played the primary role in developing the idea, researching the topic, analyzing the data, and writing the analyses in this first paper, the final product would not have been possible without Lindsay. This collaboration yielded an infinitely stronger paper.

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Abstract

The essays in this thesis examine the impact of the termination of court desegregation orders on patterns of residential choice and high-school completion. I do this by first examining decisions individual households make about where to live in the aftermath of a change in student-assignment policy using evidence from a single school district. Then, I generalize and assess trends in patterns of residential segregation and high-school dropout rates in a national study.

In the first essay, my co-author and I examine whether the legal decision to end race-conscious student assignment policies in the Charlotte-Mecklenburg school district increased the probability that families with children enrolled in the district would move to neighborhoods with a greater proportion of student residents of the same race as their own children. We make use of a natural policy experiment—a judicial decision to end court-ordered busing—to estimate the causal impacts of this policy shift on household residential decisions. We find that, for those who moved, the legal decision made white families with children in the Charlotte-Mecklenburg Schools substantially more likely than they were during desegregation to move to a neighborhood with a greater proportion of white residents than their own neighborhood.

In the second essay, I assess the impacts of the end of court desegregation orders on a comprehensive national sample of districts under court order in 1991. In a series of analyses, I conclude that the release of these districts from court desegregation orders increased the rates of black-white and, even more conclusively, Hispanic-white residential segregation. Furthermore, the declaration of districts as unitary increased rates of 16-19 year-old school dropouts in these districts by three to seven percentage points for Hispanics, one to two percentage points for blacks, and almost four percentage points for blacks living in school districts outside the South.

Taken together, these findings suggest that barring the use of race in the assignment of students to schools has deleterious effects on black and Hispanic students and the communities in which they reside.

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Introduction

American neighborhoods and schools are segregated by race and income. This current reality is a product of both historical acts of discrimination by federal, state and local agencies, as well as individual, private choices. In the late 1960s, the federal courts began an unprecedented experiment to require local school districts to take affirmative steps to desegregate their schools. Primarily in the South, but also in districts across the country, school boards were placed under judicial orders mandating that they design student assignment plans to ensure that the composition of schools matched the overall makeup of the school district.

This federal experiment was both controversial and limited in scope due to the localized nature of governance for schools in the United States. Nevertheless, the preponderance of evidence suggests the experiment had positive effects on children attending desegregated schools. However, in the early-1990s, the Supreme Court established standards to facilitate the release of school districts from 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 school systems. Since then, several studies have concluded that individual districts released from court mandates to implement race-conscious student-assignment policies experienced increases in the rates of school segregation, the sorting of more

effective teachers to white students, and increases in illegal behaviors among black males.

The essays in this thesis examine the impact of the termination of court desegregation orders on patterns of residential choice and high school completion. I do this by first examining decisions individual households make about where to live in the aftermath of a change in student-assignment policy using evidence from one school district. Then, I generalize and assess trends in patterns of residential segregation and high-school dropout in a national study.

In the first essay, my co-author and I examine whether the legal decision to grant unitary status to the Charlotte-Mecklenburg school district, which led to the end of race-conscious student assignment policies, increased the probability that families with children enrolled in the district would move to neighborhoods with a greater proportion of student residents of the same race as their own children. Motivated by the rich but inconclusive literature on the consequences of educational and residential segregation, we make use of a natural policy experiment—a judicial decision to end court-ordered busing—to estimate the causal impacts of this policy shift on household residential decisions. We find that, for those who moved, the legal decision made white families with children in the Charlotte-Mecklenburg Schools substantially more likely than they were during desegregation to move to a neighborhood with a greater proportion of white residents than their own neighborhood.

In the second essay, I leverage a comprehensive dataset constructed by Sean Reardon, Elena Grewal, Demetra Kalogrides and Erica Greenberg of Stanford University 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 of these districts from court orders increased the rates of black-white and, even more conclusively, Hispanic-white residential segregation. Furthermore, the declaration of districts as unitary increased rates of 16-19 year-old school dropouts in these districts by three to seven percentage points for Hispanics, one to two percentage points for blacks, and almost four percentage points for blacks living in school districts outside the South.

Taken together, these findings suggest that barring the use of race in the assignment of students to schools has deleterious effects on black and Hispanic students and the communities in which they reside.

DOES SCHOOL POLICY AFFECT HOUSING CHOICES? EVIDENCE FROM THE END OF DESEGREGATION IN CHARLOTTE-MECKLENBURG

David D. Liebowitz
Lindsay C. Page

DOES SCHOOL POLICY AFFECT HOUSING CHOICES? EVIDENCE FROM THE END OF DESEGREGATION IN CHARLOTTE-MECKLENBURG

1. Introduction

In 2007, the U.S. Supreme Court barred school districts from voluntarily using racial classifications in student assignment to correct de facto segregation in Parents Involved in Community Schools v. Seattle School District No. 1 et al. (Seattle), 551 U.S. 701 (2007). The plurality opinion by Chief Justice Roberts distinguished the voluntary choices of families to live in segregated communities from the governmentally mandated segregation of Jim Crow: "Where resegregation is a product not of state action but of private choices, it does not have constitutional implications." Seattle, supra at 735 quoting Kennedy, J. Freeman v. Pitts, 503 U.S. 495 (1992). Here, Roberts extended the legal theory first articulated in Pasadena City Board of Education v. Spangler, 427 U. S. 424 (1976) that has, since Board of Education of Oklahoma City v. Dowell, 498 U.S. 237 (1991), become the dominant framework for majority opinions in school desegregation and integration cases. In fact, in quoting Justice Kennedy's majority decision in Freeman, Roberts clearly intended to remind Kennedy, the swing vote in Seattle, of his words from 15 years prior: "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." Freeman, supra at 495.

The justices in these cases view educational segregation as a product of either: (1) governmental policies which explicitly assign students of different races to separate schools, in which case the state has a compelling interest to classify students by race to reassign them in an integrated fashion; or (2) individual choices and economic patterns

over which the courts have no say. The plurality in Seattle would limit the use of racial classification in student assignment policies only to instances where it is necessary to remedy the effects of past intentional discrimination.[1] Beyond these, however, a third possibility, articulated in Justice Breyer's dissent, is that "state action" which is not explicitly racially segregative may, nonetheless, lead to greater levels of residential segregation. If legal decisions and government policy actually cause residential segregation by changing the structure of incentives that drive private choices, then evidence of segregation resulting from state action might necessitate judicial remedy.

In this paper, we investigate evidence of segregative private actions in response to a court-mandated shift in student assignment policy in the Charlotte-Mecklenburg Schools (CMS). 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 Charlotte-Mecklenburg by ordering the school district to take affirmative steps to eliminate the vestiges of segregation "root and branch" (Green v County School Board, 391 U.S. 438 (1968)). The Court affirmed the constitutionality of judicially-mandated policies to re-zone attendance boundaries, transport students by bus, and pair children from different neighborhoods to ensure public school integration. Over time, school district administrators came to see this set of policies, developed by professor and NAACP consultant Dr. John Finger, as integral to promoting diverse schools. Thus, the Finger Plan remained in effect until William Capacchione sued the

[1] Most legal scholars believe that Kennedy's controlling opinion in Seattle also extends the benefits of educational diversity as a compelling interest to the K-12 educational context. In the aftermath of this opinion, the Office of Civil Rights and the U.S. Department of Education issued joint guidance in 2011 on permissible voluntary uses of race to achieve diversity and avoid racial isolation.

district to end its race-conscious assignment policy after his daughter was denied admission to a magnet school. To satisfy the legal requirements to end desegregation, the litigants were required to demonstrate that the district had eradicated its segregation-era practice of offering one set of schools to white students and another set to non-white students—a "dual" school system. After a series of appeals, the Fourth Circuit Court ruled that CMS had ended its two-track system and granted the district "unitary" status in 2001 (Belk v. Charlotte-Mecklenburg Schools, 269 F. 3d 305 (2001)). As a result, the district was released from its obligation to take proactive steps to integrate the schools. When the Supreme Court declined to hear the case in 2002, the district adopted the Family Choice Plan (FCP) for the 2002-2003 academic year and reverted to a neighborhood school system in 2005, ending three decades of race-conscious student assignment.

We argue that this court-mandated shift in student assignment policy creates a natural experiment with which to test the causal effects of the declaration of unitary status on residential segregation. Specifically, we employ an interrupted time series approach to investigate whether the judicial decisions ending court-mandated school integration policies caused families in Charlotte-Mecklenburg to move to neighborhoods in which the race of the children attending the public school there more closely matched their own child's race.

Earlier research, discussed below, suggests that the school desegregation orders from the 1960s and 70s did produce changes in housing patterns. We add to a large body of prior research on the effects of desegregation decrees and declarations of unitary status, and contribute, in particular, by modeling individual behavior and being substantially

more precise than prior studies in tracking residential movement. We find that although the end of the CMS desegregation policies had no immediate impact on the overall extent of residential segregation among families in the CMS system, for those families choosing to relocate within the county from one year to the next, it increased by half the odds that white families would select a residence located in a school attendance zone with a greater proportion of students who were white than their former residence. We take this result as evidence that "state action," which is not explicitly segregative, nonetheless has the potential to have long-run impacts on residential segregation.

This paper proceeds in six sections. In Section 2, we motivate our research and present the historical and legal background of court-ordered desegregation in Charlotte. In Section 3, we introduce our data. We discuss preliminary descriptive and graphical analysis in Section 4. In Section 5, we present results from models identifying individual household preferences. Finally, in Section 6, we discuss the implications of these findings.

2. Motivation and Context

In After Brown, the seminal quantitative work on the interrelationship between the courts and educational segregation, Clotfelter (2004) identifies three indicators that court-imposed desegregation orders from the late 1960s and 1970s led to "white flight" and increased residential segregation. First, in areas affected by desegregation home values declined in the aftermath of court orders. Second, white families with school-aged children moved out of jurisdictions with desegregated schools at a faster rate than white households without children. Finally, metropolitan regions consisting of smaller districts were more likely to experience relocation of white families following desegregation orders, because smaller districts permit families to sort themselves based on race. In

contrast, in districts covering entire metropolitan areas, white families were less likely to relocate—unless they were willing to also enter different labor markets or commute long distances—because such moves would not ensure that their children would attend racially homogenous schools. Clotfelter's descriptive findings are consistent with causal research examining housing prices along district boundaries (Boustan, 2012; Kane, Riegg & Staiger, 2006; Bogart & Cromwell, 2000), mechanisms of segregation and "white flight" (Baum-Snow & Lutz, 2011), and district coverage of metropolitan areas (Reber, 2005).

This last result is particularly important in the context of our study. The CMS school district covers the entirety of Mecklenburg County—over 500 square miles. With the caveat that some families opted for private schools,[2] few families moved out of Mecklenburg County in response to the original Swann decision (Clotfelter, 2004). Still today, CMS remains a remarkably racially and ethnically diverse district with a student population that in the 2012-2013 school year was 42 percent black, 32 percent white, 18 percent Hispanic, 5 percent Asian, and 3 percent multi-racial and other races. Thus, unlike other large metropolitan areas, many of which contain several districts serving different and racially homogenous student populations, CMS has the potential for racially integrated schools and communities without redrawing school district boundaries.

2.1 Historical and Legal Background

[2] A limitation of the analyses presented here is that we cannot observe when a student leaves for or returns to the CMS system from a private school. However, CMS enrollment has grown in a fairly linear trajectory between 1998 and 2008 (authors' analysis based on data from the National Center of Education Statistics, Characteristics of the 100 Largest Public Elementary and Secondary School Districts in the United States: 1998 – 1999 through 2008-2009). These trajectories provide no evidence that the district experienced an influx of students from private schools or a massive departure of students to private schools as a result of the declaration of unitary status.

In 1970, on the eve of the Swann case, the CMS schools, like many other Southern districts, had made only modest progress in desegregating its schools. Clotfelter (2004) uses a segregation index to describe the extent to which children are exposed to classmates of different races relative to the proportion of non-white students in a district. The index ranges from 0, indicating that all schools or neighborhoods have non-white enrollment proportional to the overall demographics of the district, to 1, indicating complete segregation of white students and non-white students.[3] The index is interpretable as the proportion of non-white 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. Figure 1, reproduced from Clotfelter's After Brown and supplemented with our own calculations, indicates that prior to the Brown decision in 1954, the CMS schools were entirely segregated, with a segregation index of 1. In 1970, the year before the Supreme Court decided Swann, the segregation index had only fallen to 0.63. By 1972, as a consequence of the Finger Plan policies that included re-zoning and gerrymandering attendance zones, pairing black, inner-city students and white, suburban students to attend the same school, and busing students between city core and suburbs, the index had fallen to nearly 0. The racial composition of every CMS school matched the racial makeup of the entire district. The index remained

[3] For a given district made up of j attendance zones, in time t, Clotfelter (2004) utilizes the following index measure of segregation: $S_t = \frac{n_t - \left(\sum_{j=1}^{n} W_{jt} n_{jt} / \sum_{j=1}^{n} W_{jt}\right)}{n_t}$. Here, for time t, n_t represents the proportion of residents who are non-white in the district, as a whole; W_{jt} represents the number of white residents in attendance zone j; and n_{jt} represents the proportion of residents who are non-white in attendance zone j. The quotient within the parentheses represents the overall exposure rate between whites and non-whites in the district. Since this value is sensitive to the overall proportion of non-whites, Clotfelter standardizes the exposure rate by the overall proportion of residents in the district who are non-white to generate a segregation index.

at or below 0.1 through the early 1990s, when CMS replaced mandatory busing for all students with a mix of busing and controlled choice among magnet schools. Rates of segregation rose gradually over the next decade before increasing sharply in the aftermath of the unitary status declaration, reaching 0.28 in 2003 and 0.33 for the 2009-2010 academic year. The increase in school segregation in the 1990s and 2000s in CMS corroborates Stroub and Richards's (2013) finding that Southern districts with prior de jure segregation experienced much more modest reintegration trends in the '00s than the rest of the country.

<<INSERT FIGURE 1 ABOUT HERE>>

The end of the race-conscious student assignment policy led to an increase in between-school racial segregation. This is unsurprising, since Charlotte's neighborhoods were still highly segregated between 1971 and 2002, despite the school desegregation order. The Family Choice Plan (FCP), implemented in 2002, afforded parents the option to apply to other schools throughout the district but guaranteed students a seat in their neighborhood school. Though many students sought to take advantage of the school choice provision of the FCP, most white students did not select out of their neighborhood schools. In fact, more than three-quarters of students in the predominantly white suburbs selected their home schools, with the rest opting for magnets. In contrast, only one-quarter of inner-city residents selected their home school (Doss & Melnik, 2002). These two patterns led suburban schools to become oversubscribed and open primarily to those families who lived in the neighborhood. Thus, while the FCP was a choice plan in name, families who did not live in a desirable school's zone had almost no chance to choose into it. In practice, therefore, the FCP was essentially a neighborhood assignment plan.

Furthermore, this policy was altered again in 2005 such that students were assigned to their neighborhood school with no formal choice options outside of limited specialty programs in magnet schools. Finally, only four charter schools had been operating in Mecklenburg County for more than two years at the time of the declaration of unitary status in 2002 (and only 10 as of the 2008-09 school year). As a consequence, we find it unlikely that charter options had a significant impact on families' ability to express their schooling choice through entry into charter lotteries rather than through residential moves. Thus, with little allowance for active school choice, CMS schools became more racially segregated after the policy change because of pre-existing patterns of residential segregation.

Numerous studies have exploited Charlotte-Mecklenburg's declaration of unitary status and the subsequent increases in school segregation to explore the impacts of the policy change on various outcomes. The new assignment policy has been identified as a cause of increases in school socioeconomic segregation (Mickelson, Smith, & Southworth, 2009), declines in the academic performance of white students and black students (Mickelson, 2003), the sorting of more effective teachers to non-minority students (Jackson, 2009), and large increases in criminality among non-white males (Billings, Deming & Rockoff, 2012). Furthermore, Hastings and colleagues (2006) find that families used the options afforded through the Family Choice Plan to select schools nearer their residence that were more racially homogenous, rather than higher performing. Despite increased school segregation, however, the policy change has not altered the black-white test score gap in CMS (Vigdor, 2011). What remains unclear is whether school segregation in CMS increased after 2002 only because families returned

to schools located in already-segregated neighborhoods, or whether the lifting of the court order actually contributed additively to segregation by affecting the residential choices of CMS families.

Weinstein (unpublished manuscript) recently found that, after the declaration of unitary status, resulting increases in the proportion of black students attending certain elementary schools led to subsequent increases in the proportion of black residents in those schools' residential assignment zones. Specifically, a 10 percentage point increase in the percent of black students assigned to an elementary school led to a 1.2 percentage point increase in the percent of CMS students who are black in the neighborhood after five years. Whereas Weinstein's identification is based on experienced post-declaration shocks to aggregate school and neighborhood composition, our dataset allows us to observe the choice each family makes between its current residence and all other available options. Consequently, whereas Weinstein finds neighborhood composition changes four and five years after the declaration of unitary status, we are able to observe the specific choices families made in the immediate aftermath of the new assignment policy. In Section 5, we discuss other key distinctions between our results and his.

