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Jason Cook
University of Pittsburgh, CESifo and IZA

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IZA – Institute of Labor Economics

Schaumburg-Lippe-Straße 5–9
53113 Bonn, Germany

Phone: +49-228-3894-0
Email: publications@iza.org

www.iza.org

ABSTRACT

Race-Blind Admissions, School Segregation, and Student Outcomes: Evidence from Race-Blind Magnet School Lotteries*

We know surprisingly little about the influence of race-blind school admissions on student outcomes. This paper studies a unique reform where a large, urban school district was federally mandated to adopt a race-blind lottery system to fill seats in its oversubscribed magnet schools. The district had previously integrated its schools by conducting separate admissions lotteries for black and non-black students to offset the predominantly black applicant pools. The switch to race-blind lotteries dramatically segregated subsequent magnet school cohorts. I show that race-blind admissions caused the more segregated schools to enroll students with lower average baseline achievement and to employ lower value-added teachers due to sorting. I also find that segregation is further exacerbated by "white flight" as white students transfer out of the district after attending more segregated schools. Ultimately, the mandated segregation decreases student standardized test scores and four-year college attendance. I provide suggestive evidence that the impact of racial segregation is partially mediated by changes to peer baseline achievement.

JEL Classification: I24, I26, I28, J15, J48

Keywords: race-blind school admissions, school racial segregation, magnet schools, peer effects, school admissions lotteries

Corresponding author:
Jason Cook
University of Pittsburgh
4117 Posvar Hall
230 S Bouquet Street
Pittsburgh, PA 15213
United States

E-mail:

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I. INTRODUCTION

The landmark ruling of *Brown v. Board of Education of Topeka* ended *de jure* school segregation and prompted integration efforts across the United States education system. Despite these early efforts, *de facto* racial segregation still persists with nearly one out of every five U.S. schools reporting over 90 percent minority enrollment shares in 2013 (Orfield et al., 2016).[1]

This persistence of *de facto* segregation in schools is not surprising given the series of Supreme Court decisions that have weakened desegregation efforts. In the early 1990s, a set of rulings facilitated the termination of court-ordered integration plans. Furthermore, over the past two decades, the Supreme Court has placed additional restrictions on how race and ethnicity can be used in school admissions decisions—limiting schools' ability to maintain racially balanced enrollments (Ellison and Pathak, 2016).[2]

A growing body of evidence shows that early school integration efforts substantially improved educational and longer-run outcomes for black students (Billings et al., 2014; Guryan, 2004; Johnson, 2015; Lutz, 2011). Yet, we lack evidence on how the more recent Supreme Court rulings on race-neutral admissions has influenced student outcomes. Understanding the implications of race-neutral admissions is key for the design of education policies such as school assignment, affirmative action, and school finance equalization and is particularly relevant as the Trump administration has recently rescinded seven policy guidelines on affirmative action, making it more difficult for schools to maintain a diverse student body.

To explore the effect of court-ordered race-blind admissions, I study a federal mandate issued to eliminate racial quotas from magnet school admissions in a large urban school district (LUSD). In this district, magnet schools were established in high-minority-share neighborhoods as an attempt to attract white student enrollment in what would otherwise be racially segregated schools.

[1]For additional evidence of the persistence of racial segregation in schools see Clotfelter et al. (2008, 2006, 2018); GAO (2016); Lutz (2011); Reardon et al. (2012); and Reardon and Owens (2014).

[2]The 2003 Supreme Court case, *Grutter vs. Bollinger*, allowed affirmative action in school admission only if it was a single factor among many others and that race did not automatically increase admission probability. *Parents Involved in Community Schools v. Seattle School District No. 1* built off of this case in 2007 and prevented schools from utilizing race in admissions decisions.

These magnet schools were oversubscribed and admissions were determined through a randomized lottery. Because black students disproportionately applied to these magnets, the schools conducted separate lotteries for black and non-black students and protected seats for non-black students to ensure racially balanced enrollment. However, in 2003, a federal mandate required the LUSD to instead use a race-blind lottery procedure under which the racial make-up of the entering cohort reflected the composition of the applicant pool.

The move to race-blind lotteries caused a large and immediate increase in magnet school racial segregation. Segregation increased by roughly seven percentage points (as measured by the Massey and Denton (1988) exposure index), which is roughly the size of the initial change in segregation resulting from the end of forced busing in Charlotte-Mecklenburg (Billings et al., 2014). Racial segregation could impact student outcomes directly if peer race effects exist or indirectly if students and teachers sort in response.

To estimate how the race-blind admissions policy impacts student outcomes through its effect on peer racial composition, I exploit the fact that magnet schools did not segregate uniformly across the district. Because a racially integrated magnet with a predominantly black applicant pool would have experienced a larger shift in racial composition after the race-blind policy than a similar school with a racially balanced applicant pool, I use the difference between the proportion of black students receiving lottery offers and the proportion of black students in the applicant pool *under race-conscious lotteries* to predict exogenous increases in magnet school segregation.

I find that racial segregation in magnet schools had deleterious effects on student outcomes. I estimate that a 10 percentage point increase in predicted black enrollment shares decreases middle school achievement by 0.04σ, which is in line with other estimates in the literature on the order of -0.04 to -0.07σ (Billings et al., 2014; Hanushek et al., 2009; Hoxby, 2000). Segregation also has persisting negative effects. Specifically, students who attend a more racially segregated school have a lower probability of later enrolling in a four-year college. A 10 percentage point increase in predicted black enrollment shares decreases college enrollment by 1.2 percentage points among black students. Lastly, I provide the first evidence that non-black students who attend magnet schools that experience an exogenous increase in racial segregation are more likely to transfer out of the district during subsequent years—thereby exacerbating segregation.

Race-blind admissions may impact other peer characteristics and may induce teacher sorting. Indeed, I find that when a magnet school becomes racially segregated, the new student body is comprised of students with lower average baseline achievement scores. Further, similar to Jackson (2009), I provide evidence that average teacher value-added (VA) declines in more segregated schools. Thus, the observed negative impact on academic outcomes could result from either peer racial composition directly or from changes to other peer characteristics and teacher quality. To disentangle whether these channels mediate the estimated peer race effects, in the spirt of Abulkadiroglu et al. (2014), I exploit the randomized lottery offers themselves to jointly estimate the causal effect of peer race along with other peer and teacher characteristics. Jointly estimating multiple causal channels is possible because lottery offers not only induce exogenous variation in magnet school enrollment, they also create exogenous variation in exposure to school-specific characteristics such as peer groups and teacher types. For example, if a student receives multiple magnet offers in 2005, *ex-ante* measures of the peer racial composition *across the set of offered magnets* in 2004 will be highly predictive of the student's realized peer racial composition.

I find that lottery offers to magnets with higher black enrollment shares decrease standardized achievement relative to offers to magnets with lower black enrollment shares. Jointly estimating multiple causal channels using lottery variation in peer and teacher characteristics suggests that peer baseline achievement partially mediates the peer race effect (a conclusion also reached by Hoxby and Weingarth, 2005). Conversely, teacher value-added plays an inconsequential mediating role for peer race effects. These estimates provide suggestive evidence that a portion of the negative impacts of racial segregation due to race-blind lotteries may be attributed to the observed changes in the peer achievement distribution.

Because race-blind admissions segregated subsequent magnet school cohorts, this meant that a higher share of desirable magnet school seats were assigned to black students. If black students benefit more from magnet schools than white students, then increasing black enrollment shares would increase aggregate student achievement—potentially offsetting the estimated segregation-induced losses. Using randomized magnet school offers in an instrumental variables framework, I find that magnet schools improve student achievement, but that the gains do not vary by race. Students induced by the lottery offer to attend a 6th grade magnet school experience middle and high school achievement gains on the order of .4σ and are more likely to graduate high school and attend college. As a result, while the race-blind admissions policy provides a higher-share of seats to black students, these estimates suggest that the reshuffling alone did not impact aggregate student achievement. Because race-blind admissions increased magnet school segregation—harming average student achievement—I conclude that the race-blind admissions policy negatively impacted aggregate achievement as well.

This paper makes four main contributions. First, it provides the first assessment of how recent court-ordered race-neutral admissions policies impact student outcomes through their effects on racial segregation. This complements the racial peer effects literature because I estimate the effect of peer race by exploiting novel sources of variation in student racial composition arising from the court-ordered termination of race-conscious lotteries and from the randomized lottery offers themselves. Existing studies have relied on quasi-random fluctuations 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); on school-switching designs where students are exposed to markedly different peer groups upon exogenously changing schools (Abulkadiroglu et al., 2014; Bergman, 2018; Billings et al., 2014; Dobbie and Fryer, 2014); or on 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), voluntary busing programs (Angrist and Lang, 2004; Cook, 1984), changes to attendance zone boundaries (Billings et al., 2014; Vigdor and Nechyba, 2007), or mandated school reassignments (Hoxby and Weingarth, 2005).

Second, this article complements the peer effects literature by exploring potential mechanisms that may drive the observed impact of peer race on student outcomes. The reduced-form nature of the empirics used throughout the literature require peer race effects to be considered an amalgamation of direct race effects as well as the indirect impacts of everything correlated with peer racial composition, e.g., teacher quality (Jackson, 2009). The main exceptions are Hoxby and Weingarth (2005), who jointly estimate the impact of multiple dimensions of peer effects driven by school reassignments, and Abulkadiroglu et al. (2014), who estimate the causal impact of attending highly-selective exam schools in a regression discontinuity setting.[3] I extend the estimation framework of Abulkadiroglu et al. (2014) by applying it to a setting with randomized lotteries. Using lottery-induced variation in peer race as well as other peer and teacher characteristics, I jointly estimate peer race effects along with other causal channels. My work further explores how teacher quality—in addition to other dimensions of peer quality—potentially mediate observed peer race effects. My empirical framework is useful for estimating the impact of peer, teacher, and school characteristics in a wide array of settings where school admissions are determined by randomized lotteries.

Third, this article estimates the long run effect of magnet middle school enrollment on post-secondary attainment using administrative lottery data. To date, magnet schools have received far less attention than charter schools and voucher programs despite being more prevalent (Engberg et al., 2014). 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 others estimate academic returns roughly half the magnitude of lottery-based charter school estimates (Betts et al., 2006; Bifulco et al., 2009; Crain et al., 1992; Hastings et al., 2012).[4] However, we know far less about long-run returns to magnet

[3] Abulkadiroglu et al. (2014) leverage the fact that the cutoffs for different schools hit at different points in the achievement and peer race distribution, each providing exogenous variation in not only exam school attendance but also exposure to different peer types.

[4] 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

enrollment in the United States.[5, 6] I add to the literature studying the short-term academic returns to magnet school enrollment and provide lottery estimates of the longer-run impact of magnets on college enrollment.

