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SCHOOL SEGREGATION, EDUCATIONAL ATTAINMENT AND CRIME: EVIDENCE FROM THE END OF BUSING IN CHARLOTTE-MECKLENBURG

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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 half of its students received a new assignment. Using addresses measured prior to the policy change, we compare students in the same neighborhood that lived on opposite sides of a newly drawn boundary. We find that both white and minority students score lower on high school exams when they are assigned to schools with more minority students. We also find decreases in high school graduation and four-year college attendance for whites, and large increases in crime for minority males. The impacts on achievement and attainment are smaller in younger cohorts, while the impact on crime remains large and persistent for at least nine years after the re-zoning. We show that compensatory resource allocation policies in CMS likely played an important role in mitigating the impact of segregation on achievement and attainment, but had no impact on crime. We conclude that the end of busing widened racial inequality, despite efforts by CMS to mitigate the impact of increases in segregation.

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I. Introduction

Since the landmark 1954 Supreme Court decision *Brown v. Board of Education,* schools have been seen by courts and policymakers 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 Charlotte-Mecklenburg schools (henceforth "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 CMS on students' achievement test scores, educational attainment, and criminal activity. 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 are done using full name and date of birth, enabling 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. Furthermore, we use students' addresses measured prior to the policy change to fix their location. This 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.

Our identification strategy compares students who lived in the same neighborhoods, but whose pre-policy addresses placed them on opposite sides of a newly drawn school boundary. We estimate the differential impact, within small geographic areas, of being *assigned* to a school with more minority students. We then examine variation in the impacts across student characteristics, grade cohorts and baseline neighborhood racial composition.[1] The main threat to identification is differential neighborhood trends that are correlated with the location of the boundaries. We show that while minority and low income students were more likely to be assigned to segregated schools overall, there is no evidence of sorting within neighborhoods across a newly drawn boundary. We also examine the impact of the boundary change on students in older grade cohorts, who should not have been exposed to the policy change. We find no evidence of pre-policy trends in school racial composition.

Our results show that the resegregation of CMS schools widened inequality of outcomes between whites and minorities. We estimate that all students, white and black, score lower on high school exams when they attend schools with more minority students. We find that a 10 percentage point increase in the share of minorities in a student's assigned school decreases high school test scores by about 0.014 standard deviations, on the low end of results from other studies of the effect of peer racial composition (e.g. Hoxby 2000, Hanushek, Rivkin, and Kain 2009, Vigdor and Ludwig 2008). Since the net impact of re-zoning was that students attended schools with a greater share of peers of their own race, our estimates imply a widening of the racial gap in test scores of about 0.04 standard deviations. We also find that white students are less likely to graduate from high school or attend a four-year college when they are assigned to schools with more minority students. Given the increase in

[1] Jackson (2009) studies teacher sorting in CMS in response to the re-zoning and constructs an instrument for changes in school racial composition using the racial composition of the surrounding neighborhood. Unlike Jackson (2009), we have address data, which allow us to measure individual-level changes in school racial composition. However, we do not pursue an instrumental variables strategy, instead estimating the reduced form impact of school assignment. We discuss the reasons for this decision in Section IV.

same-race segregation, this implies that white students had higher graduation and attendance rates after the policy change.

The re-zoning of CMS schools led to statistically significant and large increases in crime among minority males. Our estimates suggest that a 10 percentage point increase in assigned school share minority led to an increase among minority males in the probability of ever being arrested and ever being incarcerated of about 1.3 percentage points, about a 7 percent increase relative to the mean for minority males in the sample. The increase in crime is similar in magnitude across grade cohorts and persists through the end of our data in 2011, nine years after the re-zoning. We allow the impact of being rezoned to vary by both race and income, and we find that the increases in crime are driven entirely by poor minority males who are assigned to schools with higher shares of poor minority students. Additionally, the impacts on crime are concentrated among residents of neighborhoods that are more than 60 percent nonwhite. Overall, the impacts on crime align with several other studies of nonlinear peer effects, which find that grouping high-risk youth together increases the aggregate level of misbehavior and/or crime (Cook and Ludwig 2005, Carrell and Hoekstra 2010, Deming 2011, Imberman, Kugler and Sacerdote 2012).

We investigate several possible explanations for the results. First, there is little 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 find similar impacts for rising 6th and 9th grade cohorts who would have attended a new school anyway, suggesting that the results are not driven by additional school disruptions. We also test for the possibility that the results are caused by a drop in bus transit time by dropping the small subset of students who attended non-contiguous or “satellite” school zones prior to re-zoning, and find that this exclusion has no impact on our results.

We show that while the impacts on crime are persistent, the overall widening of racial inequality in academic achievement and educational attainment is driven by older students who were entering high school in the fall of 2002. We find little or no impact of re-zoning on test scores or college attendance among the younger middle school cohorts, despite the fact that these students also spent all of high school in segregated schools. We present evidence that this pattern of results is due to compensatory resource allocation by CMS, which implemented a series of programs beginning in 2006 that provided additional resources and accountability for high poverty high schools. Consistent with this story, we find that students in the high school cohorts took fewer honors and advanced placement (AP) courses when they were assigned to schools with more minority students, but that the pattern reverses for the middle school cohorts. Overall, the evidence suggests that increases in per-pupil spending and improvements in teacher staffing policies and governance may have played an important role in mitigating the impact of segregation on racial and economic inequality in test scores and attainment.

Do segregated schools inevitably lead to racial and socioeconomic inequality? Or can the consequences of school segregation be mitigated by compensatory allocation of resources? This is a critical question for education policy. Beginning in the early 1990s, school districts across the U.S. have been gradually released from court ordered desegregation plans, and school segregation has increased as a result (Lutz 2011, Reardon et al 2012). Black-white disparities in school resources were still sizable at midcentury, and thus school desegregation in the 1960s and 1970s served the dual purpose of integrating schools and narrowing racial gaps in school spending (Margo 1990, Card and Krueger 1992, Reber 2010, Cascio, Gordon, Lewis and Reber 2010). Today, as a result of Federal programs such as Title I and a variety of state and local initiatives, racial and socioeconomic gaps in school spending have narrowed considerably and in some cases reversed. Moreover, many of the highest-performing charter schools are racially segregated (e.g. Abdulkadiroglu et al 2011, Dobbie and Fryer 2011, Angrist, Pathak and Walters 2011). Our results suggest that equal or greater resources combined with active policy efforts may be able to reduce the impact of school segregation on academic outcomes, but not for crime. To the extent that crime is driven by social context and peer interactions, it will be difficult for schools to address racial and economic inequality through means other than deliberately integrative student assignment policies.

II. Background

The landmark Supreme Court decision *Brown vs. Board of Education* in 1954 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

[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.

that bused students from inner city, highly African-American, 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, and Lutz, 2011). This led to 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 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 2011, 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 for 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

[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.

[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.

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.[6]

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.

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.[7] 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.

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[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."

[6] We employ multiple definitions of neighborhood in our analysis below, including tax parcel groups, which were also not often coterminous with school boundaries and which are considerably smaller than census block groups.

[7] Exams in subjects such as Chemistry and US History are available but were only administered in some years. 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.

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. [8] 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. While these data allow us to observe future criminal behavior of CMS students, regardless of whether transfer or drop, they are limited to crimes committed within Mecklenburg County.

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. This means that we can still track attendance at colleges covered by the NSC data for students who transfer to other districts or private schools, or who drop out of CMS altogether.

We limit our analysis sample to the seven cohorts of students who were rising first-time 6^{th} grade students in the fall semesters of 1996 through 2002. Students who enter CMS after the change in boundaries are not included in the sample. Those who attended 6^{th} grade in the fall of 1996 and progressed through school at the normal rate of one grade per year would enter 12^{th} grade in the fall of 2002, and thus would have had only one year of exposure to the change in school boundaries. In contrast, students who attended 6^{th} grade in the fall of 2002 spent all of their middle and high school years in the post-busing regime. While these seven cohorts span the range of "treated" students, we also examine the impact of re-zoning on earlier cohorts in a set of specification checks.

If the youngest cohort of "treated" 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

[8] 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.

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-neighborhoods," which are based on similar real estate parcels. These two non-coterminous geographies allow us, for example, to see if micro-neighborhood characteristics are systematically different across newly drawn boundaries within the same Census Block Group.

We use address records from the spring of each school year to assign students to 2000 census geographies, micro-neighborhoods (based on tax parcel), and middle and high school zones for 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. Instead, we assign every student to pre- and post-2002 school zones based on their earliest listed address, which is observed in spring 1996 in most cases. We omit a small number of students whose first address is recorded in spring 2002, after the boundary change was announced (but before fall 2002, when the new boundaries applied). This approach minimizes the possibility that sorting will bias our estimates, but it also increases measurement error because some families will have moved to other areas by 2002. We also 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.[9] 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. While our preferred estimates use the earliest address, the results are very similar regardless of which alternative we choose.

Of our initial sample of 54,093 students, just over five percent have missing or invalid address information, which leaves us with 51,020 students.[10] Table 1 lists descriptive statistics for our sample. Overall, 44 percent of students are black, 5 percent are Hispanic, and just over half of all students come

[9] 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.

[10] While we found no systematic pattern in invalid addresses by school attended prior to busing, information was more likely to be missing in the 2002 and 2003 cohorts, which is to be expected given our sample restrictions and normal inflow and outflow of students every year. Address information was missing or invalid for about 10 percent of the latest two cohorts. Using 2002 and 2003 addresses to fill in missing data has no impact on the main results.

from poor households (i.e., receiving free or reduced price lunch). Fifth grade test scores were slightly higher than the state average in math and reading (0.01 standard deviations). Overall, 52 percent of students were assigned to a new school as a result of the 2002 change in school zone boundaries.

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 (8%), Hispanic (2%), or poor (14%), 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. Finally, Table 1 shows that the probability of being reassigned was significant across all neighborhoods, but that students living in high minority neighborhoods were much more likely to be reassigned (81%) relative to those in low minority neighborhoods (34%).

Underlying the summary statistics in Table 1 are a few 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 40 percent to about 52 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). Fifth grade math and reading scores in CMS rose from slightly below to slightly above the state average.

IV. Empirical Strategy

Our strategy uses student addresses measured prior to the policy change to generate quasi-experimental variation in exposure to schools of varying racial composition. 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 earliest known address (bottom panel). Prior to re-zoning, the vast majority of students were assigned 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 alternative schools, the *actual* distribution of school racial composition was noticeably more disperse than the *assigned* distribution both before and after re-zoning. As Figure 3 shows, segregation increased markedly in CMS schools in the 2002-2003 school year.

