Segregation, Student Achievement, and Postsecondary Attainment: Evidence from the Introduction of Race-Blind Magnet School Lotteries

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Abstract

I study the effect of racial segregation on academic achievement, college preparation, and postsecondary attainment in a large, urban school district. To achieve racial balance in its oversubscribed magnet schools, this district conducted separate admissions lotteries for black and non-black students. Because the student body was predominantly black, administrators set aside disproportionately more seats for the non-black lottery. In 2003, the federal Office of Civil Rights forced this district to instead use a race-blind lottery procedure that dramatically increased racial segregation for incoming magnet school cohorts. In an instrumental variables framework that exploits both randomized lottery offers and this unanticipated shock to racial makeup, I test whether student racial composition is a meaningful input in the education production function. As a baseline, I use admissions lotteries to estimate the effect of enrolling in a magnet middle school on student outcomes. In general, enrollment returns are comparable between magnet and traditional schools, but I estimate heterogeneous magnet school effects across student subgroups. Education production is sensitive to school racial composition in that segregation has a deleterious impact on student outcomes. I find that increasing the share of black peers in a cohort decreases student achievement in math, science, and writing for black students with losses primarily driven by high-aptitude black students. Further, racial segregation erodes high school graduation rates and also decreases college attendance by reducing enrollment at 2-year institutions among female black students. These findings suggest that policies aimed at achieving racial balance in schools will likely increase aggregate educational achievement.

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1 Introduction

The landmark ruling of *Brown v. Board of Education of Topeka* ended *de jure* school segregation and spurred integration efforts across the United States education system. The assumption underlying this significant ruling is that peer racial composition is a meaningful parameter in the education production function. Specifically, the ruling assumes that racial isolation negatively affects student outcomes, particularly for minorities. However, the effects of peer racial composition on academic outcomes are still not known. Despite early integration efforts, schools nationwide are growing increasingly *de facto* segregated (Clotfelter et al., 2008, 2006; GAO, 2016; Lutz, 2011; Reardon et al., 2012). In the 2013-2014 school year, over 6.5 million students attended schools in which over 90 percent of their peers were black or Hispanic.[1] Moreover, the proportion of these high-minority-share schools have tripled over the last two decades nationwide (Orfield et al., 2016). With *de facto* school segregation on the rise, the causal link between peer racial composition and student achievement has important implications for policy.

I directly test whether peer racial composition is a meaningful input in the education production function by studying the end of race-conscious admissions lotteries in a large urban school district (LUSD). The change in the lottery regime caused magnet middle schools that were nearly racially balanced to instead enroll a high share of minority students. Thus, more specifically, I explore how a large, exogenous increase in racial segregation impacts education production in the context of magnet middle schools.

Magnet schools provide an ideal setting to explore the impact of racial segregation on academic outcomes. Magnets were established as a voluntary alternative to compulsory desegregation efforts such as busing. While being publicly funded and operated, magnets differ from traditional public schools in that they are permitted to offer specialized programs and services. They also differ in that they lack specified catchment boundaries allowing them to attract enrollment district-wide, hence the term "magnet." In theory, districts attempting to discourage racial segregation would establish magnet schools touting specialized programs within high-minority-share neighborhoods to encourage non-resident white families to enroll

[1] Author's calculations using Common Core Data from the National Center for Education Statistics.

their children. Thus, magnets promote racial balance in what would otherwise be high-minority-share schools.[2] Despite racial balance being a founding principle motivating the creation of magnet schools, we have no understanding about the extent to which racial composition drives magnet school achievement gains.

I begin by establishing the baseline effect of enrolling in a magnet school on achievement and postsecondary attainment using two decades of LUSD admissions lotteries. To my knowledge, this is the longest panel of lotteries used in any admissions lottery study to date.

I then isolate the impact of segregation on the effectiveness of magnet enrollment using an instrumental variables design based on a change in the district's lottery system. Prior to 2003, the LUSD ran magnet school lotteries separately for black and non-black students. This provided district administrators full control over the racial composition of each magnet school's entering class, allowing them to artificially improve racial balance in their admissions by providing disproportionately more offers to non-black students. In 2003, the federal Office of Civil Rights required the district to consolidate their race-specific lotteries to a system with a combined, race-blind lottery. Under this regime, the racial composition of the entering class simply mirrored the racial makeup of the lottery pool. Importantly, the lottery consolidation only affected the admissions process and left other school policies and staffing unaffected.[3] Thus, the shift in the racial composition of a school's incoming class induced by the lottery consolidation was a function of how disproportionately non-black the pre-consolidation lottery winners were compared to the racial composition of the school's entire lottery pool.

Following this intuition, my identification strategy isolates the exogenous variation in magnet school racial composition that is induced by the district's lottery consolidation. I instrument for the racial composition in a student's enrolled school using a measure of how "disproportionately non-black" the school's lottery winners were prior to the consolidation interacted both with indicators for whether the student won a magnet lottery and whether the lottery occurred after the consolidation. The main threat to this strategy is if unobserved

[2] However in practice, Rossell (2003) finds that adding voluntary magnet programs to a district's desegregation plan has little impact on exposure to other races.

[3] Staff reshuffling could result from changes in student demographics (Jackson, 2009), however, none of these changes were structurally linked with the lottery consolidation and can be considered part of the segregation treatment.

determinants of the effect of magnet offers on student outcomes are trending differentially for magnets with more "disproportionately non-black" lottery offers in 2002. I test this assumption in an event study framework and find no evidence for the existence of such trends.

The other main threat to the validity of this strategy is if changes to the composition of the lottery pool after 2002 are correlated with "disproportionately white offers" for magnet schools. I test for changes in the composition of the lottery pool and find that magnet schools with more "disproportionately non-black offers" have a higher proportion of black and male applicants following the termination of race-based admissions. However, I find no evidence for such compositional changes in baseline achievement both with and without conditioning on student race and gender. Thus, by exploring heterogeneous effects of segregation by race and gender subgroups, I remove these compositional effects and can isolate the impact of segregation on student outcomes.

My baseline estimates reveal that the returns to magnet middle school enrollment are generally statistically indistinguishable from traditional school enrollment. However, magnet middle schools boost science achievement for non-black students as well as female students and increase ACT test taking among non-black as well as low-achieving students. The localized nature of the returns to enrolling in a magnet school relative to a traditional public school highlights the similarities between both institutions. Thus, I argue that any effects of segregation I estimate within magnet schools may generalize to traditional schools more broadly.

The end of race-conscious admissions lotteries led to an immediate 7 percentage point increase in segregation among magnet middle schools as measured by the exposure index (Massey and Denton, 1988).[4] This is slightly larger than the immediate change in exposure index resulting from the end of forced busing in Charlotte-Mecklenburg (Billings et al., 2014) or roughly half the effect of court-ordered desegregation in the 1960s and 70s (Guryan, 2004; Rossell and Armor, 1996). Racial segregation in magnet schools has deleterious effects on student outcomes. A 10 percentage point increase in the share of black peers at a student's

[4]The exposure index measures the probability that a randomly chosen peer of a minority student is also a minority.

school, which represents an increase in racial segregation, decreases student achievement by roughly 0.12 standard deviations. These effects are slightly larger than other estimates in the literature (Billings et al., 2014; Hanushek et al., 2009; Hoxby, 2000). Similar to Hanushek and Rivkin (2009), I find that the losses from segregation are concentrated among high-achieving black students. Segregation in magnet middle schools also decreases the probability of graduating from high school and later enrolling in a two-year college for black female students. I conclude that racial balance is an important input into the education production function in the magnet schools I study.

This paper makes contributions to two different literatures. First, I add to the literature studying the returns to magnet schools by providing the first lottery evidence of magnet middle school attendance on postsecondary attainment. Second, I contribute to the literature studying the effect of school segregation and peer racial composition on student academic outcomes by leveraging a natural experiment that is better suited to isolate the contribution of peer racial composition on education production. Previous studies have assessed changes to peer racial composition driven by naturally-occurring variation in cohort- or classroom-specific racial composition (Hanushek et al., 2009; Hanushek and Rivkin, 2009; Hoxby, 2000; Vigdor and Nechyba, 2007). However, by construction, these strategies exploit very small differences in peer composition, which plausibly affect student achievement differently than policy reforms that generate large changes to peer composition.[5] Other studies assess policies that induce large shifts to peer composition including the introduction of court-ordered desegregation (Guryan, 2004; Johnson, 2015) or its termination (Billings et al., 2014; Gamoran and An, 2016; Lutz, 2011), inner-city busing (Angrist and Lang, 2004), a change in attendance zone boundaries (Billings et al., 2014; Vigdor and Nechyba, 2007), or mandated school reassignment (Hoxby and Weingarth, 2006). However, these studies do not occur in settings where students are explicitly randomly assigned to schools. Because my empirical strategy leverages explicit randomization into schools both before and after a large shift in school segregation, I am able to provide a cleaner estimate than previous work of how school racial

[5] Vigdor and Nechyba (2007) estimate racial composition peer effects using classroom-specific variation, but conversely find no evidence for racial peer effects when limiting the analysis to year-to-year variation induced by changes to school assignment policies.

composition enters into the education production function.[6]

My findings are highly relevant for current education policy along a number of dimensions given the resurgence of *de facto* segregation over the past few decades in the United States. Because racial balance appears to be an important factor in the education production of magnet schools, districts may have additional justification to implement policies that promote racial balance.[7] In fact, President Obama's "Stronger Together" initiative currently proposes to double the amount of federal funding up to $120 million to improve voluntary integration efforts across the Unites States, of which magnet programs are a part.

I argue that the negative effect of segregation is likely not specific to the magnet school setting. Magnet schools in this district are statistically comparable to traditional schools with regard to school inputs, peer composition, and general returns. Moreover, my identification strategy is able to isolate the effect of segregation from any magnet-school-specific inputs. Thus, the deleterious effect of segregation on academic outcomes plausibly generalizes to other school settings.

2 Prior Literature

Why might segregation impact student outcomes? One potential explanation involves the direct influence of a student's peer group. In Florida, Carrell and Hoekstra (2010) find that black and low-income students are more likely to experience domestic violence at home and are more likely to be disruptive in the classroom as a result. Further, Carrell and Hoekstra (2010) find that exposure to these disruptive peers decreases reading and math test scores for students in this district.[8] If these findings hold in this LUSD, then increasing the share of black students in schools may increase the probability of exposure to disruptive peers.

[6] Two recent studies assess the effect of peer racial composition using regression discontinuity methods based on entrance exams scores to attend highly selective exam schools in Boston and NYC (Abulkadiroglu et al., 2014; Dobbie and Fryer, 2014). Students narrowly gaining admission experience a drastically less racially diverse peer group than narrowly failing students. However, unlike in my setting, these works are unable to disentangle the effect of peer racial composition from the effect of attending the exam school (e.g., having access to better teachers or academic resources).

[7] Enforcing racial diversity in schools is complicated due to recent court cases, such as *Parents Involved in Community Schools v. Seattle School District No. 1* in 2007, which prevents districts from utilizing race in admissions decisions.

[8] However, Hoxby and Weingarth (2006) find little support for "bad apple" models of peer effects (Lazear, 2001) in North Carolina.

In a similar vein, because black students in this district have lower average test scores compared to non-black students, increasing segregation will decrease the average baseline achievement of a student's peer group. There is a large, mixed literature testing for the presence of peer achievement effects in schools (see Sacerdote, 2011, for a detailed review). Also, if teachers adjust how they teach to the aptitude of the average student, then decreasing peer baseline achievement could affect the teacher's contribution to student outcomes (Duflo et al., 2011).

It is impossible to separately isolate the contribution of these different potential mechanisms within my setting. Instead, this study provides estimates for the combined impact of these channels on student outcomes. However, my reduced-form estimates of segregation effects are inherently interesting to researchers and policy makers faced with evaluating the impact of a policy that will influence school racial composition.

This study contributes to two different literatures. The first is a growing, but mixed literature estimating the short- and medium-run academic returns to attending magnet schools using lotteries. Several studies estimate limited-to-no academic returns to magnet attendance (Abulkadiroglu et al., 2014; Cullen and Jacob, 2007; Cullen et al., 2006; Dee and Lan, 2015; Dobbie and Fryer, 2014; Engberg et al., 2014), while other studies estimate academic returns roughly half the magnitude of lottery-based charter school estimates (Bifulco et al., 2009; Crain et al., 1992; Hastings et al., 2012).[9] However, we know far less about long-run returns to magnet enrollment particularly in the United States.[10] Several studies explore the short-, medium-, and long-run returns to open enrollment systems, of which magnet schools are a part (Deming, 2011; Deming et al., 2014; Hastings et al., 2006, 2009). However, these studies do not separately report estimates for magnet enrollment. Hoxby (2003) asserts that magnet schools should not be considered as part of the school choice movement. She argues that magnet schools predate the school choice movement and do not provide the same financial incentive structure as traditional school choice programs, which further motivates

[9] Cullen et al. (2006) and Engberg et al. (2014) estimate limited academic returns to magnet enrollment, but do find behavioral effects. Crain and Thaler (1999) find positive effects for some types of magnets and null or negative effects for other types.