Fourth, I document that magnet school racial segregation resulting from race-neutral admissions policies causes "white flight," i.e., the relocation of non-minority families out of the school district. Because magnet schools began as a non-compulsory means to integrate schools and prevent "white flight," finding that race-neutral admissions undermine this original purpose is striking. This finding contributes to the literature that studies the "white flight" response to district-wide desegregation plans (see Reber, 2005, for a review).

II. Institutional Background

Magnet schools were initially conceived as a free market means of racial integration, and thus magnets provide a natural setting to explore race-blind admissions policies.[7] The LUSD established magnet schools with special scholastic offerings within high-minority-share neighborhoods to encourage non-resident white families to enroll their children. Thus, LUSD magnets in theory promote racial diversity in what would otherwise be high-minority-share schools.

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 the time period of this study.

for other types.

[5] 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.

[6] 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)—though, these studies do not separately report estimates for magnet enrollment.

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

Because the demand for these magnet schools far outpaced supply, magnet seats were filled via randomized lotteries. To integrate schools, the district held separate school-specific lotteries for black and non-black students.[8] 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. Black students disproportionately applied to magnet schools making the offer rate more generous in the non-black lottery. In the 2002-03 school year, the federal Office of Civil Rights required the LUSD to instead utilize a race-blind lottery system, which lead to the dramatic increase in racial segregation that is the focus of this study.

III. Data

I use student-level LUSD administrative data from 2000 through 2007. In addition to statewide standardized achievement measures and student demographic information, the district also merged student information to several medium- and long-run student outcomes. The district also merged student records for each graduating class with college information collected by the National Student Clearinghouse (NSC). 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 postsecondary institutions in the United States, allowing me to observe students attending out-of-state schools.[9] The LUSD combined these student-level data with admissions lottery records that contain information on the schools to which each student applied in a given year. From waitlist information, I can infer which students were offered seats during the initial wave, hereafter denoted "initial offers." I can also observe basic demographic information for all teachers in the district and can link students to their teachers and classmates.

Prior to any sample restrictions, I observe roughly 30,000 non-special education 6th grade students from 2000 to 2007. Data are only available for students attending any of the traditional or magnet public schools within the LUSD, thus I cannot observe any students who transfer to a char-

[8]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).

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

ter or private school or who move out of the area entirely. Table I 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 93%) 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.

In columns 3 and 4, I further restrict the sample to students enrolled in a magnet school. I observe roughly 6,000 6th grade magnet students. Students enrolled in magnet schools are more likely to be black and have higher baseline achievement. I discuss the final column in Section V.A.

IV. Effect of Race-Blind Lotteries

IV.A. Race-Blind Lotteries and Racial Composition

To encourage racial diversity in its oversubscribed magnet schools, this LUSD held separate admissions lotteries for black and non-black students through the 2002-2003 school year. In subsequent years, this district instead used race-blind lotteries where the probability of winning was the same regardless of race or ethnicity. Because the race-conscious lottery procedure effectively protected magnet seats for non-black students, the change to race-blind lotteries likely increased black enrollment shares in magnet schools during subsequent years. Indeed, this shift is apparent in Figure I, which presents the proportion of black students enrolled in traditional and magnet schools across the time period of the study. From 2000 to 2002, even despite utilizing race-specific lotteries, magnet schools enrolled a higher proportion of black students than traditional schools.[10] Upon the introduction of race-blind lotteries in 2003-04, district administrators could no longer protect seats for non-black students, which resulted in roughly a 7 percentage point increase in the share of black magnet students over the next few years—increasing racial segregation. 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

[10] Some magnet schools lacked the non-black student enrollment required to ensure racial balance.

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).

Even though the lottery regime change directly impacted the racial composition of the 2003-04 entering class, the adjustment did not directly affect magnet school curricula or teaching staff.[11] As a result, understanding the implications of race-blind admissions in this context requires a careful examination of how pupils and teachers respond to changes in the student racial composition and how these changes ultimately impact student outcomes.

I estimate the effect of peer racial composition by exploiting the fact that increases in black enrollment shares are predictable based on application behavior under race-conscious lotteries. Prior to 2003, the racial composition of the students receiving initial seat offers did not necessarily reflect the composition of the full applicant pool, but it did so thereafter. I leverage the fact that the shift in racial composition for an entering cohort varied by how disproportionately non-black the pool of lottery winners were relative to all applicants. I measure how disproportionately non-black the offers were for a given school by calculating the difference between the fraction of black students in the given magnet's lottery pool and the fraction of black students receiving an offer averaged across each school year from 2000 to 2002 (denoted DB).[12] I consider traditional (non-magnet) schools as having a null DB value because most seats are filled using residential catchment zones instead of lotteries.[13]

Notice that schools with a higher DB value will likely experience a larger influx of black students upon the lottery change. To see this, consider a school (call it school A) where 80% of pre-2003 lottery applicants were black, but due to the race-specific lottery system, the school offered only 60% of its seats to black students. Also consider school B, where 60% of the students in the applicant pool were black and 60% of the students who received an initial offer were black. Suppose that the composition of the student applicant pool remains the same before and after the

[11]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 treatment.

[12]Results are robust to calculating DB based on different single years.

[13]Despite not contributing identifying variation, including traditional schools in the analysis helps identify year fixed effects (Borusyak and Jaravel, 2017).

introduction of race-blind lotteries in 2003. In that case, 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 formalize this intuition and estimate the impact of race-blind lotteries on school segregation in the following regression:

$$\text{Fraction Black}_i = \rho DB_s * \mathbb{1}(\text{Post }'02)_t + \gamma_1' X_i + \phi_s + \theta_t + \nu_i \tag{1}$$

where Fraction Black$_i$ is the leave-one-out fraction of black 6th graders enrolled in the school s that student i attends during year t (i.e., the year following the lottery).[14] DB is the difference between the fraction of black students in the lottery applicant pool and the percentage receiving an offer for school s averaged over the 2000 to 2002 school years. Specifically, $DB = \frac{1}{3}\sum_{t=2000}^{2002}\left(\frac{\sum_{i\in s,t}\mathbb{1}(\text{Black})_i}{N_{st}} - \frac{\sum_{i\in s,t}\mathbb{1}(\text{Black})_i\cdot\mathbb{1}(\text{Offer})_i}{\sum_{i\in s,t}\mathbb{1}(\text{Offer})_i}\right)$, where N_{st} is the total number of applicants to school s during year t.[15] $\mathbb{1}(\text{Post }'02)$ is an indicator variable equal to one if the student was in 6th grade after the 2002-03 school year. 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 4th grade reading and math achievement as well as indicator variables equal to one if the student is missing baseline achievement information for the given subject. ϕ_s and θ_t are school and year fixed effects. Importantly, the school fixed effects account for any direct impacts of attending a high-DB school on academic outcomes. Standard errors are clustered by school.

The coefficient of interest ρ is from the interaction between my measure of lottery racial disparity (DB) and an indicator for whether the student attended 6th grade after 2002. ρ gives the difference in the change in black enrollment shares between magnets with high and low DB val-

[14] Because the sample is limited to first-time 6th graders, every student only appears in the data once. I use student-level data instead of aggregating up to the school level so that I can include student-level baseline controls X_i.

[15] Several magnet schools have DB values near 0, while others have values ranging up to a .40 difference. The average DB is .20 among magnet middle schools.

ues. In other words, this regression 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.

Using (1), I also explore how race-blind admissions impact a variety of other student and teacher characteristics and outcomes.[16] For student achievement outcomes, to enhance precision I augment (1) by stacking test scores for students across grades 6, 7, 8, 10, and 11, additionally control for grade-of-test fixed effects, and two-way cluster standard errors by student and grade-by-year-by-school-of-test.[17]

It is important to note that while race-blind admissions explicitly augment magnet school racial composition, they also mechanically change the composition of other dimensions of student characteristics correlated with race. For example, because black students in this district tend to come from lower income families than non-black students, an exogenous increase in the percentage of black students at the school will also likely decrease the average socioeconomic status of the entering cohort. By estimating the reduced-form effect of attending high-DB relative to low-DB schools after the policy change, I remain agnostic as to whether any observed effects are directly driven by peer racial composition or whether effects are instead driven by characteristics that mechanically correlate with race or anything causally downstream from student racial composition, such as teacher sorting (Jackson, 2009). 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. As a result, while this reduced-form framework is unable to isolate the effect of peer racial composition on student outcomes, per se, I can estimate reduced-form parameters relevant to real-world desegregation and affirmative action policies.

Because the move to race-blind lotteries explicitly shifts the racial composition of entering cohorts, it is arguably sensible to model the causal effect of race-blind lotteries as operating through

[16] In some cases, outcomes are school-level s averages.

[17] For achievement regressions, school s and year t index the student's school and year where/when they first attended 6th grade.

peer racial composition in a two-stage least squares (2SLS) framework. As a result, in Online Appendix A, I also present 2SLS estimates where I use $DB * \mathbb{1}(\text{Post }'02)$ from (1) as an instrument for endogenous black enrollment shares in the following equation

$$y_i = \beta \, \widehat{\text{Fraction Black}}_i + \gamma_2' X_i + \phi_{2,s} + \theta_{2,t} + \epsilon_i. \tag{2}$$

Here, y_i is the outcome for student i and all other terms are defined as in equation (1).

The primary identification assumption for my reduced-form strategy is that unobserved determinants of student outcomes are not trending differentially by schools with varying DB levels. While the school fixed effects account for the fixed differences between high- and low-DB schools, they do not account for possible trend differences. However, in Section IV.C, I provide evidence against such differential trends in an event study framework. Further, due to the fuzzy nature of the empirical design, I must also impose a common trend condition for all potential outcomes—meaning that treatment effects are assumed to be stable over time (De Chaisemartin and D'Haultfoeuille, 2018). The event studies in Section IV.C also support this assumption.

In light of these identifying assumptions, it is worth discussing why schools differ as a function of DB, and why these level differences would not impact trends in unobserved determinants of the outcomes (Kahn-Lang and Lang, 2018). In Table II, I present the results from regressing the DB value for the school attended by each LUSD 6th grader on different school characteristics from 2000 to 2002, prior to the termination of race-based lotteries. First, higher-DB schools are located in neighborhoods with a larger share of black residents. In column 1, I show that a 10 percentage point increase in a school's DB value is associated with roughly an 11 percentage point increase in the share of black residents living in the Census block group of the given school during 2000. Presumably, the proximity of high-DB schools to high-minority-share neighborhoods makes the magnets particularly appealing to black families—explaining why black students disproportionately apply. Second, high-DB schools are better schools based on measurable inputs such as peer baseline achievement and teacher quality. In columns 2 through 4, I estimate that higher-DB

schools enroll 6th grade cohorts with higher 4th grade test scores, a higher share of which are black, and employ higher value-added teachers.[18] While the fact that higher-DB schools have better performing peers and teachers does not explain why black families disproportionately apply, it does help explain why non-black families choose to send their kids potentially long distances to attend schools located in high-minority-share neighborhoods.