The re-zoning of CMS schools in 2002 meant that students who lived in the same neighborhoods but on opposite sides of a newly drawn boundary could be assigned to schools of very different racial compositions. In an extreme case, students living on opposite sides of a street could be assigned to different schools. While the broad trend of increasing school segregation was predictable based on the court order, within small enough neighborhoods it is unlikely that families would relocate to the "preferred" side of a future boundary, or that they would anticipate the location of the boundary many years in advance. Our empirical approach formalizes this intuition by regressing outcomes of interest on the percent minority in a student's new school zone (based on the pre-policy change address) while controlling for old school zone by neighborhood fixed effects. We estimate:

$$Y_{izjc} = \beta_0 PctMinority_{izjc} + \beta_1 X_{izjc} + \eta_{zj} + \gamma_c + \varepsilon_{izjc} \tag{1}$$

where outcome *Y* for a student i living in old school zone *z*, neighborhood j and grade cohort c, is regressed on the student's new school-zone percent minority ($PctMinority_{izjc}$), a set of covariates that includes race, gender, free lunch status and a 2nd order polynomial in 5th grade math and reading scores, cohort fixed effects based on the first year a student enters the 6th grade (γ_c), and old school zone by neighborhood fixed effects (η_{zj}). We also interact the cohort fixed effects with indicators for eight demographic groups (race by gender by free lunch status) in some specifications to account for time trends in the overall demographic composition of CMS.

We define neighborhoods in two ways. 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. Even with these small spatial definitions of neighborhood, 56 percent of micro-neighborhoods in CMS had a new boundary drawn through them. The median number of students per micro-neighborhood across our eight sample cohorts is 142. For increasingly small definitions of a neighborhood, our approach 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. We use parcel groups in our main results, but in general our results are very similar with the two different neighborhood definitions.

The redrawing of CMS school boundaries led to rich variation in the racial composition of students' assigned schools. Figure 4 shows the distribution of the *change* in the percent of minority students between each student's new and old school zone, separately by race. Given the overall increase in school segregation, it is not surprising that the distribution for minority students lies to the right of non-minorities. However, a significant share of non-minority students were re-assigned to schools with *higher* minority enrollment, while a significant share of minority students were re-assigned to schools with *lower* minority enrollment. Moreover, there is considerable variation even within neighborhoods - the mean interquartile range within parcel groups with variation in the change in percent minority is about 10 percentage points for the middle school boundaries and 7 percentage points for high school.

Our main results focus on the reduced form impact of being assigned to a new school. An alternative approach is to use assigned school racial composition as an instrument for actual school racial composition as part of a two-stage least squares (2SLS) procedure. We pursue the reduced form approach for three reasons. First, we must account for differential exposure by grade cohort to the new school zone boundaries, and any choice of scaling involves strong assumptions about the nature of the treatment. For example, we could estimate the impact of cumulative exposure by multiplying students' percent minority in the new school zone by the number of years they were enrolled after the policy change. However, this assumes that the impact accumulates linearly and is the same for all age groups. Second, there is the difficult issue of how to treat students who leave CMS, since we do not know the racial composition of their new school (or if they are attending any school). If students who attend highly segregated schools are more likely to drop out, then a cumulative scaling would not be appropriate. Finally, an instrumental variable like the one described above could easily violate the assumption of monotonicity – for example, if students comply with school assignments for small changes but transfer to private schools for large changes in racial composition.

We present results with all grade cohorts pooled together, as well as separated out by "middle school" and "high school" cohorts. Students who were entering grades 6 through 8 and grades 9-12 in the fall of 2002 are in the "middle school" and "high school" cohorts respectively. One complication is that students in the "middle school" cohorts actually experienced two changes in school assignment – first at the middle school level, and then again in high school. All of our main results use the fall 2002 boundary change, even when we estimate the impact on high school outcomes. While the change in racial composition across middle school and high school boundaries is highly correlated within neighborhoods and across grade cohorts, we explore some specifications where we allow both changes to have independent impacts.

Finally, we investigate the sensitivity of our results to two key assumptions about the nature of school demographic change. First, we explore the joint impact of race and income composition by separating our independent variable of interest (formerly percent minority) into four race by poverty cells (where poverty is measured by free lunch status) and allowing for separate impacts of each student group. Second, we test for a nonlinear impact of school racial composition by allowing the impact of a new school assignment to vary by neighborhood percent minority.[11]

IV.1 Checks on Non-Random Sorting and Attrition

The key concern with our approach is that student characteristics are systematically correlated with being on a particular side of a newly drawn school boundary, even within small geographic areas. If true, this would confound our estimates of the impact of being zoned to a school with more minority students. While we cannot measure the influence of unobserved student characteristics, we can test whether students' observed characteristics such as race, income and test scores are systematically correlated with our independent variable of interest.

The results of these specification checks are shown in Table 2, which reports results from regressions like equation (1) except without the covariates in X_{izjc}, which are instead used alternately as outcomes.[12] Columns 1 and 2 show results in the full sample with prior school zone by neighborhood fixed effects. Column 1 uses census block groups as the unit of geography, while Column 2 uses parcel groups. In both cases, we find no significant differences in the characteristics of students within neighborhoods who are newly assigned to a school with a higher share of minority students. Not only are the differences statistically insignificant, they are also relatively small – for example, in Column 2 the coefficient on 5th grade math scores implies that students who are assigned to a school with 10 percentage points more minority students have scores that are only 0.0022 standard deviations lower. Columns 3 and 4 separate by high school and middle school cohorts respectively, and only two results are statistically different from zero. Overall, there is little evidence of sorting across newly drawn boundaries on prior student characteristics. While not shown, we find no impact on prior student test

[11] We use neighborhood rather than school percent minority because our identification strategy explicitly compares students in the same neighborhoods with different school assignments.
[12] When test scores, absences and suspensions are outcomes (the bottom four rows), we also include controls for race, gender and free lunch status.

scores, absences and suspensions when we allow the impact of re-zoning to vary by race. Despite the apparent balance on prior student characteristics, we still control for them in our main specifications.

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 12 percent of minority students in our sample were no longer enrolled in CMS in the fall of 2002. Since our sample is based on earliest known address for students in the relevant grade cohorts, these attrition figures also incorporate some normal turnover that is unrelated to the boundary change. Once we condition on enrollment below 12th grade in fall 2001, only 4.3 and 3.3 percent, respectively, of non-minority and minority students were not enrolled in CMS in the fall of 2002.

Table 3 examines the impact of re-zoning on short-run attrition from CMS. We estimate equation (1), with an indicator variable for being enrolled in CMS in fall 2002 as the outcome. While there is no impact on attrition in the full sample, once we condition on fall 2001 enrollment we find that the probability of staying in CMS the following year rises by about 0.002 for students assigned to a school with 10 percentage points more minority students. In Panel B (Column 2), we see that this small increase is concentrated among minorities, though estimated impacts of a 10 percent increase in minority enrollment at the assigned school are always below 0.01. Importantly, attrition from CMS is not 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 very small increases in attrition from CMS related to the re-zoning helps support the notion that data limitations do not drive our results. In Section VI, we examine the impact of re-zoning on attrition from CMS in subsequent years. The interpretation of later attrition is complicated, however, by the fact that some students who leave CMS may actually be dropping out of school altogether.

IV.2 Impact of Re-zoning on Enrollment

Another possible concern is that the location of the new school boundaries was chosen based on preexisting trends in neighborhood racial composition. In this scenario, the impact of being reassigned would be a smooth trend line that shows up in prior cohorts. We examine this possibility by estimating a series of "first stage" regressions, with (attended) school percent minority as the outcome, separately by grade and year. Concretely, we measure the impact of being reassigned in fall 2002 on the racial composition of grade cohorts from fall 1995 to fall 2006, using students' earliest addresses in all cases. In Figure 5 we estimate versions of equation (1), with each point an estimate of β_0 and the associated 95 percent confidence intervals, separately for grade and year combinations. Overall, we see no evidence of pre-trends in the percent minority of students' attended schools in cohorts prior to fall 2002. The confidence intervals for almost all those grade and year combinations include zero, and there are no obvious trends or differences across grades. For nearly all grades, we see a steep and discontinuous spike in school percent minority beginning in fall 2002 and persisting for the next 5 cohorts. The size of this "first stage" coefficient ranges from 0.2 to 0.3, indicating that a 10 percentage point increase in assigned school percent minority leads to about a 2.5 percentage point increase in attended school percent minority. The one exception is 12th grade, where we see a much smaller estimate in fall 2002 that gradually increases for later cohorts. This is most likely due to the explicit priority that rising 12th grade students were given in the Family Choice Plan (FCP) discussed earlier.

While the estimates are relatively precise, a coefficient of less than one does suggest imperfect compliance with the newly drawn boundaries. Noncompliance can happen for three reasons. First, the use of earliest known address minimizes potential bias from student sorting, but also induces some measurement error because families may no longer live in the same neighborhoods by 2002. Second, the FCP allowed for families to choose schools other than the ones to which they were assigned, including magnet schools (which have no neighborhood zone).[13] 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 much harder to attend a non-magnet school outside of one's school zone.[14] Third, noncompliance by individual students has an indirect effect because it alters the racial composition of the school for other students. For example, if within the same school zone whites are more likely than minorities to opt out and attend a magnet school, then the first stage coefficient for compliers will still be less than one because the school is not as "white" as it would have been with perfect compliance.

Table 4 presents results for a variety of first stage outcomes. Panel A shows results from estimates of equation (1) with percent minority in a student's fall 2002 school as the outcome. In Panel

[13] About 5 percent of students also attend alternative schools, usually reserved for students with disabilities or other special needs.

[14] 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).

B, which allows the impact of assignment to vary by race, we see that the first stage is a bit stronger for whites. The 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 0.03 standard deviations lower, with larger impacts for minorities. As in other studies of the impact of racial segregation on schools (e.g. Guryan 2004, 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 (LATE) 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-minorities and minorities attended their previously assigned school, 9 percent of non-minorities and 13 percent of minorities attended magnet schools, and the remaining students choose another CMS school.[15] We examine whether students' school attendance decisions under the new choice system was influenced by re-zoning, again using variation within neighborhoods (here defined using parcel groups). We find little evidence of differences in the type of school attended, save a statistically significant increase in the probability of attending a magnet program that is concentrated among non-minorities (Column 4). To the extent that magnet schools are academically similar to (or better than) a student's assigned school, this will make the results for whites look better than if we had perfect compliance. Finally, re-zoning may have also affected families' decisions to change residence *within* CMS. In Columns 5 through 7 we examine moves between three consecutive school years, from 2000-2001 to 2002-2003 (the first year of the new student assignment plan), and find no evidence of differences in residential relocation in the years leading up to the policy change.

[15] Because we use earliest address, 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.

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

V.1 Middle School and High School Outcomes

Table 5 shows impacts on 8th grade test scores, absences, and out-of-school suspensions, for students who started in grades 6 through 8 in fall 2002 (the "middle school" cohorts). We find no impact overall or by race of being assigned to a school with more minority students on outcomes in 8th grade. The standard errors allow us to rule out even relatively modest test score impacts – for example, we can reject increase of more than 0.025 standard deviations or decreases of more than 0.02 standard deviations in 8th grade math scores for a 10 percentage point increase in share minority. However, it should be noted that these results represent short-run impacts of only 1 to 3 years (depending on grade cohort) of changes in school racial composition.