[10] Crain and Thaler (1999) provide qualitative evidence about postsecondary attainment and compare in-depth survey responses between lottery winners and losers for 110 students. Park et al. (2015) find that magnet attendance increases the probability of attending college in rural China.

separately exploring the contribution of magnets. I contribute to this literature by providing the first estimates of the effect of magnet middle school enrollment on postsecondary attainment using administrative lottery data.

Second, this work contributes the to large literature assessing the impact of segregation and racial peer effects on student outcomes.[11] While there are studies that estimate little-to-no effect of student racial composition on achievement (Abulkadiroglu et al., 2014; Dobbie and Fryer, 2014; Gamoran and An, 2016; Hoxby and Weingarth, 2006), many others find that increasing the share of minority peers negatively impacts student achievement and behavioral outcomes particularly among minority subgroups and females (Angrist and Lang, 2004; Billings et al., 2014; Guryan, 2004; Hanushek et al., 2009; Hanushek and Rivkin, 2009; Hoxby, 2000; Lutz, 2011; Vigdor and Nechyba, 2007).[12]

To overcome selection biases, several studies rely on quasi-random variation in racial composition generated from naturally occurring cohort- or classroom-specific variation (Hanushek et al., 2009; Hanushek and Rivkin, 2009; Hoxby, 2000; Vigdor and Nechyba, 2007). However, the quasi-randomization that allows these studies to address selection concerns also reduces variation in racial composition. Thus, these studies identify effects from small fluctuations in racial composition, which potentially impact student outcomes differently than a large, policy-induced shift in racial composition. Other studies utilize policies that induce large shifts in racial composition such as the introduction of court-ordered desegregation (Guryan, 2004; Johnson, 2015) or its termination (Billings et al., 2014; Gamoran and An, 2016; Lutz, 2011), inner-city busing (Angrist and Lang, 2004), the change in attendance zone boundaries (Billings et al., 2014; Vigdor and Nechyba, 2007), or mandated school reassignment (Hoxby and Weingarth, 2006). These studies lack the benefits that exploiting randomized peer composition provides for identification, though each study goes to great lengths to show that

[11] See Gamoran and An (2016) for a full review of the literature estimating the effect of segregation on student achievement and Vigdor and Ludwig (2008) for a review on the literature relating neighborhood and school segregation to the black-white test score gap.

[12] Hoxby and Weingarth (2006) find evidence that peer race and ethnicity have only slight effects once conditioning on peer achievement. Vigdor and Nechyba (2007) find that school-wide racial composition does not significantly predict achievement, however, they estimate that non-black students in classrooms with a disproportionately share of black students experience lower math achievement. Johnson (2015) finds that school desegregation positively impacts adult outcomes and that school funding is the likely mechanism as opposed to any direct effects of changing the racial composition of a student's peers.

identification assumptions are met.

The work of Billings et al. (2014) is most closely related to this paper. Billings et al. (2014) study the effect of segregation induced by the end of forced busing in Charlotte-Mecklenburg. They compare students who live in the same neighborhood and school zone prior to the end of forced busing, but then live on opposite sides of newly drawn school catchment boundaries, and thus, go on to attend schools with drastically different peer racial compositions. They find that segregation decreases high school achievement for white and minority students as well as lowers graduation rates and college attendance among white students and increases crime among minority males. My study compliments this seminal work in several ways. First, schools in my setting were not allocated compensatory resources due to increased segregation, which Billings et al. (2014) show may have mitigated segregation effects for younger cohorts in their study.[13] Second, because students in my setting are explicitly randomly assigned to schools both before and after the segregation treatment, my natural experiment is better situated to cleanly isolate racial composition effects.[14] Finally, because this article and the work of Billings et al. (2014) are studying different policies, both studies generate policy implications better suited to their respective settings. The findings of Billings et al. (2014) are more relevant to assessing the effects of a policy that ends forced busing and changes school zone assignment, while the results in my setting are more relevant to understanding the implications of a policy that ends race-conscious lotteries.

Two recent papers have explored the effect of peer composition on student outcomes using regression discontinuity evidence. These studies compare students near the admissions cutoffs to top exam schools in Boston and New York where the composition of peers are drastically different for students who are provided or denied admission (Abulkadiroglu et al., 2014; Dobbie and Fryer, 2014).[15] Unlike the previous work in this literature, these studies exploit both random variation in test scores near the admissions threshold as well as markedly different peer compositions experienced by treated and untreated students. However, it is

[13] Reber (2010) also shows that desegregation effects are attributed to increased resources rather than peer racial composition.

[14] For example, Billings et al. (2014) find that white students who are assigned to school zones with a higher minority share are more likely to attend a magnet program. They note that if the relative returns to magnet attendance are positive then this would place upward pressure on segregation effects for white students.

[15] Exam schools are highly selective magnet schools with strict admissions cutoffs.

difficult to distinguish peer effects from the effect of exposure to the exam school teachers and other exam-school-specific effects. Further, students who narrowly gain and lose admission will be near the bottom and top of the baseline achievement distributions in their respective schools. Thus, these regression discontinuity studies potentially conflate any effect of a student's class ranking with peer effects.

I contribute to the literature studying segregation and racial peer effects by providing a cleaner estimate than previous work of how school racial composition enters into the education production function. My study strikes a balance between the studies exploiting explicit random variation in peer composition and those with large, policy-driven shocks to peer composition. Because students are randomized into magnet schools, my empirical strategy benefits from the virtues of randomization, while simultaneously leveraging a large shift in peer composition due to the end of race-based admissions. Further, relative to the regression discontinuity studies, my work is able to study heterogeneous effects across baseline student aptitude and does not suffer from conflating any class ranking effects.[16] Additionally, I am able to isolate the effect of peer composition from magnet-specific effects because the segregation effects I estimate are identified off of changes in the returns to magnet enrollment across the policy change.

3 Institutional Details

3.1 Magnet Schools in this Large Urban School District

Magnet schools are similar to traditional schools in that they are publicly funded and run. All LUSD schools use the same general curriculum, but magnet schools can differ in the instruction methods used. Magnets can also emphasize a particular focus of instruction, e.g., performing arts, bilingual education, STEM, or International Baccalaureate programs. Magnet schools also differ in that they lack specified catchment boundaries allowing them to attract enrollment district-wide, hence the term "magnet." In addition to the district's traditional public schools, the LUSD ran roughly 10 to 15 magnet middle schools throughout

[16] Because the 6 exam school cutoffs hit at different parts of the student baseline achievement distribution, the estimates in Abulkadiroglu et al. (2014) reflect both high- and moderate-ability students.

the time period of this study.[17]

As was the case with magnet school programs across the United States, a founding principal underlying this LUSD's magnet program was to improve racial balance and prevent "white flight."[18] Shortly after the first LUSD magnets launched in the 1970s, the district also began the mandatory busing of subsets of students to desegregate schools through the 1990s. During this time, the magnet program coexisted with forced busing as an effort to discourage middle class families from migrating to the suburbs.

Because the demand for these magnet schools far outpaced supply, magnet seats were filled via randomized lotteries. To ensure racial balance, the district held separate school-specific lotteries for black and non-black students.[19] Each year the district set a universal target for the racial composition of new enrollment that reflected the racial make-up of the district as a whole. The district then set admissions quotas for each race-specific lottery to hit the district-wide target. Black students disproportionately applied to magnet schools and so students in the non-black magnet lottery had a better chance at receiving a seat offer than students in the black magnet lottery. In the 2002-03 school year, the federal Office of Civil Rights required the LUSD to instead utilize a combined, race-blind lottery system comparable to the system in Durham County, North Carolina (Clotfelter et al., 2008).[20]

In addition to filling magnet school seats with lotteries, the LUSD allowed students to apply to transfer to other oversubscribed traditional schools via the same centralized lottery. Students applied to up to three schools and did not specify a rank ordering. Lotteries would occur at the school-grade level. Once offers were made, students had roughly one week to respond. If parents failed to respond, the seat was forfeited to the next waitlisted student. Conversely, if a student accepted a seat in a school, they were automatically withdrawn from all other waitlists. Once the district was notified that an offered seat had been declined,

[17]Exact magnet counts are purposefully withheld to maintain the anonymity of the district.

[18]See Rossell (2005) for a detailed history of the emergence of magnet schools in the United States.

[19]This was a common practice for over-subscribed magnet schools across the nation. Chicago, for example, ran separate lotteries based on both gender and race (Cullen et al., 2006).

[20]The LUSD moved to a simple race-blind lottery as opposed to a race-neutral, place-based system such as with Chicago Public Schools (Ellison and Pathak, 2016) where student need is instead determined using aggregated residential neighborhood information. Other districts approached achieving race-blind balance within schools by instead incorporating information about student socioeconomic status and achievement as in Wake County, North Carolina (Clotfelter et al., 2008; Hoxby and Weingarth, 2006).

subsequent offers were determined by moving down a randomized waitlist.[21]

To explore how race-blind lotteries impact the racial composition within LUSD schools, Figure 1 presents the percentage of black students enrolled in traditional and magnet schools across the time period of the study, 1998 to 2007. From 1998 to 2002, even despite utilizing race-specific lotteries, magnet schools enrolled a higher proportion of black students than traditional schools.[22] Upon the introduction of race-blind lotteries in 2003-04, district administrators lost their control over the racial balance of admissions resulting in roughly a 7 percentage point increase in the black-share within magnet schools over the next few years. This also equates to roughly a 7 percentage point increase in the exposure index (Massey and Denton, 1988), which is slightly larger than the immediate increase in the exposure index due to the end of forced busing in Charlotte-Mecklenburg (Billings et al., 2014) or half the size of court-ordered desegregation in the '60s and '70s (Guryan, 2004; Rossell and Armor, 1996).

3.2 No Child Left Behind

In 2002, the No Child Left Behind (NCLB) Act was signed into law as an update to the Elementary and Secondary Education Act of 1965. Because NCLB and race-blind lotteries were contemporaneously implemented, NCLB accountability measures present potential concerns for the validity of my identification strategy. In this section, I provide details about how this district implemented NCLB. I reserve discussing how NCLB may threaten the validity of my estimation strategy for section 6.3 to allow the discussion to occur in the context of my empirical method.

One of the earliest consequences for a school that fails to meet NCLB-determined academic requirements is to be subjected to increased competitive pressures through school choice. Starting in the 2003-04 school year, the LUSD required every school in the district (including magnet schools) to set aside a portion of their seats for the NCLB placement

[21]The LUSD generated separate waitlists for students with and without siblings at the school. After the initial lottery offers and responses were processed, any seats not accepted were offered to students on these waitlists in an alternating fashion. Specifically, the first seat was offered to a student on the sibling waitlist, then the next was offered from the non-sibling waitlist, the third was from the sibling waitlist, etc.

[22]Recall that magnet schools are purposefully built in particularly high-minority-share neighborhoods.

mechanism.[23]

Students across the district were ranked using two inputs: the student's baseline testing and family income, where a low ordinal ranking signified the lowest achieving, poorest students in the district. Students currently assigned to a traditional school in the district that failed to meet NCLB-determined academic measures were eligible to participate in NCLB school placement. Prior to the magnet school admissions lotteries, students from these failing schools would rank order up to three schools in the district into which they wanted to transfer. The student with the lowest rank (most disadvantaged) was placed first, followed by the next lowest ranked student, and so on. If the student's first-choice school had no more NCLB seats, then the student would be placed in their second-, or third-choice school. If all three choices were full, the student would not receive a NCLB-seat and would have to apply to the magnet school lotteries as before. After NCLB seats were determined, the (now race-blind) magnet school lotteries were carried out normally as explained in Section 3.1.

4 Data

I use student-level administrative data from a large urban school district (LUSD) from 1998 through 2007. As a condition to access their data the district requested complete anonymity. This district enrolls roughly 40 to 60 thousand students in traditional schools and 10 to 15 thousand students in magnet schools in any given school year.

In addition to statewide standardized achievement measures and student demographic information, the district also merged student information to several medium- to long-run student outcomes.[24] The district matched student records with ACT/SAT achievement from 2004 through 2011 and also merged student records for each graduating class with college information collected by the National Student Clearinghouse (NSC).[25] NSC data include the name of each college attended and the student's major as well as whether and when they graduated from college. The NSC covers all public and private, two- and four-year

[23]LUSD magnet middle schools set aside roughly 20% of their 6th grade seats for NCLB placements.