2.2 Theoretical Framework and Model

We compare the racial composition and achievement of the school attendance zone in which each student resides in a given year (year t) to the characteristics of the choice set into which each student might move in the following year (year t+1), using each zone's characteristics as measured in year t. We theorize that families with school-aged children select their neighborhoods as a function of their personal characteristics,

interacted with the school-based amenities available to residents of the neighborhood and all other non-school neighborhood amenities, subject to their household budget constraint. Formally:

$$L = f(S, N, X) \text{ s.t. } g(I, U) \quad (1)$$

Where L is a family's location, S represents school-related housing amenities, N represents all other housing amenities, X represents household characteristics, I is income, and U is unearned income. During desegregation, we posit that school-based amenities associated with housing choice within a district weight only minimally, since a family's choice of residence does not determine the public school their children attend, or does so only temporarily. Once a district is declared unitary, however, families can exert their school and Tiebout (1956) preferences through housing choice. For some families, this may entail selecting into a neighborhood associated with a school with a greater proportion of students who are of the same race as their child than the proportion in their current neighborhood's school. Thus, S in Equation (1) can be written as a function of assigned school racial composition (R) and all other characteristics (A) such as safety, facilities, student achievement, teacher and staff qualifications, and proximity: $S = f(R, A)$.

From 1971 to 2002, parents who were unwilling or unable to remove their children from the Charlotte-Mecklenburg public schools were unable to control the racial composition of the school their children attended through residential choice. In fact, the district frequently re-paired schools and re-drew assignment boundaries to preserve the integrated nature of its schools. Thus, the Finger Plan and subsequent adjustments

ensured that the demographics of individual CMS schools were nearly identical to those of the district as a whole. Given the metropolitan coverage of the CMS district, we reason that, during this period, residential choices of families with school-aged children would have been motivated by the availability, price, and quality of the housing stock, the local provision of non-educational government amenities, and a wide variety of other non-observable factors, but importantly only minimally by the perceived quality of local neighborhood schools. Once the court-mandated assignment policies ended in 2002, however, parents could exercise their preferences for neighborhoods that maximized personal utility with respect to schools. For families whose utility was heavily influenced by school quality, after 2002, they could use residential choice to select a school they perceived to be of high quality—even if one criterion was racial homogeneity of the school.

To causally attribute changes in family residential decisions to the unitary status declaration, we must show that Mecklenburg County residents could not have anticipated the policy change. The long, protracted court battle over desegregation clearly signaled to families the possibility of policy shifts. However, as the timeline in Table 1 indicates, the District, Circuit and Supreme Courts were starkly divided over this case. After the District Court declared CMS to have achieved unitary status in 1999, the Fourth Circuit Court of Appeals overturned this ruling in 2000. Then, in 2001, the full en banc panel of 11 Fourth Circuit judges overturned the 2000 decision. However, the NAACP quickly appealed the Fourth Circuit ruling to the Supreme Court, which only decided against hearing the case in April of 2002. Given this pattern of events, we argue that it is unlikely that families would have made housing choices prior to the 2002-2003 school year that

were contingent on being able to select a school through their choice of residence. Even if it were possible for families to anticipate the new assignment policy, we reason that this behavior would, in fact, bias our results downward, particularly for whites. White families who preferred more racially similar schools and who anticipated the shift in school-assignment to a neighborhood-based system would, if responsive, have moved to neighborhoods with a higher proportion of white residents prior to the policy shift and before the expected rise in housing prices.

<< INSERT TABLE 1 ABOUT HERE >>

For several reasons, we anticipate a lag in residential sorting after the introduction of the new assignment plan in August 2002. First, families might have chosen to wait to assess whether the new race-neutral assignment policy was, in fact, a permanent fixture given all of the uncertainty in the lead-up to its adoption. In July 2001, the CMS board approved, in principle, the race-neutral assignment plan. The following January, students submitted applications for up to three schools, with each student's residential zone school as a default. The change in policy also included a grandfathering clause whereby students in a terminal grade (i.e., 5^{th}, 8^{th} or 12^{th}) who wanted to remain at their current schools were given high priority but not the guarantee to remain in that school. In February 2002, the district informed families of school assignment, but made clear that this assignment was provisional pending the Supreme Court ruling. Therefore, school assignment for the 2002-2003 school year remained uncertain until April 2002, leaving families very little time to move in response. Given the timing around the finalization of this policy, families may have been more willing to take a "wait and see" attitude regarding decisions around relocation as a means of influencing school choice.

In addition, the policy left little time for families to search for suitable housing options. It takes time to search for a new home or sell an existing one. In addition, renters may have been averse to the penalties associated with breaking a lease. Further, though parents knew in February 2002 the school to which they had been tentatively assigned for the 2002-2003 school year, they knew nothing about its new racial or socio-economic makeup. As such, they would have been likely to delay any move until they had a better sense of the school to which they had been assigned.

Both our framework and theoretical choice models first articulated by Schelling (1972), developed by McFadden (1973; 1979), and applied more recently by Mare and Bruch (2003; Bruch & Mare, 2006) to patterns of residential segregation, motivate our central research question: Did the unitary status declaration and the subsequent shift in student assignment policies increase the probability that families would move to a school assignment zone where the proportion of public school children who were of the same race as their own child was higher than the proportion in their current school assignment zone?

Building on Clotfelter (2004), we hypothesize that white families, in particular, may be responsive to the change in school assignment policy because they had greater financial resources on which to capitalize in exercising residential choice, on average. In 2000, median family income (in 1999$) was $72,043 among whites, $39,479 among blacks, and $36,416 among Hispanics in Mecklenburg County (Census 2000). Due to more limited resources, it follows that black and Hispanic families, though highly mobile, would be comparatively more constrained in expressing preference for wealthier, and consequently whiter, neighborhoods. As a result of both residential preferences and the

financial capacity to express them through residential relocation, we reason that, after the declaration of unitary status, white movers would prioritize moving to school attendance zones with schools that were both higher performing and lower minority.[4] Among those unable to afford such attendance zones, we anticipate that the next most preferred option would be for schools that were not necessarily better performing but that still served a greater proportion of white students than their current school.

3. Data

Our primary data source is student-level administrative data compiled by CMS for the years 1999-2009. This dataset includes 1,440,027 student records. We exclude all students living outside the boundaries of the school district (the CMS district and Mecklenburg County are co-extensive), which leads us to eliminate a few dozen records in each year. This rich panel dataset contains information on student demographic characteristics, school identifiers, course enrollment, test performance, and—most importantly for our analysis—student race/ethnicity and student home addresses for each year of attendance. We use ArcGIS software to geocode the addresses associated with over 99 percent of the person-period observations in our dataset. Through this process, we assign a longitude and latitude coordinate to each address and then match the coordinates of each student's home address to its relevant school assignment zone. We identify school assignment zones using CMS-provided school attendance zone boundary maps (called shapefiles in geocoding vocabulary). We utilize maps that are both year-

[4] Given the similar financial means blacks and Hispanics had to express their housing preferences, we compare white and non-white individuals rather than blacks and non-blacks as does Weinstein (unpublished manuscript). Whereas his model focuses on neighborhood composition for those targeted by desegregation, ours focuses on families' ability to express their preferences.

and grade-level (i.e., elementary, middle and high school) specific, as over the time period considered, school assignment zone boundaries were changing and different zones were relevant to children according to grade level. For example, in the 2001-2002 school year, there were 67 distinct elementary school attendance zones, 21 distinct middle school attendance zones and 12 distinct high school attendance zones.

We focus our analysis in this paper on elementary schools, because we hypothesize that families with middle- and high-school-aged children may be less responsive to the policy shift, given that they would have less time to reap the perceived benefits of a more racially homogenous setting. Indeed, as we note below, results for older students are similar to, but more modest in magnitude than, those for elementary-school students.

4. Graphical and Descriptive Analysis

4.1. Rates of Segregation

Figure 2 presents the nature and extent of residential segregation of students in CMS for grades pre-kindergarten through 5 between 2000 and 2007. The dark brown areas represent elementary attendance zones where greater than 80 percent of resident students are non-white while the pale yellow sections represent zones where greater than 80 percent of the resident students are white. The orange sections represent ranges between 20 and 80 percent non-white.

<<INSERT FIGURE 2 ABOUT HERE>>

The most striking feature in these maps is the extent of residential segregation in the district; the geographic area of the highly segregated elementary attendance zones is large and located in the most densely inhabited central neighborhoods of the city.

Importantly for the purposes of our study, in the years 2003 through 2007, we observe growing numbers of attendance zones of the 80 percent non-white type throughout the county. Though not shown here, this pattern is consistent for middle school and high school attendance zones.

Increases in the number of zones with high concentrations of white or non-white students, however, does not necessarily mean increases in levels of segregation. The overall proportion of CMS students who were non-white grew from half in 1999 to two-thirds in 2009—driven in part by dramatic growth in the Hispanic population. Therefore, even if there were no changes in overall patterns of residential segregation, we might expect to see more zones with greater than 80 percent non-white residents.

We employ the segregation index used by Clotfelter (2004) to investigate trends in the overall state of residential segregation between white children and non-white children attending CMS schools between 1999 and 2009. As above, when using this index, a value of one indicates complete segregation, and a value of zero indicates perfect integration—that is, the racial makeup of each school attendance zone in the district exactly matches the racial makeup of the whole district. Figure 3 plots Clotfelter's segregation index, calculated using the formula in footnote 4, for elementary school students. We use 2001-2 boundaries for all years of this analysis. Were we to employ different boundaries for each year, we would potentially confound changes in the segregation index resulting from re-districted attendance zones with changes in the index resulting from CMS families expressing segregative residential choices in response to the

policy change. As Figure 3 demonstrates, the overall status of segregation is nearly constant over the eight years and does not appear affected by the 2002 policy change.[5]

<<INSERT FIGURE 3 ABOUT HERE>>

4.2. Patterns of Mobility

In Table 2, we present descriptive information on the mobility of elementary school students. Each row corresponds to two school years. For example, the first row corresponds to school years 1998-1999 and 1999-2000. Students included in the analysis for this row are those who were in grades 4 and under in the 1998-1999 school year and in grades 5 and under in the 1999-2000 year. In this way, we restrict ourselves to families whose residential choices would pertain to elementary schools. The third column reports the proportion of elementary school students who were present in our data in 1998-1999 but were absent in 1999-2000. We refer to these students as leavers but are unable to differentiate whether they left Mecklenburg County entirely or instead remained in the county but turned to a non-public school option. The fourth column reports the proportion of students present in both years who moved across a school attendance zone boundary between the time their address was recorded in 1998-1999 and when it was recorded again in 1999-2000.[6] The fifth column represents those who are present in both years and

[5] When we plot the segregation indices for middle and high school, the trajectories of the lines are essentially parallel, though lower overall. Given the larger geographic area covered by middle- and high-school zones, it is unsurprising that the extent of segregation appears lower among these than among the elementary zones. We also find nearly identical patterns of segregation, though higher overall, when we use smaller geographic sub-units, such as census blocks.

[6] When examining a family's zone of residence between time period t and time period t+1, we consistently utilized the zone map associated with time period t+1 to determine zone membership. Therefore, a family is indicated as having moved to a new zone if it exhibits a change of address and a change of zone. In this way, a family cannot be flagged as having moved if its home address remains the same but the family resides in a new attendance zone because of changes in zone boundaries from one year to the next.

remained in the same attendance zone. The proportion of movers is fairly stable in all years with a minimum of 12.6 percent of households in 2008 and a maximum of 19.9 percent in 2000. While there is some year-to-year fluctuation in the proportion of families who move, we observe neither a discontinuity in the proportion of moves in the aftermath of the unitary status declaration nor a clear trend over time. Further, we see no trends aligned with the assignment policy change in the proportion of students who leave the district.

<<INSERT TABLE 2 ABOUT HERE>>

Figure 4 disaggregates the results in Table 2 by racial category. Again, there is no discontinuity in the proportion of any particular subgroup in the probability of moving around the change in assignment policy. In fact, there are no apparent differences in the trends of movement from one school attendance zone to another across different races. These results are not sensitive to whether we restrict the analysis to only those families who continue to send their children to CMS schools versus including those who leave the district.

<<INSERT FIGURE 4 ABOUT HERE>>

In short, the graphical and descriptive results presented here do not suggest that overall patterns of residential segregation or decisions to move were impacted by the declaration of unitary status. In the next section, we turn to examining how it may have impacted individual household preferences nevertheless.

5. Families' Revealed Preferences

Our analytic goal is to understand whether and the extent to which families' revealed preferences for racially homogenous school attendance zones changed in the

aftermath of the unitary status declaration. To assess this change in revealed preferences, we examine, among families who move, year to year changes in the likelihood of families selecting into a school attendance zone that is more similar to their child's own race than the zone that they depart. In order to do so, we require an analytic approach that allows us to describe individual household choice as a function of characteristics that are specific to the combination of the household and of each possible option from which a household can choose. We therefore utilize McFadden's conditional logit model, which allows us to examine the factors that govern a family's decision not only of whether to move but also of where to move. Long (2004) provides an illustrative application of this approach and particularly of the model's ability to handle covariates defined in a manner that is specific to the decision-maker and a particular option.

5.1 McFadden Conditional Choice Model

In this section, we first outline in formal terms the theoretical framework for why the conditional logit model properly describes the residential choices families will make. Then, we describe how we format our dataset to permit estimating the conditional choice model. Finally, we describe the model itself.[7]

Assume that a given family i, has j school attendance zones from which to choose and that each school zone can be described by a vector of characteristics Y_j. These characteristics might include average property value, school quality, local amenities,

[7] The following section relies extensively on: Long's (2004) excellent explanation of the conditional choice model in her analysis of college-going patterns in the last quarter of the 20th century; Mare and Bruch's (2003) analysis of residential mobility and segregation in Los Angeles; and Bruch and Mare's (2006) examination of the extent to which individuals respond to the racial makeup of their neighborhoods. Note also that the McFadden conditional logit model assumes the independence of irrelevant alternatives assumption. For more detail on this assumption and for evidence that this assumption is valid in investigations of locational choice, see Dahlberg and Eklöf (2003).

proximity to public transportation, and demographic (e.g., racial) make-up of the zone residents. Let X_i represent family characteristics such as race and prior school achievement of the children in the household. The value of the j^{th} attendance zone to family i is $U(Y_j,X_i)$. U denotes utility, and $U(Y_j,X_i)$ indicates that the utility that family i would gain from residing in attendance zone j is a function of the characteristics both of attendance zone j and of family i. Following these definitions,

$$U(Y_j,X_i) = E(Y_j,X_i) + \varepsilon_{ij}, \quad (2)$$

where, $E(Y_j,X_i)$ represents the mean utility of Y_j for individuals with a vector of characteristics X_i, and ε_{ij} represents the random variation among families that depends on unobservable preferences.

We assume that the non-random portion of a family's utility for a particular zone is a function of that school zone's characteristics and the interaction between school zone and household-level characteristics. These interactions represent household-zone specific measures. In contrast, household characteristics on their own are not included in considering utility for particular school zones, as a family's characteristics, in a vacuum, should not influence choice of residence. Rather, it is only how a family's characteristics match a neighborhood of potential residence that should have an effect on whether or not a family selects a given attendance zone. We highlight below why this point is important from an analytic perspective.

We assume that for each household, school zone selection will be utility maximizing, subject to the household's budget constraint. That is, family i selects Y_k if and only if:

$U(Y_k, X_i) \geq U(Y_j, X_i)$ for all $k \neq j$, subject to the household budget constraint. (3)

Therefore, our model considers each family's choice among the j potential school attendance zones. To fit our model, we organize the data as pair-wise combinations of each family i with each school attendance zone j, for a total of i×j observations. While the number of schools (and associated school attendance zones) varied somewhat from year-to-year, organizing the data in this way in each year yields between 67 and 78 observations for each family with an elementary-aged child.[8]

Having organized the data in this way, the model that we specify is made up of j equations for each family i with each equation describing one of the elements (i.e., zones) in the choice set. In fitting this model, we estimate the probability of each family i choosing to live in school zone j, relative to all other alternatives, in year t. The outcome, $ZONE_{ijt}$, is equal to one for the school zone actually chosen by family i in year t and zero for all other zones. This allows us to model explicitly the tradeoffs between the school zone selected and the unselected alternatives. The primary predictor variable interacts $LESS\_WHITE_{ijt}$, which is equal to one for each zone in which a higher proportion of the CMS students residing within it are non-white compared to the student's current zone of residence and zero otherwise. YEAR, represents a linear time trend re-centered on 0 in the year 2002, the year of the unitary status declaration. Finally, $POST_t$ is equal to one in

[8] A simplifying assumption that we make is to ignore the presence of siblings in the data set. This could potentially lead to bias in our results as a consequence of residuals that are correlated across children within families. The data do not include information on sibling pairs. As a sensitivity check, we identify presumptive siblings by linking those students with the same last name and home address across several years of data. Among sets of presumptive siblings, we then retain only one child and rerun our analyses. Both point estimates and standard errors associated with these sensitivity analyses are largely unchanged, and substantive conclusions remain the same. Given the robustness of our results to this sensitivity check, combined with a concern that the quality and accuracy of the matches (given the possibility of multiple last names among siblings), we nevertheless prefer the full sample results. Results from this sensitivity check are available upon request. Additionally, we minimize overestimation from siblings within each set of models by estimating results separately for elementary, middle and high school zones. We address correlated residuals in more detail below.

