School Segregation, Educational Attainment and Crime

Evidence from the end of busing in Charlotte-Mecklenburg

Stephen Billings, UNC-Charlotte

David J. Deming, Harvard University and NBER

Jonah Rockoff, Columbia University and NBER

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Abstract

We study the impact of the end of race-based busing in Charlotte-Mecklenburg schools ("CMS") on academic achievement, educational attainment, and young adult crime. In 2001, CMS was prohibited from using race in assigning students to schools. School boundaries were redrawn dramatically to reflect the surrounding neighborhoods, and about half its students received a new school assignment. Using addresses measured prior to the policy change, we compare students in the same neighborhood who experienced a different change in school racial composition because they lived on opposite sides of a newly drawn boundary. We find that the resegregation of CMS schools widened racial gaps in middle school and high school math scores. We also find large increases in crime for poor minority males. We conclude that the end of busing widened racial inequality, despite efforts by CMS to mitigate the impact of increases in segregation.

I. Introduction

Since the landmark 1954 Supreme Court decision *Brown v. Board of Education,* schools have been seen by policymakers and courts as a primary social setting in which to address racial inequality. The *Brown* decision declared “separate but equal” schooling unconstitutional, yet efforts to engineer racial integration through student assignment policy have been highly controversial and not always successful. The most prominent example of court-ordered school desegregation is *Swann v. Charlotte-Mecklenburg Board of Education,* which in 1971 held that Mecklenburg County schools were *de facto* segregated even in the absence of an explicit policy, because neighborhoods were highly segregated. Most importantly, the *Swann* decision authorized the use of busing and the division of neighborhood school zones into non-contiguous areas in order to achieve racial balance in schools.

Race-based busing soon spread to school districts around the country, and court-ordered school desegregation became one of the most ambitious social policies of the 20th century. Scholars have connected the widespread implementation of school desegregation plans in the late 1960s and 1970s with increased educational attainment for black students (Guryan 2004, Reber 2010), higher income (Ashenfelter, Collins and Yoon 2006, Johnson 2011), improvements in adult health (Johnson 2011) and decreased rates of homicide victimization and arrests (Weiner, Lutz and Ludwig 2009). Many studies have found that segregation widens the racial test score gap, with most (but not all) concluding that schools play at least as important a role as neighborhoods (e.g., Cook and Evans 2000, Card and Rothstein 2007, Vigdor and Ludwig 2008).

Legal challenges to race-based busing in the late 1990s led to a reopening of the *Swann* case, and after a protracted battle CMS was declared “unitary” and ordered to disband race-based busing. The old system of student assignment, which created non-contiguous school zones and bused children around the county to preserve racial balance, was now illegal because it used race as an explicit criterion for student assignment. Beginning in the fall of 2002, CMS switched to a neighborhood-based student choice plan. The key features of the new assignment policy were that school boundaries were redrawn as contiguous areas around a school, and that students were assigned to their neighborhood school by default. Because neighborhoods in Charlotte were still highly segregated, this change led to a large and sudden increase in school segregation in the fall of 2002.

In this paper, we study the impact of the end of court-ordered desegregation in Charlotte-Mecklenburg schools (henceforth “CMS”) on students’ achievement test scores, criminal activity, and educational attainment. We match college attendance records from the National Student Clearinghouse (henceforth “NSC”) and arrest and incarceration data from the Mecklenburg County Sheriff (henceforth

“MCS”) to yearly student records from CMS. These matches enable us to track students who subsequently leave or drop out of CMS. Critically, the CMS data also include students’ exact addresses measured in the fall of each school year, which allows us to assign students to neighborhood school zones under the two policy regimes. We use students’ addresses measured prior to the policy change to fix their location, which allows us to treat exit from CMS, residential relocation during the prior school year, and other related responses as endogenous outcomes of the boundary change.

We construct an instrumental variable (henceforth “IV”) for the racial composition of students’ schools using the difference in racial composition between the new and previous school assigned to students *based on their addresses prior to the policy change*. This allows us to isolate variation in school race composition that comes from the redrawing of school boundaries. Unlike Jackson’s 2009 study of teacher sorting in CMS, where the percent of black residents in the neighborhood surrounding a school is used as a predictor of increased segregation, we construct our instrument using individual student addresses which leads to greater precision and more variation.

Our identification strategy compares students who lived in the same neighborhood but whose addresses placed them on opposite sides of a newly drawn school boundary. We show that while minority and low income students were unconditionally more likely to be assigned to segregated schools, there is no evidence of systematic student sorting across the newly drawn boundary within neighborhoods. Furthermore, the sudden change in boundaries meant that students living in the same neighborhoods but in younger grade cohorts had more exposure to newly segregated schools. Our IV strategy therefore compares the outcomes of students on either side of a boundary, across cohorts that have increasing exposure to the resulting change in racial composition.

We find that the resegregation of CMS schools widened inequality of outcomes between non-minority and minority students.[1] Students of both racial groups score lower on math exams when assigned to schools with more minority students. We find that a 10 percent increase in the share of minority students in a single school year decreases middle school math scores by about 0.016 standard deviations and high school math scores by 0.005 to 0.011 standard deviations. Appropriately scaled, these magnitudes are similar to results from other studies (e.g. Hoxby 2000, Hanushek, Rivkin, and Kain 2009, Vigdor and Ludwig 2008). Jackson (2009) finds that average teacher value-added decreased when schools acquired a higher share of minority students, but a comparison of his results to ours suggests

[1] Throughout our analysis we group white and Asian students as “non-minority” and black and Hispanic students as “minority,” though the student population in CMS is heavily dominated by white and black students.

that changes in teacher quality across schools explains only a small share of the effects we find on the racial test score gap.

The resegregation of CMS schools also led to marked increases in crime among minority males, and those from poor households in particular. For a black male who entered 6th grade in the fall of 2002, attending middle and high schools with 10 percentage points more minority students per year led to an average of 2.9 additional days incarcerated during a 12 month period in early adulthood. The increases in crime were largest for minority males who lived in neighborhoods that were between 40 and 80 percent minority, with smaller impacts in the most segregated neighborhoods (i.e. 0 to 20 and 80 to 100 percent minority). This lends some support to the idea that there might be "tipping points" in the impact of school racial composition on adult criminal activity. We find a significant negative impact of the share of minority students on high school graduation within CMS for non-minority students, with the important caveat that (unlike the college attendance data) we cannot measure graduation from another district or a private school. We find no significant impact of the re-zoning on educational attainment overall or by race, although we do find some heterogeneity in impacts by gender.

We discuss four possible explanations for the results. First, there is no evidence of differential selection out of CMS, and we show that accounting for endogenous movement out of the district and relocation to different school zones within CMS has little or no impact on the results. Second, we discuss evidence that explanations such as differential resource allocation and changing neighborhood racial composition are unlikely to account for the pattern of results. Third, we demonstrate that the impact of a change in the racial composition was similar for students who kept the same assignment and for students who were reassigned to a new school. This suggests that the disruption caused by a new school assignment is unlikely to drive our findings.

The evidence is most consistent with a peer effects explanation, where the reshuffling of students across schools led to changes in peer interactions that had different impacts for different types of students. This would explain why our results are so different by demographic group, particularly for the impacts on crime. Additionally, we find large increases in out-of-school suspensions for minority students that began in the 2002-2003 school year, immediately following the re-zoning. While differential policing of high minority schools may account for some of the increase in suspensions and reported crime (Kinsler 2011), the implication is that increased disciplinary incidents—due to closer monitoring or changes in behavior—in schools serving more minorities have long-term impacts on students' criminal behavior when they are no longer enrolled in school. We conclude with a speculative discussion of the implications of the pattern of results for peer effects, which includes recent work on the economics of identity (Akerlof and Kranton, 2002) and sorting of peer groups across different environments (Hoxby and Weingarth 2006, Cicala, Fryer and Spenkuch 2011, Carrell Sacerdote and West 2012).

II. Background

The landmark 1954 Supreme Court decision *Brown vs. Board of Education* disallowed *de jure* racial segregation of schools, but the 1971 *Swann v. Charlotte-Mecklenburg Schools* decision led to the implementation of race-based busing. Although CMS had no explicit race-based assignment policy, the Court ruled schools were *de facto* segregated due to highly segregated neighborhoods and contiguous catchment areas around each school. Following the court order, school zones in CMS were redrawn to capture non-contiguous areas with different racial compositions. It was mandated that CMS keep each school's percent black within 15 percentage points of the district average, and CMS periodically redrew boundaries to ensure that this balance was kept.[2] Racial balance was preserved using "satellite" zones that bused students from inner city, highly black neighborhoods to schools located in suburban, highly white neighborhoods.

In the early 1990s, the legal status of court-ordered desegregation became tenuous. *Board of Education of Oklahoma City v. Dowell* in 1991 outlined the conditions under which a school district can be declared unitary, or free from court control (see Tushnet 1996, Lutz 2011). This began a gradual unwinding of explicit school desegregation policies; since 1990, every contested motion for a school district to be declared unitary has resulted in a dismissal of the desegregation plan (NAACP 2000). Lutz (2011) finds that about 60 percent of the original impact on integration is reversed within 10 years after a district is declared unitary, and that this change in segregation increases dropout rates and private school attendance among black students outside of the South.

In 1997, a CMS parent sued the district because their child was denied entrance to a magnet program based on race (*Capacchione v. Charlotte-Mecklenburg Schools)*. This case escalated into a reopening of Swann in 1999 in a series of court battles which ended in April of 2002, leaving CMS no choice but to end race-based busing permanently.[3] The CMS school board discussed alternatives during

[2] Some schools located in lower density portions of Mecklenburg County did vary by more than the court benchmark of 15%. As discussed in Kane, Staiger and Riegg (2005), enforcement for outlying neighborhoods would have required long bus rides and substantial displacement of students to achieve targeted levels of integration.
[3] In September 1999, the district was ordered to discontinue the use of race in student assignment. The Swann plaintiffs appealed (Belk v. Charlotte-Mecklenburg Board of Education) and in November 2000 the ruling was overturned, holding that further review was necessary (Mickelson, 2003). In September 2001, the declaration of unitary status was affirmed, and a last-ditch appeal to the Supreme Court was denied in April of 2002.

the 1999 trial and adopted a neighborhood-based school choice plan (the "Family Choice Plan" or FCP) in December 2001.[4] New school boundaries for the fall of 2002 were drawn as contiguous areas around schools. Families were assigned to their neighborhood school by default, but could apply to attend other schools in the district, including magnet schools. Enrollment was subject to capacity constraints, and schools that were oversubscribed had admission determined by lottery (Hastings, Kane and Staiger 2008, Deming 2010, Deming, Hastings, Kane and Staiger 2011). To limit school segregation, CMS gave priority in the admissions lotteries to poor students who applied to schools that were majority non-poor. They also paired the FCP with a program called the Equity Plan, which provided high-poverty schools with additional resources such as smaller class sizes, bonuses for teachers and bond funds for renovation (Mickelson, Smith and Southworth 2009).

Under FCP, many of the previous school boundaries were redrawn. Figure 1 provides an illustration of this change for two middle schools; the top panel shows boundaries the school year 2001-2002 and the bottom panel shows the new boundaries drawn for the fall of 2002. Not only did satellite zones disappear, but the zones surrounding both schools were partially redrawn to ensure that schools were not overcrowded. Decisions about where to draw the boundaries were governed primarily by enrollment projections, with diversity taking an explicit backseat (Smith 2004, Mickelson, Smith and Southworth 2009).[5] The bottom panel of Figure 1 also shows how the new school zone boundaries were often not coterminous with census block group boundaries, creating variation in school assignments for students living in the same neighborhood.

The redrawing of CMS boundaries as contiguous neighborhood zones led to a marked increase in school segregation between school years 2001-02 and 2002-03. In a district where roughly 43 percent of students are black, the proportion of students attending a middle or high school with a high concentration of black students (over 65 percent) jumped from 12 to 21 percent, while the proportion attending a relatively integrated school—35 to 65 percent black—fell from 53 to 40 percent. As shown in Figure 2, this change did not reflect a pre-existing trend, nor did it diminish over time.

[4] The school board voted to approve the FCP in July of 2001, but the details were not worked out until December of that year. Parents were first asked to submit their school choices in the Spring of 2002.

[5] For example, at the November 9, 1999 meeting of the CMS Board, Superintendent Eric Smith described the idea behind the new process, saying, "It's a mechanical process, not a human process. It simply draws [maps] based on capacity and numbers of children, it doesn't make any sense in terms where children play, associations children naturally make as they are growing up, and it doesn't make any sense in terms of how families relate and interact."

III. Data

We use administrative records from CMS that span kindergarten through 12th grade and the school years 1995-1996 through 2010-2011. Every student who attended a CMS school for at least one semester is included, and students are tracked longitudinally across years. The data include information on student demographics such as gender, race and eligibility for free or reduced price lunch (our indicator of poverty), yearly end-of-grade (EOG) test scores for grades 3 through 8 in math and reading, and scores on end-of-course (EOC) exams in subjects such as Algebra I, Geometry, and English.[6] The EOG and EOC tests were standardized and administered across the state of North Carolina from 1993 to the present. The data also include information on graduation from CMS high schools and transfer records. Our data also include the exact address of residence in every year for every student in CMS, again from 1995 to the present. As we discuss below, this allows us to determine each student's school assignments under the busing and post-busing regimes.