In Table 6 we examine the impact of re-zoning on 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 consistent decreases in math test scores, some of which border statistical significance. To maximize statistical power, in Column 5 our outcome is the average of (standardized) scores on each of the 4 exams.[16] Here we find a small but statistically significant decrease in end-of-course test scores of about 0.014 standard deviations for a 10 percentage point increase in share minority. The decline is slightly greater for non-minorities (-0.018 SDs) than for minorities (-0.012 SDs). Columns 6 and 7 split the sample by high school and middle school grade cohorts, and we find that most of the overall decline is driven by the high school cohorts (-0.022 vs. -0.006 SDs). The uniformity of test score decline by race suggests that all students have lower scores when they attend schools with more minority peers. Although there is some variation across students in the timing of the test, 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.

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. We test the robustness of our results by probing their sensitivity to different imputation procedures for missing test scores. Our baseline imputation uses a predicted test score from a cross-sectional regression of the high school exam score on 5th grade math and reading

[16] In cases where students are missing one or more scores, we simply take the average over the available scores.

scores. 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 (available upon request) indicate that missing values are not driving our results. While the negative impacts on math scores are attenuated somewhat under the baseline imputation, they remain statistically significant, and are negative and borderline significant even under the optimistic scenario. In fact, the additional precision gained by imputing missing scores makes the results for individual scores statistically significant in 2 of 4 cases. 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).

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 6^{th} grade cohort. Since our last cohort of 6^{th} graders was expected to graduate in the spring of 2009, we limit ourselves to examining initial attendance rather than 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 re-zoning 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. Panels A and B present results separated by race and gender for the high school and middle school grade cohorts respectively. To conserve space, we do not present results that are pooled across cohorts. We find a large and statistically significant decrease in high school graduation that is concentrated among non-minority students and is greater for the high school cohorts. For non-minority students in the high school cohorts, an increase of 10 percentage points in share minority of assigned school leads to a reduction of just under 2 percentage points in the probability of graduation from a CMS high school. In Columns 3 and 4, we find that non-minority students in the high school cohorts are about 1.5 percentage points less likely to attend any four-year college or a "very competitive" four-year college (defined by the 2009 Barron's rankings) for a 10 percentage point increase in share minority.[17] Since the college attendance data include students who leave the district, they are not unaffected by differential attrition from CMS. When we estimate results for the high school cohorts separately by poverty status (not shown), we find that most of the impact on high school graduation is driven by poor non-minorities (where poverty is proxied by eligibility for free or reduced-price lunches), while most of the impact on college attendance occurs among non-poor students. In contrast, we find no consistent impact of re-zoning on the educational attainment of minority students. We also find no consistent impact on the attainment of students in the middle school cohorts, who were exposed to the new school boundaries for a longer period of time. We discuss the implications of this pattern of results in Section VI.

V.3 Criminal Behavior

In Table 8 we estimate the impact of being assigned to a school with more minority students on arrests and incarceration. Table 8 is structured identically to Table 7, with separate coefficients by race and gender and separate models for high school and middle school cohorts in Panels A and B respectively. We find large and statistically significant increases in crime among minority males. The estimates imply that a 10 percentage point increase in share minority in a student's assigned school leads to an increase in the probabilities of arrest and incarceration by 1.1 to 1.4 percentage points. In Column 2, we find a corresponding increase of about 0.15 total arrests for the high school cohorts and 0.07 arrests for the middle school cohorts. Since days incarcerated is a heavily right-skewed variable, we estimate the impact on the natural log of total days incarcerated, but with zeros for students who are never incarcerated (effectively treating 1 day and zero days incarcerated as the same). In Column 4, we find a corresponding increase in total days incarcerated of 0.065 log points for high school cohorts and 0.042 log points for middle school cohorts. While the results in Columns 2 and 4 are larger for the high school cohorts, those students are also older (our arrest data go through 2011). We find no significant impact on crime in either direction for non-minorities or for minority females.

[17] Schools in North Carolina with a rating of "very competitive" or higher include Appalachian State University, Duke University, Elon University, North Carolina State University, UNC-Asheville, UNC-Chapel Hill, UNC-Wilmington, and Wake Forest University. Four year colleges in North Carolina that are less than "very competitive" include East Carolina University, Fayetteville State University, North Carolina A&T University, North Carolina Central University, UNC-Charlotte, UNC-Greensboro, UNC-Pembroke, Western Carolina University and Winston-Salem State University.

We explore results that are broken out by type of crime (not shown) and find that the increase in crime for minority males is distributed evenly across violent and property arrests, with a smaller role for 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.

In addition to estimating the impact of re-zoning on cumulative arrests and incarceration, we also investigate the pattern of results over time by breaking the arrest data into four month windows. This acts as an additional robustness check, because being rezoned in 2002 should not affect crime prior to the announcement of the policy change. We estimate our standard specification from equation (1), where the number of arrests within each time window is regressed on the impact of being assigned to a school with more minority students. Figure 6 reports the estimates for minority males only (although other demographic groups are included in the specification) and the associated 95 percent confidence intervals. We see a statistically significant increase in arrests that begins around the time of re-zoning and persists at roughly the same level for nine years, through the end of our data in 2011.[18] Results separated out by cohort (not shown) show an age profile of increases in arrests that is broadly similar across cohorts (despite imperfect overlap). Taken together, the results in Table 8 and Figure 6 suggest a consistent and long-run increase in criminal behavior by minority males who are assigned to schools with more minority students. Unlike the results for achievement and attainment, the impacts do not diminish over time.

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 Table 6 we show estimated test score decreases for students of all racial backgrounds when they attend schools with more minority students. Since the re-zoning led to a decrease in the share of minority peers for white students and an increase for minority students, the net impact was a widening of racial inequality in test scores. To get a sense of the magnitude, we multiply the point estimates from Table 6 by the

[18] Note that there is some evidence for smaller increases in arrests prior to the fall of 2002. While they are on the border of statistical significance, it is possible that students began committing more crime (or were treated differently by school officials) once they learned of the next year's school assignment. That can be interpreted as part of the treatment and is not a threat to the validity of our results.

mean change in the assigned share minority before and after the re-zoning, separately by race.[19] This calculation implies that the re-zoning widened the racial gap in high school math scores by about 0.039 standard deviations in the oldest (high school) cohorts but only 0.014 standard deviations in the youngest (middle school) cohorts. The estimates in Table 7 imply a widening of the racial gap in high school graduation and four-year college attendance of 1.6 and 1.4 percentage points respectively in the high school cohorts, and 0.9 and 0.1 percentage points in the middle school cohorts. Finally, the crime estimates in Table 8 imply that the racial gap in male arrest and incarceration rates widened by about 1.6 percentage points in all grade cohorts.

The calculation of mean racial gaps masks considerable variation across neighborhoods. If we compare whites 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, the gaps we estimated above nearly double in magnitude. When we compare whites and minorities who live in own-race segregated neighborhoods, our estimates for the middle school cohorts imply that the racial gap in male arrest and incarceration rates widens by nearly 3.5 percentage points.

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 three 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 slightly increases the impact on crime for minority males, but leaves the rest of the results essentially unaffected. Second, we exclude the approximately 14 percent of students who relocated to a new address between fall 2001 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.

[19] For example, the mean change in assigned school percent minority is -0.071 for non-minorities and 0.102 for minorities, and the coefficients on the high school test composite are -0.248 for non-minorities and -0.208 for minorities in the high school cohorts. Multiplying the values together separately by race gives 0.0176 for non-minorities and -0.0212 for minorities, for a total gap of 0.039 standard deviations.

Third, in Table 9 we estimate the impact of re-zoning on attrition from CMS over time. Specifically, we present results where the outcome is having been enrolled in CMS in years that correspond with "on time" progression to grades 7 through 12.[20] Students who leave CMS in middle school are likely to be transfers to private schools, charter schools, or public schools outside of Mecklenburg County. While transfers also occur in high school, the impacts we estimate for older students will also include those who drop out of school altogether.[21] This is an important distinction, because the major threat to interpretation of the estimated impact on crime is that assignment to a school with more minority students is correlated with attrition *from Mecklenburg County,* not CMS. In Table 9, we see that white students who are assigned to schools with more minority students are less likely to remain in CMS. The impacts are small and relatively constant over time, suggesting that transfer to private school or out of the county may be occurring at higher rates for non-minorities. However, the impact of re-zoning in minority students follows a different pattern, staying near zero in 7th and 8th grade and rising sharply in the high school years, especially after the age of compulsory schooling. In particular, we find no impact on attrition among minority males until the years when they would be in 10th grade, or around age 16. While not conclusive, this pattern of results suggests that most of the differential attrition among minority males comes from dropout. The results imply that being assigned to a school with more minority enrollment keeps minority students in school longer, yet we find no impact on high school graduation. We also find no impact on the probability that minorities complete 11th or 12th grade (not shown), which suggests that students who stay longer are not progressing toward graduation. Overall, we conclude that the results are driven by changes in the CMS schools attended by the students in our sample rather than by differential attrition from CMS or Mecklenburg County.

The second possible explanation is that students may be harmed by additional school transitions. In this scenario, it is not the racial composition of the school that matters, but simply being rezoned to any new school. If disruption from additional school transitions was an important factor, we might expect the impacts to be much smaller for rising 6th and 9th grade cohorts, when most students are attending a new school anyway. Testing this hypothesis (in results not shown here), we find no evidence of smaller impacts for students in these two cohorts. Moreover, the pattern of results actually

[20] For example, the dependent variable in column 1 of Table 9 is equal to 1 if students who were rising 6th graders in Fall 2002 were still enrolled in CMS in Spring 2003, or if rising 7th graders in Fall 2002 were still enrolled in CMS in Spring 2002. The variable is missing for students who were in older grade cohorts in Fall 2002. Note that the sample size in Column 6 is equal to the overall sample size for the attrition outcome in Table 3, because all students in our sample would have been enrolled in 12th grade *after* the rezoning.

[21] In North Carolina, students are required to attend school until age 16, which corresponds roughly to the 10th grade year for most students.

reflects an average *increase* after the re-zoning in academic achievement and educational attainment among white students, which disruption is unlikely to explain.

Another related hypothesis is that the increase in crime among minority males is explained by the end of the long bus rides that often characterize school desegregation plans. If time on the bus after school creates an incapacitation effect on crime they would otherwise be committing (e.g. Jacob and Lefgren 2003), the re-zoning of students to "neighborhood" schools might increase crime. To test this hypothesis, we estimate our crime specifications excluding the approximately 4.5 percent of students who attended non-contiguous school zones prior to the policy change (e.g. the three grey zones in the upper lefthand area of Figure 1). The impact on crime for minority males shrinks by about 10 percent but is still large and statistically significant, which suggests that time spent on buses is unlikely to explain our findings. One reason this is not surprising is that disciplinary records collected by CMS reveal that about 40 percent of recorded incidents in middle school and high school occurred on a school bus, which calls into question the idea that bus rides prevent crime. Finally, note that any impact of disruption or incapacitation must have had far-reaching impacts on young adult crime to explain our results, since estimated increases in arrests persist for at least 9 years after the re-zoning.