2003 and subsequent years and indicates the years after the declaration of unitary status.[9] Our research question asks whether the unitary status declaration caused families to make segregative moves; however, the change in assignment policy permitted families to control the makeup of their children's school according to observable school-level characteristics other than racial make-up. In order to differentiate moves that reflect a preference for more racially segregated neighborhoods and ones that reflect a preference for higher achieving schools, we also introduce a critical control variable, HI_ACH_{ijt}. HI_ACH_{ijt} is equal to one if the school associated with a particular zone has average standardized math and reading achievement scores on the North Carolina End-of-Grade assessments that are higher than the student's initial zone of residence. To capture the fact that one choice available to families is to not move, we capitalize on Mare and Bruch's (2003) strategy and include the control variable $STAY_{ijt}$ which is equal to one for the school assignment zone in which family i initially lived, and zero otherwise. The inclusion of STAY also permits the conditional choice model to capture the non-linear jump from a family deciding whether to move as opposed to deciding where to move. In certain specifications, we also include the student-level variable, $ACHIEVE_{ijt}$, a student's performance on the North Carolina End-of-Grade mathematics assessment, to detect

[9] Of course, a linear specification of time imposes a functional form constraint on this variable. While not presented here, we first fit a completely general specification of time, with dummy variables for each year. The results from this model indicate that a linear specification of time is reasonable. Results are available upon request.

whether families with children with higher academic performance might be more motivated to move in search of more homogenous schools and neighborhoods.[10]

The zone level variables STAY, LESS_WHITE, and HI_ACH in our model correspond to the characteristics of zone Y_j in Equations (2) and (3). Where MOVE is equal to 1, these characteristics are defined as Y_k. The interaction between these zone-level features and individual characteristics such as race and ACHIEVE equate to the household-zone characteristics of X_i. Thus, in our simple specification, the utility of a residence for a given family is a product of its zone-level racial and school characteristics, the zone characteristics interacted with the child's racial and achievement profile, and family- and child-specific unobservables.

For the sake of clarity in writing out the model below, we represent the control variables STAY, HI_ACH, and ACHIEVE and their interactions with each other and with LESS_WHITE as $\mathbf{C}_{ijt}$. $(\mathbf{C}\times\mathbf{T})_{ijt}$ represents a vector of the interaction between vector $\mathbf{C}_{ijt}$ and time variables, YEAR, POST and POST× YEAR. We fit the following conditional logit choice model:

$$P\left(ZONE_{ijt+1}\right) = \frac{exp^{Z_{ijt}\beta}}{exp^{Z_{i1t}\beta} + exp^{Z_{i2t}\beta} + \cdots + exp^{Z_{iJt}\beta}}, \text{ where}$$

$$Z_{ijt}\beta = \beta_1 LESS\_WHITE_{ijt} + \beta_2 LESS\_WHITE \times YEAR_{ijt} + \beta_3 LESS\_WHITE \times$$

$$POST_{ijt} + \beta_4 LESS\_WHITE \times POST \times YEAR_{ijt} + \mathbf{C}_{ijt}\gamma + (\mathbf{C}\times\mathbf{T})_{ijt}\delta + \varepsilon_{ijt} \qquad (4)$$

[10] Individual student correlations between scores on End-of-Grade reading and mathematics exams are well above .90. As expected, therefore, results based on the inclusion of this measure of student achievement are not sensitive to the choice of the mathematics, as opposed to reading, score.

The parameters of interest are β_3 and β_4, and their estimates will be negative and statistically significant for whites if the policy shift caused an increase in segregative zone choices among families who moved.

We underscore that both LESS_WHITE and HI_ACH are binary measures. While we recognize that this modeling choice results in some loss of granularity, we choose to use binary variables in our primary specifications because our central interest is in whether families are more likely after unitary status to make segregative moves and not in the more nuanced question related to the functional form of the relationship between zone selection and the size of differentials in racial makeup. Given this goal, the binary variables allow for clear and more easily interpretable results with respect to the key question of whether the policy change increased the likelihood of segregative residential movement. Nonetheless, we test whether our results are sensitive to using a continuous variable, $\text{NONWHITE\_DIFF}_{ijt}$, constructed by subtracting the proportion of non-white residents in each of the zones in an individuals' choice set from the proportion of non-white residents in their starting zone. Because of how we construct NONWHITE_DIFF, the parameters of interest on its interactions with time will be positive for whites if the policy shift caused an increase in the probability of segregative moves.

As noted above, for each consecutive pair of years (t, t+1), this analytic procedure involves estimating a set of j equations for each family i, as for each family we are interested in the probability of selecting from among a set of j options. As a consequence of estimating within each family, student- and family-level characteristics do not enter the equations as main effects. Rather, they enter as interactions with the characteristics of specific choices. Therefore, in order to incorporate student-level racial characteristics, we

simply estimate models separately for whites and non-whites. Given that our data includes a near-census of all students served by CMS, subsetting the data in this way does not unduly threaten the precision of our estimates.

Ideally, we would define time-specific alternatives and person-period cases in the conditional logit model and then cluster standard errors at the student level to reflect the fact that some households are observed in the data for several years. Without these additions, the model is already computationally intensive. Unfortunately, when we attempt to fit this augmented specification, the model fails to converge. Therefore, we treat each period-specific observation of an individual as independent from any other-period observation of that person. Specifying the model in this way treats each individual's probability of moving in a given year as independent from choices made in any prior year. This simplifying assumption means that our residuals will necessarily be correlated, leading to an underestimate of our parameter estimate standard errors. To address this concern, we conducted sensitivity analyses to explore the extent to which our standard errors were underestimated. In order to do so, within each family's set of observations (each corresponding to a possible move from year t to year t+1), we randomly sampled a single observation and refit our model with this reduced sample. While it would be feasible, technically, to use this approach to generate standard errors empirically (similar to bootstrapping), this was impossible given the computational demands of fitting even a single iteration of the conditional logit model with a dataset of this size. Therefore, we repeated this procedure ten times in order to gauge the extent to which we should inflate our standard errors. This procedure indicated that an inflation factor of 1.5 to 2 for all standard errors serves as a suitable and conservative adjustment.

In all results, we present the unadjusted standard errors but consider the standard error inflation factor in assessing statistical significance. This sensitivity check also yielded point estimates that were essentially unchanged from those presented below, providing assurance that the repeated observation of children overtime did not lead to bias in our point estimates.

5.1 Results

Table 3 displays results of pooled-year conditional logit regressions for elementary school students. For each model, the first column presents results pertaining to white students, and the second column presents results pertaining to non-white students. Model I displays results from the simplest specification, which includes the variables STAY, LESS_WHITE, and their interactions with the time variables. Parameter estimates associated with STAY describe trends in mobility across school attendance boundaries over the eleven-year period between 1998-1999 and 2008-2009. Together, these estimates reveal that, as expected, the most frequent residential choice families made was to not move. Note that because the majority of families do not move and because of the relatively high starting level of residential segregation (Figure 2), our modeling of families' revealed preferences will describe choices of marginal movers rather than all families.

Considering results for white families, the negative coefficient on the main effect of LESS_WHITE indicates that across all years, when white families with elementary students chose to move, they had much lower odds of moving to zones with a larger share of non-white students (compared to zones with an equal or lower share of non-white students) than their current zone. LESS_WHITExPOST is the key variable of interest. The

log-odds coefficient on LESS_WHITExPOST for white students is a statistically significant -0.403.

By exponentiating the linear combination of the coefficients, we obtain the associated odds ratios. In interpreting these results for white families, we also invert in order to illustrate the preference of white families for attendance zones with greater shares of students who were white—moves that are segregative in nature. These calculations reveal that between Spring 2002 and Spring 2003, the odds that a white family who moved would relocate to a whiter school attendance zone was an estimated 3.1 times the odds that the family would move to a less white zone. Therefore, even prior to the declaration of unitary status, when white families chose to move, they exhibited a strong preference for communities that were less integrated than their starting community. After the declaration of unitary status, however, this preference became even stronger. Between Spring 2003 and Spring 2004, the estimated odds ratio was 4.5-to-1. Evidence of this elevated preference continues through the subsequent years. Whereas Weinstein (unpublished manuscript) finds small and insignificant changes in neighborhood racial composition in the immediate aftermath of the unitary status declaration, this result provides evidence of immediate effects on the revealed preferences of households as a result of the policy change.

Model II of Table 3 introduces the additional school quality variable, HI_ACH and the interaction between HI_ACH and LESS_WHITE, which we find to be a meaningful predictor of residential decisions. We continue to observe a large, robustly significant, and negatively signed coefficient on LESS_WHITExPOST. The results in Model II indicate that after 2002, white families were much more likely to select into a

whiter but worse performing zone than their current one. However, they were no more likely to select into a whiter and academically stronger neighborhood than before the new assignment policy.

While white families with elementary school students exhibited a decline in their odds of moving to a more non-white zone, non-white families' odds of making the same move was more stable during this time. The coefficient on LESS_WHITExPOST in Table 3, Model I is insignificant when the standard errors are inflated. However, when we control for the academic quality of the school in the zone in Model II, non-whites had a discontinuous decrease in the likelihood that they would move to more non-white zones after the implementation of the new assignment policy.

<<INSERT TABLE 3 ABOUT HERE>>

It is difficult to interpret any particular coefficient in Table 3 in substantive terms because of the multiple interactions. Therefore, we illustrate trends in preferences for white and non-white elementary families in Figure 5. For white families, Panel A of Figure 5 plots the odds-ratio of moving to a zone that has a greater proportion of students who are white, according to whether the zone has higher average levels of student achievement than the family's current zone. For non-white families, Panel B of Figure 5 plots the odds-ratio of moving to a zone that has a greater proportion of students who are non-white, again controlling for HI_ACH.[11] That is, these panels indicate the odds that white families and non-white families will make segregative residential choices. Panel A

[11] Panel A of Figure 5 can be reproduced from Table 3, Model II by exponentiating the inverse of the linear combination of all the coefficients on whites. Panel B is also constructed from Table 3, Model II but requires no inversion as a move in a segregative direction for non-whites is to a zone where LESS_WHITE=1.

illustrates that white families showed a clear preference for moving to zones that had a higher proportion of white students living in them, whether they were higher performing or not. If students in the zone, on average, performed equal to or worse than students in a family's current zone on state math and reading assessments, however, the effect of zone racial composition on the probability that a family would move there varied substantially before and after the unitary status declaration.

Among white families, the estimated odds of moving to a lower-performing, whiter zone compared to a lower-performing, more non-white zone following the 2001-2002 school year were 3.3-to-1. The odds of making this same move increased to 5.2-to-1 following the 2002-2003 school year. The odds of moving towards a lower-performing, whiter zone remained elevated above pre-unitary levels for the subsequent five years. White families were about twice as likely to select a zone that had a greater proportion of white student residents if student performance in the zone was either weaker or as strong as the student's current zone. Panel B of Figure 5 illustrates analogous trends among non-white families. As indicated in the discussion above, we observe less dramatic shifts in the school-zone preferences of non-white families immediately after the unitary status declaration followed by a return to levels observed prior to the policy change.

<<INSERT FIGURE 5 ABOUT HERE>>

In Model III (Table 3), we include students' academic achievement (only available for elementary students in grades 3 through 5), and the interaction between individuals' achievement and the zone's racial composition and overall performance (full sets of parameters on the ACHIEVE variable and its interactions are available upon request). First, we find that white students with higher levels of academic performance

are less likely to move. Among those white families who do move, those with higher achieving students are more likely to move to a better performing school zone. We find no relationship between white students' academic performance and whether they will move to a whiter school zone or a better, less white zone. Higher performing non-white students, however, are much less likely to move to a more non-white zone, controlling for zone-level average academic performance. Most importantly for this investigation, the coefficients on the parameters of interest are consistent with those we present in Model II. Since the inclusion of student achievement controls does not alter the substantive results of our analysis, we select the more parsimonious specification, Model II, as our preferred model.[12]

When we use the continuous predictor variable, NONWHITE_DIFF to describe the discrepancy in racial composition of individuals' starting and ending zones, our results are consistent with those in the dichotomous model. White students are more likely to move to whiter zones after the start of the new assignment policy, but this shift is specific to zones that are no stronger academically then their starting residence. We also disaggregate the category of non-white into the race-specific categories of Black and Hispanic (all other ethnic groups combined represent less than 10 percent of the population). We find no meaningful differences between these results and those using a binary definition of race.

[12] We observe similar, though less dramatic and immediate, patterns for white families with middle- and high-school students. Odds-ratios for white middle-school families to move to whiter, but no better schools increased from 2.7:1 in 2002 to 5.2:1 in 2004, an increase of six times the standard error. Odds-ratios increased at the high-school level, but not at statistically significant levels. The delay at the middle-school level may be partially explained by the grandfathering policy for 8th graders in the choice lottery.

5.3 Alternative Explanations

Though our results reveal changes in residential choices aligned to the new assignment policy, we must consider whether alternative explanations are equally plausible. It is possible that policy changes other than the unitary status declaration produced the discontinuity that we observe. A key shift in national educational policy came in January 2002 when President George W. Bush signed the No Child Left Behind Act (NCLB) into law, and states were required to release public report cards on school performance in the Fall of 2003 for the 2002-2003 school year. Thus, it is possible that our results are a consequence of families taking advantage of newly available information on school composition and quality. The fitted probabilities for better and worse performing assignment zones, however, do not show evidence of white families selecting into better schools at higher rates in the aftermath of the policy change. In fact, the coefficient on HI_ACHxPOST in Model II of Table 3 and a series of year-by-year regressions we conducted, show that white families were less likely to move to a better performing elementary or middle school zone in the aftermath of NCLB.[13] In addition, NCLB likely had little impact on information publicly available on school-level performance in CMS. Even before NCLB, North Carolina already had a strong educational accountability system based on student achievement (Carnoy & Loeb, 2003). For example, students took End-of-Grade standardized tests, teachers could earn bonuses

[13] In fact, when we measure HI_ACH as a continuous variable and compare the difference in average test scores between movers' current zone and the zone to which they move, we find that for white movers, the majority of moves are to a zone assigned to a marginally lower performing school, rather than substantially so (histogram available upon request). We take this result, combined with the much larger coefficient on LESS_WHITE as compared to HI_ACH, to suggest that white movers view race as a stronger signal for school quality than test scores.

tied to student achievement, schools were recognized publicly according to levels of test score performance, and schools faced potential sanctions or interventions for less than adequate test performance (Lauen & Gaddis, 2010). Taken together, it is unlikely that NCLB provided CMS families with much additional information with which to assess and select among the district's schools.

While the revealed preferences of white movers were for whiter neighborhoods, we cannot eliminate the possibility that families' preferences may not be explicitly driven by race. Instead, these white families could be choosing to move to whiter zones that had other preferred amenities that were also correlated with their racial composition. While our data do not permit us to exclude the possibility that household preferences were driven by zones' socio-economic makeup, most of the potentially observable zone differences are also likely correlated with school performance. The inclusion of HI_ACH in our models, and all the correlated amenities of a neighborhood with a stronger school, does not reduce white families' preference for whiter neighborhoods, post-unitary decision. Minimally, we have demonstrated that white families moved in segregative directions post-unitary declaration, even if race may not have been the sole driver.

Another major threat to valid causal inference based on interrupted time series analysis is that individuals can anticipate a policy shift and respond preemptively, prior to the policy's enactment. Here, however, the tumultuous legal history in CMS suggests this is unlikely. Nonetheless, there is a concern that those families who chose to move across school attendance boundaries prior to the declaration of unitary status were different in some meaningful way from those who chose to move afterwards. If this were the case, it would imply that the end of the race-conscious assignment plan did not affect

individuals' residential choice, but rather that it induced a different set of families with different preferences to move. This might also mean that a return to the previous policy would yield no integrative benefits. As reported in Figure 2, we find that in all years black families and Hispanic families were more likely than white families to move across school attendance zone boundaries. This may be because black families and Hispanic families are, in general, more mobile than their white counterparts. It may also be that black families and Hispanic families live in more densely populated, smaller school attendance zones such that moves that they make may be more likely to carry them across school attendance zone boundaries. Indeed, when we examine the share of families who move either within or across school attendance zones, a much larger share of moves made by black families and Hispanic families carry them across school attendance zone boundaries. Over the years considered, we observe, in general, a downward trend in the proportion of families moving across school attendance zone boundaries. This same pattern is true for white families, black families, and Hispanic families. Nevertheless, as we report in Table 2 and Figure 4, we do not observe any large, discontinuous jumps in the share of families moving across school attendance zone boundaries, such that the validity of our substantive conclusions is threatened.

6. Discussion

The end of the race-conscious student assignment policy in Charlotte-Mecklenburg increased families' ability to use residential choice to exercise school choice. For white families who moved during this time period and whose vector of housing preferences included neighborhood racial composition, the end of desegregation led them to be more likely to pick a neighborhood (or school zone) with a greater

proportion of white student residents. Two equally plausible mechanisms may drive these patterns. One explanation is that white movers fall into two groups with distinct preferences. One set of movers prioritizes improved schooling quality as measured by student achievement on state assessments. The rate at which these families selected zones with better-performing schools was unaffected by the unitary status declaration. Another set of movers prioritizes racially isolated residential and educational settings for their children, and these families were more easily able to exercise these preferences after CMS enacted the new assignment policy. A second, competing explanation is that white movers have broadly similar preferences for neighborhoods and schools that are both whiter and academically stronger. However, within white movers, only the subset of wealthier families is able to access preferred housing stock in neighborhoods that are both whiter and assigned to stronger schools. In this explanation, there was no disruption in the secular relocation trends of the movers with greater financial resources on which to draw. These families continued to consistently prefer homes that were in whiter neighborhoods and assigned to stronger schools. Their poorer counterparts, however, could not buy into their most preferred neighborhood. Instead, they selected neighborhoods that, while providing them schooling choices that were not higher performing, offered more racially similar surroundings for their children. Unfortunately, our data lack student-level socio-economic information and, therefore, do not permit us to distinguish between these explanatory mechanisms.