We match CMS administrative records to a registry of all adult (defined in North Carolina as age 16 and above) arrests and incarcerations in Mecklenburg County from 1998 to 2011. [7] The Mecklenburg County Sheriff (MCS) tracks arrests and incarcerations across individuals using a unique identifier that is established with fingerprinting. The arrest data include information on the number and nature of charges, and the incarceration data include a time and date of entry and exit, with stints in county jail and state prison both included in length of incarceration for individuals who serve concurrently. These data allow us to observe future criminal behavior of CMS students, regardless of whether they transfer to a private school or drop out, but they are limited to crimes committed within Mecklenburg County.

In our analysis, we focus on the criminal activity of each student in the 6th through 18th months after their 18th birthday (i.e., age 18.5 to 19.5). Restricting our analysis to this 12 month period has three advantages. First, it is close to the peak age of criminal activity (Levitt and Lochner 2001, Hansen 2003). Second, starting at age 18.5 means that almost all students will have already graduated high school or dropped out, limiting the possibility of systematic measurement error from differential police

[6]While test scores in subjects such as Chemistry and US History are available in some years, we restrict our attention to tests that were administered continuously throughout the sample period. Elementary and middle school math and reading test score data go back to 1993. Later we use 5th grade test scores as a control variable, which are missing for about 15 percent of the sample. Missing scores are only modestly correlated with cohort; scores are missing for 21 percent of the earliest cohort and 15 percent of the latest cohort.

[7]We use name and date of birth to link individuals across the two data sources. While close to 90 percent of the matches are exact, we recover additional matches using an algorithm for partial matches that has been used and validated in previous work (Deming, 2011). The matching algorithm creates a score that measures differences between names and dates of birth in the two data sources. Common differences are shortened versions of names (John vs. Jonathan) or apostrophes and hyphens.

presence across schools. Third, all of the cohorts we examine, the youngest of which entered 6th grade in fall 2002, are old enough to have a full 12 months of crime data by June 2011.

We also make use of a complementary source of data on the exact location, day, and time of all *reported* crimes in Mecklenburg County, regardless of whether an arrest was made. We use these data to calculate measures of crime frequency in students' residential neighborhoods. We use data on college attendance records from the National Student Clearinghouse (NSC), a non-profit organization that provides degree and enrollment verification for more than 3,300 colleges and 93 percent of students nationwide. NSC information is available for every student of college age who had ever attended a CMS school, so students who transfer or drop out of CMS can be observed in any college covered by the NSC.

We limit our analysis sample to the eight cohorts of students who were rising first-time 6th grade students in the fall semesters of 1995 through 2002. Students who enter CMS after the change in boundaries are not included in the sample. Those who attended 6th grade in the fall of 1995 and progressed through school at the normal rate of one grade per year would graduate in the spring of 2002, and thus would have been unaffected by the change in school boundaries. In contrast, students who attended 6th grade in the fall of 2002 spent all of their middle and high school years in the post-busing regime. If these students had progressed one grade per year, they would have graduated from high school in the spring of 2009 and could potentially have attended college for the first time in the fall of 2009. Because our data on college attendance and crime end in 2011, we have limited ability to look at the impact among younger cohorts of students who experienced a change in segregation in elementary school. We also cannot examine longer-run measures of educational attainment such as persistence in college and college degree completion. Thus our main measure of postsecondary attainment will be whether a student attended college *within 12 months of the fall after their expected high school graduation date.* With this measure, students who repeat a grade but still attend college immediately after graduation can be counted, as can students who delay postsecondary enrollment for up to a year after on-time high school graduation.

We define residential neighborhoods within Mecklenburg County using the 371 Block Groups from the 2000 Census. We also use data from the County Tax Assessor's Office to define 981 "micro-neighborhood" parcel groups which are based on similar plots/structures of real estate. Given that these two sets of neighborhood boundaries are developed independently and with different purposes, it is not surprising that they are generally non-coterminous.

We use address records to assign students to 2000 census geographies and to middle and high school zones based on both the pre-and post-2002 boundaries. Because families may sort in response to the policy change, it would be problematic to use their contemporaneous addresses to assign students to neighborhoods and school zones. We examine two alternatives for assigning students to neighborhoods and school zones: (a) address in the fall of 5th grade or (b) address in the latest year observed up to fall 2001.[8] These options trade off the benefits of comparing all students based on residence just before entering middle school but at different points in time vs. comparing them based on residence just before the reform but at different grade levels. One might also be concerned that residential moves just prior to the policy change were correlated with students' future academic achievement or criminal behavior, and we therefore present results using residence in 5th grade, but our results are similar regardless of which alternative we choose.

Of our initial sample of 61,061 students, about five percent have missing or invalid address information and we were unable to geocode their residential location, which leaves us with 57,682 students.[9] Nearly all of first-time 6th graders in fall 1995 did not attend CMS post-busing, and we therefore drop them from our analysis of enrollment and short-term outcomes such as test scores, leaving us with a sub-sample of 51,301. Table 1 lists descriptive statistics for our sample. Overall, 43 percent of students are black, 5 percent are Hispanic, and just under half of all students come from poor households (i.e., receiving free or reduced price lunch). Fifth grade test scores were slightly lower than the state average in math (-0.014 standard deviations) and reading (-0.040). Overall, about 50 percent of students were assigned to a new school as a result of the 2002 change in school zone boundaries. Just under half of these students attended a college, while 7 and 5 percent were arrested and incarcerated, respectively, during the windows in which we measure these outcomes.

Splitting the sample by the percentage of minority residents in the student's census block group (CBG) gives a sense of how residential segregation would lead to school segregation under a policy of contiguous neighborhood school zones. We split the sample at 20% and 66% minority, which are close to the minimum and maximum share of minority students in any CMS high school under race-based busing. In CBGs with fewer than 20% minority residents, few students are black (10%), Hispanic (3%), or

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[8] Our earliest cohort was in 5th grade in fall 1994, prior to the start of our panel; we therefore use their address in the fall of 1995, when they were in 6th grade.

[9] While we found no systematic pattern in invalid addresses by school attended prior to busing, information was slightly more likely to be missing in the earlier cohorts. Address information was missing or invalid for about 7 percent of the earliest two cohorts, and decreases gradually to about 4 percent for the latest cohort. We have explored filling in invalid addresses by using earlier (or as a last resort, 2003 and later) years, and find it changes our results very little.

poor (16%), while in CBGs with more than 66% minority residents the vast majority of students are black (86%) or Hispanic (5%), and poor (89%). While it is clear that residential and racial segregation is driven predominantly by the location of black families and students—Hispanics are a small part of the overall population and more evenly distributed across geographic areas—the court-order (and its removal) was based on the distribution of both black and Hispanic students, and we aggregate the two minority groups in much of our analysis. Table 1 also shows that students living in high minority neighborhoods were more likely to be reassigned (79%) relative to those in low minority neighborhoods (34%).

Separating statistics by residential composition also reveals large differences in college attendance and criminal behavior by students across neighborhoods. For students living in low-minority neighborhoods, (four year) college attendance rates are (35%) 46%, while college attendance for students in high minority neighborhoods is only (18%) 26%. Arrest and incarceration rates for those in CBGs with fewer than 20% minority residents are just 3 and 2 percent, respectively, while the same rates for students in CBGs with more than 66% minority are 14 and 11 percent—roughly 5 times higher.

Underlying the summary statistics in Table 1 are a few demographic trends worth mentioning here. Within our sample, cohort size grew by 32 percent over this period, and the share of minority students grew from roughly 42 percent to about 54 percent. These trends were slightly stronger than those the entire state in overall enrollment growth across cohorts (18%) and growth in share of minority students (from 31 to 38 percent). Nonetheless, fifth grade math and reading scores in CMS rose from slightly below to slightly above the state average.

IV. Empirical Strategy

Our strategy is based on identifying quasi-experimental variation in students' exposure to segregated schools due to variation in (a) students' residential location within neighborhoods, relative to newly drawn school zone boundaries, and (b) cohorts' exposure to the new school zones. Figure 3 shows kernel density plots (weighted by enrollment) of racial composition before and after the fall of 2002 for actual school *enrollment* (top panel) and school *assignment* based on residence in grade 5 (bottom panel). Prior to re-zoning, the vast majority of students was assigned to, and attended, a school where the percentage of minority students ranged between 35 and 65 percent. In the fall of 2002, these distributions show a marked shift in mass from within the 35-65 range to the more extreme parts of the distribution, consistent with the time variation shown in Figure 2. It is also interesting to note that, in line with the presence of magnet programs and other special schools (e.g. for disabled or older students), the *actual* distribution of school racial composition was noticeably more disperse than the *assigned* distribution both before and after re-zoning.

We take the difference in percent minority between each student's prior and new school zones, which we will refer to as Δ, and use this as an instrumental variable for the actual change in the percent of minority students at the school(s) the student attended.[10] Our instrument is designed to isolate changes in school racial composition due to the policy, and not to decisions or actions of students and their families, and therefore does not include residential relocation or attrition after grade 6. There are 165 unique values of Δ, which range from 0.73 (i.e., 73 percentage points more minority students in our sample for the school zone pair) to -0.43. Within each newly rezoned middle school, the median number of different values for Δ is 9, and for high schools that number is 16. Figure 4 displays the distribution of student-level values for Δ, separately by race. Given the increase in segregation shown earlier, it is not surprising that the distribution Δ for minority students lies to the right of non-minorities, but there is considerable variation (and both positive and negative values) for both groups.

School zone (re-)assignment is determined by residence and therefore is likely correlated with household and neighborhood characteristics. To account for the non-random drawing of boundaries, we compare students who lived in the same neighborhoods and were previously zoned to the same schools but whose residence in 5th grade placed them on the opposite sides of a new school boundary. For small enough definitions of a neighborhood, this converges to a boundary discontinuity design as in Black (1999) and Bayer, Ferreira and McMillan (2007), with the important difference that we examine newly drawn boundaries using addresses measured prior to the redrawing.

As mentioned above, we examine two definitions of neighborhood. First, we use census block groups (CBGs), which are the smallest geographic area for which demographic information is released by the Census Bureau. 64 percent of CBGs in CMS have a changed school boundary drawn through them. The median number of students per CBG across our eight sample cohorts is 218. Our second neighborhood definition is the "micro-neighborhood" parcel group, mentioned above, which is used for property tax assessment. These are considerably smaller and therefore trade off identifying variation in

[10] Our IV is similar to the "black differential" (BD) variable used by Jackson (2009), who studies the impact of the rezoning in CMS on movement of teachers across schools, and here we note the key differences between Jackson's measure and ours. We use percent black or Hispanic rather than percent black, though this is less important given the small population of Hispanics in CMS. Jackson (2009) used North Carolina state data and did not have student address information, so BD is the difference between the school's 2001 racial composition and the racial composition of the census area around the school. We calculate our measure using the actual school zone boundaries and student addresses. Finally, our measure is calculated at the student level, which makes the IV more precise and also provides us with more identifying variation.

our instrument for greater assurance of the validity of this variation. Still, 56 percent of micro-neighborhoods in CMS have a changed school boundary drawn through them. The median number of students per micro-neighborhood across our eight sample cohorts is about 142. For both CBGs and "micro-neighborhood" parcel groups, the mean interquartile range among areas with variation in the instrument is about 10 percentage points for the middle school boundaries and 7 percentage points for high school. Overall, there is relatively wide variation in treatment within both geographic areas.

Because we examine long-run outcomes, we must account for differential exposure of students to the new school zone boundaries. To do so, we calculate *cumulative* post-policy changes in school zone racial composition by taking our measures of Δ and multiplying by the "years of treatment" for the student's cohort in middle and high school.[11] Our years of treatment variable accounts for endogeneity in grade progression by using the first year students appeared in 6th grade and normal grade progression through middle and high school. (For example, rising 6th graders in 1997 that progress through school at a normal rate would enter 12th grade in 2002-2003 and have one year of exposure to the new boundaries, so all first-time rising 6th graders in 1997 are assigned one year of treatment. Likewise, students who were first-time rising 6th graders in the fall of 2002 are assigned seven years of treatment.) Changes in school zone racial composition occur for both middle and high school, and we therefore aggregate appropriately based on the number of years of potential treatment in grades 6 to 8 and the number of years of treatment in grades 9 to 12. We also distinguish years of treatment across outcomes. For high school graduation (from CMS), crime, and college attendance, treatment is measured using progression through grade 12. For middle school and high school exams, treatment is measured using progression through the grade at which the exam is typically taken (i.e., 9th grade in English and Algebra, 10th grade for Geometry, and 11th grade for Algebra II).[12]

IV.1 Checks on Non-Random Sorting and Attrition

It is possible that school zone boundaries were re-drawn in such a way that, even within small geographic areas, it created significant differences in unobservable attributes of students assigned to higher percent minority schools. We investigate this by testing whether students' observable characteristics (race, income and test scores) and micro-neighborhood crime report frequency prior to

[11] This cumulative measure has the advantage of simplicity but implies linearity in the impacts of exposure. Although we begin our analysis with this restriction, later we will relax it and test for non-linearity.