Because the minority share in a school's surrounding neighborhood appears to strongly predict DB values, it is possible that trends in unobservable characteristics of neighborhoods could correlate with student outcomes. For example, suppose that the schools in the neighborhoods experiencing "white flight" are also steadily declining in their effectiveness. If "white flight" is increasing in high-minority-share neighborhoods where high-DB schools are located, then trends in "white flight" and school effectiveness would bias my estimates. However, because magnet schools admit students from the district at large, they plausibly are less sensitive to neighborhood-level changes than the nearby traditional schools, which enroll students using catchment zones. As a result, I argue that while high-DB and low-DB schools differ in initial levels, they plausibly do not correlate with trends in unobserved determinants of the outcomes. This argument is empirically supported across the various event studies in Section IV.C.

This empirical strategy is able to account for a variety of potential confounders. It can handle changes to policies that are contemporaneous with the lottery consolidation. If other policy changes do not differentially affect schools across various DB levels then these potential confounders will be controlled for directly by the year fixed effects. The 2003 passage of No Child Left Behind is a possible confounder of this type.

IV.B. No Child Left Behind and Magnet Applications

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

[18] The association between DB and black enrollment shares are less pronounced than black neighborhood shares due to the race-specific admissions criteria in place during these years.

contemporaneously implemented, the NCLB school placement mechanism potentially impacts the interpretation of my estimates. In this section, I provide details about how this district implemented NCLB as well as discuss how NCLB affects enrollment across schools as a function of DB.

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 mechanism.[19]

Students across the district were ranked using two inputs: the student's baseline testing and family income, where a low ordinal ranking signified the lower-achieving, poorer students in the district. Students attending a traditional school that failed to meet NCLB-determined academic measures were eligible to participate in the NCLB school placement mechanism. Prior to the magnet school admissions lotteries, students would rank order up to three schools of their choosing. 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-, and then third-choice school. If all three choices were full, the student would not receive a NCLB-seat and would have to apply to schools through the usual magnet lotteries. After NCLB seats were determined, the (now race-blind) magnet school lotteries were carried out normally.

The implementation of NCLB will complicate my analysis insofar as it alters the composition of entering magnet school cohorts as a function of DB. If magnet schools are desired by low-ranked students, the NCLB placement mechanism will cause magnets to enroll students with lower baseline achievement and family resources. Additionally, because black students have lower average achievement than non-black students in the district, NCLB seats may be disproportionately awarded to black students. Further, if students winning NCLB seats differentially sort into schools by DB levels, then my empirical strategy will be identified off variation from the NCLB

[19] LUSD magnet middle schools set aside roughly 20% of their 6th grade seats for NCLB placements though usually not all seats were filled.

mechanism in addition to the introduction of race-blind lotteries.

In Table III, I show that the composition of students accepting NCLB seats to magnet schools differs by the school's DB value.[20] The NCLB mechanism provided a slightly higher share of seats in high-DB schools to black students, though this result is not statistically significant. Column 2 shows that high-DB schools provide NCLB seats to higher-ranked students. In other words, the lowest-income and lowest-performing students with the highest placement priority are selecting into low-DB schools. Columns 3 and 4 show that this selection occurs along baseline achievement rather than socioeconomic status—NCLB seats to high-DB schools are disproportionately accepted by students with higher 4th grade test scores.

To summarize, both the introduction of race-blind lotteries and the NCLB placement mechanism generate the variation in peer composition that is leveraged in equation (1). However, because only 10-15% of magnet seats are filled using the NCLB mechanism, I view the lottery regime change as first-order and frame the discussion in the main text accordingly.

Moreover, both the addition of the NCLB placement mechanism and the introduction of race-blind lotteries may impact magnet school application decisions, which in turn may drive or confound my later estimates. However, in Appendix Table A.1, I show that the race, sex, and baseline achievement composition of applicants to magnets as a function of DB remains stable after the policy change. As a result, I conclude that changes in the composition of the magnet applicant pools are not a first-order concern.

IV.C. Validating the Identifying Assumptions

In this section, I use event studies to test whether observable characteristics of schools trend differentially as a function of DB. To begin, I assess the relationship between DB levels and black student enrollment shares by regressing an analog of equation (1) where I interact lottery

[20] Specifically, I limit the sample to 6th grade student accepting magnet seats through the NCLB mechanism and regress each outcome on the DB value of the accepted school.

racial disparity with year indicators instead of a post-2002 binary. Specifically, I estimate

$$\text{Fraction Black}_i = \sum_{\substack{t\,=\,2000;\\ t\,\neq\,2002}}^{2007} \rho_t DB_s \cdot \mathbb{1}(\text{Year} = t)_t + \gamma_1' X_i + \theta_t + \phi_s + \nu_i \;, \tag{3}$$

where variables are defined as in equation (1). Estimates are relative to 2002, the year before the district implemented race-blind lotteries. If low-DB schools provide a valid counterfactual for high-DB schools after the introduction of race-blind lotteries then, prior to the 2003 policy change, the share of black students enrolled in both types of schools should trend similarly. Further, a marked increase in the share of black enrollment in high- relative to low-DB schools after 2003 is evidence of the first-stage relationship. Figure IIa displays estimates of ρ_t from this regression. Indeed, under race-conscious lotteries, trends in black student enrollment shares between high- and low-DB schools are not statistically significantly different. However, upon the termination of race-conscious lotteries in 2003, I estimate that increasing a school's average racial lottery disparity by 10 percentage points (i.e., $DB = 0.1$) increases the proportion of black peers attending the school by roughly 4 percentage points. The absence of systematic differences in school racial composition by DB prior to 2003 followed by the sharp increase in black enrollment is evidence for the validity of this empirical framework.

The remaining panels of Figures II and III present estimates of the same regression, but for several important dimensions of peer and teacher composition and student outcomes. Panels IIb and IIc respectively present estimates for the leave-one-out averages of baseline achievement (i.e., the average over 4th grade math and reading scores) and free or reduced lunch eligibility among peer 6th graders within the student's enrolled school.[21] As expected, peer baseline scores decrease and lunch eligibility shares increase once race-blind lotteries are implemented.[22] Figure IId shows a similar event study for average 6th grade teacher value-added. After race-blind lotteries were

[21] Free or reduced price lunch eligibility comes from school-level averages from the CCD.

[22] Peer baseline achievement would likely have fallen more rapidly had the district not concurrently implemented their NCLB placement mechanism. The NCLB mechanism tended to give seats in high-DB schools to higher-achieving students—see Section IV.B for details.

instituted, the value-added composition of 6th grade teachers steadily declined in high-DB relative to low-DB schools.

Figure III presents event studies for several student outcomes. Figures for the remaining outcomes explored in this paper can be found in Online Appendix B. Non-black students who attend more segregated schools after the policy change are more likely to transfer out of the district. Further, student achievement as well as high school graduation and college enrollment rates fall in high-DB relative to low-DB schools after 2003. In general, the absence of pre-trends across this set of peer and teacher characteristics as well as own outcomes support the identifying assumptions underlying my estimation strategy.

IV.D. Segregation and Student Outcomes

Peer and Teacher Characteristics. How do race-blind admissions impact peer and teacher characteristics? I address this question in Table IV, which presents estimates of the effect of predicted increases in black enrollment shares on the composition of other peer and teacher attributes from equation (1). Schools that I predict will have a 10 percentage point increase in black enrollment shares due to race-blind admissions (i.e., $DB = 0.1$) instead experience an average increase of 4.4 percentage points.[23] Importantly, because this LUSD is a majority-minority district, an increase in the share of black students attending magnet schools represents an *increase* in school segregation.

The main results of this section come from the reduced-form impact of predicted increases in segregation. However, because race-blind admissions directly impact the racial composition of magnet schools, another approach would be to parameterize any student outcome effects as acting through a peer race causal channel. In a two-stage least squares (2SLS) framework, this would amount to scaling the reduced-form estimates in the main text by this first-stage estimate. However, similar to Billings et al. (2014), I focus on the reduced-form approach because a 2SLS framework requires stronger assumptions about how peer race effects accumulate over multiple

[23]For reference, DB takes on values from zero to 0.4 and has a standard deviation of 0.1.

years of exposure. Still, as a reference, I present 2SLS estimates in online Appendix C.

Increasing the proportion of black students entering magnet schools will likely also shift the student composition along other dimensions. Indeed, I find that under race-blind admissions, schools that I predict will have larger black inflows also enroll students who are more likely to be free-lunch eligible and who have lower peer baseline achievement on average. In addition, these schools tend to employ less experienced and lower value-added teachers.[24] I estimate that schools that are predicted to increase their black enrollment shares by 10 percentage points due to the policy change employ teachers with .3 fewer years of experience and a $.01\sigma$ lower value-added on average. To make estimates comparable to the previous literature, scaling this reduced-form effect by the first-stage estimate of .44 implies that a 10 percentage point increase in the share of black 6th-graders decreases the average value-added of 6th grade teachers by roughly 0.03σ (see Appendix Table C.1). These effects are smaller in magnitude than estimates of a 0.13 to 0.22σ decrease due to the end of forced busing in Charlotte-Mecklenburg (Jackson, 2009).

To further explore how teacher characteristics respond to the changing student body, I alter equation (1) by instead using teacher-year data, omitting the vector of controls X, and limiting the sample to teachers with non-missing value-added information. Changes in the overall teacher composition due to an inflow of black students could result from changes in the composition of new hires and the composition of teachers who exit. Similar to the analysis in Jackson (2009), Table V explores the impact of black enrollment shares on the characteristics of incumbent teachers (i.e., teachers not in their first year at the school) and new hires. Panel A roughly mirrors the results from Table IV. I estimate that the average experience and value-added among incumbent teachers falls for schools with larger predicted black enrollment shares. A 10 percentage point increase in predicted black enrollment shares is associated with nearly a $.03\sigma$ decrease in incumbent teacher value-added. Due to the small sample size, I am unable to detect any statistically

[24]Value-added is calculated using a simple test score growth model using data from 2000-2002 (Jackson, 2009). Specifically, I regress student test scores on baseline scores, student demographics, classroom baseline score and demographic averages, and a full set of teacher, year, and school fixed effects. The coefficients on the teacher fixed effects comprise my measure of value-added.

significant changes in the composition of new hires in Panel B. Together, these results suggest that any achievement effects estimated in the next section could be the result of changing peer racial composition directly or due to these other changes in peer and teacher make-up resulting from the increase in segregation.