[24]Student demographic information is only available from 2000 onward, but I infer student race for earlier years based on which race-specific lottery they utilize.

[25]I am in the process of matching NSC records to all lottery applicants to avoid any differential attrition concerns.

postsecondary institutions in the United States allowing me to observe students attending out-of-state schools.[26] The LUSD combined these student-level data with admissions lottery records over the same time horizon which includes information on which schools each student applied in a given year and any seat offers. From waitlist information, I can deduce which students were offered seats during the initial wave, hereafter denoted "initial offers." I am also able to observe basic demographic information for all teachers in the district and, for 2000 and later, I can link students to their teachers and classmates.

Prior to any sample restrictions, I observe roughly 50,000 6th grade students from 1998 to 2007.[27] Data contain students attending any of the traditional or magnet public schools within the LUSD, thus I cannot observe any students who transfer to a charter or private school or who move out of the area entirely. Table 1 presents descriptive information about the student composition of this LUSD. Column 1 shows that for the full sample, the district is composed almost entirely of black and white students (cumulatively 92%) with a majority of the district being comprised of black students. Because race-specific admissions lotteries were conducted separately for black and non-black students, I similarly consider students of other races and ethnicities as non-black throughout the paper.

On average, black students in this district test below non-black students. Figure 2 displays the distribution of scores among black and non-black students in the district broken out by subject. The distribution of black test scores lies to the left of the non-black distribution for all subjects.

I restrict the sample to students who have applied to at least one magnet school in 6th grade and do not come from a sending school with automatic placement in a magnet middle school. The sample is further restricted to students without sibling priority in any magnet lottery. I also exclude lotteries from the 2001-02 school year because observable student characteristics fail to balance across lottery winners and losers for this year. Finally, given these restrictions, I drop any students who are the only ones in the district applying to the

[26] See Dynarski et al. (2013) for further details on NSC coverage rates across institution types.

[27] Starting in 2008-09, the LUSD set aside a third of the seats within several of the most popular magnet schools for a separate selective-admissions lottery. Students who were categorized as "Gifted" or who tested in the top 5 percent of the district on a standardized test in 6th grade were eligible to apply using this smaller lottery. Because eligible students were disproportionately white, the share of white students in magnet schools rapidly increased starting in the 2008-09 school year. As a result, for this paper, I restrict attention to lotteries occurring prior to 2008-09.

given magnet lottery after other sample restrictions are applied.

Column 3 of Table 1, shows descriptive information for all students in this baseline estimation sample. The sample for this table further requires that students have valid reading achievement outcome information. These conditions limit the sample to roughly 6,000 student-year observations for 6th grade applicants. Students in this lottery sample are more likely to be female and black. Additionally, students in the magnet school regression sample have higher baseline achievement performance across all four subjects.

5 Estimating the Baseline Returns to Magnet School Attendance

5.1 Magnet School Lottery

As a baseline, I establish how magnet schools compare to traditional schools by estimating differences in school inputs, peer composition, student achievement, and long-run outcomes for students who win a magnet school lottery seat relative to those who do not. Specifically, I estimate

$$y_{il} = \rho M_{il} + \Gamma_{2l} + \gamma' X_i + \epsilon_{il} \tag{1}$$

where y_{il} is an outcome for a student i who applies to the 6th grade magnet school lottery l.[28] X_i is a vector of pre-lottery demographics that includes indicator variables for student race (black or non-black) and gender. Similar to Billings et al. (2014), X_i also includes quadratics in pre-lottery baseline reading, math, science, and writing achievement as well as missing achievement indicators for each subject. M_{il} is an indicator equal to one if the student enrolled in a magnet school during the year following the lottery.[29] Γ_{2l} are lottery indicators, i.e., a unique application-school-by-lottery-type-by-year combination.[30] Because the unit of observation is a student-application, standard errors are two-way clustered by

[28]Note that if a student applies to multiple 6th grade lotteries the outcome is common across all lotteries.
[29]Students are counted as being enrolled in a magnet school if they are enrolled for one or more days.
[30]Lottery type refers to black, non-black, or race-blind lotteries.

student and the enrolled school after the lottery in 6th grade. Further, regressions are weighted by the inverse of the number of applications submitted by the given student so that each student contributes equally to the regression.

If magnet enrollment were randomly assigned, then ρ would give the causal effect of attending a magnet school in sixth grade on the given outcome. However, any unobserved determinants of student outcomes that also correlate with the decision to enroll in a magnet school would bias my estimate of ρ. The existence of such unobservable correlates seems likely given that magnet school applicants have higher baseline standardized test scores and are more likely to be black than other traditional public school students as shown in Table 1. As a result, I instrument for magnet enrollment using exogenous lottery offers through the following first-stage:

$$M_{il} = \Gamma_{1l} + \beta' X_i + \pi Z_{il} + \eta_{il} \ , \tag{2}$$

where Z_{il} is an indicator variable equal to one if student i receives an initial magnet offer in lottery l. In a comparable estimation framework, Angrist et al. (2016) use both initial lottery offers as well as whether the student ever receives an offer as instruments to assess the returns to charter school enrollment. However, in my setting, because students do not rank their school preferences and once a student accepts a lottery offer they are automatically removed from all other waitlists, subsequent lottery offers from randomized waitlists are endogenous. To see this, suppose that wealthier families are more willing to wait for a magnet seat in their preferred school and that low-income families are more likely to accept the first school offer they receive. If this is the case, then while the set of initial offers should have an equal share of high- and low-income students offered a seat, there would be a disproportionately larger share of lottery offers that are *ever* extended to high-income families from the waitlist because they are more likely to have waited.

To ensure that lotteries only compare students with the same probability of receiving a magnet offer, all regressions condition on a full set of lottery effects Γ_l. Students share a lottery if during the same year they apply to enter the same magnet school in 6th grade through the same type of lottery (i.e., black, non-black, or consolidated race-blind lottery).

If offers are truly random, then predetermined student characteristics should be equally represented or "balanced" across winners and losers within lotteries. I test for lottery balance by regressing student observables on an indicator for whether the student receives a magnet offer to the given lottery's reference school and a full set of lottery fixed effects. Column 5 of Table 1 presents these tests. Overall, lottery winners are comparable, on average, to losers across these observable dimensions. While not statistically different than zero, students with higher baseline reading test scores appear marginally less likely to win a seat. The combined *p*-value in the table is for a test of joint significance of the difference between lottery winners and losers across all outcomes. While this difference is statistically different at the 10 percent level, these individual offer differentials are comparable to other lottery studies in the literature (e.g., Abdulkadiroglu et al., 2011; Angrist et al., 2016). As a precaution, I include race and gender as well as baseline subject-specific achievement as controls throughout the paper. Overall, these regressions provide evidence that initial lottery offers are indeed random.

My empirical strategy is similar to Cullen et al. (2006), who estimate the reduced-form effect of receiving a school lottery offer on achievement using application-level data. Cullen et al. (2006) are inherently interested in the effect of additional schooling options and so they focus on the direct effect of receiving a lottery offer on student outcomes. Because I am specifically interested in estimating how magnet enrollment impacts student outcomes, I instead pursue a two-stage least squares approach (2SLS) that provides the causal effect of *enrolling* in a magnet school among the set of students that are induced to enroll by the randomized lottery offers (Imbens and Angrist, 1994).

Further, it is important to emphasize that just as in Cullen et al. (2006) and Cullen and Jacob (2007) the unit of observation in my setting is a student-application. Thus, students who apply to multiple magnet schools will appear in the data multiple times.[31] As a result, a student who wins one lottery and loses another will contribute to the treatment and control groups of the respective lotteries. Cullen and Jacob (2007) explain that this setup still produces consistent parameter estimates because randomization ensures that while some proportion of lottery winners also won seats in other lotteries, this is also the case among

[31] Students can apply to up to 3 magnet schools and, on average, students end up applying to 2 schools.

lottery losers. However, Cullen and Jacob (2007) go on to highlight that multiple applications do influence the magnitude of the treatment effect, because differences in outcomes between average lottery winners and losers will be more similar. An alternate strategy would be to employ a nested model that incorporates multiple magnet choices and student-year-level data similar to Angrist et al. (2016). However, subsequent segregation estimates (see Section 6) require the use of application-level data. Thus, to make baseline estimates more comparable with subsequent segregation estimates, I utilize the same application-level data in both settings.[32]

6 Estimating Peer Racial Composition Effects

To ensure a pre-determined level of racial diversity in its magnet schools, this LUSD held separate lotteries for black and non-black students to fill seats in oversubscribed schools through the 2002-2003 school year. In subsequent years, this district instead used race-blind lotteries where the probability of winning the lottery was the same regardless of race or ethnicity. Figure 3 depicts how the introduction of race-blind lotteries impacted the probability of winning a magnet lottery each year by student race. Prior to 2003, because black families disproportionately applied to magnet schools, non-black students were 15 to 20 percentage points more likely to win an initial lottery seat than black students. The introduction of race-blind lotteries, denoted by the reference line in 2003, caused both black and non-black students to have nearly identical, albeit much lower probabilities of winning.

The large drop in win probability is due to the introduction of NCLB. In addition to consolidating the lotteries, in 2003-04, the LUSD implemented NCLB school choice requirements by setting aside seats in schools across the district for the least-proficient, lowest-income students from failing schools (see Section 3.2). Because these students are disproportionately black, the NCLB placement mechanism potentially further exacerbated racial imbalance within magnets. The drop in the probability of acceptance reflects the decrease in the number of seats available to be filled via lottery.

[32] I also estimate baseline regressions using a framework comparable to Angrist et al. (2016) and find qualitatively similar results. These estimates are available upon request.

While the lottery regime change impacted the composition of the 2003-04 entering class, the adjustment did not directly affect magnet school curricula or teaching staff.[33] However, the concurrent passage of NCLB presents a possible confounder. Thus, simply comparing estimates of the effect of attending a magnet school before and after the lottery consolidation in 2003 would conflate any NCLB-driven impacts.

Prior to 2003-04, the racial composition of students receiving initial seat offers did not necessarily reflect the composition of the full applicant pool, but did so thereafter. To account for potential structural changes outside of the termination of race-based lotteries, I leverage the fact that the size of the shift in racial composition due to the lottery change varied by how "disproportionately non-black" that lottery offers were for each school. I measure how "disproportionately non-black" that offers were for a given school by calculating the difference between the percentage of black students in the lottery pool for the school to the percentage of black students receiving an initial magnet school offer during the 2002-03 school year (denoted $DPB^{'02}$).

This $DPB^{'02}$ measure is useful because the larger the difference the larger the potential shift in school racial composition upon the lottery consolidation. To see this, consider a school (call it school A) where 80% of all 2002 lottery applicants were black, but due to the dual lottery system, the school offered only 50% of the seats to black students. Conversely, consider school B, where 50% of the students in the applicant pool were black and also that 50% of the students who received an initial offer were black. Supposing that the composition of the student applicant pool remains roughly the same from 2002 to 2003, after consolidation, the composition of black students offered a seat to school A would rise to 80% to mirror the applicant pool, while the racial composition of lottery offers to school B would remain unchanged.

I isolate the exogenous shift in school segregation due to the establishment of race-blind

[33] Staff reshuffling could result from changes in student demographics (Jackson, 2009), however, none of these changes were structurally a part of the lottery consolidation and can be considered as part of the re-segregation treatment.

lotteries in an instrumental variables framework by estimating the following first-stage:

$$
\begin{aligned}
(3) \qquad \%\text{Black}_{il} &= \rho DPB_l^{'02} * \mathbb{1}(\text{Post }'02)_t * \mathbb{1}(\text{Offer})_{il} \\
&+ \kappa_1 \mathbb{1}(\text{Post }'02)_t * \mathbb{1}(\text{Offer})_{il} + \delta_1 DPB_l^{'02} * \mathbb{1}(\text{Offer})_{il} \\
&+ \theta_1 \mathbb{1}(\text{Offer})_{il} + \gamma_1' X_i + \Gamma_{1l} + \nu_{il}
\end{aligned}
$$

with the accompanying second-stage

$$
\begin{aligned}
(4) \qquad y_{il} &= \beta \ \widehat{\%\text{Black}}_{il} \\
&+ \kappa_2 \mathbb{1}(\text{Post }'02)_t * \mathbb{1}(\text{Offer})_{il} + \delta_2 DPB_l^{'02} * \mathbb{1}(\text{Offer})_{il} \\
&+ \theta_2 \mathbb{1}(\text{Offer})_{il} + \gamma_2' X_i + \Gamma_{2l} + \epsilon_{il} \ ,
\end{aligned}
$$

where $\%\text{Black}_{il}$ is the leave-one-out percentage of black students enrolled in the school that student i attends during the year following the given lottery l.[34] $DPB^{'02}$ is the 2002-03 application-school-specific difference in the percentage of black students in the lottery applicant pool relative to the percentage receiving an initial offer for the application school in lottery l. Specifically, $DPB^{'02} = 100 * \left(\frac{\sum_{i \in j} \mathbb{1}(\text{Black})_i}{N_j} - \frac{\sum_{i \in j} \mathbb{1}(\text{Black})_i \cdot \mathbb{1}(\text{Offer})_i}{\sum_{i \in j} \mathbb{1}(\text{Offer})_i} \right)$, where N_j is the total number of applicants to school j.[35] $\mathbb{1}(\text{Post }'02)$ and $\mathbb{1}(\text{Offer})$ are indicator variables respectively equal to one if the current lottery occurs strictly after the 2002-03 school year or if the student receives an initial seat offer in lottery l.[36] X_i and Γ_l are respectively the same set of pre-lottery characteristics and lottery-specific fixed effects from equation (1). Similarly, standard errors are again two-way clustered by student and school-after-lottery and regressions are weighted by one over the number of applications submitted by the given student in the given year so that each student equally contributes to the estimation.