[12] Although there is some variation across students in the timing of the test, we show later than controlling for the grade in which the exams are taken has little effect on our estimates, suggesting that timing was not sensitive to the re-zoning. In cases where a student took the exam multiple times, we only use the score from the first exam.

the boundary change are systematically related to our instrumental variable(s). Specifically, we estimate regressions of the form shown by Equations 1a and 1b:

$$Y_{izjc} = \beta_0 \Delta_i + \pi_z + \eta_j + \gamma_c + \varepsilon_{izjc} \quad (1a)$$

$$Y_{izjc} = \beta_0 \Delta_i * T_c + \pi_z + \eta_j + \gamma_c + \varepsilon_{izjc} \quad (1b)$$

where characteristic *Y* for a student i living in old school zone *z*, neighborhood j and grade cohort c, is regressed on the student's change in school-zone percent minority (Δ_i) or its interaction with years of treatment ($\Delta_i * T_c$), and fixed effects for old school zone, neighborhood, and cohort ($\pi_z, \eta_j \text{and } \gamma_c$).

The results of these specification checks are shown in Table 2. To illustrate unconditional correlations of our instrumental variables with student and neighborhood characteristics, Columns 1 and 4 show "naïve" regressions that omit fixed effects for neighborhood and previous school zone. Students who saw greater increases in the share of minority students in their assigned school zone are more likely to be black and eligible for free lunch, score lower on 5th grade tests, and live in micro-neighborhoods with higher levels of reported crime per student in the years prior to re-zoning. However, once we add CBG and prior school zone fixed effects (Columns 2 and 5), these coefficients are no longer statistically significant, save for a very small (and *negative*) effect on whether a student is black. The use of micro-neighborhood instead of CBG to define neighborhood (Columns 3 and 6) eliminates all significant coefficients, and the point estimates are all very close to zero.

As another check on potential sorting, we estimated similar regressions of students' 5th grade test scores where the instrumental variables coefficient was allowed to vary for minority and non-minority students (see Appendix Table A1).[13] Although specifications with micro-neighborhood fixed effects show no significant relationships with our instruments, specifications with CBG fixed effects show a very small but significant negative relationship for non-minorities. For example, a 10 percent increase in Δ was associated with a decrease in grade 5 scores of roughly -0.01 standard deviations. Our interpretation is that within-neighborhood variation in our instrument is unlikely correlated with student characteristics at the CBG level, particularly if we condition on observables, but that our identification strategy is more strongly supported in specifications using within-parcel group variation.

Another potential concern for our analysis is incomplete *ex post* observation of students in our sample, i.e., attrition bias. This is particularly relevant for short-run outcomes like exam scores, which only are available for students who continue to be enrolled in CMS. Overall, about 18 percent of non-minority and 13 percent of minority students in our sample were no longer enrolled in CMS in the fall of 2002. However, our sample is based on enrollment in 5th and 6th grade prior to the boundary changes

[13] These specifications also included interactions of the cohort fixed effects with student ethnicity.

(students who entered, or returned to, CMS after the changes are not included) and most of this attrition occurred prior to 2001; conditional on enrollment below 12th grade in fall 2001, only 4.5 and 3.5 percent, respectively, of non-minority and minority students were not enrolled in CMS in fall 2002.

We estimate the relationships between our instrumental variables and fall 2002 enrollment in CMS, allowing these relationships to differ for minority and non-minority students. We find no significant coefficients in specifications with either CBG or parcel group fixed effects, and the coefficients change little even if limit the sample to students enrolled in fall 2001 (Table 4). This holds regardless of whether we use the one-year change in school zone percent minority (Δ_i) or the cumulative percent change ($\Delta_i * T_c$).

Non-random attrition from CMS schools *per se* would not be a concern for our analysis of crime and college-going, which are measured outside of CMS data. Rather, the main concern in these analyses is whether the new student assignment policy is correlated with students' future criminal activity ***outside*** of Mecklenburg County or attendance at one of the few colleges not covered by the NSC. While we cannot test for this type of non-random selection directly, the fact that we find little evidence of attrition from CMS related to the re-zoning helps support the notion that our data limitations do not drive our results.

IV.2 First-Stage Estimates

For the short-run instrument (Δ), our first-stage specification is shown by Equation 2, where M_{izjc} is *actual* post-policy percent minority of the (middle or high) school the student attended in fall 2002, and X_i is a vector of demographic covariates.[14]

$$M_{izjc} = \beta_0 \Delta_i + \beta_2 X_i + \pi_z^M + \eta_j^M + \gamma_c^M + \varepsilon_{izjc} \qquad (2)$$

For each 10 percent increase in the school zone percent minority, we estimate a roughly 3 percent increase in the actual percent minority of the school the student attended in the fall of 2002 (Column 1 of Table 4). Breaking out these results by ethnicity, we see slightly larger point estimates for non-minorities (3.5 percent) than minority students (2.9 percent).[15] While the first stage is highly significant (t-statistic well above 10), one might conclude that the short-term impact of the boundary change was fairly limited. Indeed, when we use our long-term instrument ($\Delta_i * T_C$) as a first stage predictor for *cumulative* exposure to school peers from minority groups, our IV coefficient for the

[14] The control vector (X_i) includes indicators for ethnicity, free lunch eligibility, and 2nd order polynomials in students' 5th grade math and reading scores, and the percent minority in the student's school zone prior to busing.
[15] In these regressions we augment Equation 2 with indicators for our two race categories (black and Hispanic, white and other) as well as interactions of these effects with Δ_i.

pooled sample is 0.46, with effects of 0.57 for non-minorities and 0.42 for minorities when we allow these coefficients to differ by racial group. This increased power over a longer time period is consistent with the fact that students who were already well into high school were more likely to remain in their previously assigned school than younger cohorts and, as noted by Kane, Staiger and Riegg (2005), CMS made every effort to accommodate choices in the first year, in part by expanding capacity at schools where they anticipated high demand. In subsequent years it became harder to attend a non-magnet school outside of one's school zone.[16]

Our focus on the percent minority students as a policy outcome is motivated by our study of the elimination of race-based busing. Minority students tend to be poorer, have lower academic achievement, and have more disciplinary problems than non-minorities and the policy changes we study will affect the composition of schools along these dimensions as well. To illustrate, Column 2 of Table 4 shows that a 10 percentage point increase in the share of minority students in the school zone is associated with having peers with 5th grade math test scores that are roughly 0.04 standard deviations lower; an effect which is similar across all demographic groups. As in Jackson (2009), our research design cannot separate the impact of race from other factors with which it is correlated, and our results should be interpreted with this in mind. Nevertheless, most efforts to desegregate schools have focused on ethnic and racial composition and have relied on manipulation of school boundaries, so our empirical strategy is well suited to answering a question of great policy interest.

A key point of interpretation is that we are estimating a local average treatment effect based on students who comply with school zone assignments. About 73 percent of non-minority students and 55 percent of minorities attended their assigned school, 9 percent of non-minority and 11 percent of minorities attended their previously assigned school, 9 percent of non-minority and 13 percent of minorities attended magnet schools, and the remaining students choose another CMS school.[17] We examine whether students' behavior under the new school choice system was influenced by re-zoning, again using variation within neighborhoods (here defined using parcel groups). We find a small but marginally significant impact of increases in students' assigned percentage of minority peers on their

[16] CMS did not expand capacity at highly demanded schools in fall of 2003, and fewer students got their first choice. By 2005, political pressure led CMS to disband the FCP in favor of a "controlled choice" program where the only options were a neighborhood school or a magnet school (Mickelson, Smith and Southworth 2009).

[17] We define our instrument using students' residential school zones in grade 6, well before the policy change for some cohorts, so some students choosing "another" CMS school may be attending the school assigned to them based on their new residence. The small fraction of students returning to their previous schools is in itself quite significant since the open enrollment plan gave first priority to students who had attended the school in the previous year.

probability of attending their previously zoned school (Column 3), with similar point estimates for both racial groups (albeit more precise for minorities). There is little evidence of any effect on attending a magnet program or another CMS school (Columns 4 and 5).

Re-zoning may have also affected families' decisions to change residence *within* CMS. We measure changes in residence using student addresses measured from the end of one school year to the next. Moves between spring 2000 and spring 2001 appear unaffected by our instrument, supporting the notion that the exact drawing of the boundaries was unanticipated (Column 6). However, in the school year immediately preceding the policy change (spring 2001 to spring 2002), we find that minority students who were assigned larger increases in the percent of minority peers were less likely to move (Column 7), suggesting that, on average, these families may have viewed the re-zoning positively. Moves in the year after the policy change (i.e. the first year of the new boundaries) appear unaffected (Column 8), which indicates that families' experiences with the new system did not lead to a significant number deciding to relocate.

V. Impacts of Re-zoning on Outcomes in School and Beyond

Our second stage specification is shown by Equation 3, where an outcome of interest (*Y*) is regressed on the predicted change in (cumulative) racial composition ($\widehat{M}$) and the same set of controls used in the first stage regression.

$$Y_{izjc} = \delta_0 M_{izjc} + \delta_1 \Delta_i * T_c + \delta_2 X_i + \pi_z^Y + \eta_j^Y + \gamma_c^Y + \xi_{izjc} \tag{3}$$

To conserve space, we do not report first-stage regression results throughout this section, but our instrumental variables are always highly predictive. Interactions of the instrument with racial groups generally show slightly higher coefficients for non-minorities, but all of these coefficients generally range from 0.4 to 0.6 and the t-statistics on these coefficients all are well in excess of 10. The larger first-stage coefficient for non-minorities reflects these students' higher propensity to attend their assigned school, or at least a school with low percent minority. In contrast, minority students were more likely to attend a school (often a magnet school) with a very different racial composition than their assigned school. Still, given the existence of the choice plan and the focus on differential attendance within neighborhoods, we regard the first stage as relatively strong.

V.1 Middle and High School Test Scores

Table 5 shows impacts on 8^{th} grade test scores, absences, and suspensions, where our first stage dependent variable and instruments measure, respectively, actual and changes in assigned racial composition through grade 8. In the pooled sample we find a significant negative impact on math test scores and a positive impact on the number of days a student was suspended. Allowing for different effects by racial group, the negative math impacts appear to be driven equally by both groups. There is also a marginally significant negative impact on English scores for non-minority students and significant impacts on both the probability (for non-minorities) and days (for minorities) of suspension. To give a sense of the magnitudes of these effects, our estimates for math scores suggest a decrease of 0.01 standard deviations for each year during middle school that a student attends schools with 10 percent more minority students.

In order to increase our confidence that these results represent a causal impact of the re-zoning policy, we also run reduced-form regressions of our instrumental variable ($\Delta_i * T_c$) for the cumulative change in percent minority at students' assigned middle school interacted with indicator variables for (1) cohorts who entered grade 8 prior to re-zoning and (2) each of the three cohorts who entered grade 8 after re-zoning. The results of these regressions (Appendix Table 2) show that changes in assigned percent minority in students' middle school zone have no significant impact on outcomes for cohorts whose grade 8 outcomes occur prior to re-zoning, but have clear and immediate impacts on math test scores and suspensions after the end of busing, particularly for minority students. These results also suggest higher percent minority at the school level may have increased minority student absences.

We next turn to the end-of-course English and math exams typically taken during high school. We present results that pool all students in the top panel of Table 6, and then separately estimate coefficients by racial group in the bottom panel. Similar to our results for middle school, there is no statistically significant impact on English test scores. However, we do find significant decreases in math test scores. When we allow effects to differ across racial groups, we find a large negative impact on the Algebra I test scores of non-minority students, but no significant impact for minority students. In contrast, the coefficients for more advanced subjects (Geometry and Algebra II) are very similar across the two groups. Again, to provide a sense of magnitude, these results suggest that students of all racial backgrounds perform roughly 0.005 to 0.01 standard deviations lower on end-of-course math exams for each year after rezoning that they attended schools with 10 percent more minority students.

Selection into high school test taking is a more serious concern, since advanced math tests are not required for graduation and some students may drop out of high school or leave CMS prior to when they would have taken the exam. Because the timing of these exams is endogenous—advanced students take them before entering high school and struggling students usually delay or avoid taking them at all—we cannot use the same "placebo" test for robustness as we did with the middle school outcomes. Instead, we test the robustness of these results by imputing test scores for students in our sample with missing values and, re-running the regressions from Table 6, ask whether our coefficient estimates are sensitive to the choice of imputation method. Our baseline imputation uses a predicted test score from a cross-sectional regression of the high school exam score on the same-subject (i.e. English or math) score from 5th grade. Then we alternatively impute scores that are 0.5 standard deviations below or 0.5 standard deviations above these predicted values, essentially assuming that students with missing scores would have performed much worse or much better than we would predict from their performance in grade 5. The results of these imputations (shown in Appendix Table 3) indicate that missing values are not driving our results; the coefficients on math scores change little and always remain negative. In fact, in the case of Algebra II, the additional precision gained by imputing missing scores makes the estimates negative and statistically significant in all 3 imputation scenarios.