The change in revealed preferences was sudden and consistent over the following five years. Nonetheless, it does not appear to have led to substantially different overall levels of segregation in the short term. This is not surprising. Although white families

who moved after the declaration of unitary status in CMS were more likely to move in segregative ways, white families, overall, were very stable in their residential choices, with only five to ten percent of those with elementary school students moving across attendance zone boundaries in a given year. Additionally, non-white movers were marginally more likely to move to whiter neighborhoods after the changed assignment policy. These moves would serve to counterbalance some of the segregative choices white families made during these years. Ultimately, together with the already-high starting segregation levels, marginal movers alone may be insufficient to produce changes in Clotfelter's index of segregation, at least in the short run.[14] Though we do not observe a change in the status of segregation, the rate at which individual families make choices over time that contribute to or limit segregation is the first derivative of the overall trend in segregation. Thus, we should expect to observe rising values for the segregation index in the years to come. Given the current district policy that relies on assignment zones based on residence, one would expect this to compound levels of residential segregation in the district over time.

In light of the compounding effects that we find of race-neutral plans on residential choices, and the Seattle decision's limits on the use of race-conscious student assignment policies, American schoolchildren have increasingly fewer opportunities to benefit from residential and educational integration. A large body of research points to the potentially detrimental results of this pattern. First, "network" and "social contact"

[14] To examine this further, we conducted a stylized simulation of residential movement over time. While stylized, it reveals a pattern similar to that observed in the CMS data and illustrates that changes in preferences can be wholly consistent with lack of change in the overall segregation measure, particularly given the low prevalence of moving. Details on the simulation are available upon request.

theories suggest that the connections that result from the friendships and acquaintances that children form in their neighborhoods positively affect their future life outcomes (cf. Allport, 1954; Bayer, Ross, & Topa, 2008; Ellison & Powers, 1994). Further, some evidence indicates that children, particularly black children and Hispanic children, who grow up in integrated communities have higher educational attainment, stronger labor market outcomes, and better health than their peers living in segregated neighborhoods (cf. Ananat, 2007; Angrist & Lang, 2004; Borjas, 1995; Card & Rothstein, 2007; Clark & Drinkwater, 2002; Cutler & Glaeser, 1997; Darden et al., 2009; Durlauf, 2004; Schwartz, 2010; Weinberg, 2000; Williams & Collins, 2001).

A separate set of literature has documented academic, non-cognitive and pro-social benefits of integrated education (Linn & Welner, 2007; Vigdor & Ludwig, 2008); though some debate persists as to whether existent research has successfully disentangled the independent effects of racial segregation from other confounding variables (cf. Armor, Thernstrom, & Thernstrom, 2006). Recent evidence suggests that court-ordered desegregation improved black students' high school graduation rates (Johnson, 2011; Guryan, 2004), whereas post-desegregation racial sorting worsened the black-white test score gap (Hanushek, Kain, & Rivkin, 2009; Hoxby, 2000) and increased black dropout rates in the north (Lutz, 2011). A combination of cross-sectional (Braddock & Eitle, 2004), longitudinal (Johnson, 2011) and experimental (Crain & Strauss, 1985) research documents improved post-secondary, labor-market, health and incarceration outcomes for students in desegregated schools. The most rigorous meta-analysis of the impact of school diversity on race relations documents substantially reduced inter-racial prejudice (Pettigrew & Tropp, 2006). Furthermore, where educational segregation exists, there is

potential for resources and educational quality to be distributed unequally across schools and, therefore, across racial groups (Clotfelter, 2004). Therefore, all else equal, policy makers interested in racial equity should prefer school assignment policies that promote maximal integration to avoid the potential for inequitable school conditions. Indeed, research conducted by the Center for Education Policy Research (CEPR) in Charlotte-Mecklenburg found that poor students and students of color in CMS were less likely to be taught by a teacher with tenure, with National Board certification, or who had attended a competitive college. Instead, they were more likely to be taught by a teacher with less experience, who was a late hire, or who was a novice (CEPR, 2010).

In this paper, we provide evidence that "state action," which is not explicitly segregative, may nonetheless have a causal impact on individual residential choices that over time may lead to greater levels of residential segregation. Such a dynamic process might require a review of precedent barring the use of race to promote integrated schools. To be sure, race-neutral assignment policies that have disparate impacts are distinct from the stigmatizing effects of "segregation with the sanction of law" (Brown v. Board of Education, 347 U.S. 483 (1954)). Nevertheless, our findings suggest that the line is not clear between formalized de jure segregation and informal de facto segregation resulting from residential choices. It is this lack of distinction that may necessitate a reexamination of current Constitutional jurisprudence.

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Table 1. Chronology of declaration of unitary status and return to neighborhood schools.

Date	Event
Sep. 1997	William Capacchione files suit in District Court alleging that the district violated his daughter's equal protection rights when she did not gain entrance into a magnet school because she was non-Black. District opposes suit, hoping to remain under court order.
Apr. 1998	Group of six white families joins suit as "plaintiff-intervenors" to ensure standing after Capacchione family moves to California.
Aug. 1998	Two black CMS families, Belk and Collins, permitted to join suit on behalf of desegregation since Swann children had graduated from CMS.
Apr. 1999	District Court Judge Potter hears Capacchione v. CMS. CMS Board approves school choice plan as contingency against unfavorable ruling.
Sep. 1999	Judge Potter declares CMS unitary and therefore released from mandatory race-conscious student assignment policy.
Oct. 1999	CMS declares intent to appeal ruling.
Dec. 1999	4th Circuit Court of Appeals issues stay on District Court ruling.
Jun. 2000	Three-judge panel of 4th Circuit hears appeal.
Dec. 2000	Three-judge panel finds CMS to still be operating dual system of schools; overturns District Court decision and preserves race-conscious assignment plan.
Feb. 2001	4th Circuit en banc [all 11 judges] hearing of Capacchione case.
July 2001	Board approves new student assignment plan pending outcome of court case.
Sep. 2001	4th Circuit, sitting en banc overturns Dec. 2000 decision, declares district unitary.
Jan. 2002	Belk plaintiffs appeal Capacchione decision to Supreme Court.
Apr. 2002	Supreme Court denies certiorari; Sept. 2001 4th Circuit decision stands.
Aug. 2002	Charlotte-Mecklenburg schools open with race-neutral Family Choice Plan.
Aug. 2005	Charlotte-Mecklenburg schools end Family Choice Plan; return to neighborhood school assignment.

Table 2. Descriptive statistics on the proportion of elementary school students who leave CMS entirely, move to a new school attendance zone or stay in the same school attendance zone from one school year to the next (1999 to 2008).

Year	Year	p(leave)	p(move)	p(stay)	N (w/leavers)
1998-1999 to 1999-2000	1999	9.41	13.78	76.81	49,522
1999-2000 to 2000-2001	2000	13.09	19.88	67.03	49,220
2000-2001 to 2001-2002	2001	9.94	16.51	73.55	48,511
2001-2002 to 2002-2003	2002	10.06	15.85	74.09	49,848
2002-2003 to 2003-2004	2003	9.36	16.2	74.44	51,716
2003-2004 to 2004-2005	2004	9.65	18.55	71.79	54,776
2004-2005 to 2005-2006	2005	6.55	15.02	78.43	58,499
2005-2006 to 2006-2007	2006	9.03	14.34	76.64	64,086
2006-2007 to 2007-2008	2007	9.58	13.67	76.75	67,888
2007-2008 to 2008-2009	2008	10.61	12.62	76.77	66,949

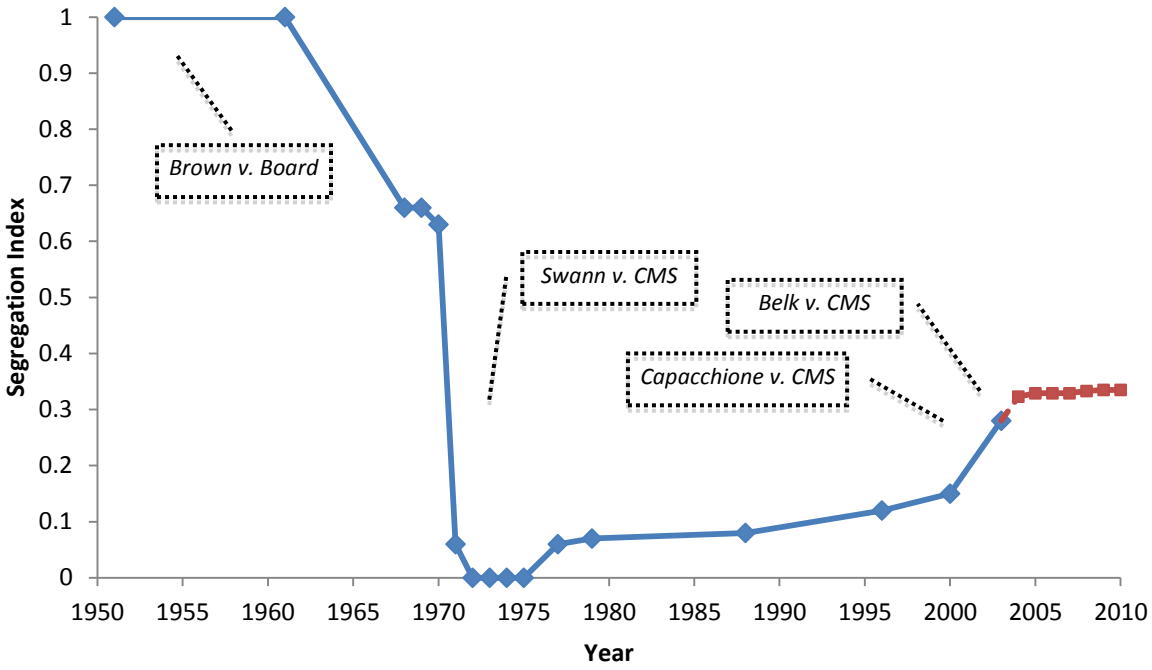
Table 3. McFadden choice models predicting log-odds of elementary CMS students selecting school zone based on whether zone has greater proportion of non-white students, controlling for whether school zone represents current residence, has better math and reading performance, and individual student achievement (1999 to 2008).

	I		II		III	
	White	Non-white	White	Non-white	White	Non-white
stay	6.038***	5.327***	5.989***	5.085***	5.866***	5.251***
	(0.031)	(0.015)	(0.042)	(0.018)	(0.070)	(0.038)
less_white	-1.116***	0.208***	-1.196***	-0.030	-1.183***	-0.139**
	(0.046)	(0.021)	(0.055)	(0.023)	(0.095)	(0.051)
hi_ach			-0.095	-0.691***	-0.237	-0.658***
			(0.059)	(0.031)	(0.104)	(0.067)
hi_achXless_white			0.466***	0.521***	0.278	0.471**
			(0.129)	(0.067)	(0.245)	(0.150)
stayXyear	0.074*	-0.010	0.070	0.001	0.017	0.010
	(0.015)	(0.008)	(0.021)	(0.010)	(0.036)	(0.021)
less_whiteXyear	0.018	0.026	0.008	0.024	0.002	0.045
	(0.023)	(0.011)	(0.028)	(0.013)	(0.049)	(0.028)
hi_achXyear			-0.006	0.036	-0.058	0.021
			(0.029)	(0.017)	(0.052)	(0.037)
hi_achXless_whiteXyear			0.094	0.090	0.030	0.116
			(0.065)	(0.036)	(0.121)	(0.084)
stayXpost	-0.342***	0.074	-0.433***	-0.133*	-0.360	-0.237*
	(0.046)	(0.023)	(0.071)	(0.027)	(0.120)	(0.057)
less_whiteXpost	-0.403**	-0.080	-0.480**	-0.271***	-0.387*	-0.342*
	(0.070)	(0.031)	(0.091)	(0.035)	(0.156)	(0.076)
hi_achXpost			-0.120	-0.381***	0.044	-0.245**
			(0.091)	(0.046)	(0.160)	(0.093)
hi_achXless_whiteXpost			0.514	-0.057	0.423	-0.044
			(0.183)	(0.096)	(0.348)	(0.209)
stayXpostXyear	0.050**	0.098***	0.086	0.120***	0.125	0.130**
	(0.018)	(0.009)	(0.026)	(0.011)	(0.044)	(0.023)
less_whiteXpostXyear	-0.002	-0.014	0.010	0.018	-0.004	0.015
	(0.027)	(0.013)	(0.034)	(0.014)	(0.059)	(0.031)
hi_achXpostXyear			0.058	0.049	0.084	0.051
			(0.035)	(0.019)	(0.062)	(0.040)
hi_achXless_whiteXpostXyear			-0.104	-0.072	0.018	-0.106
			(0.072)	(0.039)	(0.136)	(0.090)
Controls for individual achievement	**NO**		**NO**		**YES**	
N combinations	13,614,279	23,205,286	13,614,279	23,205,286	4,978,356	7,261,991
Strata (individuals)	186,694	314,882	186,694	314,882	68,382	98,638
Alternatives (zones)	728	728	728	728	728	728

Statistical significance at: ***.001 level, **.01 level, *.05 level

Note: Standard errors (SEs) presented here are the raw SEs from the non-clustered estimation procedures. A conservative inflation multiplier of 2 should be applied to all SEs. Indicators of statistical significance in this table are based on the inflated standard errors.

Figure 1. Segregation index in Charlotte-Mecklenburg Schools, 1950-2010.



Note: 1950-2003 reproduced from Clotfelter, C. (2004). After Brown: The Rise and Retreat of School Desegregation. 2003-2010 based on authors' calculations from CMS administrative data. Authors' calculations are .02 units higher for the 2002-2003 school year than Clotfelter's. The increase between 2003 and 2004 is, therefore, likely overstated by about .02 units.

Figure 2. Patterns of high-concentration white and non-white elementary attendance zones, using 2001-2002 boundaries.

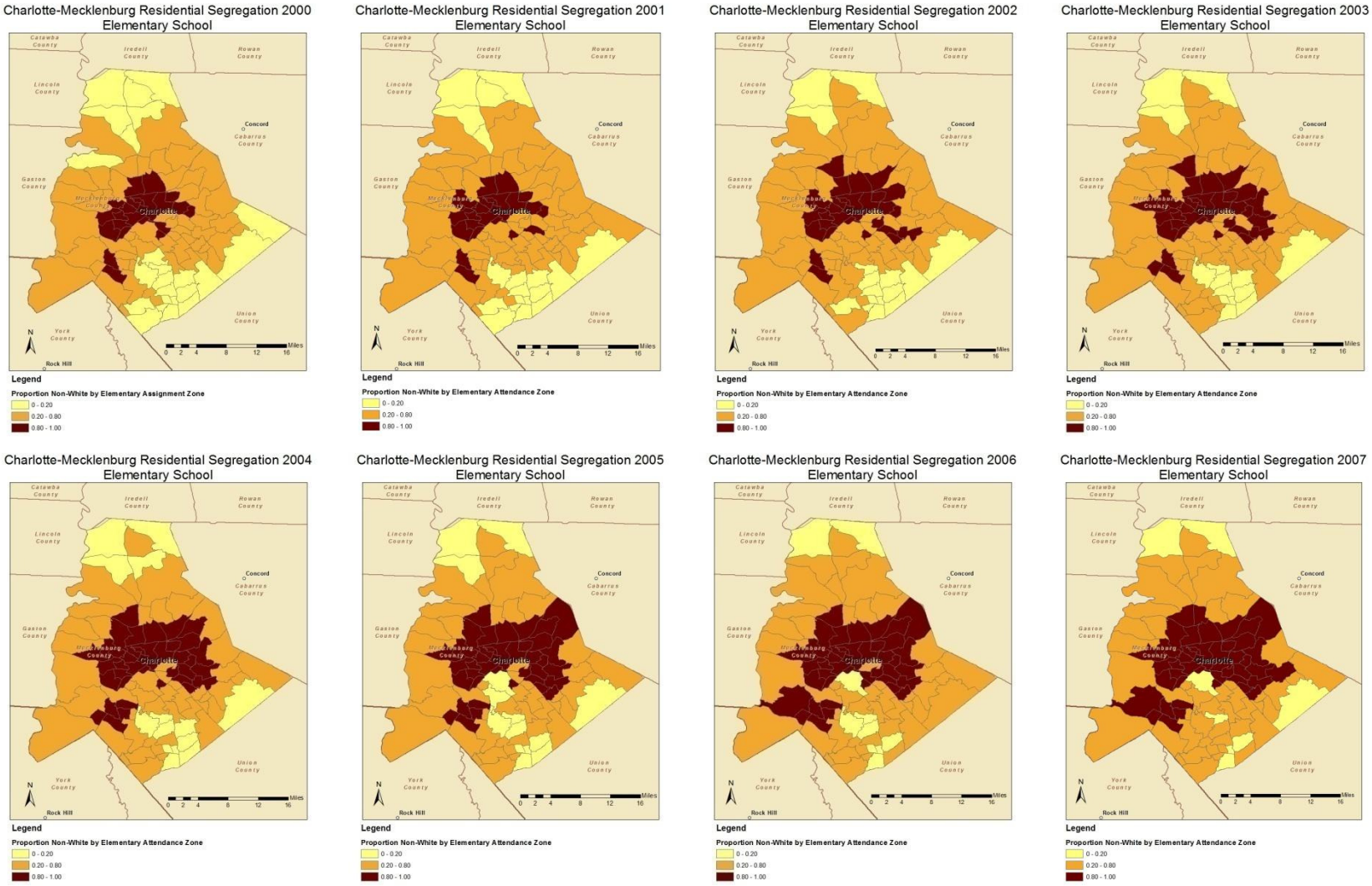


Figure 3. Residential segregation index in CMS for white families and non-white families with elementary school students using 2001-2 boundaries, by year (2000 – 2007).