I instrument for the percentage of black students using the triple interaction between my measure of lottery racial imbalance ($DPB^{'02}$), an indicator for whether the lottery occurred

[34]The leave-one-out percentage is calculated by ignoring the reference student and calculating the given statistic for the remaining 6th grade students in the school.

[35]Several magnet schools have $DPB^{'02}$ values near 0, while others have values ranging up to a 10 percentage point difference.

[36]$\mathbb{1}(\text{Post }'02)$ and $DPB^{'02}$ main effects are absorbed by lottery effects.

after 2002, and an indicator for whether the student received an initial magnet offer. To understand the interpretation of the coefficient ρ, consider two schools A and B where the 2002 lottery racial imbalance in A is one percentage point larger than B. ρ provides the differential effect of winning an initial seat after 2002 in the lottery for school A relative to B on the percentage of black students that will enroll in the student's 6th grade school. In other words, this instrument isolates the variation in racial composition that is induced by the change in the lottery regime across schools with differing levels of underlying lottery racial disparity. The identification assumption is that unobserved determinants of magnet school effects are not trending differentially by DPB'^{02}. For example, suppose that the schools in the neighborhoods experiencing "white flight" are also steadily declining in their effectiveness. If "white flight" is trending upwards in neighborhoods where high-DPB'^{02} schools are located, then trends in "white flight" and school effectiveness would bias my estimates.[37] However, in Section 6.1, I provide event studies that show little evidence for differential trends in school composition and productivity across school DPB'^{02} values.

This empirical strategy is able to account for a variety of potential confounders. First, I can handle changes to policies that are contemporaneous with the lottery consolidation. As long as other policy changes do not differentially affect schools by DPB'^{02} then these potential confounders will be controlled for directly by the interaction between the initial offer and post-2002 binaries. Further, suppose that magnet schools that have higher lottery racial disparity are generally better schools. Then, the interaction between DPB'^{02} and the initial offer binary controls for this directly as long as the effectiveness of these schools do not change after 2002.

It is important to note that while I am estimating the effect of a plausibly exogenous racial composition shock, I will be unable to disentangle any other composition changes happening simultaneously. For example, because black students in this district test lower on average than non-black students, an exogenous increase in the percentage of black students at the school will likely decrease the average baseline standardized achievement as well. Thus, if peer achievement is the actual mechanism that affects own achievement, an ability peer

[37] Gamoran and An (2016) find that upon the termination of court-ordered desegregation in Nashville, academically selective magnet schools became more white and non-selective magnets more black.

effect would appear like a race peer effect. However, this is still an interesting parameter to estimate. Policy-makers aiming to increase racial diversity in schools are simultaneously changing not only racial make-up, but also socioeconomic status, aptitude, and an array of other student and teacher demographics (Jackson, 2009). As a result, while I am unable to isolate the effect of racial diversity on student outcomes, per se, I can estimate parameters relevant to real-world desegregation policies.

6.1 Validating the Instrumental Variables Strategy

In this section, I test whether the causal effect of winning a magnet seat trends differentially by my measure of pre-consolidation lottery racial disparity. I assess the power of my first stage by regressing the reduced form analog of equation (3) where I interact initial offers and lottery racial disparity with year indicators instead of a post-2002 binary. Specifically, I estimate

$$(5) \quad \%\text{Black}_{ijl} = \sum_{\substack{t\,=\,1998;\\ t\,\neq\,2002}}^{2007} \Big\{ \rho_t DPB_l^{'02} \cdot \mathbb{1}(\text{Year} = t)_t \cdot \mathbb{1}(\text{Offer})_{il} + \kappa_t \mathbb{1}(\text{Year} = t)_t \cdot \mathbb{1}(\text{Offer})_{il} \Big\}$$
$$+ \delta_1 DPB_l^{'02} \cdot \mathbb{1}(\text{Offer})_{ijl} + \theta_1 \mathbb{1}(\text{Offer})_{il} + \gamma_1' X_{il} + \Gamma_l + \nu_{ijl}$$

where variable definitions are analogous to equation (3). Estimates are relative to 2002, the year prior to the lottery consolidation. If my empirical strategy successfully isolates the variation in racial composition driven by the lottery consolidation, then, relative to 2002, the effect of winning a seat in a more racially disparate 2002 lottery pool on the racial composition within the student's enrolled school should be zero for 2001 and earlier and positive thereafter. Figure 4 displays estimates of ρ_t for this regression.[38] Indeed, prior to the consolidation, aside from 1999, winning a seat to a magnet school with a larger $DPB^{'02}$ value has no statistically distinguishable effect on the percentage of black peers that eventually attend the lottery winner's school of enrollment. However, upon the termination of race-conscious lotteries in 2003, I estimate that winning a seat to a school with a one

[38] Recall from section 4 that I exclude lotteries from 2001 because they fail to balance on observable student characteristics.

percentage point larger DPB'^{02} value increases the proportion of black peers attending the school where the lottery winner enrolls by roughly 1.8 percentage points. Because the effect of magnet seat offers on the enrolled school racial composition does not systemically differ by DPB'^{02} prior to the consolidation, this is evidence for the absence of trends in unobservables that correlate with DPB'^{02} and also drive school racial composition.

Figure 5, presents estimates of the same regression, but for several important dimensions of school composition and student achievement. Panels 5a, 5b, and 5c respectively present estimates for the leave-one-out averages of baseline math and reading achievement and free/reduced lunch eligibility among peer 6th graders within the student's enrolled school.[39] As expected, peer baseline math and reading scores drop after the lottery consolidation, while free-lunch eligibility increases. Because black students in this district have lower baseline achievement on average and a higher proportion are free-lunch eligible, it is not surprising that an exogenous increase in the proportion of black students within a grade affects student composition along these dimensions.

Panels 5d through 5f present event studies for several student outcomes. Figures for the remaining outcomes explored in this paper can be found in Appendix A. To concisely summarize outcomes, I create indices by respectively taking averages over standardized versions of the achievement outcomes (i.e., math, reading, science, and writing achievement) and the postsecondary attainment outcomes (i.e., college enrollment, 2-year, 4-year, and "Top 50" rank enrollment). Foreshadowing future results, student achievement and postsecondary attainment is negatively affected by winning a seat to a school with a higher DPB'^{02} value after 2002, while ACT test taking is unaffected. In general, the absence of pre-trends across these regressions supports the identifying assumptions underlying my estimation strategy.

6.2 Differential Attrition

After testing whether the lotteries balance across observable student demographics, the other primary concern that potentially invalidates the lottery empirical strategy is differential attrition from the analysis sample between winners and losers. Suppose that wealthier families who lose a magnet lottery are more likely to send their child to a private school. Because I

[39] Free/Reduced price lunch eligibility comes from school-level averages from the CCD.

cannot observe students in private schools, this attrition would cause the lottery losers with valid outcome data to be disproportionately lower income, invalidating the empirical design.

In Table 2, I test whether lottery winners are less likely to be: enrolled within the district during the year following the lottery, missing math achievement outcome information, and missing NSC data.[40] In Panel A, I test differential attrition for my baseline estimates by regressing each attrition outcome on an indicator equal to one if the student was awarded an initial offer to the lottery's magnet school as well as a full set of lottery fixed effects. Lottery winners are indeed less likely to be missing from my analysis sample across all outcomes though the difference is only statistically significant for missing NSC outcome information. Magnet winners are about 2 percentage points less likely to be missing NSC outcome data.[41] In Panel B, I test whether differential attrition presents a threat to my segregation estimates from equation (3) by further interacting the initial offer variable with a post-2002 indicator and with DPB'^{02}. In order to be an issue, rates of differential attrition must vary by DPB'^{02} levels and must shift after 2002. I find no evidence that the rates of differential attrition systematically change after 2002 across lotteries with varying DPB'^{02} levels. Thus, differential attrition does not present a concern for my estimates of the effect of racial composition on student outcomes.

6.3 Accounting for No Child Left Behind and Changes in the Composition of Applicants

Both the introduction of NCLB and the lottery consolidation in 2003-04 could plausibly alter the composition of the pool of magnet lottery applicants. NCLB requires all schools in the district to reserve seats for the poorest and least proficient students enrolled in failing schools. Further, students who utilize the NCLB placement mechanism do not apply to magnet lotteries. As a result, one might expect the lottery pool to include students with higher baseline achievement and family resources than before NCLB. The lottery consolidation itself

[40] The virtue of NSC data is that students can be matched even if they leave the sample. However, because the LUSD only matched NSC data for graduating cohorts, differential attrition is still a concern in my setting. I am in the process of matching NSC data to all students in the lottery sample.

[41] The magnitude of differential attrition is comparable to the differential attrition estimated by Cullen et al. (2006) in the open enrollment system in Chicago.

may also influence the composition of the lottery pool. Recall that the lottery consolidation caused the probability of winning a seat for non-black relative to black students to fall. Thus, one might expect to see a higher share of black students in the lottery pool after the consolidation.

While randomization ensures that average lottery winners and losers are comparable along observable and unobservable dimensions, changes in the composition of the lottery pool will impact how to interpret the treatment effect. To see this, suppose that NCLB increases the baseline achievement within the lottery pool as explained above. Even though the composition of the lottery has changed, lottery offers are still randomized among the new pool. Thus, the average baseline characteristics of winners and losers will be indistinguishable, but now both average lottery winners and losers have a higher baseline aptitude. This is only a problem if the effects of magnet enrollment are heterogeneous along the same dimension.

Suppose that magnet schools relative to traditional schools are better equipped to teach high-aptitude than low-aptitude students and that the effect of attending a magnet school is constant over time. Under these assumptions, because the lottery pool is filled with higher achieving students after 2002 and magnets are better at instructing these students, then the average returns to winning a magnet lottery would be larger after 2003 than before. However, this change simply reflects the shift in the composition of the lottery pool and not any change in the underlying education production of magnet schools over time.

This consideration does not pose a problem for my baseline estimates in Section 7, but potentially threatens how I identify segregation effects in Section 8. Specifically, if treatment effects are heterogeneous and if the composition of lottery applicants changes before and after 2002 differentially based on how disproportionately non-black the school's lottery offers were in 2002 ($DPB^{'02}$), this would bias my estimated impact of segregation.

I test for compositional changes in lottery applicant pools across $DPB^{'02}$ by regressing

$$y_{jt} = \beta DPB_j^{'02} * \mathbb{1}(\text{Post } 2002)_t + \gamma_t + \theta_j + \epsilon_{jt}, \tag{6}$$

where y_{jt} is the average of a pre-lottery characteristic across students applying to enter magnet school j in year t.[42] DPB'^{02} is defined as in (3), $\mathbb{1}$(Post 2002) is an indicator variable equal to one if the lottery occurs after the lottery consolidation in 2002, while γ_t and θ_j are respectively year and school effects.[43] Consider two schools A and B, where school A has a one percentage point larger DPB'^{02} value than school B. β provides the average difference in how the lottery pool for school A changes after 2002 relative to how school B changes for the given student characteristic.

Table 3 depicts estimates of equation (6) for student race, gender, and baseline achievement. I find that the proportion of black students applying to schools with a one percentage point higher DPB'^{02} value increases by 0.8 percentage points. I also find that the proportion of female students decreases by 0.3 percentage points. The largest DPB'^{02} value in the district is roughly 10, meaning that terminating race-based lotteries shifts the share of black and female students in the lottery pool for the school with the largest 2002 lottery racial disparity upwards by 8 percentage points and downwards by 3 percentage points, respectively. Considering that on average, the proportion of black students applying to a magnet school is roughly 0.80 and the proportion of female applying is 0.55, I consider these compositional changes as second-order concerns.