Our estimates of the impact of school share minority on test scores are somewhat low relative to others in the literature, where an increase of about 10 percentage points in share minority has been found to translate to a decrease in math scores of between 0.04 and 0.07 standard deviations (e.g. Hoxby 2000, Hanushek, Rivkin and Kain 2009, Vigdor and Ludwig 2008). However, our estimate is of the one-year impact of exposure to a higher fraction of minority students, whereas most other studies consider the total impact of switching between schools of different racial compositions, irrespective of how many years a student enrolled.

V.2 High School Graduation and College Attendance

We measure college attendance as at least one semester of enrollment within 12 months of the fall after a student's expected high school graduation date. This time window allows for students to delay college enrollment for one year or to take one extra year to progress through high school based on their initial 6th grade cohort. Since our last cohort of 6th graders was expected to graduation in the spring of 2009, we are unable to measure college persistence or completion. Unlike the outcomes that are measured with CMS administrative data, we can observe college attendance for students who leave CMS so long as they attend an institution that is covered by the NSC data. Thus attrition from the district in response to rezoning is not a threat to the validity of the college attendance results. However, we only have graduation information for CMS high schools, not private schools in Mecklenburg County or public schools in other areas, so those results should be interpreted with more caution.

The results for educational attainment are in Table 7. Panel A presents pooled results and Panel B separates the impact by student race. In Panel C we further allow the impact of re-zoning to vary by race and gender. We find a significant decrease in high school graduation that is concentrated among non-minority students. However, there is no corresponding decrease in college attendance. The lack of impact on college attendance means we are unable to rule out that some non-minority students who did not graduate from CMS left to attend a private school or a public school outside the county. In Panel C, we do see a small but statistically significant decrease in four-year college attendance among non-minority females, and an *increase* in college attendance for minority males. Since these results are not affected by attrition from CMS, we can be more confident that they represent real changes in educational attainment.

V.3 Criminal Behavior

We measure the impact of re-zoning on criminal behavior in the 6th through 18th months after students' 18th birthday (i.e., age 18.5 to 19.5). As mentioned earlier, this restriction is necessary to ensure that the window of time in which outcomes are measured is constant across cohorts and unaffected by differential reporting of school-based crime. The results are in Table 8, which is structured similarly to Table 7 with Panels A through C presenting results overall, by race and by race and gender respectively. We find increases in crime that are driven entirely by minority students. In Panel C, we see that the overall increase in crime for minorities masks very different results by gender. Minority females commit fewer crimes and spend significantly fewer days incarcerated. In contrast, minority males who are assigned to schools with more minority students are significantly more likely to be arrested, have more total arrests and spend more total days incarcerated. The increase in crime for minority males is particularly large – for a 10 percentage point increase in school share minority in a single year, minority males spend about 0.42 more days incarcerated.

We explore results that are broken out by type of crime (not shown) and find that the small decreases in crime for females are driven primarily by drug crimes, while the increase in crime for minority males is distributed evenly across violent, property and drug arrests. Note that students who move outside of Mecklenburg County (perhaps by attending an out-of-town college) could in principle commit crimes that are not recorded in our data. However, the results for criminal outcomes are nearly identical when we restrict our analysis to students with no college record, or when we eliminate students who attend college outside Mecklenburg County.

VI. Discussion

After the re-zoning of CMS schools, students attended schools with a greater share of peers of their own race. Thus we can project the impact of our results on racial inequality in outcomes. In Tables 5 and 6 we show test score decreases for all students when they attend schools with more minority students. Since the re-zoning led to a decrease in the share of minority peers for non-minority students and an increase for minority students, re-zoning widened racial inequality in test scores. To get a sense of the magnitude, we multiply the mean value of the instrument by the first stage for students of each racial group, and then also by our estimates for each outcome.[18] This calculation implies that for the latest cohort (rising 6th graders in the fall of 2002), the re-zoning widened the racial gap in middle and high school math scores by about 0.04 standard deviations. This finding, which is remarkably consistent across tests, masks considerable variation across neighborhoods. If we compare non-minorities who live in census block groups that are less than 20 percent minority to minorities who live in census block groups that are more than two-thirds minority, we find that the racial gap in math scores widens by about 0.08 standard deviations. Similar calculations for crime reveal that the re-zoning widened the racial gap in days incarcerated by about 0.1 days overall and 0.5 days in the comparison across neighborhoods. However, we find no impact on racial gaps on postsecondary attainment.

VI.1 Investigation of Mechanisms

We consider four possible explanations for the pattern of results. The first is that the impact of school resegregation comes not through changes in the school itself, but from endogenous reactions such as families exiting CMS for private school, or moving to a different neighborhood to attend another public school. We examine the impact of endogenous mobility in two ways. First, we re-estimate our main results for crime and college attendance while excluding the approximately 4 percent of students who were not enrolled in CMS in the fall of 2002. While exiting CMS is part of the treatment, a large difference in the pattern of impacts with these students excluded would suggest that students' experiences outside of CMS might be driving our results. This sample modification diminishes the reduction in crime and the increases in educational attainment for white females, and the increase in crime among minority males becomes slightly larger. Other results are essentially unaffected. Second, we exclude the approximately 14 percent of students who relocated to a new address between fall 2001

[18] For example, the mean value of the instrument for non-minorities for Algebra I is -0.34, the first stage is 0.57, and the coefficient on Algebra I for non-minorities in Table 7 is -0.204. For minorities, these figures are 0.22, 0.45 and -0.008 respectively. Multiplying the values together separately by race gives 0.039 for non-minorities and -0.001 for minorities, for a total gap of 0.04 standard deviations.

and fall 2002. This also slightly magnifies the increase in crime among minority males, but no other outcomes are substantively affected. Results of these specification checks are available upon request. Overall, we conclude that the results are driven by changes in the CMS schools attended by the students in our sample.

A second possible explanation is that the end of race-based busing not only changed schools' racial compositions, but also shifted the allocation of resources across CMS schools. However, schools in high poverty, minority neighborhoods actually received additional resources. CMS paired the new student assignment policy with a program called the Equity Plan, which provided additional funds for lower student-teacher ratios, school renovation projects, learning equipment and supplies (Mickelson, Smith and Southworth 2009). While the Equity Plan also provided bonuses for teachers in high poverty schools, the evidence suggests that these incentives were insufficient to prevent the flight of effective teachers from inner city schools. Jackson (2009) studies teacher sorting in CMS following the boundary change and finds that a 10 percentage point increase in the share of black students leads to a reduction in elementary school teacher value-added of about 0.01 to 0.02 teacher-level standard deviations. Since the standard deviation of teacher value-added is typically only about 10 to 15 percent of a student-level standard deviation, the estimates in Jackson (2009) imply that teacher sorting could be responsible for a decline in student test scores of about 0.001 to 0.003 standard deviations. While we cannot say whether sorting on value-added happened to the same degree in high schools, decreases in teacher effectiveness could thus explain some relatively small share of the test score impacts. Recent research on teacher effectiveness in high school suggests that teacher effects are larger for Algebra than for English, which is consistent with our finding of effects for math but not English scores (Jackson 2012).

A related explanation for the results is that neighborhoods changed over time, which in turn affected school climate. Weinstein (2010) studies neighborhood change in Charlotte following the end of busing and finds that a 10 percentage point increase in the percent black of an assigned elementary school led to a 1.2 percentage point change in the percent black of the neighborhood five years after busing. However, there are two reasons to think that neighborhood change was not large enough to explain much of the results. First, Weinstein's results are about one-third as large (0.4 percentage points) when he uses share minority, which is what we use here (Weinstein, 2010). Second, the results for elementary schools are likely to be an upper bound for the impact of a change in racial composition of middle schools and high schools, because their catchment zones are much larger.

A third possible explanation is that the re-zoning affected students by disrupting their attendance patterns and forcing them to adapt to a new school. A fourth possible explanation is that the reshuffling of students across schools led to changes in peer interactions that had different impacts for different types of students. In Table 9 we consider these explanations by allowing the impact of re-zoning to vary by income and school assignment. Panel A examines variation in impacts by students' race, gender, and free lunch status (an indicator of poverty), while Panel B estimates a different regression that includes four-way interactions between the above and an indicator for whether a students' school assignment stayed the same before and after the re-zoning. For space reasons, we only report the coefficients for minority males, although the model is estimated with all groups. The results for other groups are available upon request.

In Panel A we see that the re-zoning had very different impacts on poor vs. non-poor minority males. Most of the small increase in postsecondary attendance shown in Table 7 is driven by non-poor minority males, who are about 0.85 percentage points *more* likely to attend a four-year college for a 10 percentage point increase in share minority in a single year. In contrast, all of the increase in crime is driven by poor minority males, who spend nearly 0.5 more days incarcerated for a 10 percentage point increase in share minority in a single year. We can reject equality of the coefficients across income groups for all seven outcomes in Panel A of Table 9. Panel B further divides poor and non-poor minority males into students whose school assignment did not change and students who were assigned to a new school after the re-zoning. Here we see that there is no significant difference within income groups in the impact by school assignment status. Poor minority males who stayed in the same school after the rezoning but received an influx of same-race peers committed just as many more crimes as demographically similar students who were given a new assignment. While not shown, the results for other race, gender and income groups are very similar by school assignment status. From this we conclude that disruption from being re-zoned is not driving our results.

The results in Table 9 and the heterogeneous impacts by race and gender in Tables 7 and 8 suggest that changes in peer interactions are driving our results, particularly for the crime and educational attainment impacts. Furthermore, the evidence presented in Table 5 and Appendix Table 2 shows that large changes in behavior measures such as absences and suspensions happened in the first school year after the boundary change. In the next section we investigate peer effects further by allowing the impact of the policy to be different for schools of different baseline racial composition.

VI.2 Heterogeneity by Neighborhood Race

Next we test for nonlinearities in the impact of a change in racial composition by allowing the impact of the instrument to vary with the racial composition of Census Block Groups. The new boundaries were drawn so that schools closely mirrored the racial composition of the surrounding neighborhood. Students who lived in highly segregated neighborhoods were especially likely to be "treated" with a large dose of school segregation (Jackson 2009). Thus the value of the instrument is positive for nearly all students in high minority neighborhoods, and negative for students in mostly non-minority neighborhoods.

We divide students into five quintiles based on the share of minority residents in their 2000 Census Block Group. Then we re-estimate our 2SLS model with interactions between neighborhood race quintiles and student race, gender and income cells. Since the result is a set of 40 coefficients for each outcome, we show selected results in graphical format in Figures 5 through 7. Each figure present coefficients and 90 percent confidence intervals for the impacts from the second stage model interacted with students' neighborhood percent minority, as well as the indicated race-gender-income category. We focus on college attendance among non-minority females and college attendance and days incarcerated among minority males, since these are among the key results in the paper.

Two findings emerge from Figure 5, which shows the four-year college attendance results for white females. First, we find a small but tightly estimated negative coefficient for white females who live in mostly white (i.e., 0 to 20 percent minority) neighborhoods. Nearly all of these students attended schools that experienced a sizeable outflow of minority students. The impact is larger for poor white females. Poor white females in mostly black (i.e., 80 to 100 percent minority) neighborhoods, while there are not very many of them, are much *more* likely to attend a four-year college after the re-zoning. Since nearly all of these students attend schools that receive an influx of minority students, the combination of the results for quintiles 1 and 5 suggest that poor white females benefited from movement away from integration in both directions.

Figures 6 and 7 present results among minority males for four-year college attendance and days incarcerated respectively. Looking at the results in Figure 6 for non-poor minority males, we see a small but significant positive coefficient for quintile 1. Again, since nearly all of these students attended schools that lost minority students, the estimates imply that non-poor minority males living in white neighborhoods were *less* likely to attend a four-year college after re-zoning. The large positive coefficients for quintiles 3 through 5 show that non-poor minority males were made better off when their schools began to match the racial composition of the surrounding neighborhood. Overall, the results imply that non-poor minority males attend four-year colleges at higher rates when their neighborhood school becomes more segregated.

Finally, Figure 7 shows that the overall increase in incarceration among poor minority males is particularly large for students who live in neighborhoods that are 40 to 80 percent minority. The estimates for highly segregated neighborhoods (0 to 20 percent and 80 to 100 percent minority) are positive and statistically significant but smaller. This provides some evidence for the idea that there are "tipping points" in the impact of school racial composition on crime. In contrast, we see negative and borderline significant impacts on incarceration for non-poor minority males in quintiles 3 through 5. Combined with the results in Figure 6, this suggests that non-poor minority males are much better off in schools with more minority students, while poor minority males are worse off.

The heterogeneity by student group and neighborhood race in Figures 5-7 is difficult to reconcile with differences in teacher effectiveness, school resources or even monotonic peer effects (i.e., students perform better when their peers have higher average levels of achievement). Hoxby and Weingarth (2006) explore different functional forms of peer effects in student achievement and find support for their "Boutique" and "Focus" models, both of which imply that diversity through integration of students by ability (which is correlated with race) would reduce overall achievement. Yet results for test scores that are separated out by neighborhood race (not shown) are nearly linear-in-means. Hoxby and Weingarth (2006) offer homogeneity of classroom instruction and practice as possible explanations for the efficiency gains from ability segregation. The analog for peer effects in crime and educational attainment in high school might be homogeneity of social groups.