A third possible explanation is that the end of race-based busing shifted the allocation of resources across CMS schools. Recent work on the impact of court-ordered school desegregation in the 1960s and 1970s shows that racial integration was also accompanied by a narrowing of racial gaps in per-pupil spending and class size (Reber 2010, Johnson 2011, Cascio, Gordon, Lewis and Reber 2010). This suggests that increased resources, beyond changes in school racial composition itself, may have been an important mechanism for improvements in the outcomes of African-American students during this period (e.g. Margo 1990, Card and Krueger 1992). Archival expenditure data from CMS show that per-pupil instructional spending (net of capital costs, transportation, etc.) was roughly equal across high schools in the 2001-2002 year, prior to re-zoning. Yet Jackson (2009) shows that teachers, who are the main factor in school expenditures, sorted across schools in response to the policy change - he 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.[22]

To address equity concerns and perhaps in anticipation of teacher sorting, CMS paired the new student assignment policy with a program called the Equity Plan, which provided additional funds to high poverty schools for recruitment bonuses for teachers, lower student-teacher ratios, school

[22] Because of the difficulty of calculating teacher value-added in high schools (see Jackson 2012), we cannot say whether similar sorting occurred at the high school level.

renovation projects, learning equipment and supplies (Mickelson, Smith and Southworth 2009). However, budget cuts limited the impact of the Equity Plan in the first years after re-zoning (Mickelson 2005). In 2006, CMS began a program called the High School Challenge, which targeted the four high schools with the highest shares of poor and minority students. The High School Challenge was created as a response to the schools' inability to meet state accountability benchmarks for achievement on End-of-Course (EOC) tests, and was funded by the Mecklenburg Board of County Commissioners and the Federal Teacher Incentive Fund (TIF) (CMS Schools, 2005). The program increased teacher salaries in these schools by 15 percent and offered signing bonuses of up to $15,000, but also increased accountability, allowing for expedited removal of teachers and principals as well as restructuring of schools that were not meeting performance standards (CMS Schools, 2008). Partially as a result of the High School Challenge program, by the 2009 school year the four targeted high schools spent about 20 percent more per pupil (not including capital costs) than the average CMS high school (CMS Schools, 2011). More generally, the relationship between the share of minority students in a CMS high school in 2009 and two measures of resources (per-pupil spending net of capital costs and the student/teacher ratio) was strongly positive.[23] By 2008, the High School Challenge schools had increased their performance on a composite of EOC tests by an average of 12 percentile ranks. The one school that did not make substantial gains (Garinger High School, which moved from 41 to 43 percent from 2005 to 2008) was reconstituted as five small "academy" programs with themes such as technology, business and finance, and international studies.

In Table 10 we provide some information on high school student experiences across cohorts by measuring the impact of re-zoning on upper level and advanced placement (AP) course-taking. An important caveat is that we only observe these data for students who remained in CMS through 12th grade. The top panel of Table 10 presents results for the high school cohorts, while the bottom panel shows results for the middle school cohorts. For students who were in rising grades 9 through 12 in the fall of 2002, being assigned to a school with more minority students led to decreases in the number of honors and advanced math courses (defined as pre-calculus and above) taken, as well as decreases in the probability of taking an AP science or English course. This pattern is broadly consistent with the declines in achievement on high school EOC tests we saw in Table 6, as well as the declines in educational attainment for non-minority students in Table 7. However, we do not find the same pattern of results for the middle school cohorts – if anything, being assigned to a school with more minorities leads to *increases* in advanced course-taking among these students. Moreover, recall that there was no

[23] The rank-order correlation between each resource measure and share minority was about 0.5.

impact of being rezoned on high school test scores or college attendance in the middle school cohorts. Thus the pattern of results in Table 10 is broadly consistent with the impacts on test scores and educational attainment in Tables 6 and 7. Since students in the middle school cohorts would have taken AP and advanced classes roughly from 2006 through 2009, the pattern of results over time suggests that compensatory increases in school resources may have played some role in mitigating the longer-run impacts of re-zoning on test scores and educational attainment.

A fourth possible explanation is that the re-zoning of CMS schools led to changes in peer interactions and/or school context. The key difference from a resource allocation story is that peer interactions and school context may be very difficult for a school district to manipulate. If most of the impact of school segregation were driven by associated changes in resources – either financial, or in terms of nonfinancial inputs such as teacher quality - then policymakers could address racial inequality through compensatory policies (such as the High School Challenge) instead of manipulating student assignments directly. On the other hand, if changes in racial composition lead to different outcomes for students because of peer effects, then providing additional resources alone will not be sufficient to prevent widening inequality. In Table 8, we show that being rezoned to a school with more minority students causes large and statistically significant increases in crime among minority males. Unlike the results for achievement and attainment, we also found that the impacts on crime are equally large among minority males in the middle school cohorts, and that they persist through the end of our data in 2011. Thus the increased allocation of resources to high poverty high schools did not diminish the impact of re-zoning on the criminal activity of minority males. Moreover, the social nature of criminal activity and the concentration of impacts for one particular demographic group (minority males) suggest that peer effects may be an important mechanism.

We investigate the possibility of peer effects further in Table 11 by allowing the impact of re-zoning to vary across four race-by-income combinations. We estimate the impact of being rezoned to a school with higher shares of poor non-minorities, non-poor minorities and poor minorities (with non-poor non-minorities as the reference category), and we further allow those impacts to vary by students' own race and gender. Because of our focus on peer effects and crime, we only present the results for minority males in Table 11, although all the estimates are included in the model and available upon request. The results show clearly that it is the combination of race and income segregation that leads to increases in crime. Minority males have significantly more arrests and days incarcerated when they are assigned to schools with more poor minorities. However, we find no impact on crime of being assigned to schools with more non-poor minorities or poor non-minorities (compared to non-poor non-minorities, the reference group). If we further separate students into eight race by gender by poverty groups, we find that the impacts on crime in Table 8 and Table 11 are driven entirely by increases among *poor* minority males.[24] Taken together, the results show that poor minority males commit more crimes when they are assigned to schools with more poor minority males. Crime is not significantly affected among any other groups for any possible combination of demographic changes.

Past work on the measurement of segregation, as well as on peer effects, has focused on nonlinear impacts of changes in school racial composition (e.g. Massey and Denton 1988, Hoxby 2000, Card and Rothstein 2007, Vigdor and Ludwig 2008). We cannot directly stratify the analysis by school racial composition because our analysis compares students on either side of a newly drawn school boundary. However, we can examine nonlinearities indirectly by allowing the impacts to vary by neighborhood racial composition.[25] Table 12 presents crime results for minority males where the impact is allowed to vary by share minority of the student's 2000 Census Block Group (again, other groups are included in the model and those results are available upon request). In essence, we are examining the impact of linear changes in share minority of assigned school, but from different baselines. The results are highly nonlinear and indicate that all of the impact on crime is driven by minority males who live in neighborhoods that are 60 percent minority or greater. Combined with Table 11, this suggests that the overall impacts on crime are driven by relatively high concentrations of poor minority males being grouped together. This confirms results from several other studies which find that concentrating high-risk youth together increases the aggregate level of misbehavior and/or crime (Cook and Ludwig 2005, Carrell and Hoekstra 2010, Deming 2011, Imberman, Kugler and Sacerdote 2012).

Changes in school context could come from students themselves or from institutional reactions to peer group composition. Kinsler (2011) shows that while African-American students are about twice as likely to be suspended from school compared to white students, all of the racial gap in school discipline is driven by variation across (rather than within) schools. Thus it is possible that students who are assigned to schools with more minority students exhibit similar behavior but are treated differently. If schools with more poor and minority students have a greater police presence, for example, this would raise the probability of disciplinary action conditional on behavior. If school suspensions lead to long-run increases in crime, then differences in schools' treatment of student behavior could explain our findings. Yet in Table 5 we found no impact on total days suspended in 8th grade, and in results not shown we also

[24] We did not separate by poverty in our main results to conserve space, but those results are available upon request.
[25] The court order that race was not allowed to be a factor in the re-zoning effectively mandated that neighborhood racial composition be strongly correlated with school racial composition.

find no impact on 9th grade suspensions.[26] This suggests that, despite large differences in suspension rates across schools, the marginal student is not more likely to be suspended when they are assigned to a school with more minority students. Importantly, all arrests stemming from behavior on school grounds also result in out-of-school suspension, which suggests that our crime results are not driven by criminal offenses that occur on school grounds.

We also investigate the possibility of a "sensitive period" for the impact of school racial composition on crime by looking for differences in estimates across grade cohorts with different exposure to re-zoning, but find no evidence for nonlinearities by exposure. Additionally, for students in the middle school cohorts, we can allow the impact of re-zoning to be different for middle school and high school assignments, rather than using the one-year change from fall 2001 to fall 2002 for all cohorts as we do in our previous results. Not surprisingly, individual students' changes in assigned school race are strongly correlated for middle schools and high schools (about 0.75). Still when we include them together, the impacts on crime among minority males show up as positive and statistically significant for both boundary changes, and are roughly equal in size. From this we conclude that increases in share minority have similar impacts at the middle school and high school levels.

A final possible explanation for the results is that neighborhoods changed over time, which in turn affected school context. Weinstein (2011) 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, 2011). 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 areas are larger.

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

[26] While we also find no impact on school suspensions in grades 10 through 12, selective school dropout makes it difficult to interpret those results.

to concerns that subsequent resegregation of schools will cause blacks to give back some of the gains made in the 1960s and 1970s (e.g. Mickelson 2003).

We find that the resegregation of CMS schools led to an increase in racial inequality. Both whites and minorities score lower on high school exams when they are assigned to schools with more minority students. 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 white students are about 1.5 percentage points less likely to graduate from high school or attend a four-year college when they are assigned to schools with 10 percentage points more minority students. However, we find that the widening of the racial gap in achievement and attainment was short-lived. For younger cohorts of students, who were in rising 6th through 8th grades in the fall of 2002, we find much smaller and often statistically insignificant impacts on racial inequality. An important exception to this pattern is the criminal activity of minority males, where we find that a 10 percentage point increase in the share of minorities in a student's assigned school raises the probability of ever being incarcerated and ever being arrested by about 1.3 percentage points. The impacts on crime for minority males are equally large across grade cohorts and last through the end of our sample period, nine years after the re-zoning of CMS schools. Moreover, they are driven equally by increases in racial and economic segregation, and are concentrated among poor minority males who live in racially segregated neighborhoods.