Additionally, I find no statistically significant shifts in baseline achievement. Because the lottery composition is only changing with respect to race and gender, estimating the effect of segregation separately by these two groups will eliminate these compositional effects. In order for this to be successful, it needs to be the case that conditional on race/gender the composition of students is fixed. In Panel B, I test for changes in achievement within race and gender categories by restricting the sample appropriately. Indeed, I am unable to detect significant changes to the baseline achievement of the lottery applicants within these groups. Thus, I can abstract from compositional changes to the lottery pool by simply exploring sub-group analyses of segregation effects.

Aside from these composition issues, NCLB also potentially changes how to interpret the treatment. Because black students have lower average achievement than non-black students in the district (see Figure 2), NCLB seats may be disproportionately awarded to black

[42]The regression is weighted by the number of students applying to the given school in the given year.

[43]Main effects for $\mathbb{1}$(Post 2002) and DPB'^{02} are absorbed by these indicator variables.

students. I consider DPB'^{02} to proxy for the underlying black student demand for the given magnet school. Thus, if a higher proportion of black students fill NCLB seats for schools with higher DPB'^{02} values, then the lottery consolidation will appear to induce additional racial imbalance into the school. While this poses no threat to internal validity, it impacts how to interpret the treatment. In Table 4, I show that the composition of students awarded NCLB seats to magnet schools does not statistically significantly differ by the school's value of DPB'^{02}.[44] As a result, I interpret the results from equation (3) as isolating the exogenous change in racial composition solely resulting from the termination of race-blind lotteries.

7 Magnet Enrollment Effects

7.1 Effects of Magnet Enrollment on Teacher and Peer Characteristics

Before estimating the impact of racial segregation on magnet school effects, I first benchmark the returns to magnet enrollment within the LUSD. I begin by analyzing how magnet school enrollment changes a student's exposure to different dimensions of teacher quality, school institutional details, and peer composition. Together these effects help characterize the magnet enrollment treatment, which will be useful in determining whether the racial composition effects I estimate in magnet schools may generalize to traditional schools as well. Table 5 presents the effect of magnet enrollment on a variety of teacher and peer characteristics from equation (1). Recall that the endogenous variable of interest is an indicator variable equal to one if the student enrolled in a magnet school during 6th grade. I instrument magnet enrollment with an indicator equal to one if the student won a seat in the given magnet school lottery during the first wave of offers.

Panel A displays two-stage least squares (2SLS) estimates along with the accompanying first stage estimates for the pooled sample. The first stage estimates characterize the take-up rate of the lottery offer treatment. There are many reasons why magnet school enrollment

[44] Specifically, I regress each outcome on DPB'^{02} values and year indicators among students who accept NCLB-provided seats to magnet schools in 6th grade.

may not perfectly correlate with initial offers. First, anyone who receives an initial magnet school offer still has the prerogative to enroll elsewhere. Further, students who do not receive an initial lottery offer may eventually receive a seat after being waitlisted, letting them gain entry to the magnet despite losing the initial lottery. Depending on the estimate, students receiving an initial middle school magnet offer are anywhere from 15 to 19 percentage points more likely to be enrolled in a magnet school. These take-up rates are comparable to other lottery studies in the literature.[45]

In columns 1 through 3, I assess the differences between magnet and traditional schools along several classroom measures. Specifically, among the set of classrooms a given student takes in 6th grade, I calculate the average teacher experience and class size as well as the proportion of teachers with a Masters degree that the student is exposed to throughout the year. Students that attend magnet schools are assigned to classrooms where the teachers are no more likely to have a Masters degree. However, students in magnet schools are taught by less-experienced teachers and attend classes that are on average about 4 students larger than in traditional public schools. Panel B displays 2SLS estimates separately for black and non-black student subgroups. Interestingly, non-black students who attend magnet schools are exposed to a higher fraction of teachers with Masters degrees, but who are less experienced on average.

Because classroom-specific information is only available from 2000 and later, in columns 4 through 7, I estimate the effect of magnet attendance on peer composition at the school level (as opposed to the classroom level) to exploit a larger sample more in line with subsequent analyses. These outcomes are school-year-grade-specific averages of peer compositions that omit the student's own characteristic. From the perspective of non-black students, enrollment in a magnet school increases the proportion of black students in the cohort. Conversely, for black students, magnet enrollment decreases the share of students in the cohort qualifying for free/reduced lunch (FRL) and increases peer baseline academic achievement across both reading and math. In general, exposure to magnet schools in this LUSD affects teacher- and school-level educational inputs, but the imprecise estimates prevent me from ruling

[45] For example, Angrist et al. (2016) estimate that winning an initial lottery seat to attend a Boston charter school increases subsequent charter enrollment by 15 to 22 percentage points.

out relatively large differences in the composition of peers between magnet and traditional schools.

In summary, the effect of attending a magnet school is an amalgamation of school practices, teacher characteristics, and substantial changes to the composition of a student's peer group. Each present possible mechanisms driving the effects estimated in the following sections. While I am unable to isolate the role of school and teacher inputs, in Section 8, I exploit a natural experiment that isolates the effect of changing the peer racial composition on magnet returns.

7.2 Effects of Magnet Enrollment on Student Outcomes

In this section, I test whether magnet school enrollment impacts student outcomes relative to traditional schools in this LUSD. The value of this analysis in the context of studying the effect of segregation on student outcomes is twofold. First, because I am exploring how magnet school productivity changes due to increased racial segregation, baseline estimates for magnet school productivity relative to traditional schools are useful to interpret subsequent segregation effects. Second, these estimates help inform whether segregation effects are externally valid. If the returns to attending a magnet school are similar to that of traditional schools in this district, then the segregation effects that I find in the magnet school setting may more plausibly generalize to traditional schools as well.

Table 6 presents the effect of magnet enrollment on several student outcome summary measures. Adapting the method used by Billings et al. (2014), I create indices that summarize student achievement in column 1, postsecondary attainment in column 2, and total student academic outcomes in column 3. The achievement index is the simple average across student middle school math, reading, science, and writing achievement.[46] The postsecondary index is a simple average over standardized versions of whether the student enrolled in any postsecondary institution as well as a 2-year, 4-year, or "Top 50" ranked institution.[47] In addition to these outcomes, the total index also averages over standardized versions of high

[46]If a student is missing outcome information for a subject, then the average is taken over the remaining subjects only.

[47]Postsecondary outcomes are based on enrollment decisions made within 18 months after high school graduation.

school graduation status, ACT test taking status, and ACT composite scores. Because the focus of this article is assessing how segregation influences education production, I relegate estimates of magnet effects on individual outcomes and the accompanying discussion to Appendix B.

Panel A provides estimates for the effect of magnet enrollment on student outcomes among the pooled regression sample. I am unable to detect statistically significant differences between magnet and traditional enrollees across all three indices though I am unable to rule out relatively large effects across each outcome. Panel B presents these estimates among sub-groups by student race, gender, and whether baseline math achievement is above or below the district's median.

Magnet attendance improves achievement respectively by .10 and .15 standard deviations for black and non-black students as well as male and female students though the effect is not statistically significantly different than zero for most estimates. The achievement gains to magnet enrollment are primarily driven by improvement in science (see Appendix B). It is worth emphasizing that magnet school enrollment conditional on being offered a seat varies dramatically for black and non-black students. Table C.1 shows that winning the magnet lottery increases subsequent magnet enrollment by roughly 30 and 17 percentage points for non-black and black students, respectively. Anecdotally, magnet schools in this district were historically marketed to non-black students as a way to prevent "white flight" to the suburbs, which could help explain the difference in acceptance rates. As a result, the statistical power of the instrument also varies by student subgroup (e.g., Kleibergen-Papp F statistics for tests of weak instruments range from 3 to 12 depending on the specification – see Table C.3). Thus, caution should be given to interpreting under-powered sub-group estimates.

I estimate that enrolling in a magnet middle school tends to decrease (increase) postsecondary outcomes high-aptitude (low-aptitude) students. Again, these estimates are imprecisely estimated and should only be considered as suggestive. Column 3 shows that across the outcomes explored in this paper, the returns to magnet schools relative to traditional schools are negligible in the pooled sample, with imprecise heterogeneous returns to certain subgroups.

Together, these results build upon the findings of the previous section. In general, magnet schools do not generate educational benefits to students over other traditional public schools in the district. However, magnet schools generate both positive and negative heterogeneous effects across student subgroups. The localized and seemingly contradictory nature of these effects could be driven by differences in magnet-school-specific teaching strategies and specialties, where particularly (in)effective schools could be driving estimates for certain student subgroups. However, because the focus of this paper is estimating segregation effects, I leave the exploration of the heterogeneous impacts across magnet school types to future work. I read these results as suggestive evidence that magnet schools generate returns similar to the traditional schools in the district and I argue that studying the effect of racial segregation within these magnet schools may reasonably provide insight into how an increase in racial segregation would influence traditional public schools more generally. With these baseline estimates in hand, I now turn to the focus of this study by estimating the effect of a sharp increase in school segregation on student outcomes.

8 The Effect of Segregation on Student Outcomes

In this section, I explore one of the fundamental assumptions underlying the ruling of the landmark 1954 case *Brown v. Board of Education* in that school racial segregation negatively impacts minority student outcomes. I test this assumption by providing a causal estimate of how the peer racial composition parameter enters into the magnet school education production function.

Table 7 presents instrumental variables estimates in Panel A from equation (4) of the effect of a one percentage point increase in the share of black peers at a student's school on the composition of other peer characteristics. Recall from Table 1 that roughly 80 percent of magnet school enrollment is comprised of black students. As a result, an increase in the share of black students attending magnet schools should be thought of as an increase in school segregation. Panel B provides first-stage estimates as well as F statistics for tests of weak instruments. The interpretation of the first stage estimate is that winning a seat after 2002 to a magnet school with a one percentage point larger disparity (DPB'^{02}) increases the percentage of black peers in the student's school by roughly two percentage points.[48]

Increasing the proportion of black students entering magnet schools also shifts the student composition along the dimensions of socioeconomic status and prior achievement. Segregation increases the share of free-lunch eligible students at a one-to-one ratio and decreases average peer baseline achievement in reading, math, and science. A ten percentage point increase in the share of black peers decreases average peer baseline achievement from 0.11 to 0.14 student-level standard deviations.

Middle school standardized testing provides an early measure to assess whether education production is sensitive to school racial composition. In Table 8, I estimate that a 10 percentage point increase in the share of black peers at a student's school decreases achievement across math, science, and writing by 0.12 standard deviations. This is equivalent to the estimated achievement losses that would accompany permanently increasing class sizes by roughly 6 students (Angrist and Lavy, 1999).[49] The current literature estimates segregation-induced-losses to math achievement of 0.04 to 0.07 standard deviations, making my estimates somewhat larger (Billings et al., 2014; Hanushek et al., 2009). However, direct comparisons are obfuscated by the methodological differences and the unique educational setting in each study. My estimates are most closely aligned with Hoxby (2000) who estimates that a 10 percentage point increase in the share of black peers in a student's class decreases math achievement by 0.19 points for black students.

Heterogeneous effects also confirm patterns in the segregation literature.[50] Math and science losses estimated among the sample of black students are even more pronounced, though statistically indistinguishable, from the pooled sample estimates. This suggests that achievement losses may be larger for black students than non-black students as in Hoxby (2000) and Hanushek et al. (2009). Decreases in math and science achievement are concentrated among black students with the highest baseline achievement as in Hanushek and Rivkin (2009), while segregation negatively impacts writing achievement for low-achieving black students

[48] The shift in the share of black peers is greater than unity because the lottery consolidation increased the proportion of black students applying to the magnet lotteries (see Table 3).

[49] Angrist and Lavy (1999) estimate that a class reduction of 8 pupils increases reading achievement by about .18 student-level standard deviations.

[50] I exclude non-black-student-specific results because I am severely under-powered due to the low counts of non-black students in the district.

the most. Science and writing achievement losses are larger for black male students, while losses to math achievement for black female students are slightly more pronounced than for black male students.

Segregation has a clear negative impact on student achievement, but these short-term losses do not guarantee longer-term penalties to important education milestones such as high school graduation and postsecondary attainment. However, in Table 9, I show that the negative consequences of racial segregation are visible across several important medium-to-long-run educational outcomes. I estimate that a 10 percentage point increase in the share of a student's peers that are black erodes high school graduation rates by 4 percentage points. These losses are driven by female students whose high school graduation rates fall by twice the magnitude of the pooled sample.