One hypothesis is that assignment to a school with more or fewer minorities has a particularly large impact on students who are on the margin of belonging to a particular social group. This story is consistent with the model in Akerlof and Kranton (2002), where students exert discontinuous effort in school when they are on the margin between two social categories (e.g., high achieving "nerds", low achieving "burnouts," etc.) They suggest identifying the model based on nonlinearity in effort, yet here we have instead an exogenous change in the social environment holding constant the characteristics of individual students. Combining this model with linear-in-means peer effects of the kind found in other studies would be sufficient to generate the pattern of results in our study. For example, suppose that many poor minority males are likely to be "burnouts" in a wide variety of possible school environments, so they are inframarginal in the model of Akerlof and Kranton (2002). Yet non-poor minority males are more likely to be on the margin of social groups, and thus may identify with the "nerds" when they attend mostly minority schools and the "burnouts" when they attend mostly non-minority schools. Cicala, Fryer and Spenkuch (2011) develop a model of social interactions based on comparative advantage in the market for peers. The general implication of their model is that multiple equilibria may exist across social settings and the impact of peers can be non-monotonic, a finding that is borne out in a recent study of peer effects at the US Air Force Academy (Carrell, Sacerdote and West 2012). However, given our lack of direct evidence on the nature of peer interactions, our discussion on this issue is naturally speculative.

VII. Conclusion

Few would argue today with the basic argument laid out in *Brown v. Board of Education* that state-enforced segregation through "separate but equal" is unconstitutional and inequitable. Yet the remedy authorized later by *Swann v. Mecklenburg County Schools* of forced busing proved controversial and difficult to enforce (Armor and Rossell 2002). The end of court-ordered school desegregation has led to concerns that subsequent resegregation of schools will cause blacks to give back some of the gains made in the 1960s and 1970s (Mickelson 2003).

We find that the resegregation of CMS schools led to an increase in racial inequality. Our estimates imply that re-zoning in CMS widened the racial gap in math scores by about 0.04 standard deviations, with larger gaps for students who lived in segregated neighborhoods. Similarly, we find that poor minority males were arrested more often and spent more days incarcerated when they attended schools with more minority students. While there is some evidence of smaller offsetting reductions in crime among other students, the net results was to further widen racial gaps in criminal involvement. We find no impact on racial gaps in educational attainment, primarily because both minority and non-minority students appear more likely to attend a four-year college when their neighborhood school becomes more segregated.

The pattern of results is most consistent with a peer effects story, where the reshuffling of students across schools led to changes in peer interactions that had different impacts for different types of students. This suggests the need for further study of the role of social groups and identity in determining outcomes for high school students. We find little evidence that our results are explained by the disruption from reassignment, exit from CMS and into other schools, or changes in school resources or neighborhood racial composition. Sorting of teachers across schools, as in Jackson (2009), could explain at most a very small share of the test score impacts.

Finally, it is worth noting that achievement among both minority and non-minority students was increasing overall in CMS during this period (Vigdor 2011). Our results show that racial gaps are larger than they otherwise would have been if the court order was still in place. Yet CMS implemented a number of innovative policy changes over the last decade, including the development of an intensive monitoring and support program for low-performing schools and a district-wide pay-for-performance program. In 2011, CMS won the Broad Prize for Urban Education. Taken together, CMS's efforts to ease the transition from busing to neighborhood schools and to improve the achievement of minority children suggest that our results could be a lower bound on the impact of school resegregation in other settings.

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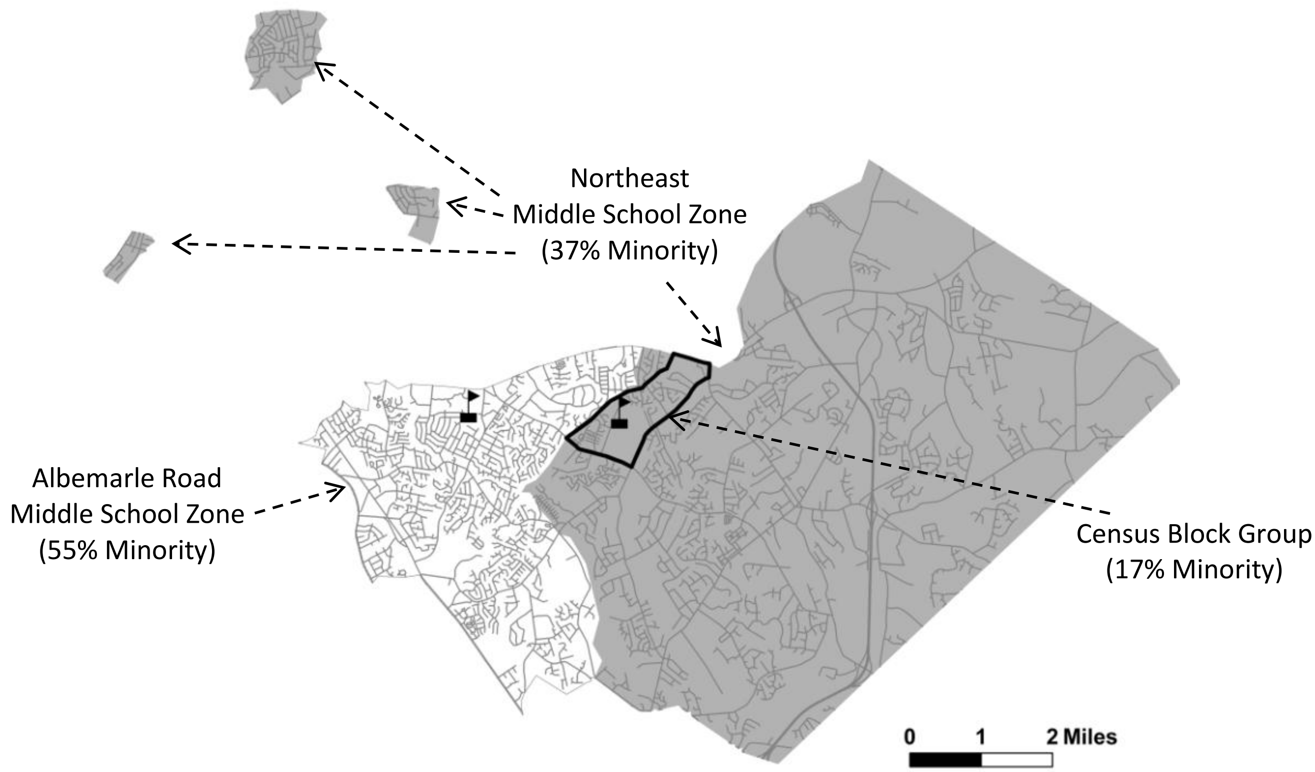
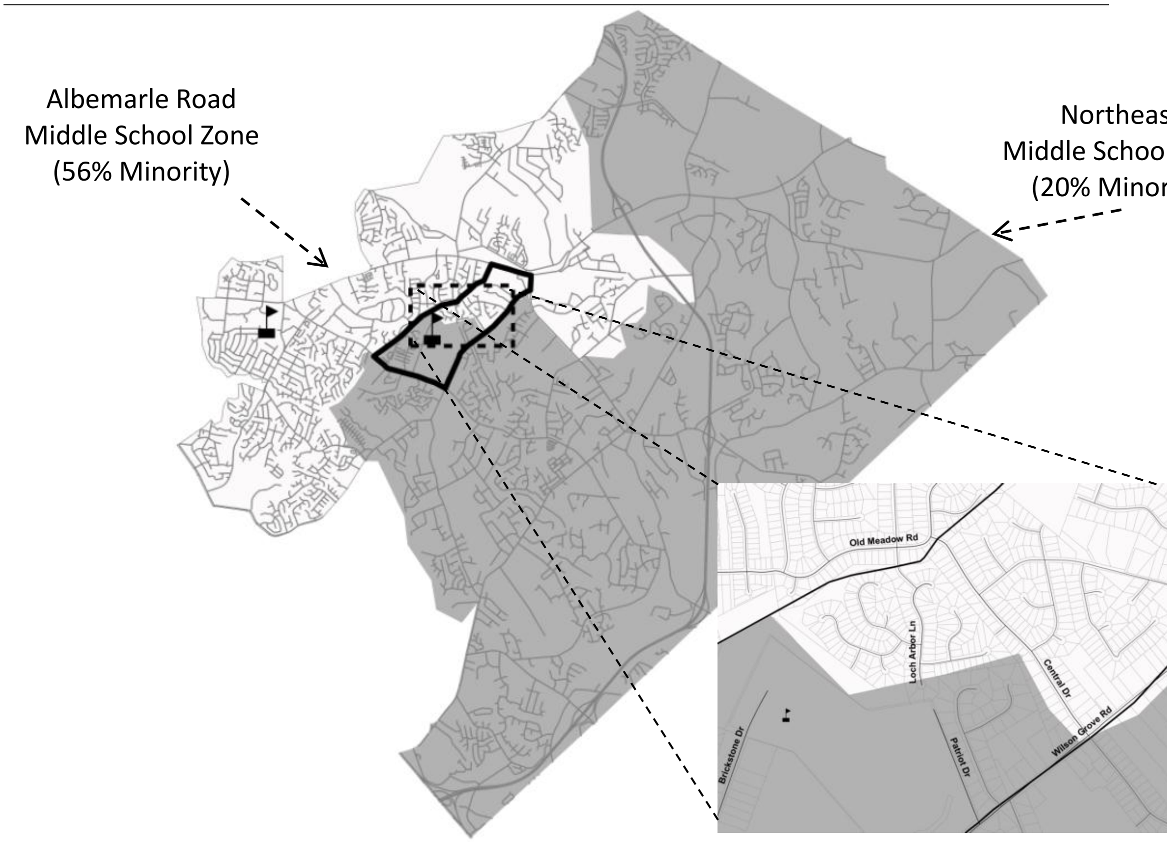
Figure 1 –Re-Zoning for Two Middle Schools

Before (2001-2002)

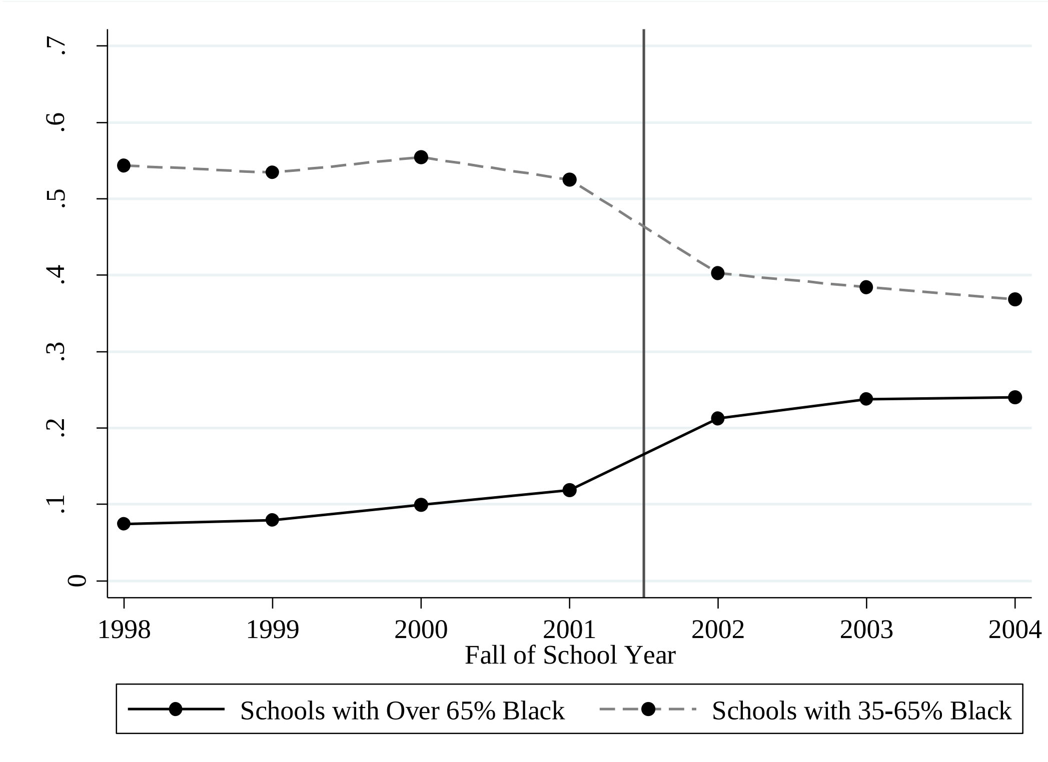


After (2002-2003)