Based on this pattern of results, we argue that the diminishing impacts on test scores and attainment are most likely driven by compensatory allocation of educational resources. The impacts on crime, which we argue are most consistent with peer effects and/or school environment, do not seem to have been dampened by the investment of additional resources in high poverty schools by CMS after the re-zoning. Importantly, the impact on crime was highly concentrated among poor minority males from segregated neighborhoods who attended schools with more students like them. We find little evidence that our results are explained by exit from CMS and into other schools, by disruption from increased school transitions, or by a decline in the incapacitation effect of long bus rides before and after school.

If policymakers wish to prevent a widening of racial and economic inequality, our findings suggest that explicit efforts to offset the impacts of school segregation may be necessary. CMS implemented a number of innovative policy changes over the last decade, including the allocation of additional resources to and intensive monitoring of high schools with concentrations of disadvantaged students. The pattern of results suggests that these efforts halted an initial widening of racial inequality in achievement and attainment. However, if peer effects are responsible for increases in crime among minority males, then widening racial inequality in crime may be linked inextricably with segregation. Policies that allocate additional resources to segregated schools can improve classroom instruction and course offerings, but only deliberately integrative student assignment policies can change the racial or socioeconomic backgrounds of students who walk in the doors of the school.

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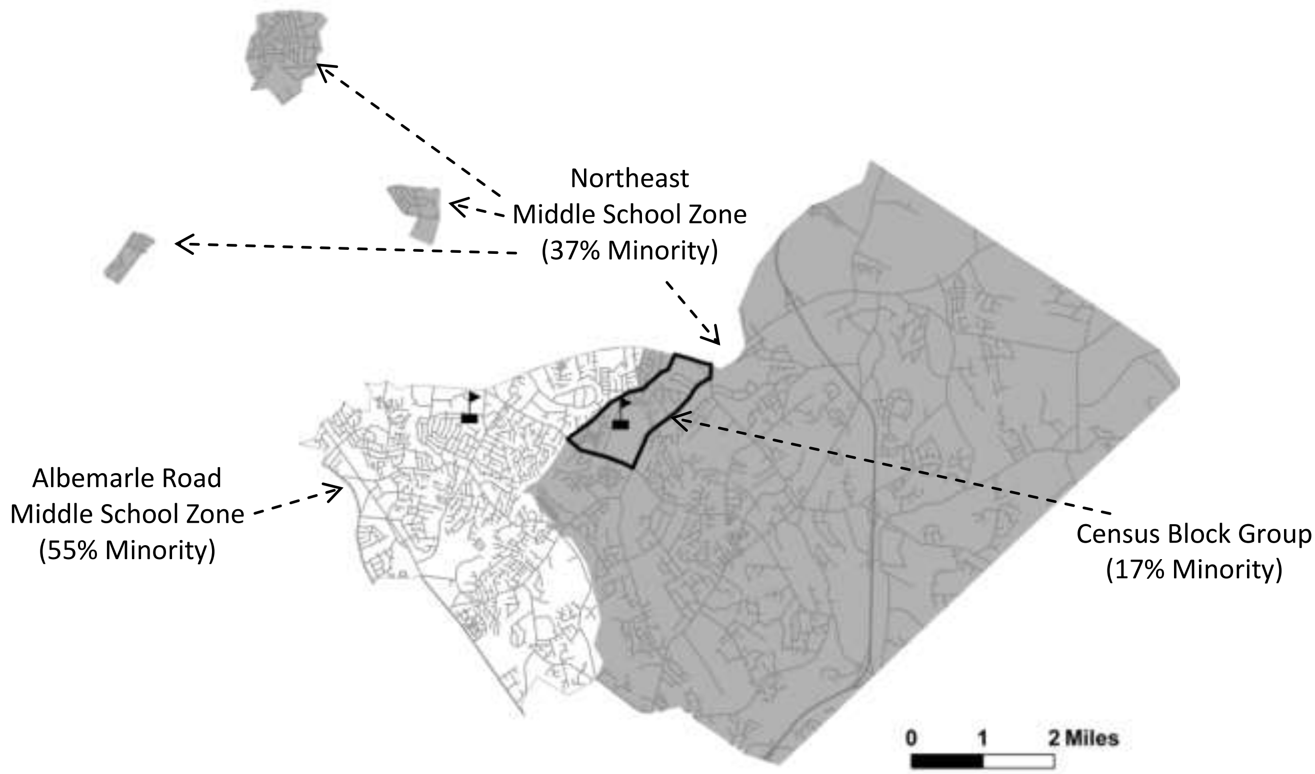
Figure 1 –Re-Zoning for Two Middle Schools

Before (2001-2002)



After (2002-2003)

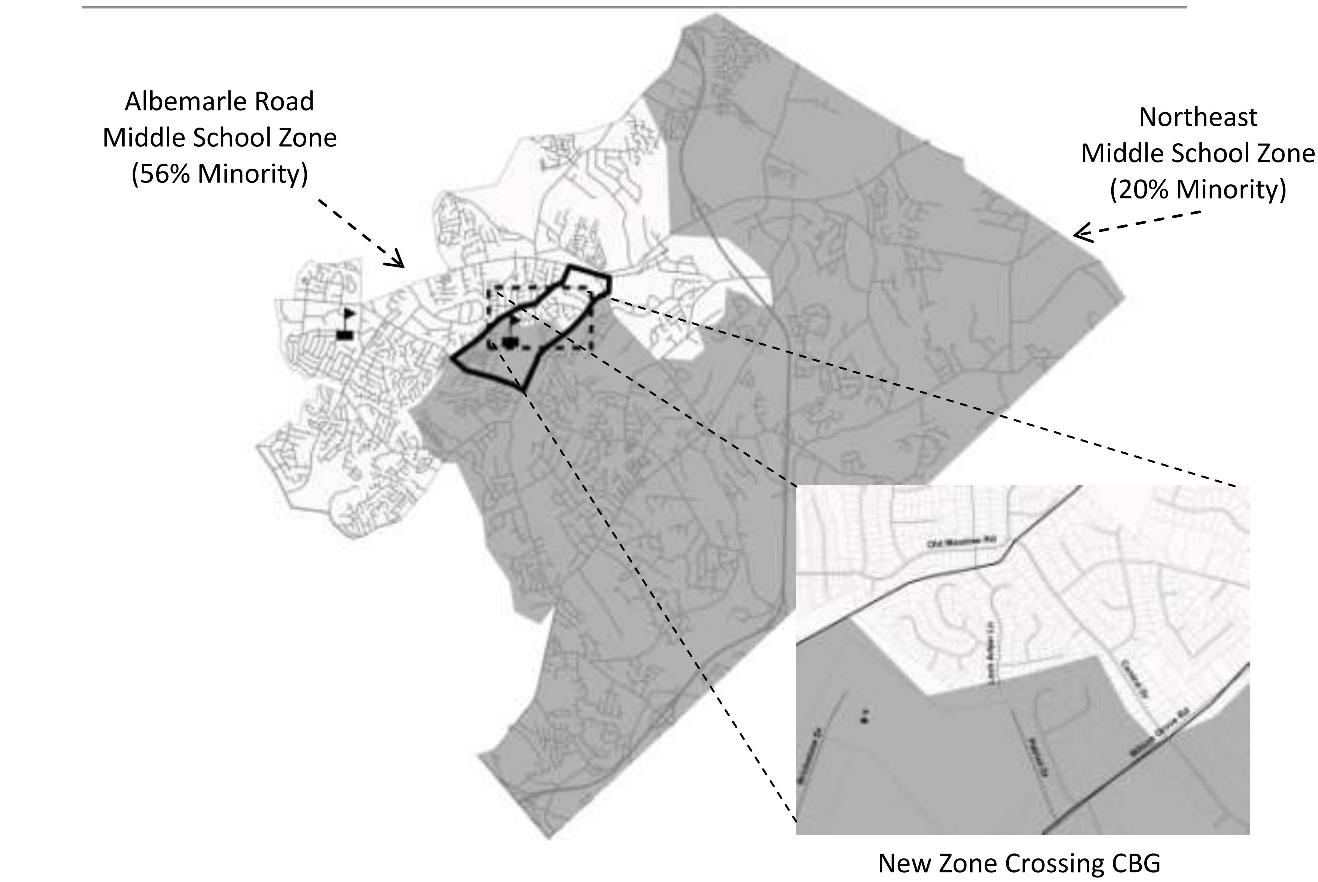
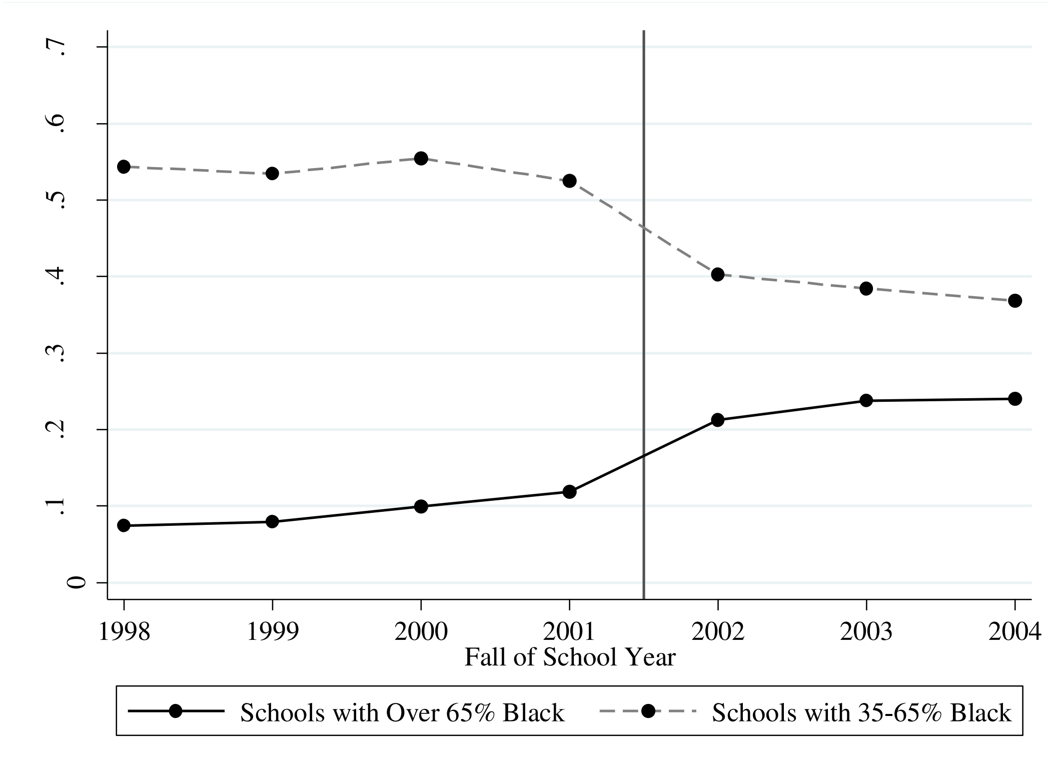
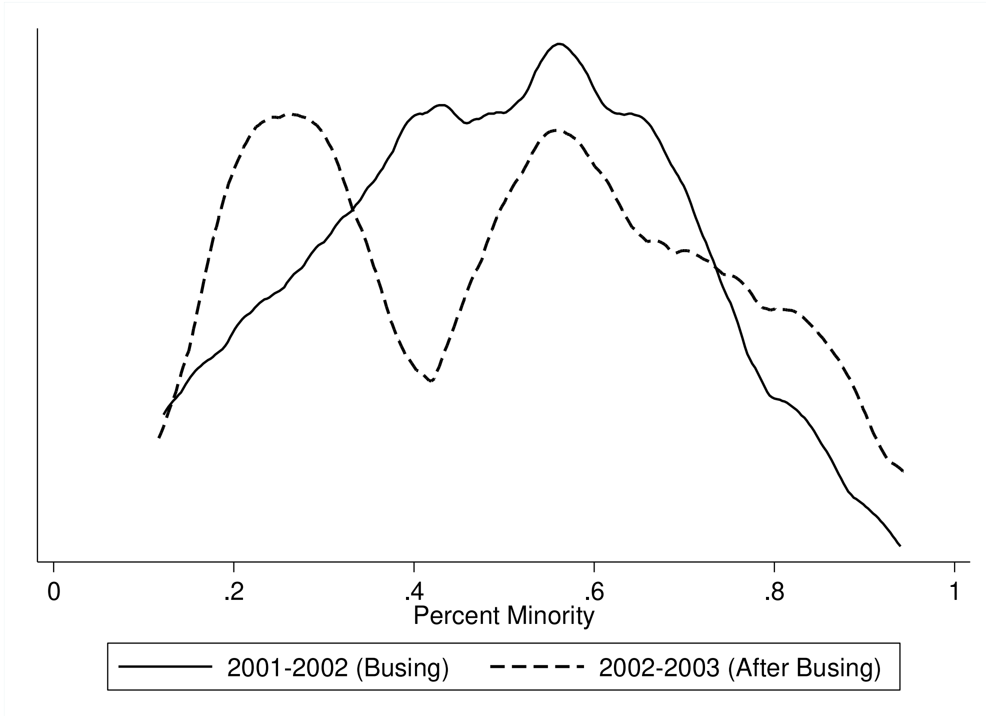