School segregation also has deleterious impacts on postsecondary attainment. A 10 percentage point increase in school segregation decreases student enrollment in any postsecondary institution 18 months after high school graduation by 5 percentage points. The magnitude of this effect is comparable to exposing a student from 6th through 12th grade to teachers having nearly a standard deviation lower value-added (Chetty et al., 2014).[51] These losses are driven by low-aptitude black students as well as black female student subgroups and add to the myriad studies showing that the effects of educational interventions are often driven by particular subgroups, namely by minority and female students (Anderson, 2008; Andrews et al., 2016; Angrist et al., 2009; Angrist and Lavy, 2009; Deming et al., 2014; Hastings et al., 2006; Jackson, 2013). Further, these heterogeneous effects are interesting because they remove any issues relating to the changing composition of magnet school applicants after 2002 (see Section 6.3).

Unlike in Charlotte-Mecklenburg where Billings et al. (2014) find that school segregation impacts four-year college attainment, I find that segregation instead discourages prospective college students from enrolling in 2-year institutions, particularly for black female students. This could be a direct result of the impact that segregation has on high school graduation

[51] Chetty et al. (2014) estimate that one year of being exposed to a teacher with a one standard deviation lower value-added decreases college enrollment at age 20 by .82 percentage points. If you assume these effects accumulate linearly over time this yields a decrease of 5.74 percentage points for continual exposure from 6th through 12th grade.

rates. Students on the margin of graduating high school are more likely potential candidates to attend 2-year rather that 4-year postsecondary institutions. As a result, because segregation reduces high school graduation rates it is intuitive that I find that racial isolation subsequently diminishes postsecondary attainment through 2-year enrollment.

Finally, I assess whether college quality is affected by middle school segregation by estimating the impact on the probability of a student enrolling in a US News and World Report "Top 50" ranked university. While I estimate that segregation has no statistically significant impact on general college quality, I find that a ten percentage point increase in the share of black peers decreases student enrollment at a "Top 50" institution by 1 percentage point among black female students. Conversely, I estimate that black, male students are more likely to enroll in a "Top 50" institution by the same magnitude.

From these results, I conclude that racial balance is a meaningful input into the magnet school education production function both for immediate achievement as well as for long-run college outcomes. Magnet schools in this district generate similar returns to the traditional schools, and further, school-specific contributions to student achievement are intentionally removed by my estimation strategy. As a result, it is plausible that these results are not limited to the magnet school setting, rather, racial balance is likely an important input in education production functions more generally.

9 Conclusion

The United States education system has grown increasingly segregated since the end of court-ordered desegregation (Clotfelter et al., 2008, 2006; GAO, 2016; Lutz, 2011; Reardon et al., 2012), which makes assessing the causal link between *de facto* racial segregation and student achievement important to both researchers and policy-makers alike. I isolate how the effect of attending magnet schools in a large urban school district (LUSD) changes once schools are no longer allowed to artificially maintain the racial balance of incoming cohorts through race-base admissions lotteries. This setting provides an excellent natural experiment that reflects the growth in racial imbalance spreading across the nation's school system.

I find that achievement gains to magnet enrollment are local to certain subjects and student subgroups. The localized nature of these returns leads me to conclude that, in general, the LUSD magnet schools have similar returns to other traditional public schools in this district. Further, my main estimates suggest that racial balance is an important input in the magnet school education production function. Exogenously increasing segregation decreases achievement among black high-achievers and negatively influences postsecondary outcomes among black females and black low-achievers.

While school assignment policies that explicitly use race in admissions decisions have been declared unconstitutional (*Parents Involved in Community Schools v. Seattle School District No. 1* – 2007), my results suggest that more creative policies aimed at improving racial balance in schools can generate large improvements in education production. For instance, many districts utilize information about residence instead of race to ensure their schools enroll a diverse student body from rich and poor neighborhoods. This work is particularly timely given President Obama's current "Stronger Together" initiative that proposes to double the amount of federal funding up to $120 million to support voluntary integration programs across the United States of which magnet schools are a part. Education interventions of any variety that incorporate achieving racial balance as either an explicit goal or simply a byproduct of the policy stand to improve student outcomes.

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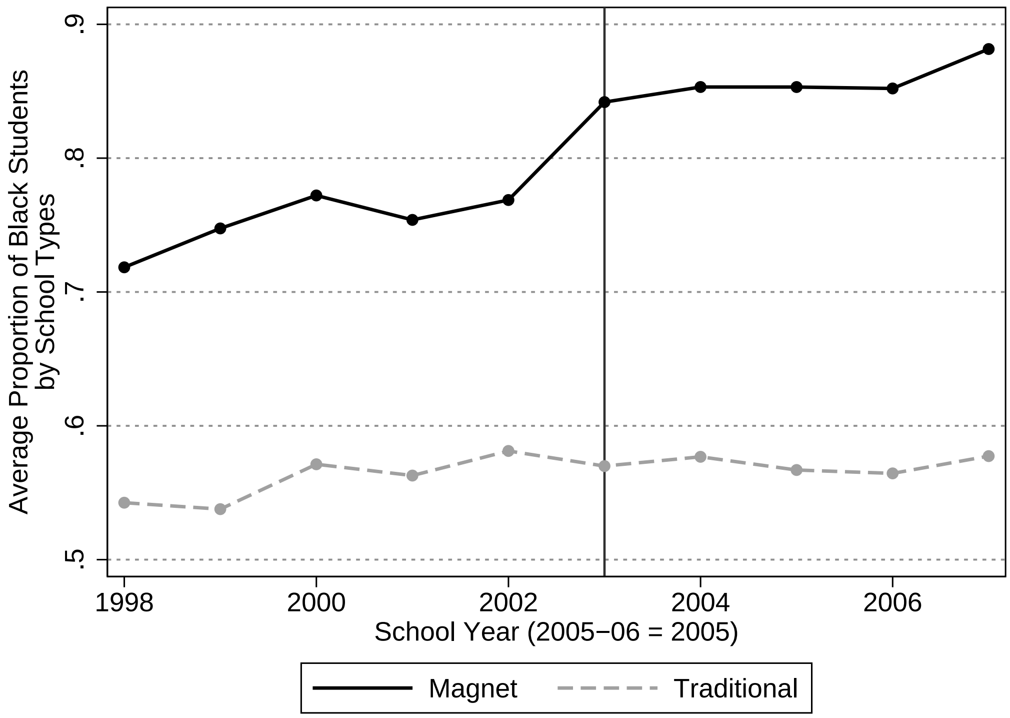
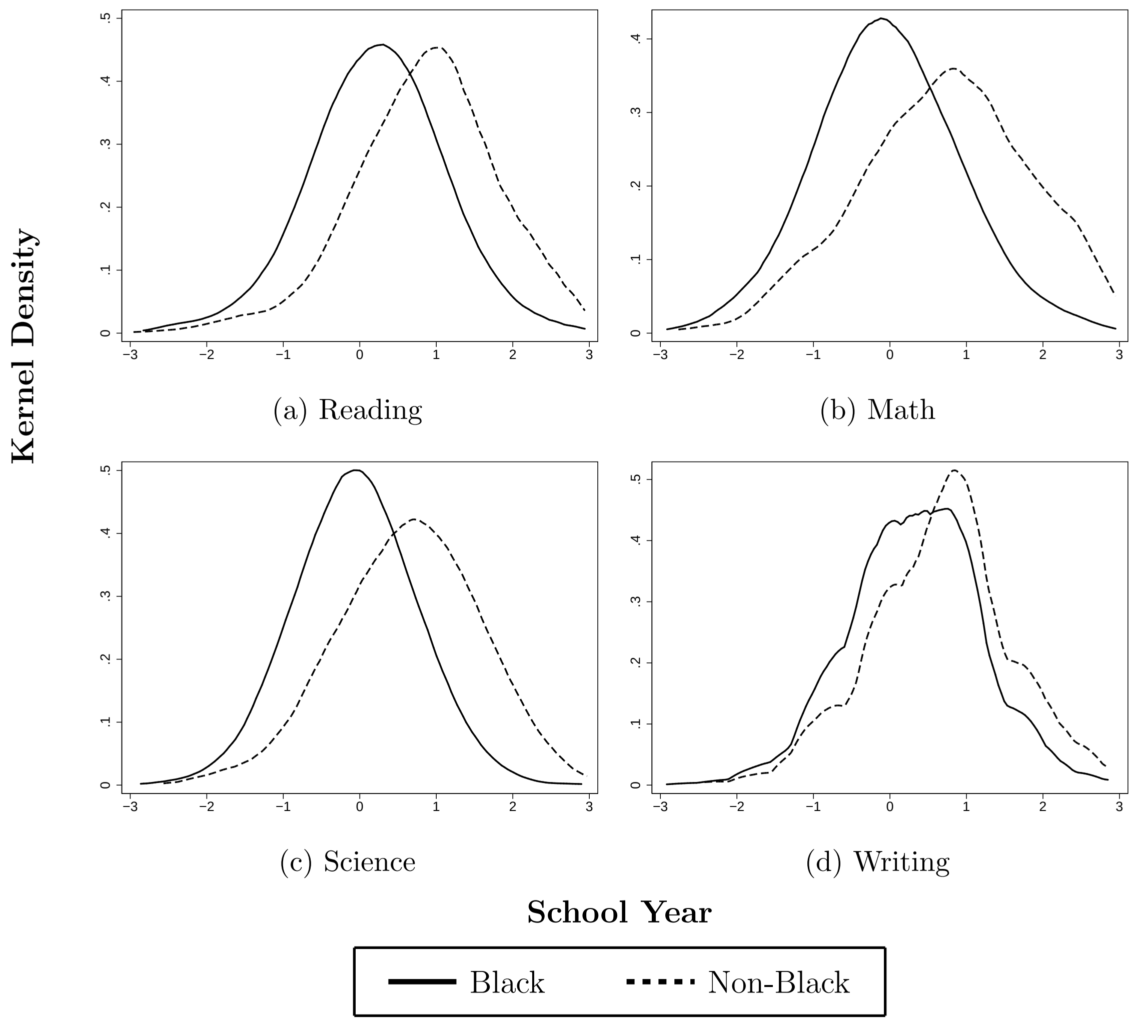
10 Figures and Tables

Figure 1: Racial Composition of Enrollment by School Type



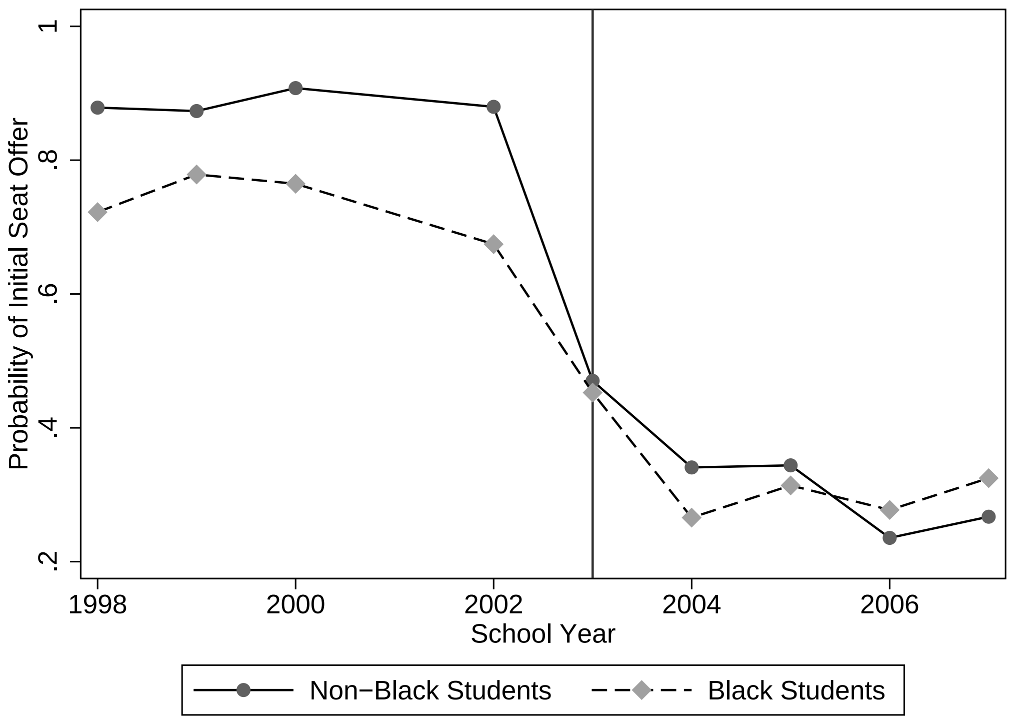
Notes: The figure plots the average black 6th grade enrollment shares across magnet and traditional schools in the LUSD. The vertical line represents the first year in which race-blind lotteries were used to determine enrollment in oversubscribed schools.