Student Attrition. Magnet schools were originally conceived as a non-compulsive means to integrate districts. Magnets were designed to discourage non-minority families from relocating out of urban districts to enroll their children in suburban schools. The unusually detailed nature of this LUSD's enrollment data allows me to observe the extent to which these types of transfers occur. When a student withdraws from this LUSD, a record is kept on the type of transfer. I can distinguish between transfers to another district within the state; a district outside of the state; a private, charter, or home school within district boundaries; or whether the student was truant or expelled. These data uniquely allow me to test whether predicted increases in segregation cause students to leave the school district and whether these effects are heterogeneous across race. Further, because students who leave the district do not have outcome information, this exercise also determines the extent that sample attrition may impact my estimates.

Table VI provides OLS estimates of the impact of predicted changes in black enrollment shares on an array of attrition outcomes. Regression outcomes from columns 1 and 2 are whether the student is missing 10th grade achievement information and missing postsecondary outcome information, respectively. Panel A shows that higher predicted black enrollment shares (i.e., more racial segregation) increases the probability of missing outcome information among the full sample of students, but estimates are imprecise. Separate estimates for black and non-black students in Panel B reveal that this increase is entirely driven by non-black attrition. Non-black students who attend a 6th grade school that is predicted to enroll a 10 percentage point higher share of black students are roughly 3 percentage points more likely to be missing college outcome information after the implementation of race-blind admissions. As a result, in the following sections, I interpret any long-run estimates for non-black student subgroups with caution.

The remaining columns show to where these non-black students transfer. Nearly all of the non-black student attrition is accounted for by students transferring to another school district that is within the state. A 10 percentage point increase in predicted black enrollment shares causes non-black students to leave to another in-state school district by 4 percentage points (a 9% increase). This is consistent with Reber (2005), who finds that "white flight" due to the introduction of desegregation plans was more pronounced in districts with other public school districts available in the same metropolitan area—as is the case with this LUSD. This behavior is also consistent with evidence that families exhibit preferences for same-race peers (Glazerman and Dotter, 2017; Hastings et al., 2007). Because these magnets are majority-minority schools, non-black student attrition to outside districts further compounds the racial segregation stemming from the race-blind admissions. Higher predicted black enrollment shares causes both black and non-black students to be less likely to transfer to private schools, but has no detectable impact on other transfer types.[25] Finally, non-black students are more likely to leave the district due to being truant or expelled, but this estimate is not statistically significant. Together these results are consistent with the idea that school segregation resulting from race-blind lotteries undermines the ability of LUSD magnet schools to prevent "white flight."

Achievement and Postsecondary Attainment. Thus far, I have provided evidence that race-blind admissions procedures in this LUSD substantially increased black enrollment shares within entering magnet school cohorts, which in turn, caused higher-VA teacher attrition from the more segregated schools and induced non-black student attrition from the district. Next, I explore whether changing the racial makeup of the student body through race-blind lotteries impacts student achievement and postsecondary attainment. Table VII, presents reduced-form estimates from equation (1) for the full sample of students in Panel A and for particular subgroups in Panel B. As seen in column 1, I estimate that a student who attends a magnet that is predicted to experience a 10 percentage point increase in its black enrollment share will perform .04σ lower on 6th through 8th grade standardized tests after race-blind lotteries are implemented. This estimate is comparable to the previous literature. Scaling by the effect of predicted on realized black enrollment shares

[25] Other transfers include: home schools, charter schools, and voucher recipients.

(i.e., .44), the estimate is roughly $.1\sigma$ which is comparable to the relevant literature that relates a 10 percentage point increase in black enrollment shares to math achievement losses of 0.04 to 0.07 standard deviations (Billings et al., 2014; Hanushek et al., 2009). Panel B shows that estimated effects are relatively homogeneous by student race and sex, but that the effects of segregation from race-blind lotteries are more pronounced for students with below-median baseline achievement. I estimate that black enrollment has little impact on 10th grade achievement, though my imprecise estimates prevent ruling out relatively large impacts.

While segregation has a negative influence on student achievement, these potential short-term losses do not guarantee longer-term penalties to important education milestones such as high school graduation and postsecondary attainment. In columns 3 through 6, I show that the negative consequences of racial segregation are visible across several important medium-to-long-run educational outcomes. Estimates suggest that a 10 percentage point increase in the predicted share of a student's 6th grade peers who are black erodes high school graduation rates by 1.4 percentage points.

Heterogeneous estimates reveal large decreases for non-black students. However, due to the substantial attrition of non-black students who attend more segregated schools this estimate is likely biased downward. Using 4th grade baseline achievement as the outcome, I estimate equation (1) among the set of non-black students who have missing postsecondary outcomes. I estimate that racial segregation makes the composition of non-black attriters higher-achieving. A school with a 10 percentage point higher DB value loses students with an average of $.047\sigma$ ($p < .01$) higher baseline achievement than prior to 2002. As a result, the large negative effect of black peers on non-black graduation rates is likely at least partially driven by sample attrition.[26] While it is possible that peer racial composition has negative long-run consequences on non-black student outcomes,

[26]Similar to Lee (2009), I attempt to bound this estimate by trimming the sample. Specifically, I categorize schools as being high-DB and low-DB schools by whether the school has a $DB > .20$. I then trim the sample of low-DB schools after 2002 so that the following equality holds for non-black students, $\frac{\#High-DB,Post}{\#High-DB,Pre} = \frac{\#Low-DB,Post}{\#Low-DB,Pre}$. For upper bounds, I drop observations at the top of the outcome distribution. For the lower bound, I drop students at the bottom of the outcome distribution. The extent of the attrition generates uninformatively large bounds. The lower-bound estimate is -2.97 ($p < .01$) and the upper-bound estimate is .55 ($p < .10$).

due to the substantial non-black attrition, this analysis provides little insight. Conversely, because black student attrition was minimal, this analysis is better suited to explore the long-run impacts of segregation among this subgroup.

Among black students, school segregation negatively impacts postsecondary attainment. A 10 percentage point increase in predicted black enrollment shares decreases student enrollment in any postsecondary institution 6 months after high school graduation by 1.2 percentage points.[27] The magnitude of this effect is comparable to exposing a student from 6th through 12th grade to teachers having roughly $.2\sigma$ lower value-added (Chetty et al., 2014).[28] These losses are equally pronounced across student sex and baseline aptitude.

Columns 5 and 6 disaggregate overall postsecondary enrollment effects into 2-year and 4-year enrollment impacts. Segregation from race-blind lotteries primarily deteriorates black student enrollment in 4-year institutions. Lastly, the decrease in 4-year college enrollment is driven by black female and black high-achieving students.

Together, these results provide evidence that the racial segregation generated by race-blind admissions procedures negatively impacts education production in both the short- and long-run. However, it is unclear to what extent these negative effects are driven directly by black enrollment shares or by the changes to peer baseline achievement and teacher quality resulting from racial segregation. In the next section, I exploit cross-sectional lottery variation to disentangle the direct effect of peer racial composition from these other causal channels.

[27] This result is in line with Bergman (2018) who finds that being randomly offered a chance to transfer to a nearby low-minority, high-income school district increases college attendance.

[28] 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.

V. Estimating Magnet School and Peer Effects using Lottery Variation

V.A. Lottery Framework

Race-blind admissions policies directly manipulate the racial makeup of entering cohorts. As a result, understanding the effect of substantial changes in black enrollment shares gives insight into a primary policy impact. However, race-blind admissions also provide a higher share of desirable magnet school seats to black students who tend to be more disadvantaged than non-black students in this LUSD. If black students benefit more from magnet attendance than non-black students, this reshuffling could generate aggregate achievement gains—possibly offsetting the losses caused by racially segregating the district's magnet schools.

In this section, I leverage the randomized magnet school seat offers themselves to estimate the causal returns to magnet enrollment and test whether these returns are heterogeneous by race. Along the way, I also document how magnet schools compare to traditional schools. I accomplish this by estimating differences in peer and teacher composition, student achievement, and long-run outcomes for students who win a magnet school lottery seat relative to those who do not. Specifically, following Abulkadiroglu et al. (2014) and Angrist et al. (2016), I estimate:

$$y_i = \sum_j \delta_j d_{ij} + \rho M_i + \gamma' X_i + \epsilon_i \tag{4}$$

where y_i is an outcome for a student i who applies to any 6th grade magnet school lottery. X_i is a vector of pre-lottery demographics identical to equation (1).[29] M_i is an indicator equal to one if the student enrolled in a magnet school during the year following the lottery for at least one day. The d_{ij}'s are "risk-set" indicators as in Angrist et al. (2016), i.e., a unique application-portfolio-by-year combination. The inclusion of risk-sets ensures that comparisons are only made between students who apply to the same set of magnet schools. Standard errors for non-achievement outcomes are

[29] Similar to equation (1), for achievement outcomes, I stack test scores for grades 6, 7, 8, 10, and 11; add grade-of-test fixed effects; and instead two-way cluster standard errors by student and grade-by-school-by-year-of-test.

clustered by 6th-grade-school-by-year (Angrist et al., 2016).

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 I. As a result, I instrument for magnet enrollment using exogenous lottery offers through the following first-stage:

$$M_i = \sum_j \mu_j d_{ij} + \beta' X_i + \pi Z_i + \eta_i \ , \tag{5}$$

where Z_i is an indicator variable equal to one if student i receives at least one initial magnet offer. 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 risk set indicators. If offers are truly random, then predetermined student characteristics should be equally represented or “balanced” across winners and losers within risk sets. 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 risk set fixed effects. Column 5 of Table I presents these tests.[30] Lottery winners are comparable, on average, to losers across these observable dimensions. 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 and is also statistically insignificant. These regressions provide evidence that initial lottery offers are indeed random.

Another primary concern that potentially invalidates lottery-based empirical strategies is the differential attrition from the analysis sample between winners and losers. In Online Appendix D, I test for and find evidence of minor differential attrition for lottery losers. However, I show that results are robust to trimming procedures used by Abdulkadiroglu et al. (2015) and conclude that differential attrition is not a first-order concern in this setting.

V.B. Using Lotteries to Estimate Peer Race Effects

Section IV examines how students and teachers respond to a large, persistent shock in racial segregation and how these changes ultimately impact student outcomes. However, the reduced-form nature of these analyses attribute all direct and mediating effects to the increase in black enrollment shares. While I established that higher black enrollment shares mechanically lowered peer baseline achievement and led to decreased teacher quality, I can only say that the combination of those changes harm student outcomes. I cannot isolate separate causal channels.