Figure 2: Impact of the 2002 Re-zoning on the Concentration of Black Students



Source: NCES Common Core of Data

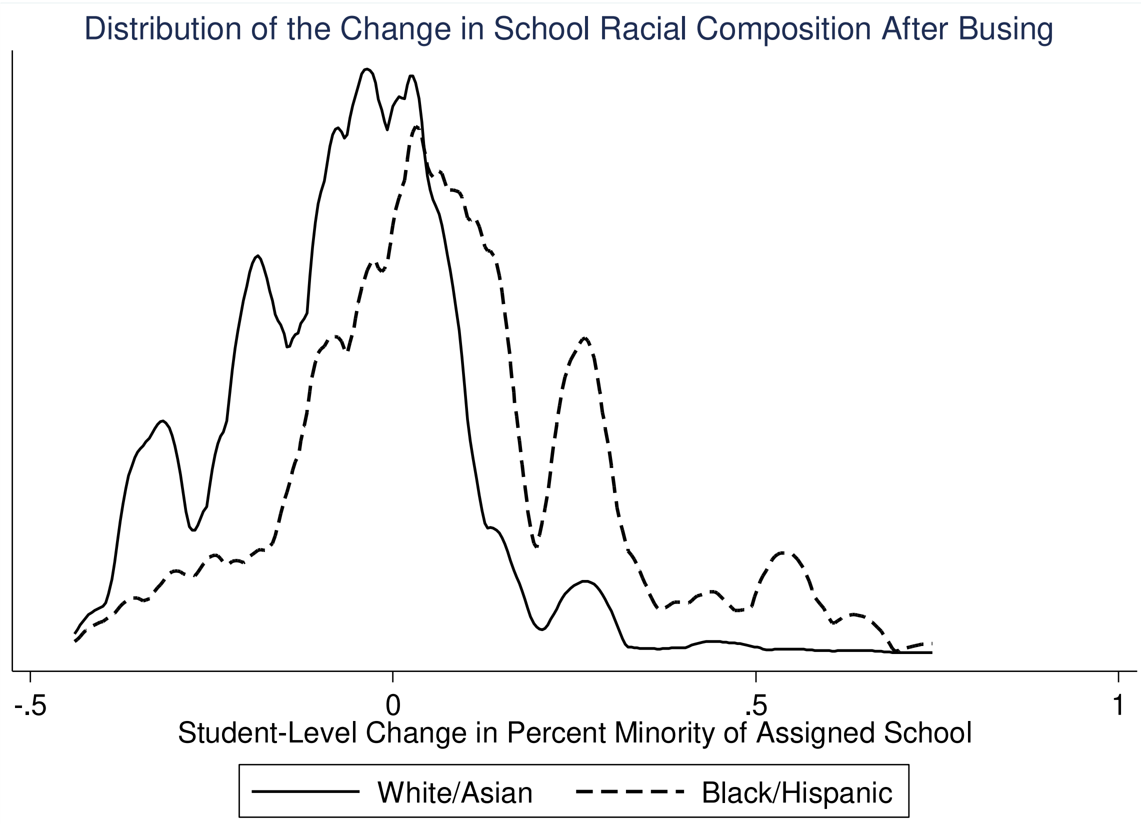
Figure 3: Density of Middle/High School Racial Composition (Enrollment Weighted)
Actual School Attended



School Assigned Based on Residence

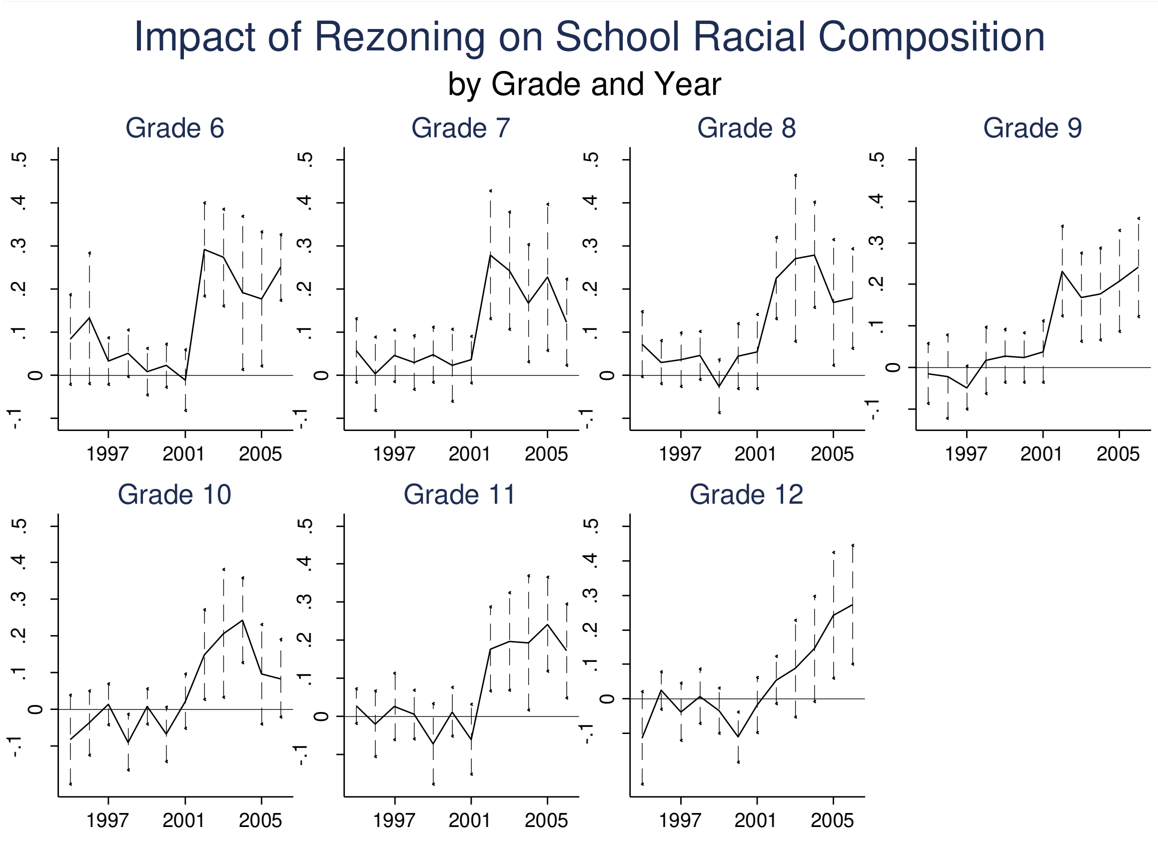
Notes: The top panel shows kernel density plots of the distribution of the racial composition of the schools attended by students in the sample in the years immediately before and immediately after the re-zoning. The bottom panel shows the same thing, except for assigned school. Differences in assigned 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



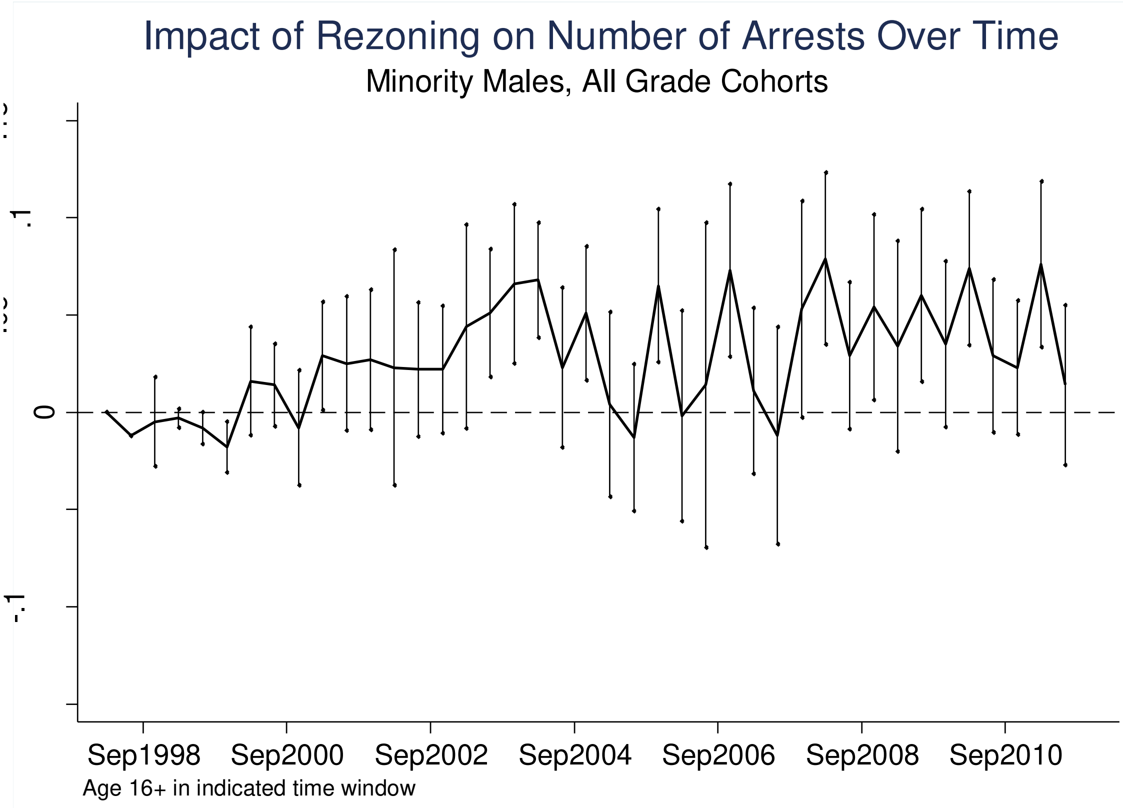
Notes: This figure plots the student-level change in the racial composition of the assigned school before and after the re-zoning, separately by race. The mean values for Percent Minority are -0.07 for non-minorities and +0.08 for minorities, with standard deviations of 0.15 and 0.21 respectively.