Figure 2: Standardized achievement Distribution by Subject and Race



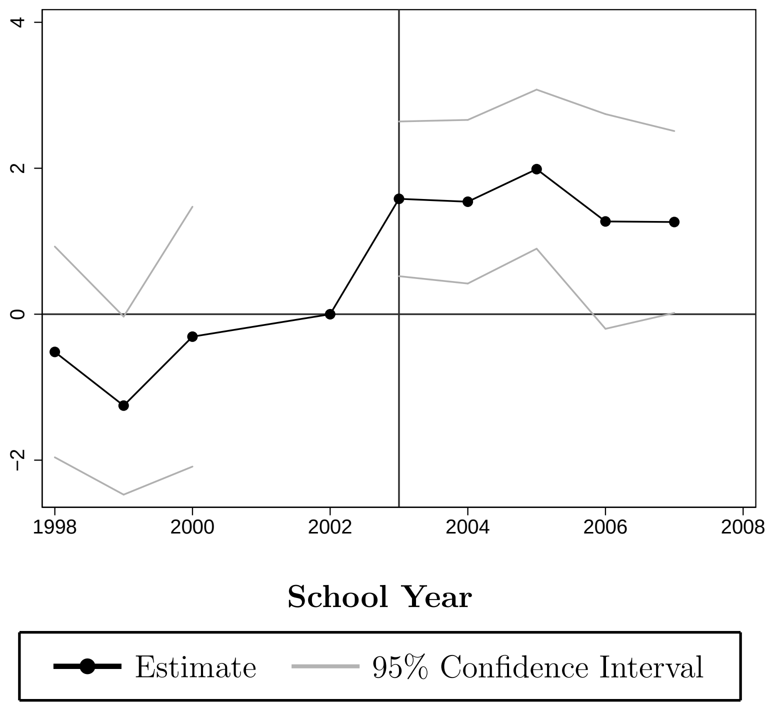
Notes: The figures present the distribution of subject-specific standardized achievement for 6th grade students who applied to magnet school lotteries prior to 2007.

Figure 3: Changes in Probability of Winning a Magnet Seat by Race



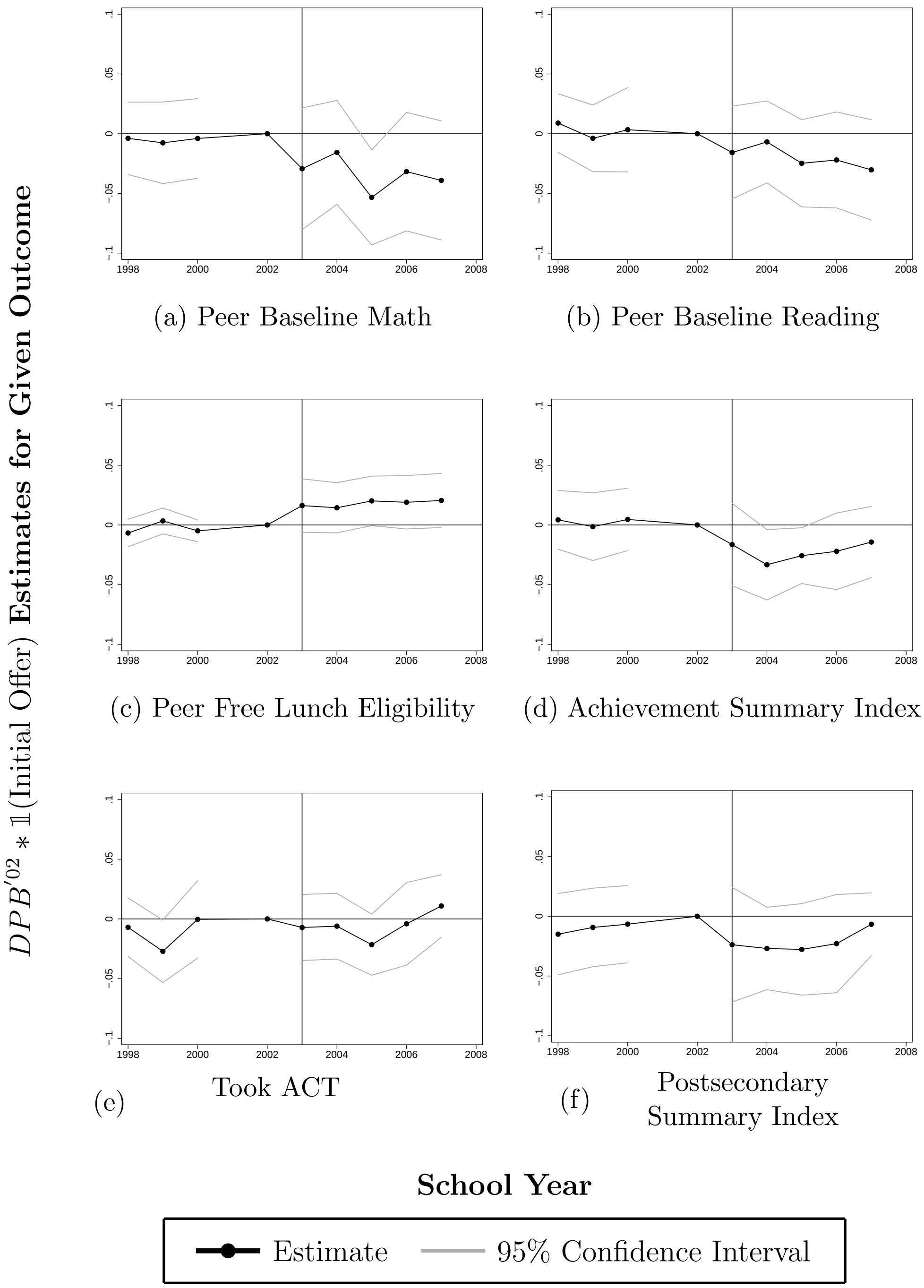
Notes: The figure plots the probability of winning an initial seat to attend a magnet school in 6th grade over time for by student race. The reference line in 2003 denotes the first year that the LUSD utilized the consolidated lottery to determine magnet seat offers. Prior to 2003, the LUSD ran separate lotteries for black students and non-black students (i.e. any student not considered a black student).

Figure 4: Trends in Black Student Composition by Lottery Racial Disparity (DPB'^{02})



Notes: The figure presents the effect of receiving an initial seat offer to a magnet school through a lottery with a 1 percentage point larger disparity between the percentage of black students in the lottery pool and the percentage of black student receiving offers in 2002 (DPB'^{02}) on the percentage of black students of the student's subsequent school of enrollment. Regressions are estimated using (5) as explained in Section 6.1. The regression is run using the sample restrictions in the footnote of Table 6. The reference line in 2003 denotes the first year the LUSD implemented the consolidated lottery system.

Figure 5: Trends in Various Outcomes by Lottery Racial Disparity ($DPB^{'02}$)



(a) Peer Baseline Math

(b) Peer Baseline Reading

(c) Peer Free Lunch Eligibility

(d) Achievement Summary Index

(e) Took ACT

(f) Postsecondary Summary Index

Notes: Each figure presents the effect of receiving an initial seat offer to a magnet school through a lottery with a 1 percentage point larger disparity between the percentage of black students in the lottery pool and the percentage of black student receiving offers in 2002 ($DPB^{'02}$) on the given current outcome. Regressions are estimated using (5) as explained in Section 6.1. Each regression is respectively run using the sample restrictions for the given outcome in the footnotes of Tables 5 through 6. Similar Figures for the remaining outcomes explored in the paper are located in Appendix A. The reference line in 2003 denotes the first year the LUSD implemented the consolidated lottery system.

Table 1: Descriptive Statistics and Balance Test

	All LUSD Students		Magnet Lottery Sample		
	Mean (1)	N (2)	Mean (3)	N (4)	Initial Offer Gap (5)
Female	.482	53,833	.554	6,615	-0.005 (0.012)
Black	.620	52,915	.817	6,564	-0.009 (0.007)
White	.304	52,915	.155	6,564	0.011 (0.007)
Baseline Math	-.011	41,289	.282	5,127	0.001 (0.017)
Baseline Reading	.023	44,750	.231	5,205	-0.026 (0.019)
Baseline Science	-.000	36,753	.221	4,572	0.013 (0.017)
Baseline Writing	.018	31,353	.264	4,440	0.010 (0.016)
			Combined p-value:	0.084	
			Initial Offer Rate:	.514	
			Magnet Attendance Rate:	.783	

Notes: The sample for columns 1 and 2 includes student-year observations for students attending a school 6th grade from 1998 to 2007. Columns 3 through 5 further restrict the sample to students who applied to a magnet school through the lottery and do not come from a sending school with automatic placement into a magnet school. The sample is further restricted to students without sibling priority in any lottery application and also to risk sets that have more than one student. Finally, the sample is further restricted to students with non-missing reading outcome test scores who are not receiving special education and are not repeating the given grade. Column 5 regresses each student demographic on an indicator equal to one if the student received an initial offer to a magnet school and a full set of lottery risk set fixed effects ($N = 13,315$). Standard errors (in parentheses) are clustered on students. p-values test the hypothesis that all coefficients on initial offer indicators are zero.

Table 2: Differential Attrition as a Function of DPB'^{02}

	Not Enrolled Following Lottery	Missing Math Outcomes	Missing NSC Outcomes
	(1)	(2)	(3)
Panel A: *Baseline Estimates*			
$\mathbb{1}$(Initial Offer)	-0.003	-0.003	-0.020*
	(0.009)	(0.007)	(0.011)
Panel B: *Segregation Estimates*			
$DPB'^{02} * \mathbb{1}(\text{Post }'02) * \mathbb{1}(\text{Initial Offer})$	-0.007	-0.003	0.002
	(0.006)	(0.003)	(0.006)

Notes: $N = 28,463$. The outcomes for each column are respectively a binary for whether the student appears in the LUSD enrollment records during the year following the 6th grade lottery, whether the student is missing math achievement information, and whether the student is missing in the National Student Clearinghouse data. Panel A presents the results from regressing the given outcome on a binary for whether the student was offered an initial magnet seat and a full set of lottery fixed effects. Panel B presents the results from a similar regression where instead of a binary for receiving an initial lottery offer, I include a triple interaction of the differential in 2002 percent black in the pool and winning a lottery (i.e., DPB), an indicator for if the lottery occurred after 2002, and the initial offer indicator, as well as the main effects. Standard errors for both regressions are clustered at the lottery level. Together these regressions show that there are no differential attrition problems in general (Panel A) and specifically for my racial segregation identification strategy (Panel B). Regressions are limited to observations from the baseline sample (see table note for Table 6).

Table 3: Differential Effect of Consolidation by DPB'02 on Middle School Applicant Composition

	Black	Female	Baseline Testing			
	Black (1)	Female (2)	Reading (3)	Math (4)	Science (5)	Writing (6)
Panel A: Pooled Composition Changes						
$DPB'^{02} * \mathbb{1}(\text{Post 2002})$	0.008***	-0.003**	0.004	-0.003	-0.004	-0.007
	(0.002)	(0.001)	(0.012)	(0.012)	(0.015)	(0.008)
Observations	62	62	61	54	48	48
Panel B: Baseline Achievement Composition by Race and Gender						
		Black	0.008	0.003	0.003	-0.004
			(0.010)	(0.010)	(0.012)	(0.007)
		Non-Black	0.009	0.004	-0.009	-0.007
$DPB'^{02} * \mathbb{1}(\text{Post 2002})$			(0.017)	(0.019)	(0.022)	(0.014)
		Female	0.006	0.002	-0.004	-0.004
			(0.011)	(0.008)	(0.013)	(0.009)
		Male	0.003	-0.008	-0.005	-0.009
			(0.013)	(0.016)	(0.016)	(0.008)

Notes: *, **, and *** denote statistical significance at the 10, 5, and 1 percent levels, respectively. Regressions follow equation (6) where school-year averages of each outcome is regressed on an indicator equal to one if the lottery occurred after 2002 interacted with the school-specific DPB'^{02} value as well as indicators for the application school and lottery year. Regressions are weighted by the number of applicants in the given school-year pool. Standard errors (in parentheses) are clustered by application school. Each regression sample is limited to baseline sample restrictions specified in the notes for Table 6 who are also applying to enter a magnet school in the 6th grade.

Table 4: Composition of NCLB Magnet Seats as a Function of $DPB^{'02}$

				Baseline Testing			
	Black	Female	NCLB Rank	Reading	Math	Science	Writing
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
$DPB^{'02}$	-0.007	-0.007	4.420	0.013	-0.001	0.013	0.001
	(0.008)	(0.005)	(40.940)	(0.015)	(0.018)	(0.016)	(0.016)
Observations	702	696	699	488	305	305	295

Notes: Each outcome is regressed on school-specific $\text{DPB}^{'02}$ values as well as indicators for the lottery year. Standard errors (in parentheses) are clustered by application school. Each regression sample is limited to baseline sample restrictions specified in the notes for Table 6 and is additionally limited to students who accepted a NCLB position in a 6th grade magnet school from 2002-2007. NCLB rank refers to the district's internal ranking of the lowest-achieving, poorest students in the district. The lower the rank value the higher the priority that student receives for NCLB placement.