In this section, I extend the estimation framework from equation (5) to estimate the impact of having a higher share of black peers by isolating exogenous lottery-induced variation in several dimensions of peer characteristics as well as teacher quality. This strategy allows me to jointly estimate multiple causal channels to help disentangle which effects from Section IV are driving the estimated losses. In addition to jointly estimating several causal channels, this analysis complements the results from Section IV by testing whether racial composition impacts student outcomes

[30] 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. Finally, given these restrictions, I drop any students who are the only ones in the district applying to the given magnet lottery after other sample restrictions are applied.

using a different source of variation.[31]

Despite the race-conscious lotteries utilized prior to 2003, there is still substantial variation in peer racial composition across magnet middle schools and between magnet and traditional schools during that period.[32] In addition to providing random variation in magnet enrollment, lottery offers induce exogenous variation in exposure to peers, providing a natural instrument for peer characteristics. To see this, consider two students who apply to the same two magnet schools, but student 1 receives a seat only to magnet A and student 2 only to magnet B. As a result, both students have the option to attend a magnet school though the *ex-ante* peer group composition of the magnet school in their offer sets likely differ. Suppose magnet A enrolled a higher proportion of black students than magnet B during the year before the lottery was held. Even if both students prefer schools with a higher proportion of non-black students and thus would like to attend school A, only student 1 has the option to attend. Thus, lottery offers provide an instrument for magnet attendance, while *ex-ante* measures of peer characteristics across offer sets provide additional instruments for realized peer groups.

I operationalize this rationale by calculating, for each student i, the school with the lowest baseline black share among the schools in i's lottery offer set.[33] For students who receive no offers, I assign to them the racial composition of their assigned 6th grade school based on residence during the previous year. Specifically, for student i who is offered seats to enter the set of magnet schools $\mathbb{M}_i$ during year t, I calculate:

$$\text{(6)} \qquad \text{Offered Peer Group}_{it}^{black} = \mathbb{1}(\text{Any Offer})_{it} * \min_{j \in \mathbb{M}_i} \{\text{Frac. black}_{j,t-1} * 100\} + (1 - \mathbb{1}(\text{Any Offer})_{it}) * (\text{Frac. black}_{j=R,t-1} * 100)$$

[31] In Online Appendix E, I replicate this analysis using only pre-NCLB years, which provides an additional robustness check that NCLB was not driving the previous estimates. The main results carry through, though with less statistical precision.

[32] From 2000 to 2002, magnet middle school student bodies were roughly 75% black (with a standard deviation of 0.29) — nearly 20 percentage points higher than traditional middle schools.

[33] If students prefer schools with lower black enrollment shares (i.e., more integrated), then assigning the value of the school with the lowest *ex-ante* black enrollment share will best predict the student's realized peer group.

where $\mathbb{1}(\text{Any Offer})$ is an indicator equal to one if the student won any magnet offer and R is student i's default 6th grade school based on residence. This exogenously determined baseline measure should highly correlate with the realized *ex-post* 6th grade peer composition in the school where student i enrolls—providing a natural instrument. Note that Offered Peer Group$_{it}^{black}$ varies by the set of magnets that offer a seat as well as the default school of residence and is mediated by the take-up rate. I also use a similar measure of lottery-induced peer baseline achievement as an instrument for the realized peer achievement composition, e.g., Offered Peer Group$_{it}^{reading}$. Baseline achievement measures are calculated similarly, but for students offered a magnet seat, I calculate the *maximum* average peer baseline achievement over offered schools during $t-1$.[34] Finally, I calculate similar *ex-ante* measures of teacher value-added.

To estimate the impact of peer racial composition on student achievement and postsecondary attainment, I augment equations (4) and (5) by adding different combinations of the endogenous variables and the corresponding instruments described above. This procedure estimates several causal channels simultaneously by including endogenous regressors for magnet enrollment and realized peer composition while instrumenting with lottery offers and the *ex-ante* baseline offered peer groups. Jointly estimating multiple causal channels allows for the possibility of countervailing or complementary effects. This procedure extends the analyses of Abulkadiroglu et al. (2014), who perform a similar analysis but in a regression discontinuity framework to test for peer effects in Boston and New York's selective exam schools. The identifying assumptions are more demanding for instrumental variables models with multiple endogenous variables. For instance, I must assume that treatment effects are homogeneous due to the lack of a local average treatment effect theorem for 2SLS models with multiple endogenous variables. As a result, I focus the discussion in the main text on reduced-form regressions that do not impose these assumptions and I present the 2SLS counterparts in the Online Appendix.

[34] If students prefer schools with higher-achieving peers, then assigning the value of the school with the highest peer achievement will best predict the student's realized peer group.

V.C. *Magnet Enrollment and Peer Effects*

I estimate that the move to race-blind admissions increased school segregation in LUSD magnet schools—ultimately harming student outcomes. However, in addition to changing the racial composition of the student body, the race-blind admissions also provided a higher share of desirable magnets seats to black students. From a social planner's perspective, providing more seats to black students could offset the observed segregation losses assuming that magnet effects are heterogeneous across race. The analysis in Section IV is silent on the returns to magnet enrollment. As a result, in this section, I leverage lottery offers to estimate the causal returns to magnet enrollment. Along the way, I also utilize the cross-sectional lottery variation to provide suggestive evidence for the existence of mediators of the segregation effects estimated in Section IV.

Teacher and Peer Characteristics. I begin by analyzing how enrolling in a magnet school changes a student's exposure to different dimensions of peer composition and teacher quality. Together these effects help characterize the magnet enrollment treatment. The previous estimates from Section IV abstract away from characterizing how magnet schools differ from traditional schools due to the inclusion of school fixed effects. In this section, I instead use cross-sectional lottery variation to provide insight into how magnet schools generally differ from traditional schools in this district.

The odd-numbered columns in Table VIII present the effect of a magnet lottery offer on a variety of peer and teacher characteristics at the student's enrolled school using equation (4). I display cohort averages of peer characteristics in Panel A and teacher characteristics in Panel B. Receiving an initial offer to attend a magnet school increases enrollment in *any* magnet by roughly 20 percentage points.[35] While this may seem low considering the popularity of this district's magnet schools, because I only consider initial offers, students who enroll via a waitlisted offer drive my first-stage estimate towards zero.

How do magnet schools differ from traditional schools in this district? Magnet offers expose students to a higher share of black and high-achieving peers and a lower share of low-income peers

[35] See Table C.9 for the full set of 2SLS estimates.

(as measured by free/reduced lunch eligibility). Students who receive offers to attend a magnet school are taught by more-experienced and more-effective teachers (as measure by value-added), an equal share of whom are black.

To characterize the implications of enrolling in schools with differences in predicted racial compositions, in the even columns, I present reduced-form estimates of two causal channels by introducing the offered share of black peers from (6) as an additional variable. Increasing the minimum *ex-ante* share of black students within the set of magnet offers by 10 percentage points increases the *ex-post* black share in the student's 6th grade cohort by roughly 3 percentage points showing this exogenous measure is able to predict a portion of the variation in the experienced *ex-post* peer racial composition.

The impact of the predicted share of black peers on other peer characteristics shows how student attributes are interconnected, reinforcing the idea that a change in the racial composition of students will likely change other demographic dimensions. In Panel A, I estimate that being offered a higher share of black peers in a cohort mechanically generates exposure to peers with lower average peer baseline aptitude, a higher share of whom are free/reduced lunch eligible. Thus, a shock to the student racial composition should be expected to change student demographics along these dimensions as well. Indeed, in Section IV, I showed that schools with an increase in black enrollment shares experienced an increase in free-lunch eligible students shares and a decrease in average peer baseline achievement.

Estimated impacts of offered peer race on teacher traits reveal how characteristics of teachers vary across magnets enrolling higher shares of black students. Students that are randomly offered seats to attend magnets with a higher share of black peers experience a group of teachers that are more likely to be black, less-experienced, and are lower quality on average (based on value-added).

Taken together, these results using cross-sectional lottery variation confirm the relationships from Section IV. The most consistent findings are that schools with a higher share of black peers enroll a higher share of free-lunch eligible and lower achieving students, as well as employ lower-value-added teachers. These estimates show how the magnet school treatment conflates many dimensions of peer and teacher quality. With this in mind, I now turn to estimating the causal impact of magnet middle school enrollment on student outcomes.

Student Achievement and Postsecondary Attainment. I begin with a straightforward assessment of the returns to attending a magnet middle school in this LUSD by presenting 2SLS estimates from equation (4). In Panel A of Table IX, I estimate that magnets generate substantial increases to achievement in the short- and medium-run, and imprecise gains in the longer-run. Students induced by the lottery offer to enroll in a 6th grade magnet school experience roughly a $.4\sigma$ increase in middle school and high school standardized scores relative to students attending traditional schools. These estimates are much larger than the magnet returns estimated in the recent literature on the order of $.14$-$.20\sigma$ (Betts et al., 2006; Bifulco et al., 2009; Hastings et al., 2012). Panel B shows that these gains are statistically indistinguishable between black and non-black students.

Because black students are not gaining substantially more from magnet enrollment relative to non-black students, the fact that LUSD race-blind admissions provided a higher share of seats to black students arguably had little impact on aggregate achievement along this dimension. Against the backdrop of Section IV, it appears that providing more seats to black students does not offset the losses incurred from magnet school segregation.[36]

Potential Mediators of Peer Race Effects: Suggestive Evidence. In this section, I exploit exogenous lottery variation in peer and teacher characteristics to explore potential mediators of the observed peer race effects from race-blind admissions estimated in Section IV. Table X presents reduced-form versions of equation (4) where I further augment (4) by simultaneously estimating causal channels for general magnet impacts along with other peer and teacher causal channels. For comparison, columns 1 and 5 present reduced-form counterparts to the 2SLS estimates from Table IX. In columns 2 and 6, I include a measure of offered peer racial composition using (6). I find

[36]Implicit in this argument is that the social planner puts equal weight on the achievement of each student regardless of race. If instead, the social planner put more weight on disadvantaged students, a higher share of which happen to be black in this LUSD, then providing more seats to black students would increase social welfare.

evidence that the causal effects of magnet schools and peer racial composition are offsetting for 6th and 10th grade achievement. In these specifications, offered peer race offsets a larger, positive magnet lottery offer effect of roughly .09σ. If having a higher share of minority peers generates negative effects, then because the average magnet enrolls a higher black share than the average traditional school, it is sensible that this peer effect could offset general magnet returns.[37]

I estimate that increasing the offered share of black peers decreases both 6th and 10th grade achievement, though estimates are only statistically significant for the former. Receiving offers to attend a school with a 10 percentage point higher share of black peers, i.e., more racially segregated, leads to reductions in 6th and 10th grade scores of roughly 0.02 and 0.01σ, respectively. To put these effect sizes into context, this is roughly equivalent to the estimated achievement losses that would accompany permanently increasing class sizes by 1 student (Angrist and Lavy, 1999).[38] The corresponding 2SLS estimate from Table C.13 is roughly -.07σ, which is comparable to recent work estimating segregation-induced losses to achievement on the order of 0.04 to 0.07σ (Billings et al., 2014; Hanushek et al., 2009). Further, these estimates are smaller but comparable to my estimates of achievement losses due to the introduction of race-blind lotteries from Section IV.