Figure 5



Notes: Each point is the key coefficient and associated 95 percent confidence interval from a regression like equation (1), estimated separately by grade and year, and is interpreted as the impact of a 100 percentage point increase in the share minority of a student's assigned school on the share minority of the school the student actually attended in the fall of 2002. Thus coefficients for years prior to 2002 act as a check for pre-policy trends in school racial composition.

Figure 6



Notes: Each point is the key coefficient and associated 95 percent confidence interval from a regression like equation (1) on the full sample of age-eligible students, estimated separately for four-month intervals. The coefficients are interpreted as the impact of a 100 percentage point increase in the share minority of a student's assigned school on the number of arrests for minority males in the indicated time period.

Table 1: Sample Descriptive Statistics

	Full Sample	CBG Percent Minority <20%	CBG Percent Minority 20%-66%	CBG Percent Minority >66%
Sample Size	51,020	17,931	17,989	15,100
Black	0.44	0.08	0.44	0.86
Hispanic	0.05	0.02	0.07	0.05
Free/Reduced Lunch	0.51	0.14	0.54	0.89
5th Grade Math	0.01	0.54	-0.06	-0.57
5th Grade Reading	0.01	0.53	-0.06	-0.57
Reassigned	0.52	0.34	0.46	0.81
Any College	0.47	0.65	0.45	0.28
4 Year College	0.35	0.53	0.31	0.19
Arrested	0.09	0.04	0.09	0.17
# Arrests	0.50	0.13	0.41	1.03
Incarcerated	0.08	0.03	0.07	0.15
Days Incarcerated	8.56	1.45	6.74	19.18

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 (~96% 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. Crime outcomes are measured beginning at age 16. 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 Re-zoning Predict Student Characteristics?

	Full Sample	Full Sample	High School Cohorts	Middle School Cohorts
	(1)	(2)	(3)	(4)
Black	-0.047	-0.008	0.074	-0.012
	[0.044]	[0.028]	[0.140]	[0.092]
Hispanic	0.008	0.028	0.090**	0.069
	[0.014]	[0.019]	[0.043]	[0.103]
Free/Reduced Lunch	0.001	0.014	0.093	0.126
	[0.031]	[0.025]	[0.071]	[0.110]
5th Grade Math Score	0.020	-0.022	-0.212	-0.060
	[0.051]	[0.046]	[0.140]	[0.121]
5th Grade Reading Score	-0.004	-0.025	-0.178	-0.117
	[0,.051]	[0.045]	[0.133]	[0.096]
5th Grade Days Absent	-0.410	-0.027	-0.459	2.148**
	[0.389]	[0.375]	[0.998]	[0.888]
5th Grade Days Suspended	0.07	0.062	-0.038	0.182
	[0.080]	[0.080]	[0.192]	[0.165]
Prior Zone by Census Block Group Fixed Effects	√			
Prior Zone by Parcel Group Fixed Effects		√	√	√
Sample Size	51,020	51,020	28,465	22,555

Notes: Each cell shows the coefficient and standard error from a separate estimate of equation (1), and is interpreted as the impact of being assigned to a school with 100 percentage points more minority students. Variables listed in the first column (e.g., Black, Hispanic) are dependent variables; all regressions include cohort fixed effects. Standard errors are clustered at the same levels of geography as the fixed effects indicated in each column. *** p<0.01, ** p<0.05, * p<0.10

Table 3: Impact of Re-zoning on Short-run Attrition from CMS

Panel A: Pooled Sample	Full Sample		High School Cohorts		Middle School Cohorts	
	(1)	(2)	(3)	(4)	(5)	(6)
Share Minority in New Zone	0.013	0.022**	-0.035	0.029	0.064*	0.033
	[0.018]	[0.010]	[0.066]	[0.031]	[0.034]	[0.023]
Panel B: Effects by Racial Group						
Share Minority in New Zone *						
Non-Minority Student	-0.011	0.009	-0.083	0.011	0.042	0.021
	[0.025]	[0.015]	[0.070]	[0.033]	[0.037]	[0.026]
Minority Student	0.029	0.031**	-0.001	0.040	0.080**	0.042*
	[0.019]	[0.011]	[0.069]	[0.032]	[0.035]	[0.024]
Prior Zone by Parcel Group Fixed Effects	√	√	√	√	√	√
Limit to Students Enrolled in 2001-2002		√		√		√
Sample Size	51,020	43,949	28,465	22,329	22,555	21,620

Notes: In Panel A, each cell shows the coefficient and standard error from a separate estimate of equation (1), and is interpreted as the impact of being assigned to a school with 100 percentage points more minority students. Panel B presents results where the impact is allowed to vary by student's own race. 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; all regressions also control for race by cohort fixed effects, parcel group by prior middle and high school zone fixed effects, and quadratics in 5th grade math and reading scores plus dummies for missing scores. Standard errors are clustered at the Prior Zone by Parcel Group level. *** 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)
Share Minority in New Zone	0.251***	-0.317***	0.007	0.060**	-0.012	-0.023	0.021	0.013
	[0.028]	[0.044]	[0.022]	[0.027]	[0.022]	[0.016]	[0.017]	[0.020]
Panel B: Effects by Racial Group								
Share Minority in New Zone *								
Non-Minority Student	0.313***	-0.391***	-0.006	0.130***	0.014	-0.012	0.002	0.010
	[0.030]	[0.063]	[0.029]	[0.032]	[0.025]	[0.019]	[0.020]	[0.020]
Minority Student	0.210***	-0.268***	0.015	0.014	-0.030	-0.031	0.035	0.016
	[0.024]	[0.034]	[0.021]	[0.024]	[0.027]	[0.020]	[0.022]	[0.025]
Sample Size	42,274	42,186	42,274	42,274	42,274	39,557	41,863	39,451

Notes: In Panel A, each cell shows the coefficient and standard error from a separate estimate of equation (1), and is interpreted as the impact of being assigned to a school with 100 percentage points more minority students. Panel B presents results where the impact is allowed to vary by student's own race. Each column shows the results of a separate regression where the dependent variable is indicated in the column heading above; all regressions also control for race by cohort fixed effects, parcel group by prior middle and high school zone fixed effects, and quadratics in 5th grade math and reading scores plus dummies for missing scores. Standard errors are clustered at the Prior Zone by Parcel Group level. *** p<0.01, ** p<0.05, * p<0.10

Table 5: Impacts of Re-zoning on 8th Grade Achievement and Behavior

Panel A: Pooled Sample	Math Score	Reading Score	Total Absences	Days Suspended
	(1)	(2)	(3)	(4)
Share Minority in New School Zone	-0.029	0.008	-1.04	0.208
	[0.115]	[0.099]	[1.84]	[0.747]
Panel B: Effects by Racial Group				
Share Minority in New School Zone *				
Non-Minority Student	0.026	0.037	-0.66	-0.204
	[0.115]	[0.115]	[1.89]	[0.773]
Minority Student	-0.053	-0.009	-1.26	0.435
	[0.105]	[0.097]	[1.93]	[0.798]
Observations	18,800	18,802	19,444	19,444

Notes: In Panel A, each cell shows the coefficient and standard error from a separate estimate of equation (1), and is interpreted as the impact of being assigned to a school with 100 percentage points more minority students. Panel B presents results where the impact is allowed to vary by student's own race. Each column shows the results of a separate regression where the dependent variable is indicated in the column heading above; all regressions also control for race by cohort fixed effects, parcel group by prior middle and high school zone fixed effects, and quadratics in 5th grade math and reading scores plus dummies for missing scores. The sample is restricted to students in rising middle school grades in the fall of 2002. Standard errors are clustered at the Prior Zone by Parcel Group level. *** 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	Average of 4 HS Tests: Pooled	Average of 4 HS Tests: HS Cohorts	Average of 4 HS Tests: MS Cohorts
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Share Minority in New School Zone	-0.038	-0.048	-0.130*	-0.125	-0.137**	-0.220*	-0.063
	[0.097]	[0.081]	[0.077]	[0.133]	[0.060]	[0.120]	[0.088]
Panel B: Effects by Racial Group							
Share Minority in New School Zone *							
Non-Minority Student	-0.066	-0.124	-0.126	-0.124	-0.182**	-0.248*	-0.143
	[0.106]	[0.100]	[0.099]	[0.152]	[0.080]	[0.149]	[0.107]
Minority Student	-0.025	-0.017	-0.132*	-0.126	-0.115*	-0.208*	-0.025
	[0.099]	[0.085]	[0.076]	[0.133]	[0.061]	[0.117]	[0.085]
Observations	23,387	21,378	21,613	21,525	31,675	13,340	18,335

Notes: In Panel A, each cell shows the coefficient and standard error from a separate estimate of equation (1), and is interpreted as the impact of being assigned to a school with 100 percentage points more minority students. Panel B presents results where the impact is allowed to vary by student's own race. Each column shows the results of a separate regression where the dependent variable is indicated in the column heading above; all regressions also control for race by cohort fixed effects, parcel group by prior middle and high school zone fixed effects, and quadratics in 5th grade math and reading scores plus dummies for missing scores. Columns 5 through 7 construct averages across all non-missing scores. Standard errors are clustered at the Prior Zone by Parcel Group level. *** p<0.01, ** p<0.05, * p<0.10