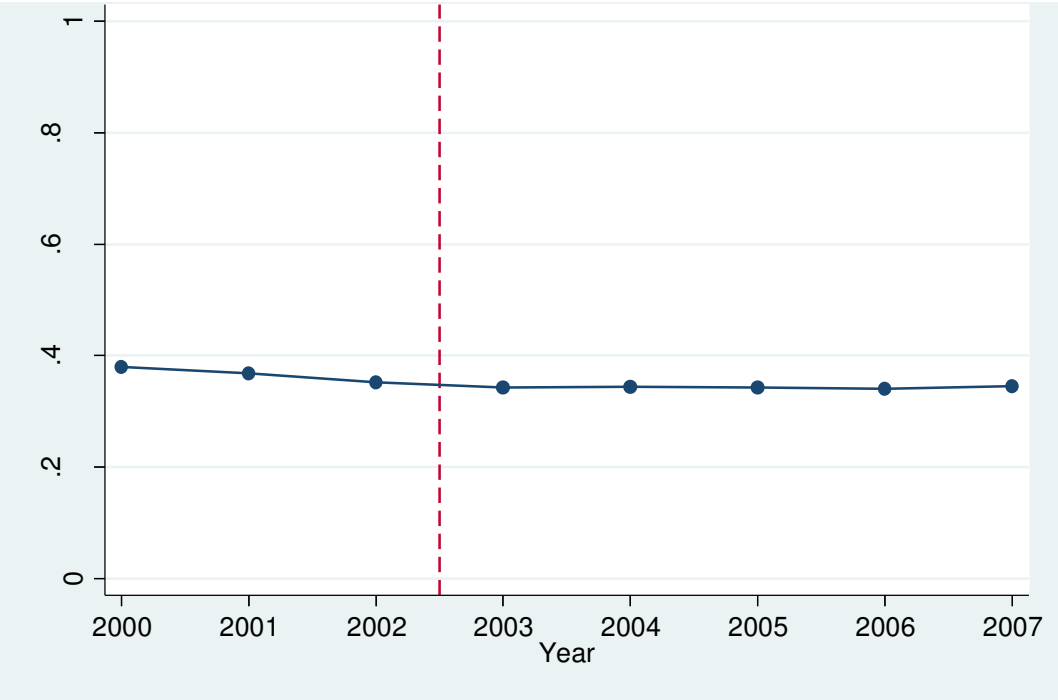
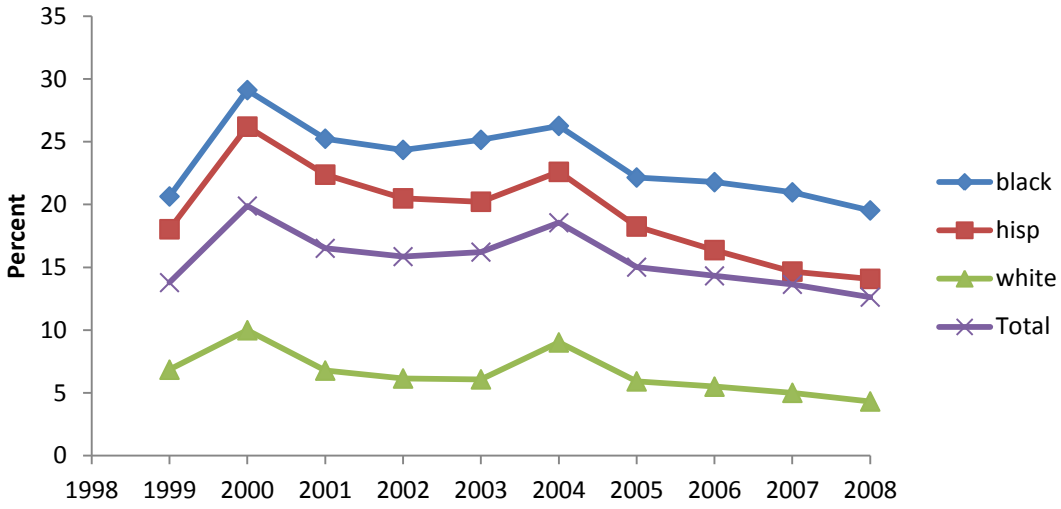
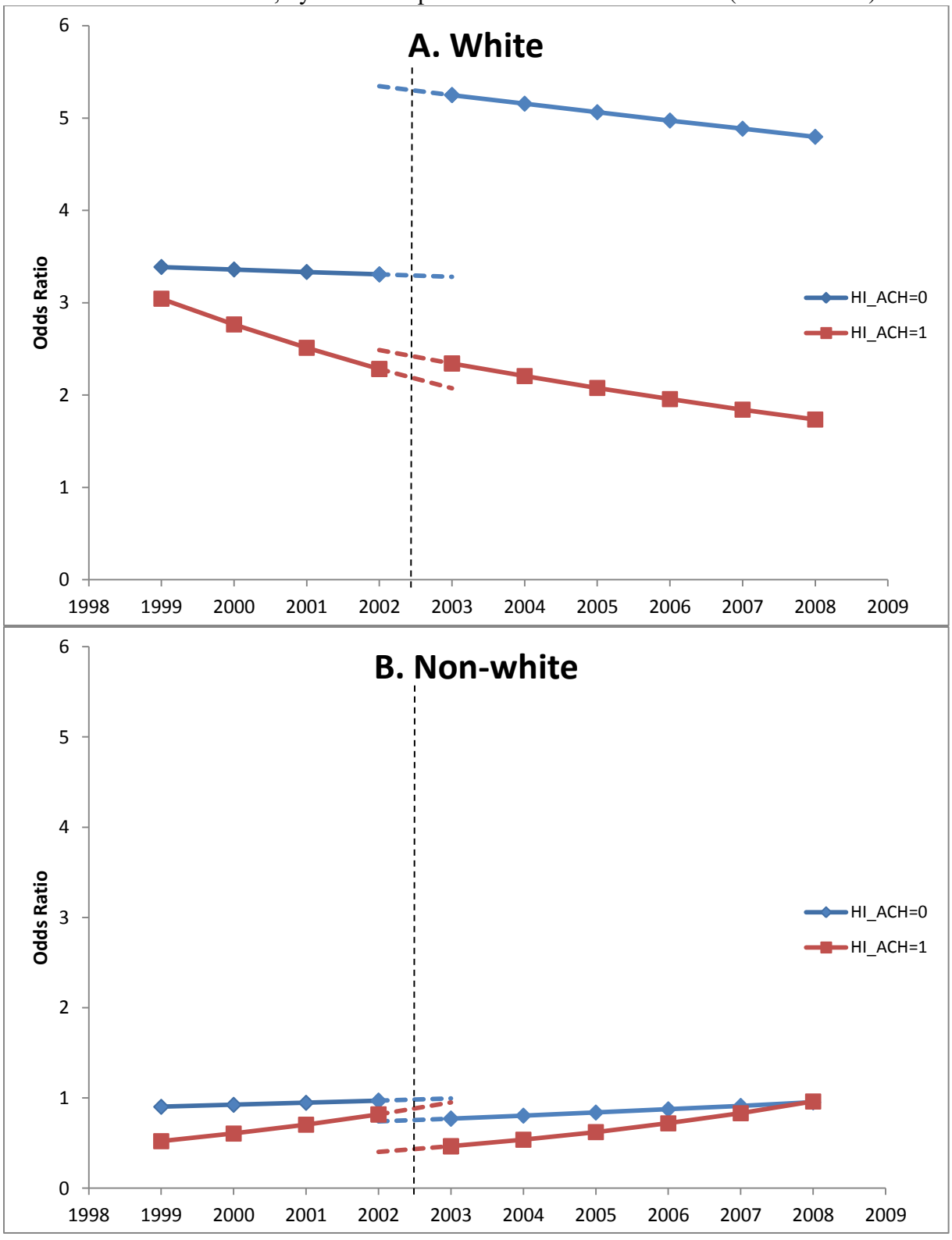


Figure 4. Trends in the share of elementary school families who move to new school attendance zone, by race.



Note: Percentage of movers is based on a categorical variable which includes three categories: stay (in same attendance zone), move (to new attendance zone), and leave (school district entirely).

Figure 5. Fitted odds-ratio of moving to an elementary zone that has a greater proportion of white residents (for white families) and of non-white residents (for non-white families) than student's current zone, by academic performance of school in zone (1999 to 2008).



THE IMPACT OF THE TERMINATION OF COURT DESEGREGATION ORDERS ON RESIDENTIAL SEGREGATION AND SCHOOL DROPOUT RATES: EVIDENCE FROM A NATIONAL SAMPLE

David D. Liebowitz

THE IMPACT OF THE TERMINATION OF COURT DESEGREGATION ORDERS ON RESIDENTIAL SEGREGATION AND SCHOOL DROPOUT RATES:

EVIDENCE FROM A NATIONAL SAMPLE

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. The Green factors required that: (1) student assignment policies, (2) faculty assignment to schools, (3) staff assignments to schools, (4) transportation, (5) facilities; (6) extracurricular activities; and (7) the quality of education offered in all schools must be devoid of the effects of past segregation.

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. Echoing the Western Oklahoma District Court, Chief Justice Rehnquist clarified that vestiges of segregation that result from "private decision making and economics…[are] too attenuated to be a vestige of former school segregation" Dowell, supra, at 243. Once the above steps towards unitary status were accomplished, the district would no longer be legally responsible for remedying subsequent segregation resulting

from demographic patterns that did "not violate the Constitution or flow from such a violation," Milliken v. Bradley, 433 U.S. 267 (1977) as cited in Dowell, supra, at 247.

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" Freeman v. Pitts, 503 U.S. 495 (1992). If districts could demonstrate that they had made incremental efforts to resolve one or more of the Green factors, 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 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.

The justices in these cases view educational segregation as a product of either: (1) governmental policies that explicitly assign students of different races to separate schools; or (2) individual choices and economic patterns over which the courts have no say or influence. Beyond these, however, a third possibility is that "state action" that is not explicitly racially segregative may, nonetheless, increase residential segregation and cause educational quality to differ by the racial composition of the schools.

In this essay, I extend prior research on the impact of the end of court-ordered desegregation in a single school district, or within a limited data-set, 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. Further, 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 conclude that the end of court desegregation orders resulted in an increase in at least one measure of black residential segregation, though these results are not consistent across all measures. The end of the desegregation plans also had a significant effect on the dropout rate for black residents of these districts aged 16 to 19, and this effect was greater for blacks living outside of the South. In a break from prior research, I explore the impact of unitary status declarations on another historically disadvantaged minority group who were a primary target of some but not all initial desegregation plans: Hispanics.[1] I find a significant and substantial increase in the rates of residential segregation and school dropout for Hispanics aged 16 to 19 in districts released from desegregation orders.

Policy makers and jurists should be keenly interested in these estimates. If the "state action" of requiring student-to-school assignment plans that have the associated

[1] Throughout this paper, I use the term "Hispanic" to refer to the current Office of Management and Budget 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."

pernicious effects of causing residential racial segregation and increasing the rate at which students of color dropout of high school, such state action should be revisited. Instead, districts should take advantage of the flexibility afforded by Kennedy's controlling opinion in Parents Involved in Community Schools v. Seattle, 551 U.S. 735 (2007) and echoed by the Office of Civil Rights and the U.S. Department of Education in joint guidance in 2011 to use race-, income-, or place-conscious assignment plans to achieve racial diversity and avoid racial isolation in schools.

I include five additional sections in this paper. In Section 1, I introduce the historical and legal background of court-ordered desegregation and present my research questions. Section 2 describes the data sources, analytic sample, key measures, and analytic strategies. In sections 3 and 4, I present results for my questions related to residential segregation and school dropout rates, respectively. Section 5 discusses the implications of my results.

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 re-zoning 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 to levels indicating close-to-perfect integration in school districts under desegregation orders. Several studies have found positive effects of desegregation on black students' high-school graduation rates (Johnson, 2011; Guryan, 2004), post-secondary labor-market outcomes (Crain & Strauss, 1985), adult earnings, incarceration and health outcomes (Johnson, 2011), and inter-racial prejudice (Pettigrew & Tropp, 2006).

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 socio-economic 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 non-minority students; however, Vigdor (2011)

observed no effect of the policy change on the gap between black and white students in average test scores. Weinstein (2011) and Liebowitz and Page (2014) concluded that the declaration increased segregative residential moves, 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.

While 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 (2011) 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. Further, 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 non-white racial/ethnic 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 co-authors also used levels of residential segregation in a sub-sample 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.

Race-Based Student-Assignment Policies and Housing Preferences

There is strong evidence that the court desegregation orders of the late 1960s and early 1970s increased 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) presents 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. Additionally, 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 non-school amenities to which that home entitles them, subject to their household-budget constraint. Formally:

(1) $L = f(S, N, X)$ subject to $g(I, U)$

Where L is a family's location, S represents school-related housing amenities, N represents all other housing amenities, X represents household characteristics, I is income, and U is unearned income. During desegregation, I posit that school-based amenities associated with housing choice within a district weight only minimally, since a family's choice of residence does not determine the public school their children attend, or does so only temporarily. Once a district is declared unitary, however, families can exert their school and Tiebout (1956) preferences through housing choice. For some families, this may entail selecting into a neighborhood associated with a school with a greater proportion of students who are of the same race as their child than the proportion in their current neighborhood's school. Thus, S in Equation (1) can be written as a function of assigned school racial composition (R) and all other characteristics (A) such as safety, facilities, student achievement, teacher and staff qualifications, and proximity: $S = f(R, A)$. 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, post-unitary declaration from both the starting values of other school and neighborhood characteristics and the secular trends of changing economic and social conditions over a twenty year period.

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,[2] and living in a neighborhood,[3] with children of different racial backgrounds. More limited evidence suggests benefits for white children as well (Linn & Welner, 2007). Theorists and jurists have advanced various explanations for these benefits. First, the allocation of more resources to integrated schools can increase opportunities to learn. In the 1960s and 70s this occurred as a consequence of mandates in the Civil Rights Act and court desegregation orders. When desegregation ended, the reverse occurred when more effective and experienced teachers sorted into schools with higher proportions of white students (Jackson, 2009). Second, the creation of networks of high-social-capital peers can increase access to higher education and labor-market opportunities (cf. Bayer, Ross & Topa, 2008). Third, social contact among racial groups can decrease negative stereotypes (cf. Allport, 1954). Fourth, exposure to students from multiple racial and cultural backgrounds prepares students for productive careers and citizenship in a pluralistic

[2] See Ashenfelter, Collins & Yoon, 2004; Braddock & Eitle, 2004; Crain & Strauss, 1985; Deming, Billings & Rockoff, 2014; Guryan, 2004; Hanushek, Kain, & Rivkin, 2009; Lutz, 2011; Pettigrew & Tropp, 2006; Vigdor & Ludwig, 2008.

[3] See Ananat, 2007; Borjas, 1995; Card & Rothstein, 2007; Chetty & Hendren, 2015; Chetty, Hendren & Katz, 2015; Clark & Drinkwater, 2002; Cutler & Glaeser, 1997; Darden et al., 2009; Durlauf, 2004; Schwartz, 2010; Weinberg, 2000; Williams & Collins, 2001.

society (cf. Grutter v. Bollinger, 596 U.S. 306 (2003)). Finally, to the extent that race and socioeconomic status correlate with student academic achievement and behavioral disruptions, peer effects can limit opportunities to learn in highly segregated environments (cf. Aizer, 2008; Angrist & Lang, 2004; Carrell & Hoekstra, 2010; Hoxby & Weingarth, 2005). Carrell and Hoekstra in particular find that high concentrations in schools of students who have experienced trauma can be particularly disruptive to the learning environment of their classmates, particularly for low-income minority students.

I theorize that ending student-assignment policies intended to generate diverse learning environments will lower average educational attainment for all 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, that this will generate a deterioration in the learning environment, and this will lead to 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 70s led to white flight. Guryan (2004) and Johnson (2011) showed that these 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 impacted 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:

1. Did the end of court-ordered, race-based student-assignment policies increase levels of residential racial segregation in affected school districts?
2. Did the end of court-ordered, race-based student-assignment policies increase rates of high-school dropout in affected school districts?

Research Design

In order 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 the resulting data using difference-in-differences estimation, implemented in a standard 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 need to demonstrate that there are no unobserved differences between districts that were, and were not, declared unitary. Reardon and co-authors (2012) show that while Circuit court jurisdiction, size, and Northern racial composition are predictive of dismissal, demographic and segregation trends are not. Thus, there is no observable evidence that the identification assumption for my research questions is flawed. Further, my

identification strategy relies, in part, on differences in the timing of when courts released school districts from desegregation orders. Lutz (2011) notes that the timing of release was marked by "an element of randomness" (p. 134). This randomness was a product of having 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 a large element of unpredictability to when each district was finally declared unitary. Thus, there is both theory (Lutz, 2011) and evidence (Reardon et al., 2012) to suggest that there are no unobservable factors driving the timing of unitary declarations across districts.

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.[4]

I merge this 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 in order 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 5-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 while conducting the ACS survey, the public reporting

[4] 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 (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. The census block is such a small unit of geography with so few residents that it is imprecisely estimated and public reports censor much of the data.

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 onwards. By request, the American Community Survey Office at the Census Bureau provided me with a custom tabulation for School Enrollment for the Population 16-19 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 this data was heretofore unavailable. The difference in geographic level of data collection (school district versus 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 to the values I obtain by summing across census block groups, they correlate nearly perfectly (0.998). Nevertheless, it is important to note that the information was aggregated differently for the 2010 dropout outcome.

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 a 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.[5]

<<INSERT FIGURE 1 ABOUT HERE>>

In Table 1, I report summary statistics for the population and demographic characteristics of my sample of school districts. In it, I include weighted averages and weighted medians for the 480 districts in my sample. Both statistics are informative, but the median is particularly instructive 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. For instance, the average number of total residents for districts in the sample grew from 1.7 million residents in 1990 to 1.8 million in 2000 and then shrunk by a few thousand in 2010. However, the median-sized school district grew by over 100,000 residents between 1990 and 2010. This suggests that the largest cities in my sample had relatively flat or declining total populations, whereas the median statistic suggests that the typically sized school districts in the sample grew larger over time.