To explore whether the substantive peer racial effects are mediated by peer baseline achievement, in columns 3 and 7, I estimate these reduced-form causal channels simultaneously. For both 6th and 10th grade achievement, my estimates suggest that peer baseline aptitude mediates a portion of the offered peer race effect. After allowing peer baseline aptitude to have its own causal channel, point estimates on black enrollment shares change signs, but are noisy—a finding that supports the conclusion reached by Hoxby and Weingarth (2005). I take this as suggestive evidence that some of the negative impact of racial segregation stemming from race-blind admissions could potentially be due to the observed decrease in peer baseline achievement.

Teacher value added (VA) is another potentially important mediator. In columns 4 and 8, I

[37]For example, in Connecticut, Bifulco et al. (2009) show that urban magnets enroll a higher share of white students relative to traditional schools, while the opposite is true for suburban magnets. They also find that across most specifications, math magnet achievement gains are more pronounced for the urban than suburban magnets.

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

replace the offered peer baseline achievement measure with an *ex-ante* measure of offered VA composition constructed analogously to the other instruments in this section.[39] The coefficients on black enrollment shares become larger in magnitude when jointly estimating a teacher VA causal channel. Because teacher VA does not mediate the impact of peer racial composition on achievement in this setting, the shift in teacher quality following race-blind lotteries possibly also played a smaller mediating roll in that context.

Black enrollment shares impact short-term achievement as measured by standardized tests, but do these effects persist and influence high school graduation and postsecondary attainment? To explore this question, I present estimates on these outcomes from the same empirical design in Table XI.[40] Across the outcomes, results are imprecisely estimated. Columns 2 and 6 show that black enrollment shares negatively impact high school graduation rates and college attendance, but these estimates are not statistically significant. The peer baseline achievement and teacher VA compositions appear to have substantial implications for college enrollment. I estimate that increasing offered peer quality (teacher VA) by $.1\sigma$ increases the likelihood of attending college by 1.2 (3.2) percentage points relative to the mean college attendance rate of 0.58. Again, peer baseline achievement appears to mediate the negative high school and postsecondary race effects. Estimating a separate causal channel for teacher VA has little impact on how peer race affects high school graduation and college enrollment.

To summarize, the analyses in this section compliment my estimates of the effect of racial segregation from Section IV. First, because magnet returns are not heterogeneous by student race, the fact that race-blind admissions provided more seats to black students likely had little impact on aggregate achievement. Second, despite using different sources of exogenous variation in peer racial composition, estimates from both empirical frameworks lead to similar conclusions—namely, that

[39]Specifically,

$$\begin{aligned}\text{Offered Teacher Value-Added}_{it} &= \mathbb{1}(\text{Any Offer})_{it} * \max_{j\in\mathbb{M}_i}\left\{\text{Avg. Teacher Value-Added}_{j,t-1} * 100\right\} \\ &\quad + (1-\mathbb{1}(\text{Any Offer})_{it}) * \left(\text{Avg. Teacher Value-Added}_{j=R,t-1} * 100\right),\end{aligned}$$

where variables are defined as in (6).

[40]See Online Table C.15 for two-year and four-year school outcomes.

racial segregation generates achievement and postsecondary attainment losses. Finally, these analyses provide suggestive evidence that the losses caused by the racial segregation following race-blind admissions may have been mediated by changes in peer baseline aptitude (Hoxby and Weingarth, 2005) as opposed to teacher quality.

VI. Conclusion

The changing legal landscape over the past several decades has increasingly weakened desegregation efforts across the United States. In the early 1990s, a set of Supreme Court rulings enabled school districts to dismantle their court-ordered integration plans. More recently, the Supreme Court ruled that school assignment policies that explicitly use race in admissions decisions were unconstitutional (*Parents Involved in Community Schools v. Seattle School District No. 1* – 2007). Just this year, the Trump administration rescinded seven policy guidelines on affirmative action put forth under the previous Obama administration—making it even more difficult for schools to consider race in admissions decisions (United States Department of Justice, 2018). In this context, it is not suprising that the United States education system has grown increasingly racially *de facto* segregated since the end of court-ordered desegregation (Clotfelter et al., 2008, 2006; GAO, 2016; Lutz, 2011; Reardon et al., 2012).

While we have growing evidence that early school integration efforts substantially improved educational and adult outcomes for black students (Billings et al., 2014; Guryan, 2004; Johnson, 2015; Lutz, 2011), we understand very little about the more recent rulings that require race-neutral admissions criteria. I study an unusual policy change where a large urban school district (LUSD) was no longer allowed to integrate incoming cohorts through race-based admissions lotteries, which effectively acted as an affirmative action policy for white students. Because magnet schools became racially segregated following this change to race-blind lotteries, this setting provides an excellent natural experiment that reflects the growth in segregation spreading across the nation's school system.

A first-order consequence of the LUSD race-blind admissions was a substantial change in the racial composition of incoming magnet cohorts. Along this dimension, I find the policy change has several meaningful impacts on the magnet school landscape. Policy-induced segregation causes magnets to enroll lower-achieving students, teachers to sort in a way that reduces the average value-added, and white students to later transfer out of the district—further exacerbating segregation. Ultimately, the change in peer racial composition driven by race-blind admissions decreases student achievement and postsecondary outcomes.

A second primary consequence of race-blind admissions was that it provided a higher share of magnet seats to black students, which could lead to aggregate achievement gains if black students disproportionately benefit from magnet schools. However, using random lottery offers, I find that the causal returns to magnet enrollment are statistically indistinguishable between black and non-black students. Taken together, these results suggest that the main consequences of race-blind admissions in this LUSD decreased aggregate achievement.

Further, using exogenous variation in peer groups arising from randomized magnet seat offers, I find suggestive evidence that the negative peer race effects are possibly mediated by the accompanying changes in peer baseline aptitude. As a result, the decrease in academic outcomes resulting from segregation induced by race-blind admissions may partially be explained by the concurrent decline in peer baseline achievement.

While school assignment policies that explicitly use race in admissions decisions have been declared unconstitutional, my results suggest that more creative policies aimed at integrating schools can generate 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. Ellison and Pathak (2016) analyze and propose several alternative race-neutral mechanisms, but they also note that race-neutral alternatives are not as effective at integrating schools as race-conscious policies. The school district explored in this study provides evidence that integration efforts through race-conscious admissions can improve student outcomes. More generally, these findings underscore the importance of accounting for peer groups when determining the anticipated impacts of education interventions that change the student body composition.

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VII. Tables and Figures

TABLE I: Descriptive Statistics and Balance Test

	All LUSD Students		Magnet Students		Magnet Lottery Sample
	Mean	N	Mean	N	Initial Offer Gap
	(1)	(2)	(3)	(4)	(5)
Black	0.634	30,223	0.827	6,150	0.001 (0.007)
White	0.300	30,223	0.132	6,150	-0.001 (0.007)
Female	0.507	30,778	0.545	6,234	-0.016 (0.014)
Baseline Math	0.512	18,501	0.659	4,069	-0.003 (0.030)
Baseline Reading	0.234	21,358	0.362	4,752	0.013 (0.027)
			Combined p-value:		0.973

Notes: The sample for columns 1 and 2 includes all non-special education 6th grade students in the LUSD from 2000 to 2007. Columns 3 through 4 further restrict the sample to students who enrolled in a magnet school. The sample is further restricted to the magnet lottery sample as detailed in section V.A. 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. Heteroskedasticity-robust standard errors (in parentheses). p-value tests the hypothesis that all coefficients on initial offer indicators are zero.

TABLE II: Correlates of DB Prior to 2003

	Fraction Black in School's Census Block Group ('00)	Peer Composition: 4th Grade Achievement	Peer Composition: Black	Avg. Teacher Value-Added
	(1)	(2)	(3)	(4)
DB	1.097**	0.860***	0.499	0.628***
	(0.476)	(0.277)	(0.308)	(0.139)
Observations	10,840	9,925	12,616	12,666

Notes: *, **, and *** denote statistical significance at the 10, 5, and 1 percent levels, respectively. I regress each outcome specified in the column heading on the average difference between the share of black applicants and black winners in the given school's admissions lottery (i.e., DB) among 6th graders from 2000-2002. Standard errors are clustered by school. Each regression excludes students qualifying for special education. The outcome in Column 1 is the fraction of black residents living in the given school's Census block group in 2000.

TABLE III: Characteristics of Students Accepting NCLB Offers by DPB

	Black (1)	NCLB Rank (2)	FRL-Eligible (3)	Average of 4th Grade Math & Reading Achievement (4)
Lottery Disparity (DB)	0.096	1137.945***	0.035	0.584**
	(0.077)	(402.215)	(0.111)	(0.257)
Outcome Mean	0.916	2,730	0.849	0.261
N	903	916	913	683

Notes: *, **, and *** denote statistical significance at the 10, 5, and 1 percent levels, respectively. Estimates are obtained by regressing the variable in the column header on DPB among the set of NCLB applicants accepting a seat through the NCLB mechanism. Heteroskedasticity-robust standard errors are in parentheses.

TABLE IV: Effect of Student Racial Composition on Student and Teacher Characteristics

	Peer Composition			Teacher Composition		
	Black	FRL-Eligible	Baseline Achievement	Black	Experience	Value-Added
	(1)	(2)	(3)	(4)	(5)	(6)
$DB \times \mathbb{1}(\text{Post 2002})$	0.441***	0.228*	-0.193***	-0.000	-3.011*	-0.137**
	(0.087)	(0.123)	(0.071)	(0.060)	(1.599)	(0.059)
Outcome Mean	0.634	0.732	0.346	0.191	9.422	-0.146
Observations	30,138	30,083	26,589	30,043	30,043	30,090

Notes: *, **, and *** denote statistical significance at the 10, 5, and 1 percent levels, respectively. Regressions follow equation (1) where each outcome is regressed on school and year fixed effects and indicators for student gender and race. The regressor of interest is the interaction between whether the observation is after 2002 and the average difference in the share of black students (DB) between the total magnet lottery pool and the pool of initial offers from 1999-2002. Standard errors are clustered by school. Each regression excludes students qualifying for special education.

TABLE V: Effect of Segregation on Teacher Sorting

	Black (1)	Experience (2)	Value-Added (3)
Panel A: *Incumbent Teachers*			
$DB \times \mathbb{1}(\text{Post 2002})$	0.052	-3.479	-0.271***
	(0.146)	(2.824)	(0.105)
Outcome Mean	0.209	10.062	-0.155
Observations	3,269	2,700	3,269
Panel B: *New Hires*			
$DB \times \mathbb{1}(\text{Post 2002})$	-0.095	0.482	0.069
	(0.794)	(6.282)	(0.437)
Outcome Mean	0.228	5.738	-0.200
Observations	661	511	661

Notes: *, **, and *** denote statistical significance at the 10, 5, and 1 percent levels, respectively. Regressions follow equation (1) where each outcome is regressed on school and year fixed effects. The regressor of interest is the interaction between whether the observation is after 2002 and the average difference in share of black students (DB) between the total magnet lottery pool and the pool of initial offers from 1999-2002. Standard errors are clustered by school. Panel A presents estimates among the set of teachers not in their first year, while Panel B restricts attention to teachers in their first year at the given school.