Table 5: Lottery Estimates of Effects of Magnet Attendance on Teacher, School, and Peer Characteristics

	Teacher/School Characteristics			Student Characteristics of Entire School			
						Average Peer Baseline Achievement	
	Fraction Masters (1)	Average Experience (2)	Average Class Size (3)	Fraction Black (4)	Fraction FRL (5)	Math (6)	Reading (7)
Panel A: *Pooled Sample*							
Enrolled in Magnet (2SLS)	0.020	-1.256	3.945***	0.011	-0.202*	0.434*	0.284
	(0.119)	(1.000)	(1.307)	(0.123)	(0.122)	(0.226)	(0.239)
	[0.578]	[11.364]	[23.035]	[0.808]	[0.771]	[0.163]	[-0.013]
Lottery Offer (First-Stage)	0.155**	0.155**	0.155**	0.193***	0.193***	0.193***	0.196***
	(0.062)	(0.062)	(0.062)	(0.067)	(0.067)	(0.067)	(0.067)
Observations	9,994	9,962	9,970	13,405	13,429	13,403	12,937
Panel B: *Subgroup Estimates*							
Black	-0.026	-0.908	4.216***	-0.053	-0.263*	0.529**	0.410*
	(0.150)	(1.145)	(1.617)	(0.129)	(0.135)	(0.221)	(0.237)
	[0.576]	[11.311]	[22.994]	[0.833]	[0.777]	[0.153]	[-0.024]
Non-Black	0.153***	-1.888**	3.223***	0.198*	-0.032	0.144	-0.063
	(0.057)	(0.917)	(1.115)	(0.103)	(0.078)	(0.199)	(0.183)
	[0.594]	[11.740]	[23.323]	[0.639]	[0.733]	[0.234]	[0.060]

Notes: *, **, and *** denote statistical significance at the 10, 5, and 1 percent levels, respectively. Outcome means among students not offered a magnet lottery seat are in brackets. Regressions follow equation (1) where each outcome is regressed on a indicator equal to one if the student attended a magnet school during the year following the lottery as well as indicators for student gender, race, and risk-sets. I instrument for endogenous magnet attendance variable with whether the student receiving an initial lottery offer. Standard errors are two-way clustered by student and the enrolled school after the lottery. Each regression sample is limited to baseline sample restrictions specified in the notes for Table 6. Outcomes in columns 1-3 are averages over the actual classrooms that the given student attended in 6th grade, while remaining outcomes are averages over the entire school. FRL data are from CCD school averages. Regressions are weighted by one over the number of lotteries applied to by the student in the given year.

Table 6: Lottery Estimates of Effects of Magnet Enrollment on Student Outcomes

	Achievement Index (1)	Postsecondary Index (2)	Total Index (3)
Panel A: *2SLS Estimates for Pooled Sample*			
Enrolled in Magnet	0.120	-0.039	0.050
	(0.084)	(0.128)	(0.073)
Panel B: *2SLS Estimates by Subgroup*			
Non-Black	0.147	-0.031	0.037
	(0.105)	(0.218)	(0.081)
Black	0.099	-0.060	0.048
	(0.091)	(0.128)	(0.079)
Male	0.151	0.102	0.147
	(0.158)	(0.169)	(0.136)
Female	0.111**	-0.115	-0.005
	(0.049)	(0.164)	(0.054)
Above Median Baseline Math Score	0.135	-0.068	0.007
	(0.114)	(0.178)	(0.087)
Below Median Baseline Math Score	0.067	0.141	0.123
	(0.140)	(0.218)	(0.140)
Black, Male	0.106	0.189	0.182
	(0.168)	(0.151)	(0.158)
Black, Female	0.111	-0.202	-0.037
	(0.068)	(0.183)	(0.058)
Black, Above Median	0.169	-0.116	0.018
	(0.151)	(0.195)	(0.102)
Black, Below Median	0.029	0.150	0.137
	(0.152)	(0.272)	(0.166)

Notes: *, **, and *** denote statistical significance at the 10, 5, and 1 percent levels, respectively. Regressions follow equation (1) where each outcome is regressed on a indicator equal to one if the student attended a magnet school during the year following the lottery as well as indicators for student gender, race, and risk-sets. I instrument for endogenous magnet attendance variable with whether the student receiving an initial lottery offer. Standard errors are two-way clustered by student and the enrolled school after the lottery. Each regression sample is limited to baseline sample restrictions i.e., the student must have applied to a magnet school lottery in the 6th grade from 1998-2007, must be in their first year attending the grade of the lottery application (no grade retention), and must not be eligible for special education. Further, the baseline sample restriction also excludes students from a school with automatic placement into a magnet school. Achievement index is the simple mean of math, science, reading, and writing achievement. Post-secondary index is a simple mean over standardized versions of whether 18 months after high school graduation the student enrolled in any postsecondary institution, a 2-year, 4-year, or Top 50 ranked institution. Regressions are weighted by one over the number of lotteries applied to by the student in the given year. First-stage estimates, observation counts, weak IV tests, and outcome means are provided in Tables C.1, C.2, C.3, and C.4, respectively. See Appendix B for disaggregated estimates.

Table 7: Lottery Estimates of Effects of School Peer Racial Composition on Peer Characteristics

	School Peer Composition				
		Baseline Testing			
	FRL (1)	Reading (2)	Math (3)	Science (4)	Writing (5)
Panel A: *2SLS Estimates*					
Fraction Black in School ×100	0.010***	-0.011**	-0.014***	-0.013***	-0.008
	(0.003)	(0.005)	(0.005)	(0.004)	(0.005)
Non-offer Outcome Mean	0.695	0.170	0.053	0.050	0.108
Panel B: *First-Stage Estimates*					
$\mathbb{1}$(Initial Offer) × $\mathbb{1}$(Post 2002) × DPB	2.091***	2.087***	2.187***	2.277***	2.275***
	(0.492)	(0.492)	(0.496)	(0.461)	(0.462)
F Statistic (Weak IV)	18.042	17.974	19.424	24.413	24.234
Observations	13,398	13,396	12,930	11,316	11,285

Notes: *, **, and *** denote statistical significance at the 10, 5, and 1 percent levels, respectively. Regressions follow equation (4) where each outcome is regressed on the percentage of black students in the grade of the school the student attended during the year following the lottery as well as indicators for student gender, race, year-of-test and lottery. I instrument for endogenous racial composition with a triple-interaction between whether the student received an initial lottery offer, whether the observation is after 2002, and by the 2002 difference in percentage of black students ($DPB^{'02}$) between the total magnet lottery pool and the pool of initial offers. Standard errors are two-way clustered both by student and by school-after-lottery. Because observations are at the student-application level for these regressions, a given student-year combination can appear multiple times. Each regression sample is limited to the sample restrictions specified in the notes for Table 6. 2SLS estimates and the outcome mean for students not offered a magnet seat are provided in Panel A. First-stage estimates and weak instrument tests using the Kleibergen-Paap Wald F statistic are reported in Panel B.

Table 8: Lottery Estimates of Effects of School Peer Racial Composition on Student Achievement

	Achievement Testing			
	Reading (1)	Math (2)	Science (3)	Writing (4)
Panel A: *2SLS Estimates for Pooled Sample*				
Fraction Black in School ×100	-0.007	-0.012***	-0.012***	-0.012***
	(0.005)	(0.005)	(0.004)	(0.004)
Panel B: *2SLS Estimates by Subgroup*				
Black	-0.006	-0.015***	-0.015***	-0.010**
	(0.005)	(0.005)	(0.005)	(0.004)
Male	-0.007*	-0.010**	-0.011**	-0.012**
	(0.004)	(0.005)	(0.006)	(0.006)
Female	-0.007	-0.016**	-0.012**	-0.011**
	(0.008)	(0.007)	(0.005)	(0.005)
Above Median Baseline Math Score	-0.008**	-0.012**	-0.013**	-0.013
	(0.004)	(0.006)	(0.006)	(0.008)
Below Median Baseline Math Score	-0.004	-0.008	-0.011	-0.022*
	(0.010)	(0.008)	(0.009)	(0.011)
Black, Male	-0.005	-0.014***	-0.016**	-0.013**
	(0.004)	(0.004)	(0.007)	(0.006)
Black, Female	-0.007	-0.017*	-0.012**	-0.007
	(0.009)	(0.009)	(0.005)	(0.006)
Black, Above Median	-0.008*	-0.015**	-0.020***	-0.010
	(0.005)	(0.007)	(0.007)	(0.007)
Black, Below Median	-0.003	-0.007	-0.009	-0.023*
	(0.010)	(0.009)	(0.009)	(0.013)

Notes: *, **, and *** denote statistical significance at the 10, 5, and 1 percent levels, respectively. Regressions follow equation (4) where each outcome is regressed on the percentage of black students in the grade of the school the student attended during the year following the lottery as well as indicators for student gender, race, year-of-test and lottery. I instrument for endogenous racial composition with a triple-interaction between whether the student received an initial lottery offer, whether the observation is after 2002, and by the 2002 difference in percentage of black students ($DPB^{'02}$) between the total magnet lottery pool and the pool of initial offers. Standard errors are two-way clustered both by student and by school-after-lottery. Because observations are at the student-application level for these regressions, a given student-year combination can appear multiple times. Each regression sample is limited to the sample restrictions specified in the notes for Table 6. First-stage estimates, observation counts, weak IV tests, and outcome means are provided in Appendix Tables C.5, C.6, C.7, and C.8, respectively.

Table 9: Lottery Estimates of Effects of School Peer Racial Composition on High School Graduation and Postsecondary Attainment

		College Attendance (18 Months after Graduation)			
	HS Grad. (1)	Any (2)	2-year (3)	4-year (4)	Top 50 (5)
Panel A: *2SLS Estimates for Pooled Sample*					
Fraction Black in School ×100	-0.004**	-0.005**	-0.007**	-0.000	0.000
	(0.002)	(0.003)	(0.003)	(0.003)	(0.000)
Panel B: *2SLS Estimates by Subgroup*					
Black	-0.004	-0.006*	-0.007**	-0.000	0.000
	(0.002)	(0.003)	(0.003)	(0.003)	(0.000)
Male	-0.001	-0.001	-0.003	0.004	0.001**
	(0.004)	(0.004)	(0.003)	(0.004)	(0.001)
Female	-0.008**	-0.009*	-0.011***	-0.002	-0.001**
	(0.004)	(0.005)	(0.004)	(0.005)	(0.000)
Above Median Baseline Math Score	-0.005	-0.002	-0.007*	0.001	-0.000
	(0.003)	(0.002)	(0.004)	(0.003)	(0.001)
Below Median Baseline Math Score	-0.006	-0.010*	-0.005	-0.001	0.000
	(0.005)	(0.006)	(0.005)	(0.006)	(0.001)
Black, Male	-0.001	-0.002	-0.005	0.003	0.001*
	(0.004)	(0.004)	(0.003)	(0.004)	(0.001)
Black, Female	-0.007*	-0.008	-0.012**	-0.001	-0.001***
	(0.004)	(0.006)	(0.005)	(0.005)	(0.000)
Black, Above Median	-0.006*	-0.002	-0.008**	0.002	0.000
	(0.003)	(0.003)	(0.004)	(0.003)	(0.001)
Black, Below Median	-0.004	-0.014*	-0.008	-0.003	0.001
	(0.004)	(0.008)	(0.007)	(0.007)	(0.001)

Notes: *, **, and *** denote statistical significance at the 10, 5, and 1 percent levels, respectively. Regressions follow equation (4) where each outcome is regressed on the percentage of black students in the grade of the school the student attended during the year following the lottery as well as indicators for student gender, race, and lottery. I instrument for endogenous racial composition with a triple-interaction between whether the student received an initial lottery offer, whether the observation is after 2002, and by the 2002 difference in percentage of black students (DPB'^{02}) between the total magnet lottery pool and the pool of initial offers. Standard errors are two-way clustered both by student and by school-after-lottery. Because observations are at the student-application level for these regressions, a given student-year combination can appear multiple times. Each regression sample is limited to the sample restrictions specified in the notes for Table 6. Top 50 denotes an indicator equal to one if the student attends a top 50 ranked school based on the U.S. News and World Report. First-stage estimates, observation counts, weak IV tests, and outcome means are provided in Appendix Tables C.9, C.10, C.11, and C.12, respectively.