<<INSERT TABLE 1 ABOUT HERE>>

As of 2000, the Census Bureau began collecting racial and ethnic information separately, so it is not possible to build a non-overlapping race category that includes

[5] Note that for all my analyses, I use the year of release from court order as the unitary shock, defined by Reardon el al. (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. (2012). 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.

Hispanics. Therefore, I construct race categories of white, black, and non-white which includes all one-race categories other than white and all multi-race individuals. Further, 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 U.S. has experienced over the past 20 years. The proportion of white residents in these districts declined by seven percentage points, while the proportion of black residents remained steady. The overall proportion of non-white residents, including Asian and multi-racial residents grew five percentage points. Most dramatically, the proportion of Hispanic residents in these districts increased by seven to ten percentage points, depending on the use of the mean or median statistic. Thus, these districts are becoming increasingly multi-chromatic.

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 standard racial- and ethnic-dissimilarity indices (Population Studies Center, 2000). Panel C of Table 1 presents the average over the 20 years of study for three separate measures of residential segregation. The exposure measure assesses the extent of interracial contact within a neighborhood. However, it is sensitive to the overall proportion of white and non-white

residents in the district. That is, if the proportion of non-white residents in the district increases, the exposure index will mechanically increase if housing patterns remain constant. In fact, as the entries in Panel C indicate, the exposure measure rose at similar rates to the proportion of blacks and Hispanics over the 20 year period of my sample. Thus while I report some descriptive evidence of this measure since it is widely used in the segregation literature (cf. Orfield & Lee, 2007; Lutz, 2011; Baum-Snow & Lutz, 2011), I do not ultimately include it as an outcome of interest in my formal statistical modeling.

Instead, 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:

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

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.[6] 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 black

[6] 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 downwards-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 under-reported in 2010 when the white racial group also includes residents of Hispanic ethnicity. However, there is no reason to believe that this will impact the answer to my research question, since the rate of Hispanic-White overlap should not vary based on whether a district was declared unitary.

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 can 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 match 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 (2) 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. Thus, changes in the dissimilarity index are independent of changes in the overall racial composition of the district. In fact, in my sample the values of the index move in the opposite direction from the values of the exposure index, indicating that levels of residential integration in these districts have increased over time net of changing composition, though this reduction in residential segregation has been only marginal for Hispanics.

As Massey and Denton (1988) highlight, 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 different definitions of the index by testing the impact of the dismissal of the districts on the residential Isolation Correlation index (also known as eta-squared), defined as:

(3) $$IC = \frac{I_{jt} - P_{jt}}{1 - P_{jt}}$$

where $I_{jt} = \sum_{i=1}^{n} \left[\left(\frac{b_{it}}{B_{jt}} \right) \left(\frac{b_{it}}{t_{it}} \right) \right]$ and $P = \frac{B_{jt}}{T_{jt}}$. The notation here is similar to that above, but I examine the extent to which one racial group, blacks (b) or Hispanics (h), lives in isolation compared to the total population (t). The isolation correlation index measures "the extent to which minority members are exposed only to one another" (Massey & Denton, 1988). The number estimates the probability that a member of the minority will share a housing unit area with another member of that minority. The isolation correlation index, in parallel to the dissimilarity index, declined considerably for blacks during the time period under study, suggesting that blacks lived in neighborhoods that were more representative of the larger district population and they experienced less spatial isolation. These trends in my sample mirror those found by Reardon and Bischoff (2011) across all communities. Hispanics in my sample, however, became more spatially isolated from whites and Asians over these twenty years.

In Figure 2, I plot the median dissimilarity index for blacks and Hispanics comparing districts that were never released from their court order, districts released by 2000 and districts released by 2010. For blacks, all three types of districts experienced gradual declines in the rates of residential segregation. However, districts that were never released from court order experienced gradual declines in the dissimilarity index for Hispanics, whereas districts that were released in either 2000 or 2010 from court order experienced flat, or in some years modest increases, in this measure of residential segregation. These patterns are even more pronounced for the isolation correlation index as evident in Panels C and D of Figure 2, suggesting that Hispanics in districts that were declared unitary became more geographically isolated over time.

<<INSERT FIGURE 2 ABOUT HERE>>

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 four distinct time-varying outcomes: the sample proportions of (1) all, (2) white, (3) black, and (4) 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) notes, they conflate 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-difference 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 not 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 downwards-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 recent immigrants aged 16 to 19 who may not be attending school but have not dropped out of an American 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 1 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 old in my sample who had dropped out of school declined precipitously from 14.2 percent in 1990, to 12.4 percent in in 2000, and to 7.8 percent 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 dropout rate. There is also some potential noise in the 1990 and 2000 data due to very small numbers of Hispanics residing in many of the Census block groups, resulting in non-reports due to privacy concerns.

Figure 3 compares dropout rates in districts that were and were not ever declared unitary. No obviously differential trend is evident in Panels A and B between overall and white dropout rates for districts that were and were not declared unitary. However, Panel C and especially Panel D show that the median dropout rate was highest (or tied for highest) for blacks and Hispanics in 1990 in districts that were not to be declared unitary over the next 20 years. However, 20 years later, these never-unitary districts had the lowest median dropout rates for blacks and Hispanics. This descriptive evidence motivates the specification of these dropout patterns in the formal statistical model described below.

<<INSERT FIGURE 3 ABOUT HERE>>

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 ten years prior to that row of Census data collection, and 1 if the district has been declared unitary in the previous ten 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 1 indicates, there were a total of 76 districts, representing over 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 non-parameterized sets of predictors to capture the short- and long-term 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 unitary in 1995. In the 2000 row of data collection, I code YRS_UNITARY equal to 5 because DPS had been unitary for five years at that data collection point. In 2010, YRS_UNITARY equals 15 for DPS.

I have no pre-existing 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 non-parametric approach of creating a vector of dichomotous predictors, $UNITARY\_PLUS_t$, where t runs from 0 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.

Exploratory analysis with my outcomes suggests a valuable modification to Baum-Snow and Lutz's (2011) strategy of using a delayed indicator of unitary status. Whereas they use a lagged indicator to test whether residential segregation patterns had changed five years after districts were placed under desegregation order, I use a secondary predictor variable, UNITARY_3, coded as 1 if it has been three years since the district was first declared unitary by the time of that decennial Census collection. This permits me to assess whether there may have been a lag in the manifestation of the hypothesized outcomes.

Table 2 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 asterisks in Table 2 indicate that the starting value of the mean in Columns 1 or 2 differs statistically from the mean of districts never declared unitary in Column 3. 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. While the difference-in-difference 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 dismissed. Table 2 provides no particular evidence that the districts not declared unitary are fundamentally unlike the unitary districts. The non-unitary 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 ten years under examination.

<<INSERT TABLE 2 ABOUT HERE>>

There are two key covariates necessary for implementing my difference-in-differences strategy in a regression framework. They are: (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 as 1 in each respective district, zero 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 controls for 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 in order 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 homogenous 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 in order 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:

(4) $$D_{jt} = \Gamma_j + \Phi_t + \beta_1 \text{UNITARY}_{jt} + \boldsymbol{\gamma}\mathbf{X_{jt}} + \varepsilon_j,$$

where **X** is the vector of district-level time-invariant and time-varying covariates defined above. The district-level error term (ε_j) will be heteroskedastic 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.[7] 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

[7] Though a common challenge in difference-in-difference estimates with panel data is that standard errors tend to be understated because of serial inter-correlation (Bertrand, Mullainathan and Duflo, 2004), the district-level fixed effects I include address this problem.

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 impacted 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 Denver Public Schools 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, while 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 adopting a non-race-based student-assignment policy on residential segregation. It will be positive and statistically significant if the declaration of unitary status increased levels of residential racial sorting.

In supplemental models, I analyze the extent to which districts released from court order longer experience different patterns of residential segregation or dropouts than those with less time free from the desegregation order. In these models, I examine a second causal parameter of interest on YRS_UNITARY or $UNITARY\_PLUS_t$. The parameter represents the causal effect of either operating for an additional year free from court order or of being t years removed from unitary declaration, where t=0, 1, 2, 3,…19.

To address my second research question, I rely on the same difference-in-differences framework as in Equation (4), 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 of that race or ethnicity in the school district.

Residential Segregation Results

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

The difference-in-differences analyses provide some, though not conclusive, evidence that the declaration of unitary status increased the rate of residential segregation for blacks. It provides strong evidence that it did so for Hispanics. I discuss each of these findings below in detail.

Table 3 reports a taxonomy of fitted regression models from Equation (4) with the Black-White/Asian dissimilarity index as the primary outcome. Model 1 is the most basic model and is interpretable as a declaration of unitary status causes a district to experience a decline in the 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 zero in the population. Additional models include measures of the relationship between the district's

physical size and that of the surrounding MSA (Model 2),[8] variation in housing affordability in 1990 (Model 3), whether the district is in the South Census region (Model 4), and a lag measure of the declaration of unitary status for three years after the fact (Model 5). In none of these specifications am I able to reject the null hypothesis that the declaration of unitary status had no impact on rates of black residential segregation in the population. However, Model 6 reports results on the black isolation correlation index and indicates a significant increase of 5.1 percentage points in the probability that black residents in districts declared unitary will reside in a census block group with another black resident. In districts that had minimal differences across census block groups in the affordability of housing, the effect of being released from court order was that blacks had a greater likelihood of living in a census block group with other blacks, net of overall trends in the district's demographics. However, in districts at or above the median level of unequal starting home values, there was no effect on the isolation correlation index of being declared unitary.

<<INSERT TABLE 3 ABOUT HERE>>

Table 4 shows the same taxonomy of regressions for the Hispanic-White/Asian dissimilarity index. In contrast with the black results, I observe consistent, positively signed impacts of being declared unitary on a district's rate of Hispanic residential segregation, particularly in Southern census region districts where Hispanics became more geographically isolated. Specifically, in the simplest form, the impact of a declaration of unitary status results in a 0.030 increase in the dissimilarity index,

[8] The introduction of AREA into the model results in a reduction in the total number of observations because some school districts are not located within Metropolitan Statistical Areas.

interpretable as a further 3.0 percentage point increase in the number of Hispanics who would need to move to new census block groups to be perfectly integrated, on top of the 36.9 percent average across all districts in 2010. A district's physical size relative to its Metropolitan Statistical Area, interacted with when a district was declared unitary has a negative impact on dissimilarity values. This contradicts my hypothesis that geographically larger districts would experience greater increases in rates of segregation post-unitary declaration. Nevertheless, the main effect of UNITARY remains positively signed and statistically significant in Model 2; however, the rate of post-unitary Hispanic residential segregation was concentrated in districts that were small relative to their surrounding MSAs. Unlike for blacks, 1990 home affordability is unrelated to dissimilarity rates (Model 3). The introduction of the South Census region in Model 4 is statistically insignificant, but positively signed, prefacing an important interaction when I test the sensitivity of these results to the correlation isolation index of segregation in Model 6. The inclusion of the 3-year lag indicator for unitary declaration in Model 5 adds no additional insight. However, across all five models, the coefficient on UNITARY remains positively signed between 2.3 and 4.5 percentage points. Model 6 presents an interesting nuance to the results. Here, the main effect of the declaration of unitary status on the isolation correlation index is statistically indistinguishable from zero in the population, but the parameter estimate on SOUTHxUNITARY is statistically significant, positively signed and similar in magnitude to the coefficient on UNITARY in the dissimilarity models. This suggests that, at least for the measure of spatial isolation, the segregative effects of the release of districts from court order on Hispanics are concentrated in South Census region districts.

<<INSERT TABLE 4 ABOUT HERE>>

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 4 plots the non-parametric results of the linear combination of UNITARY with a series of year-by-year indicators for the estimates from Tables 3 and 4 which generate significant and substantively interesting estimates of the coefficient on UNITARY. I use year-specific indicators for years 1 through 9, followed by a final indicator for years 10 through 19. This final binning has both substantive and statistical value. First, conceptually it is unlikely that there will be ongoing changes altering the residential choice set a family is facing ten years after a change in assignment policy. Second, there are fewer districts with more than 10 years of unitary status, so binning them together helps with the precision of my estimates. Finally, exploratory analysis suggests that there is no meaningful difference in the point estimates for any year after Year 10. Panel A of Figure 4 plots the impact of the unitary declaration on Hispanic dissimilarity rates. In every year, there is a positive-signed effect on the dissimilarity index, and most are significant at the 99 percent confidence threshold. The rate of increased segregation appears to peak three and four years after the initial declaration of unitary status. Panel B shows very similar patterns for the Hispanic isolation index in South Census region school districts, though these are much less precisely estimated. Finally, Panel C shows that the unitary declaration had in every year a positive-signed and significant effect on the rate of residential isolation for blacks every year through Year 9. The effects are strong, and for blacks continue to peak seven years after the declaration of unitary status.

<<INSERT FIGURE 4 ABOUT HERE>>

In sum, in both categorical and time-trend models, there is evidence that the declaration of unitary status increased the black isolation correlation index, the Hispanic dissimilarity index, and the Hispanic isolation correlation index in South Census region districts.

High-School Dropout Rates

2. Did the end of court-ordered, race-based student-assignment policies increase rates of high-school dropout in affected school districts?

I find that for both blacks and Hispanics there is strong evidence that the declaration of unitary status caused an increase in the dropout rate, with the effects for black students concentrated in districts outside of the South census region. There are minimal effects on white dropout rates.

Table 5 presents a taxonomy of regression models predicting the overall status dropout rate for all individuals aged 16 to 19.[9] In all specifications, the declaration of unitary status generates an increase the overall dropout rate of 1 to 3 percentage points for students of all races. Any analysis of a policy's impact must consider its general welfare effects in addition to the effects on specific groups of individuals. In this case, the causal impact of unitary declarations is to increase the overall status dropout rates. As Table 6 indicates, there is no measurable impact of unitary declaration on the white

[9] I also conduct the same analyses restricting the sample to districts that are unified or secondary. This excludes elementary districts where a change in unitary status might not generate results for over a decade. Excluding these districts from the results generates no meaningful differences in my estimates. Tables available upon request.

dropout rate immediately after unitary declaration. Thus, the short-term welfare effects are driven primarily by the impact of the unitary declaration on black and Hispanic residents' dropout rates. However, as Model 4 indicates, after three years, white residents' status dropout rates in unitary districts are 2.9 percentage points higher as well, though this effect is concentrated in districts that had less variation in housing prices prior to being declared unitary.

<<INSERT TABLES 5 AND 6 ABOUT HERE>>

Table 7 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 percent. 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 to a district still under court order which I estimate would have an 11.8 percent black dropout rate, this dismissed district would have a 13.1 percent dropout rate. The inclusion of additional explanatory variables in Models 2 and 3 does not show any additional relationship between the spread of housing affordability or district size and the black dropout rate, but the magnitude of the point estimate on UNITARY increases, though it is less precisely estimated. Model 4 shows a 2.3 percentage point increase in the

status dropout rate three years after the unitary declaration, significant at the 90 percent confidence level (as is Model 3).[10]

<<INSERT TABLE 7 ABOUT HERE>>

The results in Table 8 show an even stronger, though less precisely estimated, 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 percent confidence threshold. The estimated Hispanic dropout rate in 1990 was 24.2 percent. 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 percent. 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 percent. The models including variation in home affordability across Census block groups, school district area relative to MSA, and the three-year lagged unitary indicator (Models 2 through 4) are all similarly signed and imprecisely estimated, though in the case of Models 3 and 4 meet the 90 percent confidence threshold.

A sizeable number of school districts reported no Census-block-group counted Hispanic dropouts aged 16 to 19 years old in 1990 and 2000. As a robustness check, Model 5 excludes districts that reported no Hispanic dropouts aged 16 to 19. Similar to

[10] The N for LEAs for the dropout rate results is lower than 480 in Tables 7 and 8 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 and ACS do not report on estimates below a threshold of five, 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.

the previous models, I find a 7.5 percentage point increase in the status dropout rate post-unitary declaration for these 313 districts.

<<INSERT TABLE 8 ABOUT HERE>>

Though the precision of my estimates varies, across all specifications, I find that the declaration of unitary status causes an increase in the black and Hispanic high-school status dropout rate in affected school districts. To explore further the impact that these declarations have over time, I estimate parametric and non-parametric models. Though the linear and higher-order polynomial terms I include in my specifications yield trivial results, the non-parametric analysis proves interesting. Figure 5 presents linear combinations of the coefficients on UNITARY and those on the year-by-unitary dummies, with confidence intervals. All point estimates in Panel A of Figure 5 are positively signed and suggest that the additive impact of unitary declaration on rates of black dropout peaks three years after the release from desegregation order and ranges from 1.4 to 3.0 percentage points. The estimates on the black status dropout rate over time are much more precisely estimated due to the larger number of black than Hispanic residents, particularly in the early years of my sample. However, similarly to blacks, all point estimates in Panel B are positively signed. The coefficients suggest that the impact on dropout rates was highest in the first six years after the declaration of unitary status, with three of those years distinguishable from zero at the 95 percent confidence threshold.

<<INSERT FIGURE 5 ABOUT HERE>>

As Lutz (2011) notes, the contrast between black and white results (with the addition of Hispanic results in this 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" (Lutz, 2011, p. 157). The discrepancy indicates that these 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.