TABLE VI: Effect of Student Racial Composition on Sample Attrition and Withdrawal Reason

	Missing Outcome		Reason for Withdrawal			
			Attend School			
	10th Grade (1)	NSC (2)	Outside LUSD (3)	Private (4)	Other (5)	Truant or Expelled (6)
Panel A: Pooled Sample						
$DB \times \mathbb{1}(\text{Post 2002})$	0.111	0.069	0.127**	-0.100***	0.028	0.008
	(0.078)	(0.087)	(0.056)	(0.031)	(0.046)	(0.035)
Outcome Mean	0.499	0.570	0.404	0.027	0.140	0.105
Panel B: Subgroups						
Non-Black	0.430**	0.318*	0.362***	-0.239*	0.027	0.084
	(0.170)	(0.180)	(0.088)	(0.123)	(0.153)	(0.059)
Black	-0.011	0.007	0.068	-0.065***	0.018	-0.022
	(0.096)	(0.090)	(0.086)	(0.016)	(0.030)	(0.038)

Notes: N=30,138. *, **, and *** denote statistical significance at the 10, 5, and 1 percent levels, respectively. Regressions follow equation (1) where each outcome is regressed on school and year fixed effects and indicators for student gender and race. The regressor interest is the interaction between whether the observation is after 2002 and the average difference in the share of black students (DB) between the total magnet lottery pool and the pool of initial offers from 2000-2002. Standard errors are clustered by school. Each regression excludes students qualifying for special education.

TABLE VII: Effect of Peer Racial Composition on Student Achievement, High School Graduation, and College Enrollment

	Achievement			College Enrollment (6 months after HS)		
	Middle School (1)	High School (2)	Grad. HS (3)	Any (4)	2-year (5)	4-year (6)
Panel A: *Pooled Sample*						
DB$*\mathbb{1}$(Post′02))	-0.444***	0.063	-0.140**	-0.092	-0.033	-0.046
	(0.126)	(0.114)	(0.061)	(0.067)	(0.046)	(0.048)
Panel B: *Subgroups*						
Non-Black	-0.511***	0.328	-0.449***	0.052	0.134	-0.052
	(0.157)	(0.218)	(0.076)	(0.230)	(0.133)	(0.172)
Black	-0.463***	0.094	-0.040	-0.124**	-0.022	-0.099**
	(0.144)	(0.119)	(0.054)	(0.056)	(0.036)	(0.045)
Black, Male	-0.457***	0.114	-0.107	-0.150	-0.094	-0.049
	(0.137)	(0.186)	(0.071)	(0.146)	(0.120)	(0.050)
Black, Female	-0.431**	0.068	0.012	-0.130	0.032	-0.164**
	(0.179)	(0.129)	(0.091)	(0.097)	(0.072)	(0.082)
Black, Below Median	-0.376*	0.020	-0.049	-0.083	-0.075	0.018
	(0.199)	(0.173)	(0.082)	(0.107)	(0.078)	(0.132)
Black, Above Median	-0.156	0.126	-0.056	-0.126	0.031	-0.168
	(0.134)	(0.151)	(0.066)	(0.126)	(0.057)	(0.140)

Notes: *, **, and *** denote statistical significance at the 10, 5, and 1 percent levels, respectively. Regressions follow equation (1) where each outcome is regressed on school and year fixed effects and indicators for student gender, race, and quadratics for baseline math and reading achievement. The regressor of interest is the interaction between whether the observation is after 2002 and the average difference in the share of black students (DB) between the total magnet lottery pool and the pool of initial offers from 1999-2002. Achievement testing is conducted in grades, 6, 7, 8, 10, and 11. Columns 1 and 2 pool achievement across these grades and additionally control for grade-of-test fixed effects. Students only appear once in each of the regressions in columns 3 through 6. Standard errors in columns 1 and 2 are two-way clustered by school-of-test-by-grade and student while standard errors in the remaining columns are clustered by school. Each regression excludes students qualifying for special education. Observation counts and outcome means are provided in Appendix Tables C.6 and C.8, respectively.

TABLE VIII: Effect of Magnet Offer and Offered Peer Composition on Teacher and Peer Characteristics (2000-2007)

	Fraction Black		Fraction FRL		Peer Baseline Achievement	
Panel A: *Peer Characteristics*	(1)	(2)	(3)	(4)	(5)	(6)
Magnet lottery offer	0.025***	-0.012	-0.016**	-0.027***	0.041**	0.063***
	(0.009)	(0.008)	(0.007)	(0.009)	(0.020)	(0.023)
Offered share of black peers		0.281***		0.086***		-0.168***
		(0.034)		(0.020)		(0.047)
	Fraction Black		**Average Exp.**		**Average VA**	
Panel B: *Teacher Characteristics*	(7)	(8)	(9)	(10)	(11)	(12)
Magnet lottery offer	-0.006	-0.020***	0.239**	0.374**	0.038***	0.048***
	(0.006)	(0.008)	(0.110)	(0.149)	(0.008)	(0.010)
Offered share of black peers		0.111***		-1.018**		-0.075***
		(0.021)		(0.451)		(0.025)

Notes: *, **, and *** denote statistical significance at the 10, 5, and 1 percent levels, respectively. Each column presents results from separate regressions. Regressions follow equation (4) 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. The variable of interest is whether the student received an initial lottery offer as well as the lottery-induced peer/teacher offerings from equation (6). Standard errors are clustered by 6th-grade-school-by-year. Each regression sample is limited to students applying to a 6th grade magnet school lottery from 2000-2007.

TABLE IX: 2SLS Estimates of Magnet Enrollment on Achievement, High School Graduation, and Postsecondary Enrollment (2000-2007)

	Achievement			
	Middle School	High School	Graduate High School	Attend College (6 Months after Graduation)
	(1)	(2)	(3)	(4)
Panel A: *2SLS Estimates (Pooled Sample)*				
Enrolled in Magnet	0.392^{***}	0.415^{***}	0.148	0.085
	(0.090)	(0.126)	(0.099)	(0.117)
Observations	12,479	6,630	3,592	2,871
First-Stage F	76.034	74.302	25.902	22.161
Panel B: *2SLS Estimates (Subgroups)*				
Non-Black	0.434^{***}	0.446^{**}	0.233	-0.147
	(0.162)	(0.215)	(0.174)	(0.210)
First-Stage F	42.927	21.356	11.986	7.353
Black	0.391^{***}	0.404^{***}	0.144	0.100
	(0.106)	(0.151)	(0.108)	(0.140)
First-Stage F	62.241	59.916	23.055	20.506

Notes: *, **, and *** denote statistical significance at the 10, 5, and 1 percent levels, respectively. Each column presents results from separate regressions. Regressions follow equation (4) 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 and quadratics in math and reading baseline achievement. I instrument for the endogenous magnet attendance variable with whether the student received an initial lottery offer. Standard errors are clustered by 6th-grade-school-by-year. Each regression sample is limited to students applying to a 6th grade magnet school lottery from 2000-2007. Achievement testing is conducted in grades, 6, 7, 8, 10, and 11. Achievement regressions stack scores over these grades and additionally control for grade-of-test fixed effects. Standard errors in achievement regressions are two-way clustered by school-of-test-by-grade-by-year-of-test and student.

TABLE X: Effect of Magnet Offer and Offered Peer Composition on Achievement (2000-2007)

	Middle School Achievement				High School Achievement			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Magnet lottery offer	0.071***	0.094***	0.033	0.076***	0.068***	0.090***	0.046*	0.066**
	(0.015)	(0.018)	(0.023)	(0.023)	(0.020)	(0.025)	(0.028)	(0.027)
Offered share of black peers		-0.158***	-0.065	-0.260***		-0.103	-0.020	-0.205**
		(0.052)	(0.064)	(0.065)		(0.065)	(0.074)	(0.082)
Other offered characteristic			0.133***	0.179			0.065	0.118
			(0.036)	(0.122)			(0.043)	(0.158)
Characteristic Type			Mean Peer Reading	Teacher VA			Mean Peer Reading	Teacher VA
Observations	12,479				6,630			
Outcome Mean	0.190				0.071			

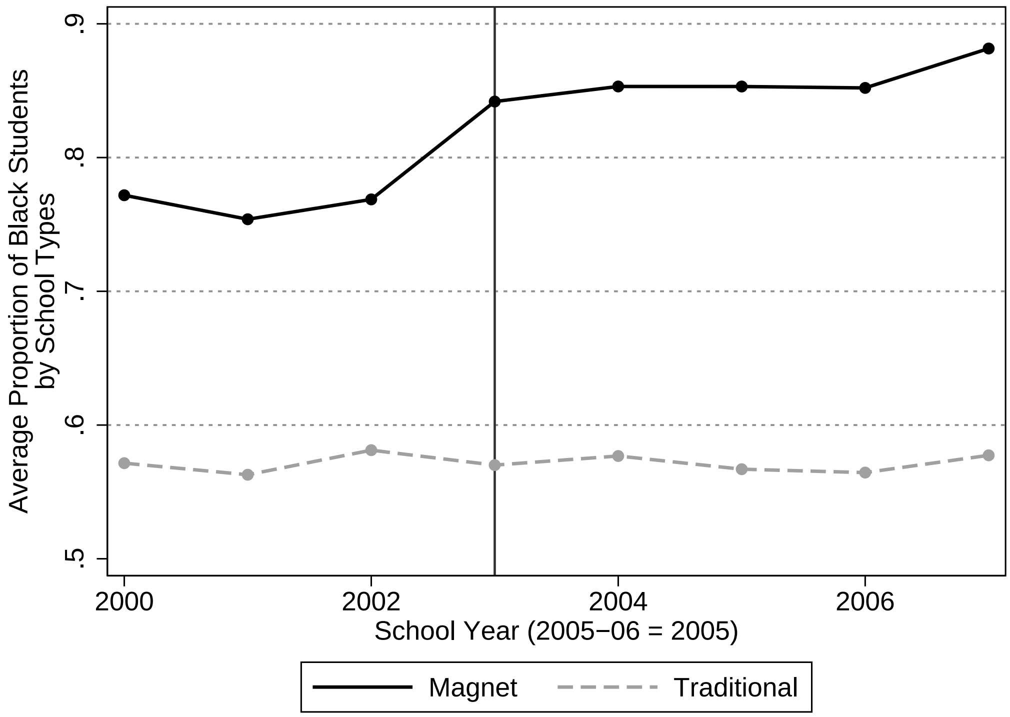
Notes: *, **, and *** denote statistical significance at the 10, 5, and 1 percent levels, respectively. Each column presents results from separate regressions. Regressions follow equation (4) where each outcome is regressed on a indicator equal to one if the student attended a magnet school during the year following the lottery as well as indicators for student gender, race, and risk-sets and quadratics in math and reading baseline achievement. The variable of interest is whether the student received an initial lottery offer as well as the lottery-induced peer/teacher offerings from equation (6). Achievement testing is conducted in grades, 6, 7, 8, 10, and 11. Regressions pool achievement across these grades and additionally control for grade-of-test fixed effects. Standard errors are two-way clustered by school-of-test-by-grade-by-year-of-test and student. Each regression sample is limited to students applying to a 6th grade magnet school lottery from 2000-2007.