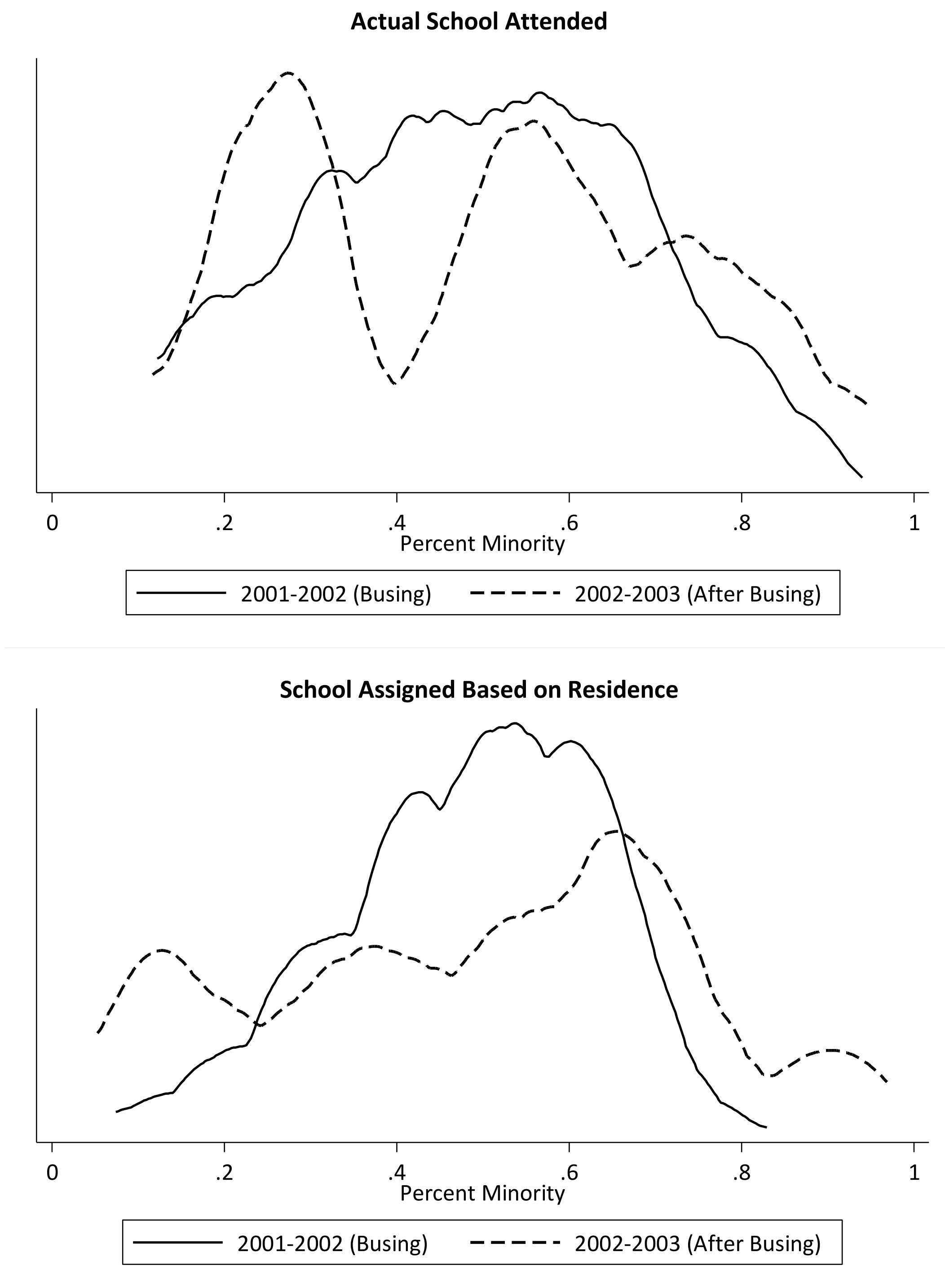
New Zone Crossing CBG

Figure 2: Impact of the 2002 Re-zoning on the Concentration of Black Students



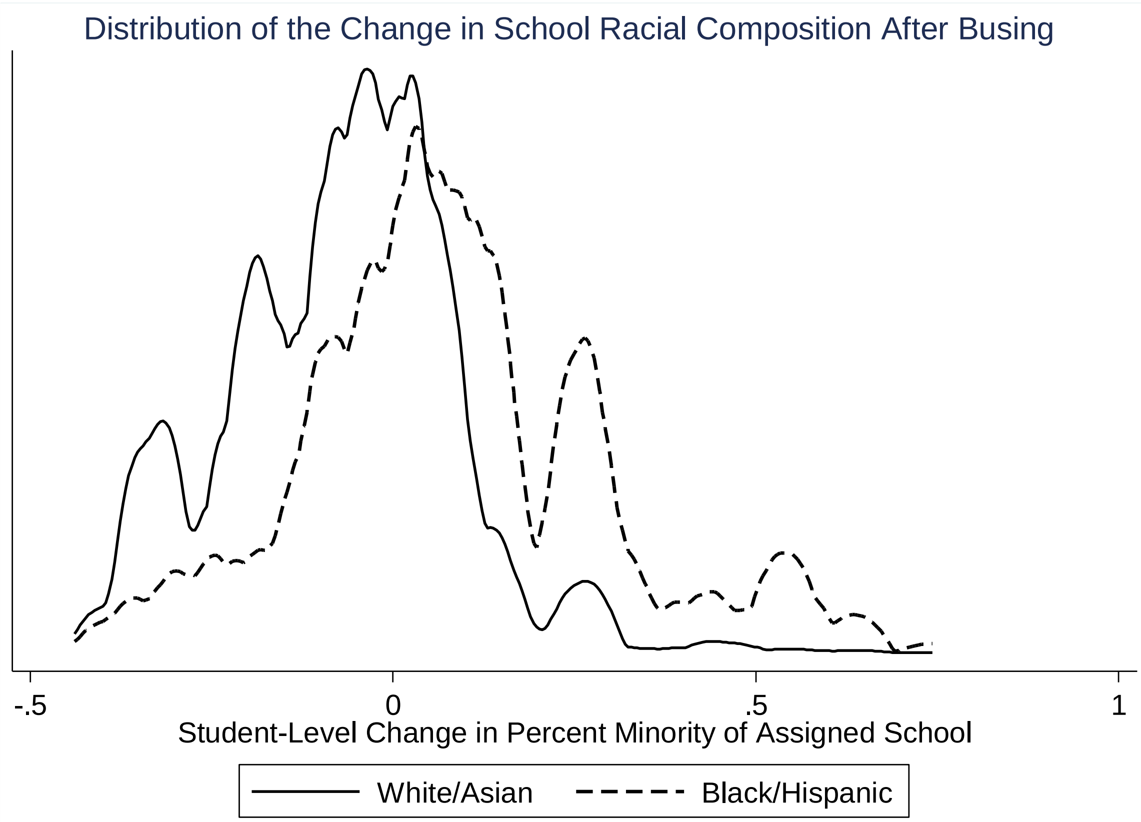
Source: NCES Common Core of Data

Figure 3: Density of Middle/High School Racial Composition (Enrollment Weighted)



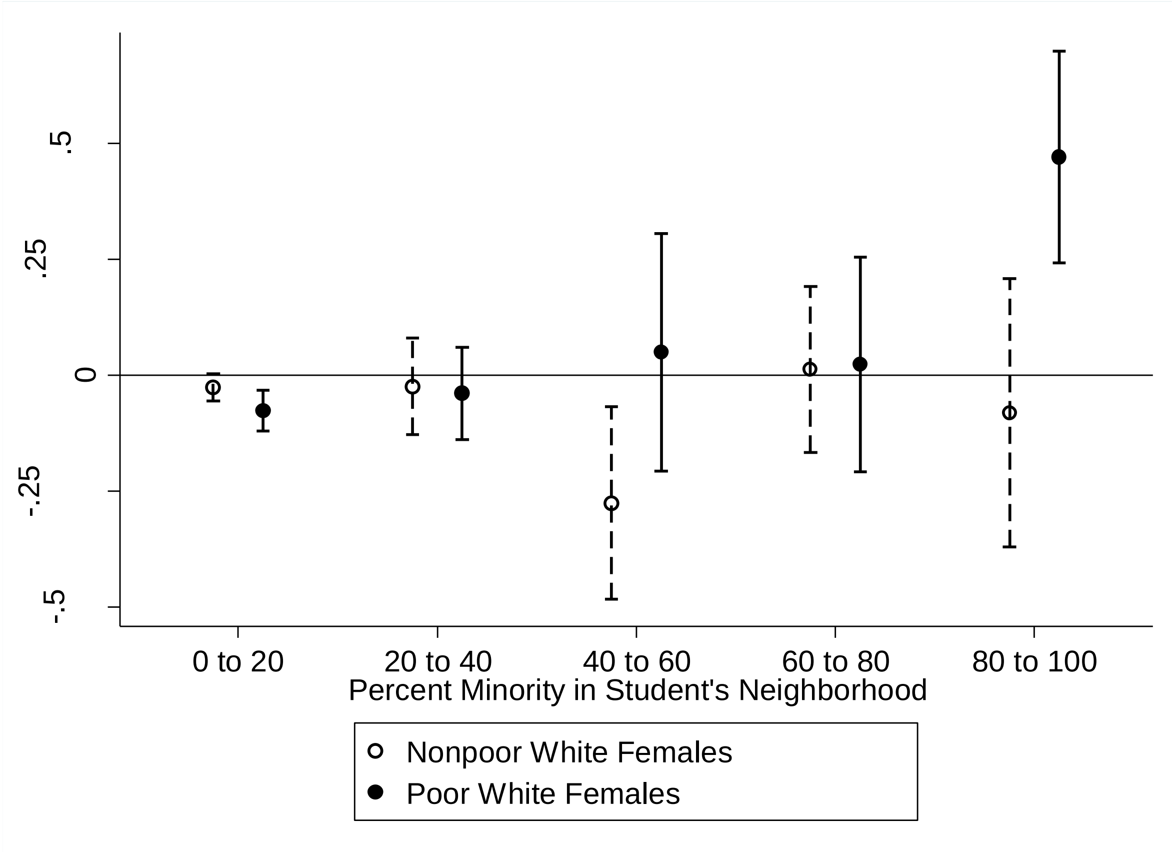
Notes: The top panel shows kernel density plots of the distribution of the racial composition of the schools attended by students in the sample in the years immediately before and immediately after the re-zoning. The bottom panel shows the same thing, except for assigned school. Differences in assigned and actual school occur because of magnet schools, schools for children with special needs, and the Family Choice Plan that was implemented in the 2002-2003 year.

Figure 4



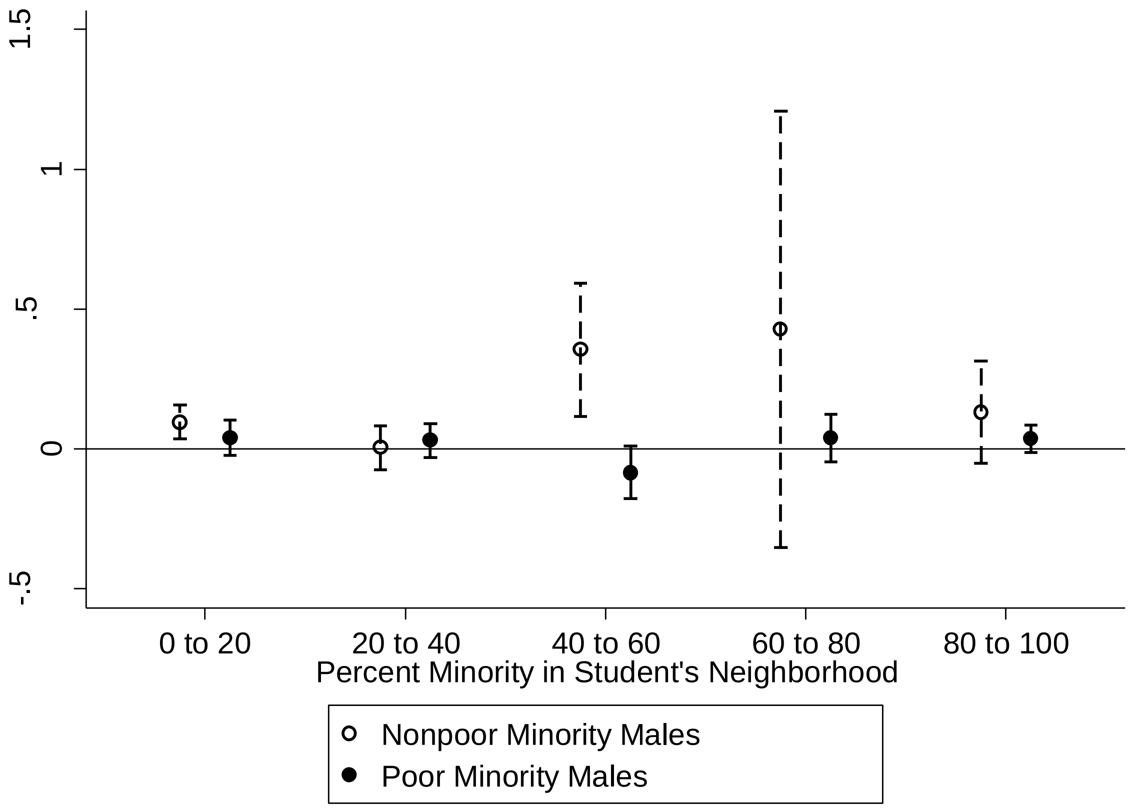
Notes: This figure plots the student-level change in the racial composition of the assigned school before and after the re-zoning, separately by race. This is equivalent to the short-run IV used in Tables 2 through 4 and estimated in equation 2. See the text for details. The mean of the IV is -0.07 for non-minorities and +0.08 for minorities, with standard deviations of 0.15 and 0.21 respectively.