Whereas Lutz (2011) finds an impact on black dropout rates only for non-Southern districts, I find in Reardon et al.'s (2012) larger sample of districts with data extending an additional four years that there are effects on black dropout rates for all dismissed districts. Additionally, I find even stronger (though less precisely estimated) results for Hispanics. Nevertheless, Table 9 presents dropout rate results for blacks and Hispanics limiting the sample to non-Southern Census region school districts. In alignment with Lutz, the effects of unitary status declaration on dropout rates in my most parsimonious models are twice as strong for blacks outside of the South and strongly significant. The magnitude of the effect is the same outside the South for Hispanics (whether looking at all districts or only those that had some Hispanic dropouts), but less precisely estimated.

<<INSERT TABLE 9 ABOUT HERE>>

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. While 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. Further, in contrast to other studies on this subject, I analyze the impact of these shifts on the fastest growing minority group in our nation's schools—Hispanics. I conclude that barring districts from using race-conscious mechanisms for assigning students to schools increased the rates of residential segregation for blacks and Hispanics across most measures, and increased the dropout rate for blacks (particularly outside of the South) and Hispanics by sizeable amounts.

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 impacts school outcomes (cf. Borjas, 1995; Card & Rothstein, 2007; Schwartz, 2010), the first outcome of interest (residential segregation) caused the second (school dropout). There is some evidence suggestive of this hypothesis, but it is decidedly mixed.

I estimate the impact of an interaction between the dissimilarity and isolation measures for both black and Hispanic residential segregation and UNITARY on high-school dropout rates. The coefficient on the interaction is interpretable as what additional effect does being more racially segregated have on the dropout rate in districts that are declared unitary. If this hypothesis is correct the interaction between the black dissimilarity rate and whether a district is unitary should not impact the dropout rate because districts released from court order did not experience an increase in the dissimilarity rate. However, the black isolation correlation, the Hispanic dissimilarity,

and the South Census region Hispanic isolation indices should predict higher rates of post-unitary dropouts. Models 2 and 4 in Table 10 confirm this hypothesis as it relates to the isolation correlation index. The coefficients on the black and Hispanic isolation index and are positive and significant in unitary districts, indicating that districts experiencing more residential segregation after unitary declaration saw increases in their status dropout rates. However, I find the same positive associations between the black dissimilarity rate and the school dropout rate (Model 1), even though the declaration of unitary status had no impact on the black dissimilarity rate. Even more perplexingly, the impact of the Hispanic dissimilarity index on Hispanic dropout rates (Model 3) is negatively signed, though imprecisely estimated. This implies that districts that experienced more segregation after being declared unitary had lower dropout rates. Since this test of a potential explanatory mechanism is inconclusive, more research is required to explain why these assignment policies that resulted in more segregated schools (Reardon et al., 2012) also yielded higher dropout rates.

<<INSERT TABLE 10 ABOUT HERE>>

While the evidence suggests that there were deleterious effects of releasing districts from desegregation orders on black and Hispanic dropout rates, these dropout rates were nevertheless quite high even when districts were under court order to desegregate. This suggests that far more must be done to understand and implement strategies that leverage the benefits of racially and ethnically diverse schools to raise graduation rates for all. In fact, as ethnographic studies demonstrate (cf. Tatum, 1997), while desegregated schools may have educated students of different races within the same walls, they often did not associate with each other socially and did not take the

same classes. Further, if they did develop overlapping social networks, these frequently faded after students left school (Wells, 2009). Research findings that provide causal and practice-based guidance on how to create schools in which all students benefit maximally from racial and ethnic diversity remain elusive.

Despite these limitations, this study advances the policy and legal discussion on the impact of the end of race-based student assignment. As our nation struggles to create schools that are representative of its rich cultural diversity rather than to replicate patterns of residential segregation, my findings indicate that "state action" that does not explicitly take into account race in assigning students to schools increases the rate at which black and Hispanic students drop out 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.

Further, 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 to do neighborhoods and the composition of the graduating high school class, it is in local officials' best interests to design plans which seek to limit racial isolation. Justice Marshall Harlan argued in his famous dissent to Plessy v. Ferguson, 163 U.S. 537 (1896) that "our Constitution [is] color blind," providing motivation to modern-day arguments that race-conscious policies

are antithetical to our founding principles. But Justice Harlan also wrote immediately preceding those words that, “there is no caste here.” The results of this study suggest that our current legal doctrine and policy may perpetuate such a caste system and should be subject to a searching review.

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Appendix A. Tables

Table 1. 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			
Avg. District Population	1,717,975	1,844,657	1,828,796
Median District Population	[507,235]	[558,719]	[600,158]
% change 90 to 00		0.074	
		[0.101]	
% change 00 to 10			-0.009
			[0.074]
% change 90 to 10			0.065
			[0.183]
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]
% nonwhite	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 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 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
Unitary LEAs (3 yrs+)	0	32	195
Unitary Block Groups (3 yrs+)	0	6,144	21,846
Non-Unitary	480	404	265
Non-Unitary Block Groups	50,007	34,856	25,968

Cells in Panels A-D contain means with medians displayed in brackets. Averages and medians weighted by total residents within school district boundaries. Cells in Panel E contain counts.

Table 2. School district characteristics in 1990, by whether and when they were declared unitary (n=480)

	Dismissed 1991 to 2000	Dismissed 2001 to 2010	Never Dismissed
Total Residents	514,519	582,511	2,695,443
	(79,936)	(218,672)	(1,110,249)
% White Residents	0.703	0.699	0.621
	(0.270)	(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 from the mean in column 3.

Table 3. Linear regression models estimating the effect of declaration of unitary status on black-white/Asian dissimilarity and the black isolation correlation indices, controlling for variation in housing affordability, Census region, and relative size (1990 to 2010).

	1	2	3	4	5	6
unitary	-0.017	-0.020	-0.017	-0.018		0.051*
	(0.011)	(0.013)	(0.026)	(0.025)		(0.026)
areaXunitary		0.009	0.009	0.009		0.006
		(0.019)	(0.019)	(0.021)		(0.023)
gini_housevalueXunitary			-0.013	-0.013		-0.298**
			(0.097)	(0.097)		(0.090)
southXunitary				0.001		-0.002
				(0.014)		(0.017)
unitary_3					-0.011	
					(0.025)	
areaXunitary_3					0.006	
					(0.023)	
gini_housevalueXunitary_3					-0.024	
					(0.098)	
southXunitary_3					-0.002	
					(0.014)	
District & Year Fixed Effects	X	X	X	X	X	X
Observations	1,440	1,080	1,080	1,080	1,080	1,080
LEAs	480	360	360	360	360	360
R-Squared	0.952	0.952	0.952	0.952	0.952	0.969

* significant at 5%, ** significant at 1%, *** significant at 0.1%

Note: the table displays coefficients from equation (4). Standard errors (in parentheses) are adjusted to account for the serial inter-correlation caused by the clustering of observations within districts. The dependent variable in Models 1-5 is the dissimilarity index for black/African-American defined in the text. Model 6 reports the isolation correlation index.

Table 4. Linear regression models estimating the effect of declaration of unitary status on Hispanic-white/Asian dissimilarity and the Hispanic isolation correlation indices, controlling for variation in housing affordability, Census region, and relative size (1990 to 2010).

	1	2	3	4	5	6
unitary	0.030**	0.045**	0.044	0.023		-0.006
	(0.011)	(0.014)	(0.037)	(0.045)		(0.021)
areaXunitary		-0.043**	-0.043**	-0.055**		-0.044**
		(0.015)	(0.016)	(0.018)		(0.015)
gini_housevalueXunitary			0.007	0.002		0.051
			(0.177)	(0.186)		(0.072)
southXunitary				0.036		0.045**
				(0.022)		(0.015)
unitary_3					0.026	
					(0.041)	
areaXunitary_3					-0.052**	
					(0.016)	
gini_housevalueXunitary_3					-0.047	
					(0.174)	
southXunitary_3					0.036	
					(0.020)	
District & Year Fixed Effects	X	X	X	X	X	X
Observations	1,440	1,080	1,080	1,080	1,080	1,080
LEAs	480	360	360	360	360	360
R-Squared	0.881	0.892	0.892	0.894	0.892	0.971

* significant at 5%, ** significant at 1%, *** significant at 0.1%

Note: the table displays coefficients from equation (4). Standard errors (in parentheses) are adjusted to account for the serial inter-correlation caused by the clustering of observations within districts. The dependent variable in Models 1-5 is the dissimilarity index for Hispanic-White defined in the text. Model 6 reports the isolation correlation index.

Table 5. Linear regression models estimating the effect of declaration of unitary status on the overall dropout rate, controlling for variation in housing affordability and relative size (1990 to 2010).

	1	2	3	4
unitary	0.012	0.023*	0.027*	
	(0.006)	(0.010)	(0.011)	
gini_housevalueXunitary		-0.049	-0.046	
		(0.041)	(0.041)	
areaXunitary			-0.008	
			(0.005)	
unitary_3				0.032**
				(0.011)
gini_housevalueXunitary_3				-0.07
				(0.038)
areaXunitary_3				-0.009
				(0.006)
District & Year Fixed Effects	X	X	X	X
Observations	1,440	1,440	1,080	1,080
LEAs	480	480	360	360
R-Squared	0.837	0.838	0.854	0.855

* significant at 5%, ** significant at 1%, *** significant at 0.1%

Note: the table displays coefficients from equation (4). Standard errors (in parentheses) are adjusted to account for the serial inter-correlation caused by the clustering of observations within districts. The dependent variable is the status dropout rate as defined in the text.

Table 6. Linear regression models estimating the effect of declaration of unitary status on the white dropout rate, controlling for variation in housing affordability and relative size (1990 to 2010).

	1	2	3	4
unitary	0.002	0.015	0.018	
	(0.005)	(0.010)	(0.010)	
gini_housevalueXunitary		-0.056	-0.047	
		(0.042)	(0.041)	
areaXunitary			-0.01	
			(0.005)	
unitary_3				0.029**
				(0.010)
gini_housevalueXunitary_3				-0.083*
				(0.040)
areaXunitary_3				-0.011
				(0.006)
District & Year Fixed Effects	X	X	X	X
Observations	1,440	1,440	1,080	1,080
LEAs	480	480	360	360
R-Squared	0.817	0.818	0.834	0.836

* significant at 5%, ** significant at 1%, *** significant at 0.1%

Note: the table displays coefficients from equation (4). Standard errors (in parentheses) are adjusted to account for the serial inter-correlation caused by the clustering of observations within districts. The dependent variable is the status dropout rate as defined in the text.

Table 7. Linear regression models estimating the effect of declaration of unitary status on the black dropout rate, controlling for variation in housing affordability and relative size (1990 to 2010).

	1	2	3	4
unitary	0.013**	0.020	0.025	
	(0.005)	(0.013)	(0.013)	
gini_housevalueXunitary		-0.032	-0.042	
		(0.053)	(0.052)	
areaXunitary			0.000	
			(0.008)	
unitary_3				0.023
				(0.013)
gini_housevalueXunitary_3				-0.041
				(0.049)
areaXunitary_3				0.001
				(0.008)
District & Year Fixed Effects	X	X	X	X
Observation	1,428	1,428	1,068	1,068
LEAs	476	476	356	356
R-Squared	0.709	0.71	0.741	0.739

* significant at 5%, ** significant at 1%, *** significant at 0.1%

Note: the table displays coefficients from equation (4). Standard errors (in parentheses) are adjusted to account for the serial inter-correlation caused by the clustering of observations within districts. The dependent variable is the status dropout rate as defined in the text.

Table 8. Linear regression models estimating the effect of declaration of unitary status on the Hispanic dropout rate, controlling for variation in housing affordability and relative size (1990 to 2010).

	1	2	3	4	5
unitary	0.035	0.066	0.071		0.075
	(0.018)	(0.041)	(0.038)		(0.039)
gini_housevalueXunitary		-0.124	-0.180		-0.200
		(0.153)	(0.142)		(0.143)
areaXunitary			0.014		0.015
			(0.009)		(0.009)
unitary_3				0.072	
				(0.040)	
gini_housevalueXunitary_3				-0.206	
				(0.143)	
areaXunitary_3				0.014	
				(0.009)	
District & Year Fixed Effects	X	X	X	X	X
Observations	1,422	1,422	1,074	1,074	939
LEAs	474	474	358	358	313
R-Squared	0.805	0.805	0.818	0.816	0.847

* significant at 5%, ** significant at 1%, *** significant at 0.1%

Note: the table displays coefficients from equation (4). Standard errors (in parentheses) are adjusted to account for the serial inter-correlation caused by the clustering of observations within districts. The dependent variable is the status dropout rate as defined in the text.

Table 9. Linear regression models estimating the effect of declaration of unitary status on dropout rate by race in non-Southern Census regions, controlling for variation in housing affordability and relative size (1990 to 2010).

	Black		Hispanic	
	1	2	1	2
unitary	0.024**	0.034	0.036	0.005
	(0.008)	(0.025)	(0.031)	(0.088)
gini_housevalueXunitary		-0.046		0.061
		(0.115)		(0.352)
areaXunitary		0.055		0.348
		(0.149)		(0.254)
District & Year Fixed Effects	X	X	X	X
Observations	276	261	282	270
LEAs	92	87	94	90
R-Squared	0.839	0.844	0.849	0.85

* significant at 5%, ** significant at 1%, *** significant at 0.1%

Note: the table displays coefficients from equation (4). Standard errors (in parentheses) are adjusted to account for the serial inter-correlation caused by the clustering of observations within districts. The dependent variable is the status dropout rate as defined in the text.

Table 10. Linear regression models estimating the effects of unitary status declaration on dropout rate for black and Hispanic residents, by rates of residential segregation (1990 to 2010)

	1	2	3	4
unitary	0.001	0.015	0.091*	0.119*
	(0.015)	(0.013)	(0.041)	(0.058)
dissimilarity_blackXunitary	0.061*			
	(0.025)			
isolation_blackXunitary		0.062**		
		(0.019)		
dissimilarity_hispanicXunitary			-0.076	
			(0.064)	
isolation_HispanicXunitary				0.088
				(0.097)
gini_housevalueXunitary	-0.098	-0.106*	-0.141	-0.418
	(0.058)	(0.054)	(0.136)	(0.253)
District & Year Fixed Effects	X	X	X	X
Observations	1,428	1,428	1,422	1,140
LEAs	476	476	474	380
R-Squared	0.711	0.712	0.806	0.792

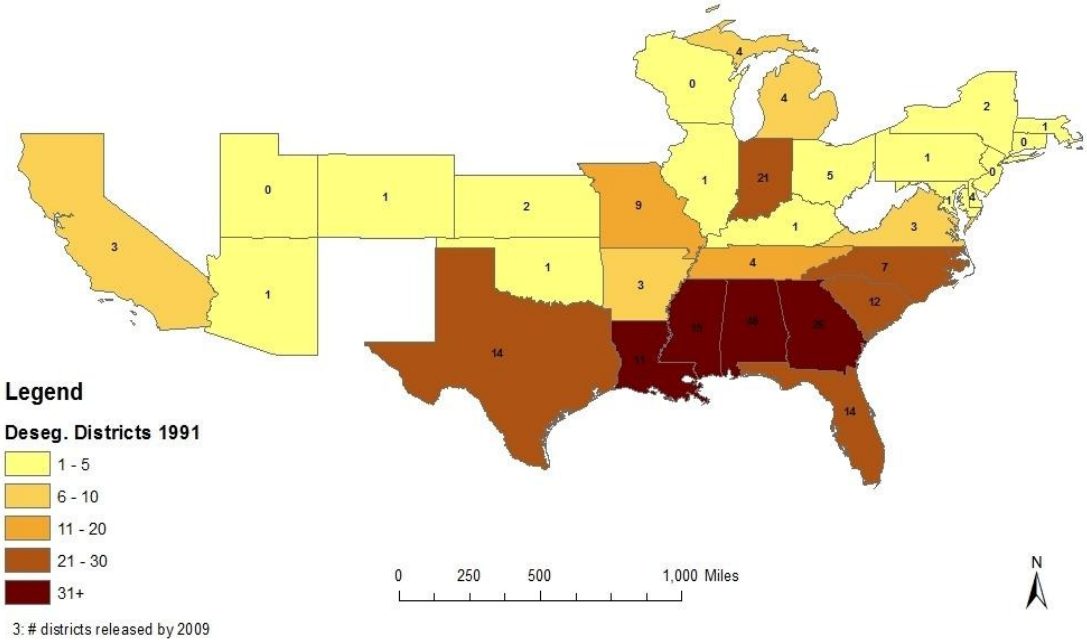
* significant at 5%, ** significant at 1%, *** significant at 0.1%

Note: the table displays coefficients from equation (4). Standard errors are clustered by district in parentheses. Observations represent the total number of 16-19 year-olds living in the sampled districts. The dependent variable is the dropout rate defined in the text.