Table 7: Impacts of Re-zoning on Educational Attainment

Panel A: High School Cohorts	Graduated from CMS	Attend Any College	Attend 4 Year College	Attend Very Competitive
	(1)	(2)	(3)	(4)
Share Minority in New School Zone *				
Non-Minority Female	-0.183***	-0.032	-0.164**	-0.149***
	[0.071]	[0.082]	[0.077]	[0.051]
Non-Minority Male	-0.202***	-0.104	-0.168**	-0.163***
	[0.073]	[0.080]	[0.076]	[0.047]
Minority Female	0.012	0.039	-0.040	-0.021
	[0.077]	[0.070]	[0.065]	[0.038]
Minority Male	-0.050	0.070	-0.008	-0.043
	[0.081]	[0.075]	[0.072]	[0.038]
Observations	22,329	22,329	22,329	22,329
Panel B: Middle School Cohorts				
Share Minority in New School Zone *				
Non-Minority Female	-0.145*	-0.114	-0.107	0.011
	[0.075]	[0.078]	[0.078]	[0.050]
Non-Minority Male	-0.083	-0.089	-0.015	0.083*
	[0.080]	[0.080]	[0.076]	[0.046]
Minority Female	0.027	-0.021	0.026	0.057
	[0.076]	[0.066]	[0.063]	[0.041]
Minority Male	-0.040	-0.026	0.056	0.074*
	[0.078]	[0.071]	[0.069]	[0.040]
Observations	21,620	21,620	21,620	21,620

Notes: Within panels, each column shows coefficients and standard errors from a separate estimate of equation (1), and is interpreted as the impact of being assigned to a school with 100 percentage points more minority students, where the impact is allowed to vary by the race and gender combinations indicated in each row. Panel A presents results for rising 9th through 12th graders in the fall of 2002, while Panel B presents results for rising 6th through 8th graders. All regressions control for race by cohort fixed effects, parcel group by prior middle and high school zone fixed effects, and quadratics in 5th grade math and reading scores plus dummies for missing scores. College attendance records are obtained from the NSC data, which can track students who leave CMS schools. Standard errors are clustered at the Prior Zone by Parcel Group level. *** p<0.01, ** p<0.05, * p<0.10

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

Panel A: High School Cohorts	Ever Arrested	Number of Arrests	Ever Incarcerated	Ln (Total Days Incarcerated)
	(1)	(2)	(3)	(4)
Share Minority in New School Zone *				
Non-Minority Female	0.014	0.082	0.023	0.189
	[0.044]	[0.446]	[0.042]	[0.187]
Non-Minority Male	0.032	0.284	0.042	0.276
	[0.045]	[0.461]	[0.043]	[0.193]
Minority Female	-0.005	-0.006	-0.001	0.057
	[0.042]	[0.479]	[0.041]	[0.201]
Minority Male	0.144***	1.534**	0.109**	0.654***
	[0.050]	[0.606]	[0.049]	[0.249]
Observations	22,329	22,329	22,329	22,329
Panel B: Middle School Cohorts				
Share Minority in New School Zone *				
Non-Minority Female	0.010	-0.126	0.016	-0.030
	[0.048]	[0.276]	[0.049]	[0.152]
Non-Minority Male	0.034	-0.103	0.030	-0.001
	[0.052]	[0.297]	[0.054]	[0.164]
Minority Female	0.030	-0.161	0.035	-0.070
	[0.052]	[0.304]	[0.053]	[0.161]
Minority Male	0.128**	0.704*	0.136**	0.417**
	[0.061]	[0.372]	[0.062]	[0.205]
Observations	21,620	21,620	21,620	21,620

Notes: Within panels, each column shows coefficients and standard errors from a separate estimate of equation (1), and is interpreted as the impact of being assigned to a school with 100 percentage points more minority students, where the impact is allowed to vary by the race and gender combinations indicated in each row. Panel A presents results for rising 9th through 12th graders in the fall of 2002, while Panel B presents results for rising 6th through 8th graders. All regressions control for race by cohort fixed effects, parcel group by prior middle and high school zone fixed effects, and quadratics in 5th grade math and reading scores plus dummies for missing scores. Criminal records are obtained from the Mecklenburg County Sheriff, and can track students who leave CMS schools. Standard errors are clustered at the Prior Zone by Parcel Group level. *** p<0.01, ** p<0.05, * p<0.10

Table 9: Impacts of Re-Zoning on Enrollment in CMS over Time

	Expected grade for on-time progression, based on 6th grade cohort					
	Grade 7	Grade 8	Grade 9	Grade 10	Grade 11	Grade 12
	(1)	(2)	(3)	(4)	(5)	(6)
Share Minority in New School Zone *						
Non-Minority Female	-0.046	-0.121**	-0.055	-0.047	-0.051	-0.060*
	[0.061]	[0.054]	[0.036]	[0.038]	[0.036]	[0.033]
Non-Minority Male	-0.063	-0.084	-0.060	-0.032	-0.020	-0.029
	[0.063]	[0.055]	[0.038]	[0.041]	[0.040]	[0.038]
Minority Female	0.016	-0.006	0.088**	0.094***	0.132***	0.137***
	[0.061]	[0.051]	[0.032]	[0.031]	[0.034]	[0.035]
Minority Male	0.012	-0.033	0.026	0.062*	0.063**	0.070**
	[0.065]	[0.047]	[0.029]	[0.031]	[0.031]	[0.032]
Sample Size	14,570	21,620	27,995	34,007	39,365	43,949

Notes: Each column shows coefficients and standard errors from a separate estimate of equation (1), and is interpreted as the impact of being assigned to a school with 100 percentage points more minority students, where the impact is allowed to vary by the race and gender combinations indicated in each row. The outcome is an indicator variable for being enrolled in any CMS school in the "expected grade" in each column. "Expected grade" is calculated as being enrolled in any CMS school in the year that a student should have been in each grade, based on the first time that student entered 6th grade. Sample sizes increase across columns because more cohorts were enrolled in the higher grades post-rezoning. All regressions control for race by cohort fixed effects, parcel group by prior middle and high school zone fixed effects, and quadratics in 5th grade math and reading scores plus dummies for missing scores. Standard errors are clustered at the Prior Zone by Parcel Group level. *** p<0.01, ** p<0.05, * p<0.10

Table 10: Impacts of Re-zoning on High School Course-Taking

Panel A: High School Cohorts	AP Math	# Advanced Math Courses	Honors / IB Math	AP Science	AP English
	(1)	(2)	(3)	(4)	(5)
Share Minority in New School Zone *					
Non-Minority Student	-0.023	-0.235*	-0.205**	-0.109*	-0.248**
	[0.081]	[0.127]	[0.103]	[0.063]	[0.097]
Minority Student	-0.025	-0.195	-0.155*	-0.090*	-0.162*
	[0.076]	[0.125]	[0.087]	[0.053]	[0.083]
Observations	15,490	16,423	15,482	16,423	16,423
Panel B: Middle School Cohorts					
Share Minority in New School Zone *					
Non-Minority Student	0.242***	0.351	0.159	0.072	0.108
	[0.091]	[0.234]	[0.101]	[0.064]	[0.125]
Minority Student	0.139*	-0.042	0.133	0.058	0.029
	[0.080]	[0.220]	[0.104]	[0.053]	[0.119]
Observations	13,026	13,102	12,964	13,102	13,102

Notes: Within panels, each column shows coefficients and standard errors from a separate estimate of equation (1), and is interpreted as the impact of being assigned to a school with 100 percentage points more minority students, where the impact is allowed to vary by race as indicated in each row. Panel A presents results for rising 9th through 12th graders in the fall of 2002, while Panel B presents results for rising 6th through 8th graders. All regressions control for race by cohort fixed effects, parcel group by prior middle and high school zone fixed effects, and quadratics in 5th grade math and reading scores plus dummies for missing scores. Advanced math courses are defined as pre-calculus and above. All results are conditional on being enrolled in CMS in 12th grade. Standard errors are clustered at the Prior Zone by Parcel Group level. *** p<0.01, ** p<0.05, * p<0.10

Table 11: Impacts of Re-zoning by Race and Income

Impact on minority males of an increase in:	Ever Arrested (1)	Number of Arrests (2)	Ever Incarcerated (3)	Ln (Total Days Incarcerated) (4)
Poor non-minorities	-0.008	-1.038	-0.037	-0.446
	[0.117]	[0.858]	[0.101]	[0.390]
Non-poor minorities	-0.054	-0.831	-0.035	-0.380
	[0.117]	[0.908]	[0.107]	[0.388]
Poor minorities	0.073**	0.637***	0.070**	0.321***
	[0.030]	[0.225]	[0.028]	[0.101]
Observations	43,949	43,949	43,949	43,949

Notes: Each column shows coefficients and standard errors from a separate estimate of equation (1), where the results are interpreted as the impact of being assigned to a school with a 100 percentage point greater share of students in the demographic group indicated in each row. Non-poor non-minorities are the reference group. Results are pooled by grade cohort. While only the coefficients for minority males are shown, the model includes all other race and gender combinations. All regressions control for race by cohort fixed effects, parcel group by prior middle and high school zone fixed effects, and quadratics in 5th grade math and reading scores plus dummies for missing scores. Criminal records are obtained from the Mecklenburg County Sheriff, and can track students who leave CMS schools. Standard errors are clustered at the Prior Zone by Parcel Group level. *** p<0.01, ** p<0.05, * p<0.10

Table 12: Impact of Re-zoning by Neigborhood Racial Composition

Impact on minority males who live in neighborhoods that are:	Ever Arrested	Number of Arrests	Ever Incarcerated	Ln (Days Incarcerated)
	(1)	(2)	(3)	(4)
0 to 20 percent minority	-0.042	0.113	-0.049	-0.072
	[0.049]	[0.352]	[0.047]	[0.158]
20 to 40 percent minority	0.064	0.355	0.068	0.114
	[0.046]	[0.284]	[0.042]	[0.130]
40 to 60 percent minority	-0.050	-0.136	-0.076	-0.159
	[0.066]	[0.546]	[0.057]	[0.189]
60 to 80 percent minority	0.063	0.591*	0.071*	0.307**
	[0.047]	[0.334]	[0.042]	[0.142]
80 to 100 percent minority	0.066*	0.689***	0.071**	0.414***
	[0.035]	[0.257]	[0.032]	[0.117]
Observations	43,949	43,949	43,949	43,949

Notes: Each column shows coefficients and standard errors from a separate estimate of equation (1), where the results are interpreted as being assigned to a school with 100 percentage points more minority students. The impacts are allowed to vary by five categories (indicated in each row) of the percent of minority residents in a student's 2000 Census Block Group. Results are pooled by grade cohort. While only the coefficients for minority males are shown, the model includes all other race and gender combinations. All regressions control for race by cohort fixed effects, parcel group by prior middle and high school zone fixed effects, and quadratics in 5th grade math and reading scores plus dummies for missing scores. Criminal records are obtained from the Mecklenburg County Sheriff, and can track students who leave CMS schools. Standard errors are clustered at the Prior Zone by Parcel Group level. *** p<0.01, ** p<0.05, * p<0.10