Figure 5 – Heterogeneous Impacts of Re-Zoning on the Four-Year College Attendance Rates of White Females



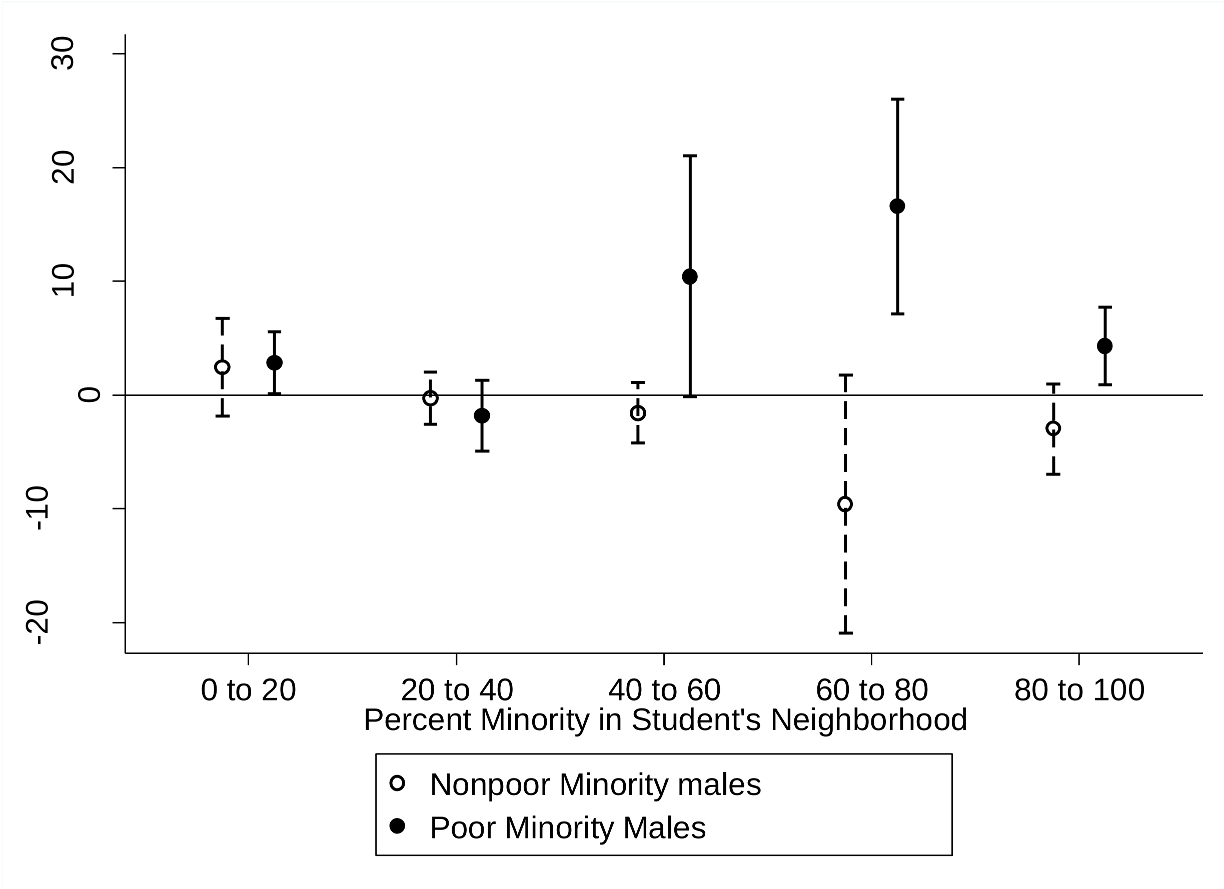
Notes: Figure 5 shows estimates and 90 percent confidence intervals for the results of the 2SLS model in equation 3, with the long-run IV interacted with indicators for students' race, gender, income and 5 quintiles of neighborhood percent minority based on 2000 Census block groups. See the text for more details. We only report results for the indicated groups but the regression model includes all 40 race-gender-income-neighborhood race interactions.

Figure 6 – Heterogeneous Impacts of Re-Zoning on the Four-Year College Attendance Rates of Minority Males



Notes: Figure 6 shows estimates and 90 percent confidence intervals for the results of the 2SLS model in equation 3, with the long-run IV interacted with indicators for students' race, gender, income and 5 quintiles of neighborhood percent minority based on 2000 Census block groups. See the text for more details. We only report results for the indicated groups but the regression model includes all 40 race-gender-income-neighborhood race interactions.

Figure 7 – Heterogeneous Impacts of Re-Zoning on the Total Days Incarcerated of Minority Males



Notes: Figure 5 shows estimates and 90 percent confidence intervals for the results of the 2SLS model in equation 3, with the long-run IV interacted with indicators for students' race, gender, income and 5 quintiles of neighborhood percent minority based on 2000 Census block groups. See the text for more details. We only report results for the indicated groups but the regression model includes all 40 race-gender-income-neighborhood race interactions.

Table 1: Sample Descriptive Statistics

	Full Sample	CBG Percent Minority <20%	20%-66%	>66%
Sample Size	57,682	22,236	21,254	14,192
Black	43%	10%	48%	86%
Hispanic	5%	3%	7%	5%
Free/Reduced Lunch	49%	16%	57%	89%
5th Grade Math	-0.014	0.510	-0.147	-0.677
5th Grade Reading	-0.040	0.479	-0.168	-0.711
Reassigned	50%	34%	48%	79%
Any College	46%	63%	42%	26%
4 Year College	35%	51%	30%	18%
Ever Arrested	7%	3%	7%	14%
# Arrests	0.12	0.04	0.11	0.24
Ever Incarcerated	5%	2%	5%	11%
Days Incarcerated	1.94	0.46	1.61	4.76

Notes: These descriptive statistics are for first-time, rising 6th grade students in CMS between fall 1996 and fall 2002 for whom we possess valid address data (~95% of enrolled students in these cohorts). Student eligibility to receive free or reduced price lunch is an indicator of poverty. 5th grade math and reading scores are in standard deviation units and are normed at the state-year level. Reassignment is an indicator for whether a student was assigned to a new school in the Fall of 2002, relative to the previous year. College outcomes are measured using any attendance within the 18 month period after the student would have graduated on-time from high school. Criminal outcomes are measured between the ages of 18.5 and 19.5 years. CBG Percent Minority reflects percentage of residents who are Black or Hispanic in the 2000 Census block groups in which student addresses were located.

Table 2: Does the Re-zoning Instrument Predict Student Observables?

	Short-Run IV (Δ_i)			Long-Run IV (Δ_i*T_c)		
Black	0.777***	-0.055	-0.000	0.178***	-0.016**	0.001
	[0.060]	[0.034]	[0.023]	[0.016]	[0.007]	[0.006]
Hispanic	-0.012	-0.014	-0.015	-0.002	0.001	0.000
	[0.010]	[0.014]	[0.017]	[0.003]	[0.003]	[0.004]
Free/Reduced Lunch	0.721***	-0.041	-0.019	0.167***	-0.008	-0.001
	[0.052]	[0.032]	[0.025]	[0.013]	[0.007]	[0.007]
5th Grade Math Score	-1.185***	-0.011	-0.038	-0.277***	-0.003	-0.009
	[0.088]	[0.058]	[0.044]	[0.023]	[0.015]	[0.011]
5th Grade English Score	-1.182***	-0.025	-0.040	-0.272***	0.001	-0.001
	[0.085]	[0.059]	[0.048]	[0.022]	[0.013]	[0.011]
Local Crime Per Capita	1.270***	-0.093		0.172	-0.120	
	[0.429]	[0.597]		[0.110]	[0.128]	
Prior Zone Fixed Effects		√	√		√	√
CBG Fixed Effects		√			√	
Parcel Group Fixed Effects			√			√

Notes: Each cell shows the coefficient and standard error on the rezoning instrument (short-run or long-run) from a separate regression; variables listed in the first column (e.g., Black, Hispanic) are dependent variables; all regressions include cohort fixed effects. Crime per Capita is the total number of crimes reported divided by the total number of students in the student's micro-neighborhood. Standard errors are clustered at the CBG level in Columns 1,2, 4, 5 and at the Parcel Group Level in Columns 3 and 6. *** p<0.01, ** p<0.05, * p<0.10

Table 3: Do the Re-zoning Instruments Predict Short-Run Attrition?

Panel A: Short Run Instrument	(1)	(2)	(3)	(4)
Short-Run IV (Δ_i) * Non-Minority	-0.033	-0.005	-0.008	-0.003
	[0.023]	[0.004]	[0.025]	[0.015]
Short-Run IV (Δ_i) * Minority	0.016	0.016	0.020	0.016
	[0.020]	[0.013]	[0.020]	[0.013]
CBG Fixed Effects	√			
Parcel Fixed Effects		√	√	√
Limit to Students Enrolled in 2001-2002		√		√
Sample Size	51,275	43,803	51,275	43,803
Panel B: Long Run Instrument	(1)	(2)	(3)	(4)
Long-Run IV (Δ_i*T_c) * Non-Minority	-0.000	-0.004	0.005	-0.004
	[0.005]	[0.004]	[0.006]	[0.004]
Long-Run IV (Δ_i*T_c) * Minority	0.005	0.002	0.006	0.002
	[0.005]	[0.002]	[0.006]	[0.002]
CBG Fixed Effects	√			
Parcel Fixed Effects		√	√	√
Limit to Students Enrolled in 2001-2002		√		√
Sample Size	51,275	43,803	51,275	43,803

Notes: Within each panel, each column shows the results of a separate regression where the dependent variable is an indicator for enrollment in CMS on the 20th day of school in fall 2002 and the independent variables of interest are the interactions of the short-run (Di) or long-run (Di*Tc) rezoning instrument with indicator variables for being a non-minority and being a minority student; all regressions also control for race and cohort fixed effects, fixed effects for middle by high school zones prior to re-zoning, percent minority in previous year's school, quadratics in math and reading scores plus dummies for missing scores. Standard errors are clustered at the CBG level in Column 1 and at the Parcel Group Level in Columns 2 to 4. *** p<0.01, ** p<0.05, * p<0.10

Table 4: First-Stage Impacts of Re-zoning in the Short Run

Panel A: Pooled Sample	Percent Minority	Peer Prior Math Score	Previous School	Magnet School	Other School	Moved 00-01	Moved 01-02	Moved 02-03
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Short-Run IV (Δ_i)	0.307***	-0.438***	0.061*	-0.010	0.018	-0.008	-0.061**	-0.001
	[0.021]	[0.036]	[0.031]	[0.034]	[0.030]	[0.020]	[0.030]	[0.021]

Panel B: Effects by Racial Group	Percent Minority	Peer Prior Math Score	Previous School	Magnet School	Other School	Moved 00-01	Moved 01-02	Moved 02-03
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Short-Run IV (Δ_i) * Non-Minority	0.346***	-0.411***	0.070	0.021	0.044	0.007	-0.028	0.002
	[0.022]	[0.050]	[0.049]	[0.040]	[0.037]	[0.026]	[0.027]	[0.023]
Short-Run IV (Δ_i) * Minority	0.284***	-0.454***	0.055*	-0.028	0.003	-0.017	-0.081**	-0.003
	[0.021]	[0.035]	[0.030]	[0.035]	[0.031]	[0.022]	[0.033]	[0.025]
Sample Size	43,353	41,756	43,353	43,353	43,353	40,724	41,193	39,902

Notes: Within each panel, each column shows the results of a separate regression where the dependent variable listed in the column heading and the independent variables of interest are (in Panel A) the short-run rezoning instrument (Di) and (in Panel B) the interactions of the short-run rezoning instrument (Di) with indicator variables for being a non-minority and being a minority student; all regressions also control for race and cohort fixed effects, fixed effects for middle by high school zones prior to re-zoning, percent minority in previous year's school, quadratics in math and reading scores plus dummies for missing scores, and parcel group fixed effects. Standard errors clustered at the parcel group level. *** p<0.01, ** p<0.05, * p<0.10

Table 5: Impacts of Re-zoning on Middle School Achievement and Behavior

Panel A: Pooled Sample	Math Score	Reading Score	Total Absences	Ever Suspended	Days Suspended
	(1)	(2)	(3)	(4)	(5)
Cumulative School % Minority	-0.164***	-0.058	0.796	0.061	0.952**
	[0.037]	[0.037]	[0.735]	[0.037]	[0.447]

Panel B: Effects by Racial Group	Math Score	Reading Score	Total Absences	Ever Suspended	Days Suspended
	(1)	(2)	(3)	(4)	(5)
Cumulative School % Minority *					
Non-Minority Student	-0.178**	-0.119*	0.420	0.098**	0.084
	[0.075]	[0.066]	[1.110]	[0.046]	[0.441]
Minority Student	-0.161***	0.039	0.922	0.047	1.241**
	[0.039]	[0.042]	[0.936]	[0.044]	[0.582]
Observations	39,642	39,610	40,925	43,819	40,925
R-squared	0.708	0.657	0.146	0.170	0.135

Notes: All regressions include fixed effects for race, cohort, parcel group, middle by high school zones prior to re-zoning, quadratic controls for 5th grade math and reading scores, and indicator variables for missing 5th grade scores. *** p<0.01, ** p<0.05, * p<0.10