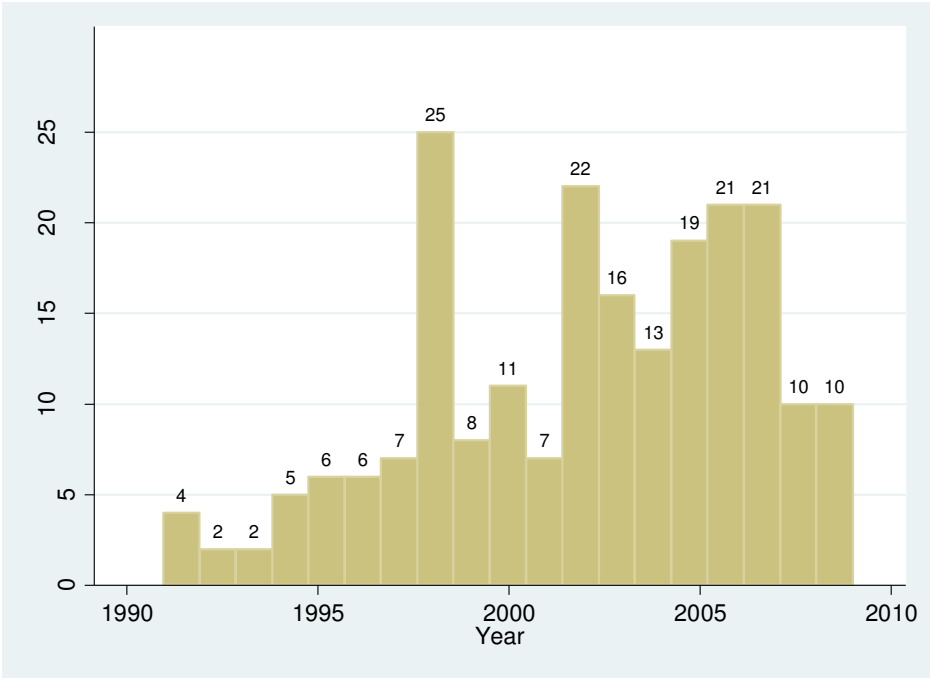
Appendix B. Figures

Figure 1. School districts under court desegregation order and declared unitary by 2010.

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



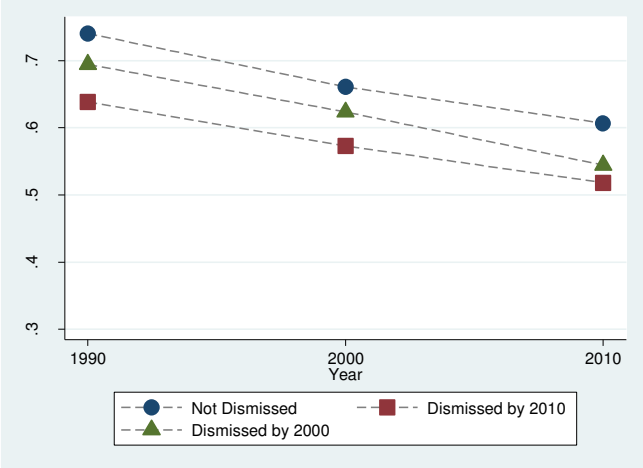
Panel B. Timing of release of districts from court order (n=215), by year



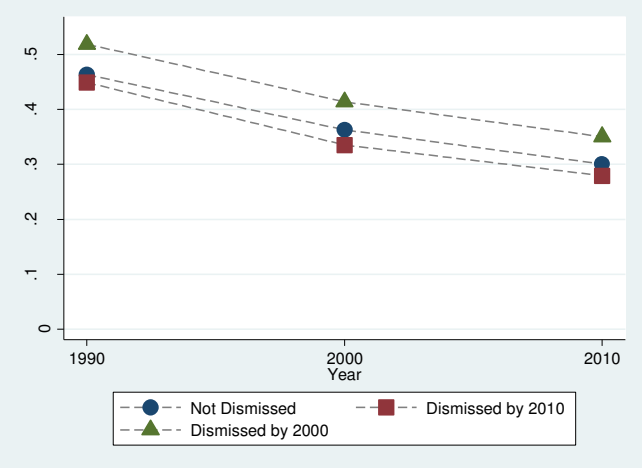
Source data is Reardon et al. (2012) with updates as described in footnote 5.

Figure 2. Dissimilarity and isolation correlation index rates, by unitary status

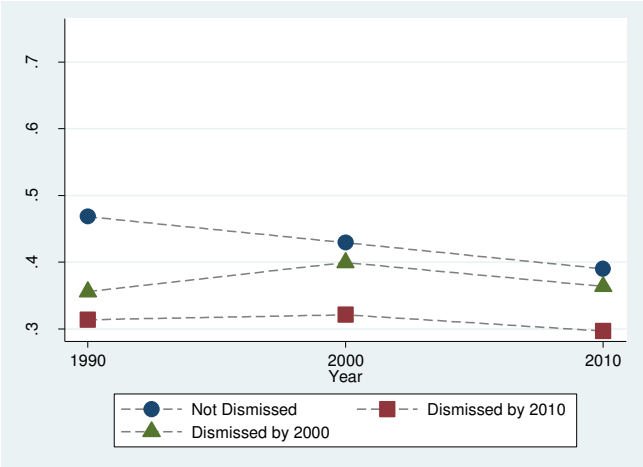
Panel A. Black-White/Asian Dissimilarity



Panel C. Black Isolation Correlation



Panel B. Hispanic-White/Asian Dissimilarity



Panel D. Hispanic Isolation Correlation

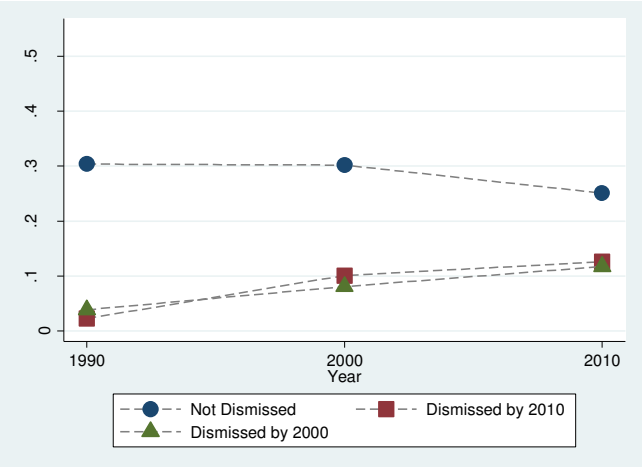
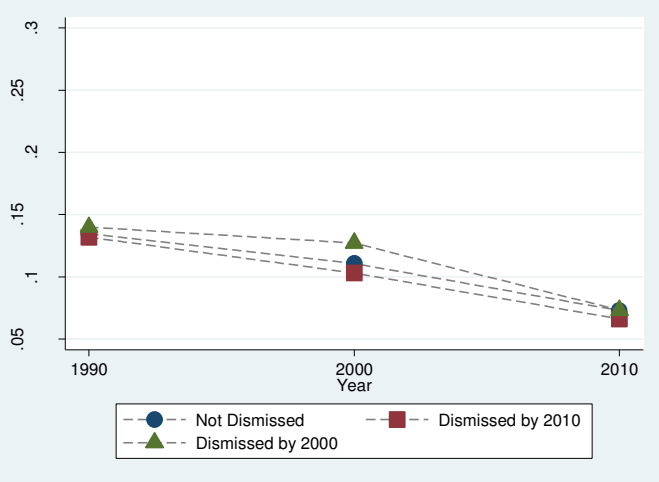
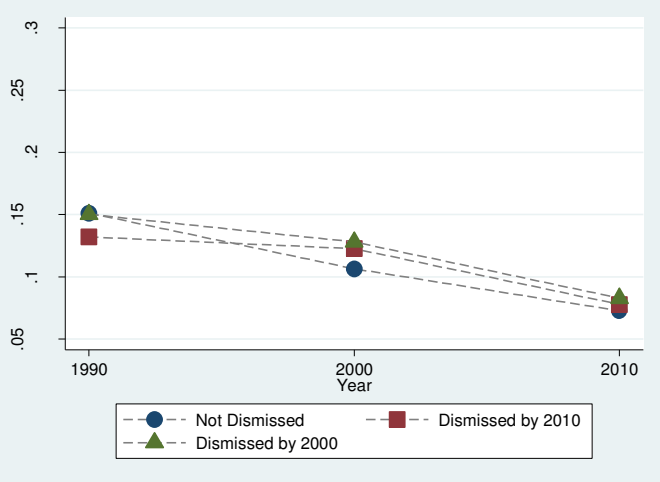


Figure 3. Status dropout rates for the population aged 16 to 19 years, by race and unitary status

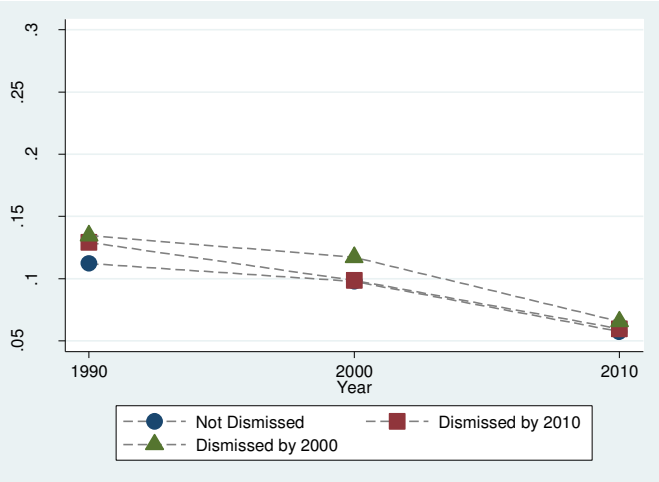
Panel A. Overall dropout rate



Panel C. Black dropout rate



Panel B. White dropout rate



Panel D. Hispanic dropout rate

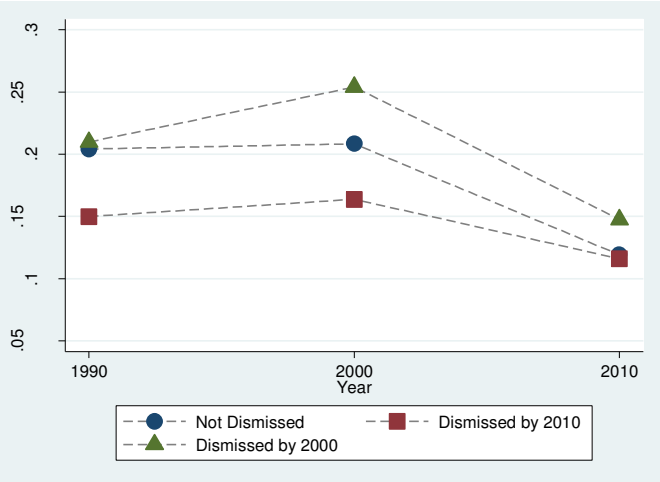
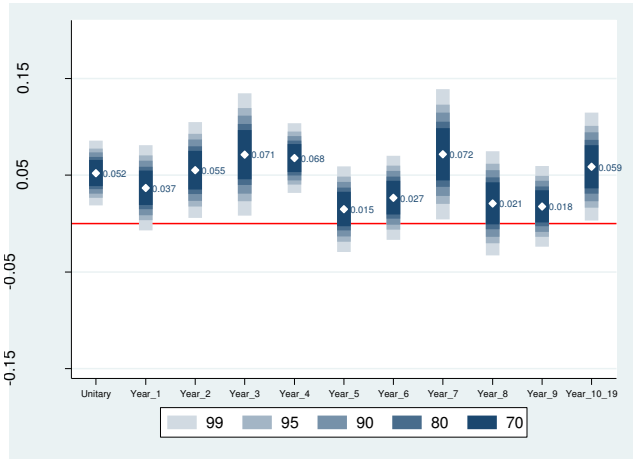
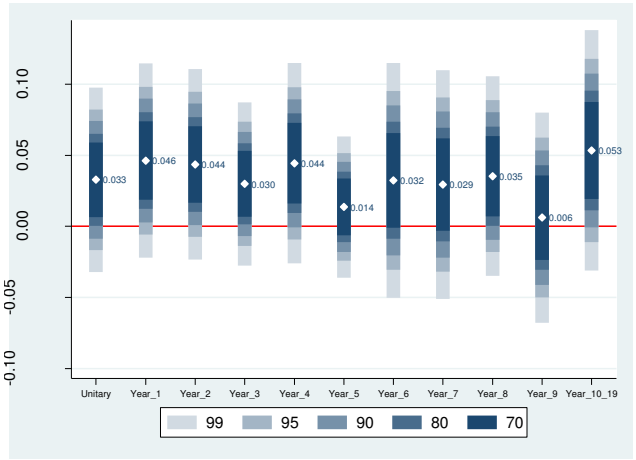


Figure 4. Non-parametric regression estimates of dissimilarity and isolation correlation indices after unitary declaration, by race

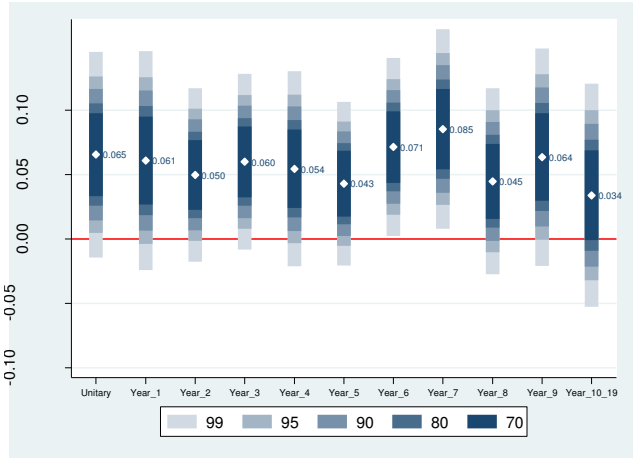
Panel A. Hispanic Dissimilarity



Panel B. Hispanic Isolation—South Census Region



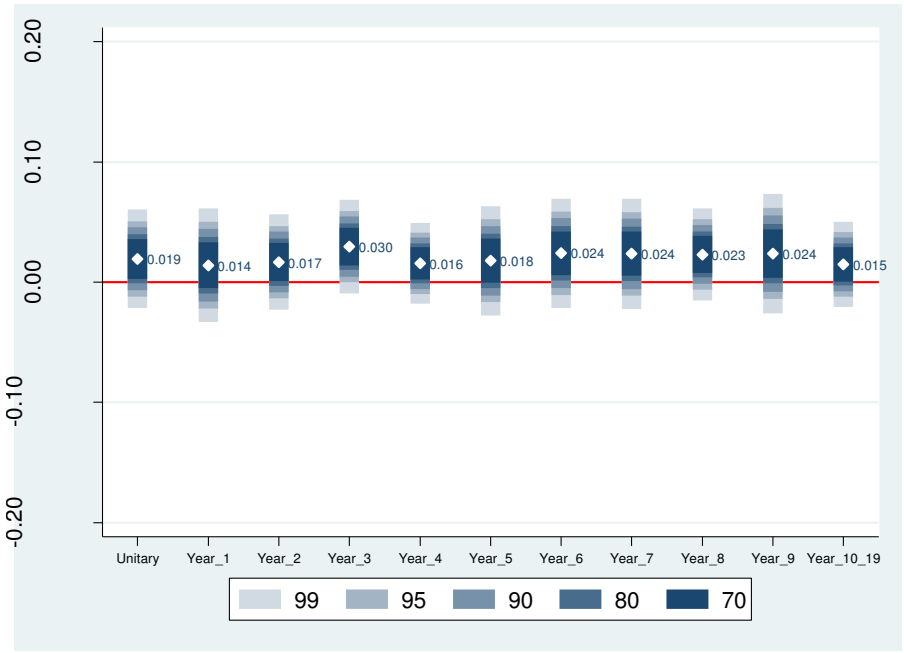
Panel C. Black Isolation



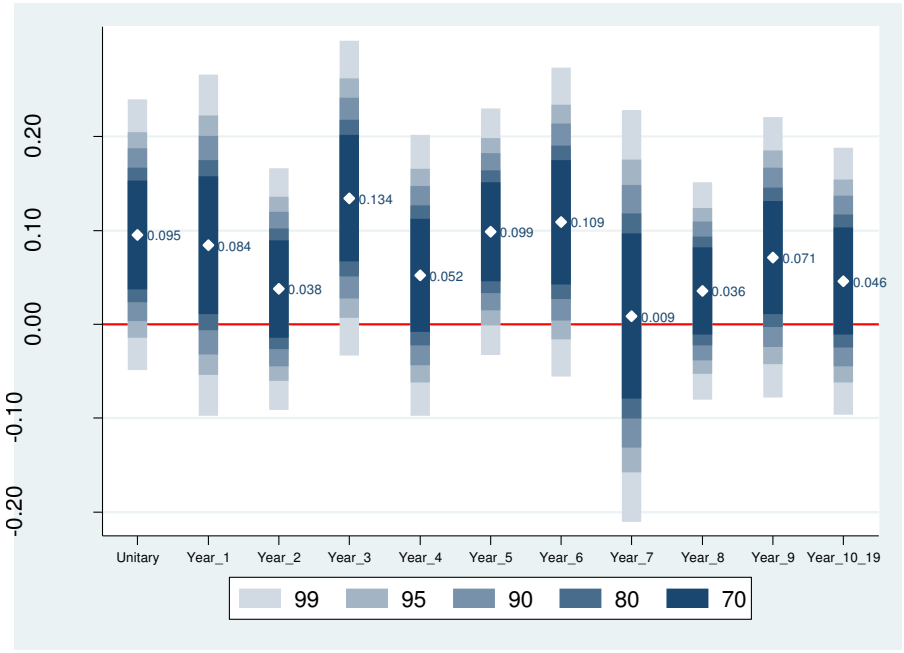
Note: Figure 4 generated from point estimates using the most comprehensive, while parsimonious, models from Tables 3 and 4. Panel A is Table 4, Model 2, Panel B is Table 4, Model 6, restricted to South Census region, and Panel C is Table 3, Model 6. Full tables for these estimates as well as the non-significant linear time trend available from author.

Figure 5. Non-parametric regression estimates of dropout rates after unitary declaration, by race.

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



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



Note: Figure 5 generated from point estimates from Tables 7 and 8. Panel A is Table 7, Model 3, Panel B is Table 8, Model 3. Full tables for these estimates as well as the non-significant linear time trend available from author. Shaded regions represent 70, 80, 90, 95 and 99 percent confidence thresholds.

VITA

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