TABLE XI: Effect of Magnet Offer and Offered Peer Composition on High School Graduation and Postsecondary Enrollment (2000-2007)

	Graduate High School				College Attendance (6 Months after Graduation)			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Magnet lottery offer	0.028	0.033*	-0.006	0.042**	0.015	0.023	-0.017	0.012
	(0.018)	(0.018)	(0.025)	(0.020)	(0.022)	(0.024)	(0.032)	(0.029)
Offered share of black peers		-0.040	-0.014	-0.055		-0.055	-0.018	-0.099
		(0.044)	(0.058)	(0.052)		(0.065)	(0.071)	(0.067)
Other offered characteristic			0.065	-0.051			0.115**	0.323***
			(0.047)	(0.085)			(0.057)	(0.120)
Characteristic Type			Mean Peer Reading	Teacher VA			Mean Peer Reading	Teacher VA
Outcome Mean	0.743				0.579			
Observations	3,592				2,871			

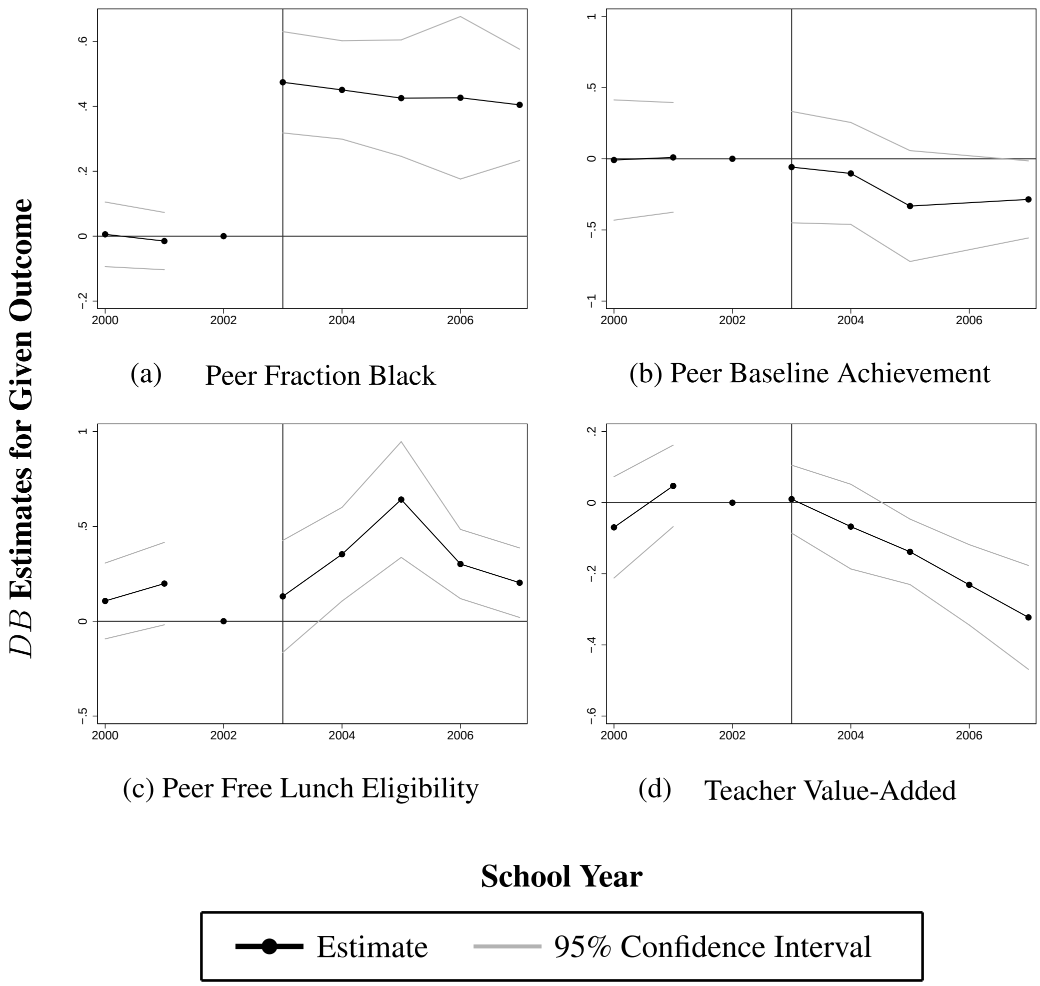
Notes: *, **, and *** denote statistical significance at the 10, 5, and 1 percent levels, respectively. Each column presents results from separate regressions. Regressions follow equation (4) where each outcome is regressed on a indicator equal to one if the student attended a magnet school during the year following the lottery as well as indicators for student gender, race, and risk-sets and quadratics in math and reading baseline achievement. The variable of interest is whether the student received an initial lottery offer as well as the lottery-induced peer/teacher offerings from equation (6). Standard errors are clustered by 6th-grade-school-by-year. Each regression sample is limited to students applying to a 6th grade magnet school lottery from 2000-2007.

FIGURE I: Racial Composition of Enrollment by School Type



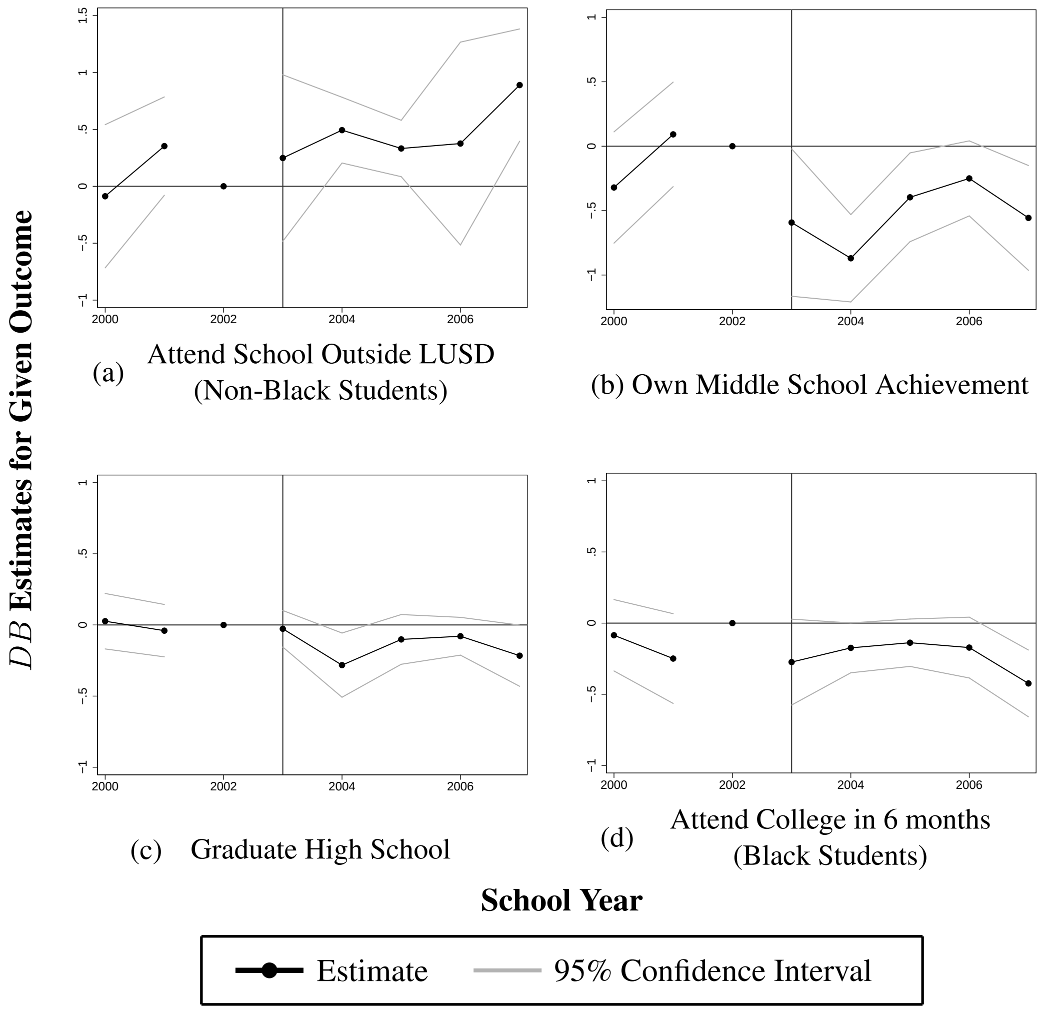
Notes: The figure plots the average black 6th grade enrollment shares across magnet and traditional schools in the LUSD. The vertical line represents the first year in which race-blind lotteries were used to determine enrollment in oversubscribed schools.

FIGURE II: Trends in Various Peer and Teacher Characteristics by Lottery Racial Disparity (DB)



Notes: Each figure presents the effect of enrolling in a magnet school with a 100 percentage point larger disparity between the share of black students in the lottery pool and the share of black students receiving offers averaged over 2000-2002 (DB) on the given outcome. Regressions are estimated using (3) as explained in Section IV.C. Similar Figures for the remaining outcomes explored in the paper are located in Appendix B. The reference line in 2003 denotes the first year the LUSD implemented the race-blind lottery system.

FIGURE III: Trends in Various Outcomes by Lottery Racial Disparity (DB)



(a) Attend School Outside LUSD (Non-Black Students)

(b) Own Middle School Achievement

(c) Graduate High School

(d) Attend College in 6 months (Black Students)

Notes: Each figure presents the effect of enrolling in a magnet school with a 100 percentage point larger disparity between the share of black students in the lottery pool and the share of black students receiving offers averaged over 2000-2002 (DB) on the given outcome. Regressions are estimated using (3) as explained in Section IV.C. Similar Figures for the remaining outcomes explored in the paper are located in Appendix B. The reference line in 2003 denotes the first year the LUSD implemented the race-blind lottery system.