Table 6: Impacts of Re-zoning on High School Achievement Test Scores

Panel A: Pooled Sample	English	Algebra I	Geometry	Algebra II
	(1)	(2)	(3)	(4)
Cumulative School % Minority	0.055	-0.050	-0.110***	-0.064
	[0.048]	[0.053]	[0.037]	[0.042]

Panel B: Effects by Racial Group	English	Algebra I	Geometry	Algebra II
	(1)	(2)	(3)	(4)
Cumulative School % Minority *				
Non-Minority Student	0.042	-0.204**	-0.114**	-0.082
	[0.067]	[0.083]	[0.055]	[0.057]
Minority Student	0.058	-0.008	-0.111**	-0.053
	[0.053]	[0.055]	[0.039]	[0.049]
Observations	23,567	21,616	21,712	21,505
R-squared	0.660	0.608	0.634	0.534

Notes: All regressions include fixed effects for race, cohort, parcel group, middle by high school zones prior to re-zoning, quadratic controls for 5th grade math and reading scores, and indicator variables for missing 5th grade scores. *** p<0.01, ** p<0.05, * p<0.10

Table 7: Impacts of Re-zoning on Educational Attainment

Panel A: Pooled Sample	Graduated from CMS	Attend Any College	Attend 4 Year College
	(1)	(2)	(3)
Cumulative School % Minority	-0.022*	0.011	0.004
	[0.013]	[0.015]	[0.011]
Panel B: Effects by Racial Group	Graduated from CMS	Attend Any College	Attend 4 Year College
	(1)	(2)	(3)
Cumulative School % Minority *			
Non-Minority Student	-0.067***	0.000	-0.016
	[0.016]	[0.016]	[0.014]
Minority Student	0.002	0.017	0.014
	[0.017]	[0.019]	[0.014]
Panel C: Effects by Racial Group and Gender	Graduated from CMS	Attend Any College	Attend 4 Year College
	(1)	(2)	(3)
Cumulative School % Minority *			
Non-Minority Female	-0.083***	0.005	-0.035*
	[0.018]	[0.019]	[0.019]
Non-Minority Male	-0.051***	-0.005	0.003
	[0.017]	[0.017]	[0.016]
Minority Female	0.019	0.007	-0.002
	[0.016]	[0.021]	[0.016]
Minority Male	-0.017	0.027	0.036**
	[0.019]	[0.019]	[0.015]
Observations	47,668	47,668	47,668
R-squared	0.201	0.285	0.312

Notes: All regressions include fixed effects for race-gender cells, cohort, parcel group, middle by high school zones prior to re-zoning, quadratic controls for 5th grade math and reading scores, and indicator variables for missing 5th grade scores. *** p<0.01, ** p<0.05, * p<0.10

Table 8: Impacts of Re-zoning on Criminal Behavior in Early Adulthood

Panel A: Pooled Sample	Arrested	# Arrests	Incarcerated	Total Days
	(1)	(2)	(3)	(4)
Cumulative School % Minority	0.007	0.020	0.007	0.650*
	[0.007]	[0.013]	[0.007]	[0.387]

Panel B: Effects by Racial Group	Arrested	# Arrests	Incarcerated	Total Days
	(1)	(2)	(3)	(4)
Cumulative School % Minority *				
Non-Minority Student	-0.007	-0.012	-0.005	-0.246
	[0.007]	[0.012]	[0.007]	[0.318]
Minority Student	0.014	0.036*	0.014	1.107**
	[0.009]	[0.019]	[0.009]	[0.513]

Panel C: Effects by Racial Group / Gender	Arrested	# Arrests	Incarcerated	Total Days
	(1)	(2)	(3)	(4)
Cumulative School % Minority *				
Non-Minority Female	-0.016**	-0.023**	-0.012	-0.346
	[0.007]	[0.011]	[0.007]	[0.294]
Non-Minority Male	0.002	0.000	0.002	-0.047
	[0.009]	[0.016]	[0.009]	[0.403]
Minority Female	-0.014	-0.028	-0.014	-1.547***
	[0.012]	[0.021]	[0.009]	[0.439]
Minority Male	0.044***	0.109***	0.049***	4.146***
	[0.012]	[0.029]	[0.012]	[0.927]
Observations	47,668	47,668	47,668	47,668
R-squared	0.098	0.087	0.101	0.053

Notes: All regressions include fixed effects for race-gender cells, cohort, parcel group, middle by high school zones prior to re-zoning, quadratic controls for 5th grade math and reading scores, and indicator variables for missing 5th grade scores. *** p<0.01, ** p<0.05, * p<0.10

Table 9: Distinguishing Impacts of Re-zoning on Poor and Non-Poor Minority Males

	Graduated from CMS	Attend Any College	Attend 4 Year College	Arrested	# Arrests	Incarcerated	Total Days
Previous Results in Tables 7 and 8:	(1)	(2)	(3)	(4)	(5)	(6)	(7)
*Cumulative School % Minority **							
Minority Male	*-0.017*	*0.027*	*0.036***	*0.044****	*0.109****	*0.049****	*4.146****
	[0.019]	*[0.019]*	*[0.015]*	*[0.012]*	*[0.029]*	*[0.012]*	*[0.927]*
Panel A: Effects by Free Lunch Status	Graduated from CMS	Attend Any College	Attend 4 Year College	Arrested	# Arrests	Incarcerated	Total Days
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
*Cumulative School % Minority **							
Nonpoor Minority Male	0.021	0.071**	0.085***	0.003	0.024	0.014	0.560
	[0.035]	[0.031]	[0.031]	[0.014]	[0.026]	[0.012]	[0.962]
Poor Minority Male	-0.026	0.018	0.028*	0.052***	0.127***	0.054***	4.866***
	[0.021]	[0.018]	[0.016]	[0.016]	[0.034]	[0.016]	[1.080]
Panel B: Effects by Free Lunch Status and School Assignment	Graduated from CMS	Attend Any College	Attend 4 Year College	Arrested	# Arrests	Incarcerated	Total Days
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Cumulative School % Minority *							
Nonpoor Minority Male * Same School	0.028	0.078*	0.072*	-0.004	0.001	0.000	-1.653
	[0.037]	[0.044]	[0.039]	[0.021]	[0.027]	[0.019]	[1.394]
Nonpoor Minority Male * New School	0.017	0.072*	0.097**	0.008	0.036	0.017	1.526
	[0.044]	[0.040]	[0.042]	[0.015]	[0.034]	[0.013]	[1.176]
Poor Minority Male * Same School	-0.075	0.009	0.024	0.062**	0.142***	0.064**	4.137**
	[0.053]	[0.042]	[0.031]	[0.026]	[0.058]	[0.024]	[1.962]
Poor Minority Male * New School	-0.008	0.027	0.038**	0.044***	0.117***	0.046***	4.396***
	[0.025]	[0.022]	[0.016]	[0.016]	[0.038]	[0.016]	[1.251]
Observations	47,668	47,668	47,668	47,668	47,668	47,668	47,668
R-squared	0.202	0.286	0.313	0.101	0.090	0.103	0.055

Notes: All regressions include fixed effects for race-gender-income cells, cohort, parcel group, middle by high school zones prior to re-zoning, quadratic controls for 5th grade math and reading scores, and indicator variables for missing 5th grade scores. *** p<0.01, ** p<0.05, * p<0.10

Appendix Table A1: Re-zoning and Student Observables by Ethnicity

Panel A: Short-Run IV (Δ_i)	5th Grade Math Score		5th Grade Reading Score		Parcel Crime Per Capita
Short-Run IV (Δ_i) * Non-Minority	-0.113*	-0.031	-0.094	-0.021	0.616
	[0.067]	[0.058]	[0.064]	[0.058]	[0.624]
Short-Run IV (Δ_i) * Minority	-0.013	-0.069	-0.041	-0.077	-0.631
	[0.054]	[0.045]	[0.059]	[0.050]	[0.826]
CBG Fixed Effects	√		√		√
Parcel Group Fixed Effects		√		√	

*Panel B: Long-Run IV (Δ_i *T_c)*	5th Grade Math Score		5th Grade Reading Score		Parcel Crime Per Capita
Long-Run IV (Δ_i*T_c) * Non-Minority	-0.041**	-0.017	-0.033**	-0.013	0.027
	[0.017]	[0.015]	[0.015]	[0.015]	[0.165]
Long-Run IV (Δ_i*T_c) * Minority	-0.005	-0.011	-0.011	-0.011	-0.200
	[0.014]	[0.012]	[0.014]	[0.011]	[0.166]
CBG Fixed Effects	√		√		√
Parcel Group Fixed Effects		√		√	

Notes: Within each panel, each column shows the coefficients and standard errors on interactions of the rezoning instrument (short-run or long-run) with student ethnicity; variables listed in the column headers are dependent variables; all regressions include race by cohort fixed effects and fixed effects for students' middle by high school zones prior to re-zoning. *** p<0.01, ** p<0.05, * p<0.10

Appendix Table A2: Reduced-Form Effects on Grade 8 Outcomes by Pre-Post Cohort

	Math Score	English Score	Total Absences	Ever Suspended	Days Suspended
	(1)	(2)	(3)	(4)	(5)
Interaction of Δ_i*T_c and Non-Minority with...					
...Grade 8 Prior to Fall 2002 (Placebo)	0.014	-0.050	0.201	-0.010	0.163
	[0.081]	[0.081]	[0.883]	[0.030]	[0.365]
...Grade 8 in Fall 2002	-0.028	-0.012	-0.215	0.063	0.704
	[0.127]	[0.106]	[1.354]	[0.043]	[0.450]
...Grade 8 in Fall 2003	-0.114	-0.134	-1.389	-0.029	0.381
	[0.134]	[0.138]	[1.444]	[0.042]	[0.496]
...Grade 8 in Fall 2004	-0.105	-0.059	0.678	0.058	0.639
	[0.125]	[0.121]	[1.436]	[0.047]	[0.528]
Interaction of Δ_i*T_c and Minority with...					
...Grade 8 Prior to Fall 2002 (Placebo)	-0.049	-0.134	-0.447	0.006	0.415
	[0.071]	[0.082]	[1.044]	[0.035]	[0.485]
...Grade 8 in Fall 2002	-0.156**	-0.029	3.357***	0.102**	2.935***
	[0.068]	[0.083]	[1.247]	[0.043]	[0.708]
...Grade 8 in Fall 2003	-0.132*	-0.107	1.053	0.076*	1.748*
	[0.069]	[0.083]	[1.191]	[0.039]	[0.925]
...Grade 8 in Fall 2004	-0.261***	-0.113	1.961*	0.129***	2.238***
	[0.070]	[0.076]	[1.125]	[0.041]	[0.670]
Observations	42,779	42,750	44,300	44,300	44,300
R-squared	0.396	0.375	0.157	0.183	0.143

Notes: All regressions include fixed effects for race, cohort, prior school zone, and parcel group. Students are considered to enter grade 8 two years after entering grade 6 in CMS for the first time. *** p<0.01, ** p<0.05, * p<0.10

Appendix Table A3: Robustness Checks on HS Test Score Impacts Using Imputation

	English I				Algebra I			
	Actual Score	Predicted - 0.5	Predicted Score	Predicted + 0.5	*Actual Score*	Predicted - 0.5	Predicted Score	Predicted + 0.5
Cumulative School % Minority *								
Non-Minority Student	*0.042*	-0.023	0.035	0.092	*-0.204***	-0.226***	-0.186***	-0.148**
	[0.067]	[0.074]	[0.050]	[0.060]	*[0.083]*	[0.074]	[0.070]	[0.067]
Minority Student	*0.058*	0.071	0.041	0.010	*-0.008*	0.008	-0.021	-0.049
	[0.053]	[0.046]	[0.046]	[0.051]	*[0.055]*	[0.047]	[0.044]	[0.047]
Observations	*23,567*	27,314	27,314	27,314	*21,616*	24,878	24,878	24,878
	Geometry				**Algebra II**			
	Actual Score	Predicted - 0.5	Predicted Score	Predicted + 0.5	*Actual Score*	Predicted - 0.5	Predicted Score	Predicted + 0.5
Cumulative School % Minority *								
Non-Minority Student	*-0.114***	-0.164***	-0.117***	-0.070*	*-0.082*	-0.096***	-0.070**	-0.045
	[0.055]	[0.043]	[0.039]	[0.039]	*[0.057]*	[0.035]	[0.033]	[0.033]
Minority Student	*-0.111***	-0.084***	-0.086***	-0.091***	*-0.053*	-0.033	-0.038	-0.046*
	[0.039]	[0.030]	[0.027]	[0.029]	*[0.049]*	[0.026]	[0.026]	[0.028]
Observations	*21,712*	29,848	29,848	29,848	*21,505*	34,300	34,300	34,300

Notes: All regressions include fixed effects for racial group, cohort, parcel group, middle by high school zones prior to re-zoning, quadratic controls for 5th grade math and reading scores, and indicator variables for missing 5th grade scores. Columns labeled "Actual Score" display results from Table 6; Columns labeled "Predicted" are based on samples where we impute scores for students with missing test scores using a bivariate regression of high school test scores on 5th grade test scores in the same subject (i.e., English or math). We use either the predicted score itself, or the predicted score plus or minus 0.5 standard deviations. *** p<0.01, ** p<0.05, * p<0.10