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

Jason B. Cook*

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Abstract

In 2007, the Supreme Court declared race-conscious school admissions unconstitutional. This paper provides the first evaluation of a related federal mandate where a school district was forced to adopt a race-blind lottery system for its magnet schools. Lottery-estimated magnet school returns fall substantially under race-blind admissions. I explore a plausible mechanism: the dramatic increase in racial segregation following the mandate. More segregated schools spend less per-pupil, enroll lower achieving students, employ lower value-added teachers, and perpetuate "white flight" out of the district. Ultimately, segregation arising from mandated race-blind admissions causes student achievement and college attendance rates to decline.

*Cook: University of Pittsburgh, CESifo, and IZA, jbc50@pitt.edu, (412) 648-8746. I am grateful to Francine Blau, Michael Lovenheim, and Jordan Matsudaira for careful advising. I am also grateful to Joshua Angrist, Peter Bergman, Sandra Black, Julie Cullen, Clément de Chaisemartin, Will Dobbie, David Figlio, Robert Garlick, Eric Hanushek, Caroline Hoxby, Rucker Johnson, Andrew Johnston, Vladimir Kogan, Stéphane Lavertu, Hugh Macartney, Richard Mansfield, Steven Rivkin, Allison Shertzer, and Lise Vesterlund as well as seminar participants at CESifo (Munich), the NBER Fall Education Program, APPAM, University of Colorado-Boulder, Cornell University, Stanford University (CEPA), University of California-Davis, University of Oregon, The Ohio State University, University of Pittsburgh, University of Illinois Urbana-Champaign, University of California-Merced, the US Military Academy, the Naval Postgraduate School, and several anonymous referees for helpful comments. Any remaining errors are my own.

> "The way to stop discrimination on the basis of race is to stop discriminating on the basis of race." — Chief Justice Roberts, *Parents Involved v. Seattle Sch. Dist.*, 2007

A dominant voice in American legal theory argues that racial equality requires the law to be colorblind. This sentiment motivated decades of Supreme Court rulings that have curtailed the acceptable methods to achieve racial diversity in public education. In 1991, a Supreme Court decision began the process of dismantling court-ordered district integration plans.[1] Despite fewer regulations, many districts continued their efforts to integrate majority-black schools through voluntary market-based strategies that relied on race-conscious admissions procedures. Yet attempts to diversify schools through race-based admissions faced significant challenges. Following the turn of the century, the Supreme Court systematically outlawed practices historically used by districts to promote racial diversity—ultimately ruling that school admissions must be altogether blind to race.[2] Against this legal backdrop, the persistence of *de facto* school segregation is not surprising.[3]

This article provides the first empirical evidence on whether eliminating race-conscious policies impact the achievement returns to magnet school attendance and to what extent any effects are driven by racial segregation. Using rich administrative data, I evaluate a key federal mandate that eliminated racial quotas from magnet school admissions in a large urban school district (LUSD).

The magnet school setting is ideal for exploring the consequences of mandated race-blind admissions. Magnets provide special scholastic offerings and permit district-wide enrollment to encourage nonresident white families to enroll their children in schools that would otherwise be predominantly black. Magnets frequently relied on race-aware admissions practices such as racial

[1] *Board of Education v. Dowell* 48 U.S. 237 (1991).

[2] *Parents Involved in Community Schools v. Seattle School District No. 1* 127 S.Ct. 2738 (2007).

[3] See for instance: Clotfelter, Ladd and Vigdor (2008); Clotfelter, Vigdor and Ladd (2006); Clotfelter et al. (2018); Gamoran and An (2016); GAO (2016); Lutz (2011); Orfield et al. (2016); Reardon and Owens (2014); Reardon et al. (2012); and Welch and Light (1987).

quotas to diversify enrollment. Thus, the race-blind mandate has first-order implications for the efficacy of magnets as a tool for integration. While magnet schools are common to voluntary integration plans, we have no evidence on the impact of race-blind admissions on the returns to magnet schools, nor do we know how these policies change the school landscape more generally.[4] Understanding the implications of race-blind admissions is key for the design of education policies such as school assignment, affirmative action, and school finance equalization.

To begin, I document that race-blind admissions led to sizable increases in segregation within LUSD magnets. In the most extreme case, the percentage of black students enrolled in a magnet jumped from 60 to over 90 percent in a single year. Overall, the increased segregation mirrored the change experienced in Charlotte-Mecklenburg after forced busing was first eliminated (Billings, Deming and Rockoff, 2014). Why did segregation increase? In short, because black students disproportionately applied to LUSD magnets. This occurred because magnet schools in this district were opened in high-minority-share neighborhoods to attract white student enrollment in schools that would otherwise be racially segregated. Thus, to ensure racial balance, the schools conducted race-conscious admissions lotteries and used racial quotas to protect seats for non-black students. When the Office of Civil Rights forced the LUSD to use a race-blind lottery, the racial make-up of the entering cohort simply reflected the composition of the applicant pool. Black enrollment shares soared as a result.

While racial segregation within magnets rose after race-blind lotteries were instituted, the returns to magnet attendance fell. Students induced by the lottery offer to attend a 6th grade magnet school under race-conscious admissions experienced an increase in middle school achievement of roughly 0.49σ. However, under race-blind admissions, the returns fell to 0.28σ, bringing the estimates more in line to those from the previous literature of 0.14-0.32σ (Betts et al., 2006; Bifulco, Cobb and Bell, 2009; Hastings, Neilson and Zimmerman,

[4]By 1991, 40 percent of all district voluntary integration plans included a magnet school component (Rossell, 2003).

2012). These falling returns suggest a negative association between race-blind admissions and magnet value added.

If race-blind admissions reduce the gains from attending a magnet school, the resulting increase in segregation provides a plausible mechanism. The federal mandate directly manipulated the racial composition of incoming magnet cohorts, but left other district practices and policies intact. Thus, any impact that race-blind admissions have on magnet returns is likely either a direct consequence of the increased racial segregation or an indirect consequence of student and teacher responses to segregation. I explore these possible channels by testing whether segregation impacts student and teacher sorting and school spending.

To estimate how the mandate-induced segregation affects student outcomes, I exploit the fact that magnet schools did not segregate uniformly across the district. An integrated magnet with a predominantly black applicant pool will undergo a larger shift in racial composition after the race-blind mandate than a similar school with a racially balanced applicant pool. Thus, I use the difference between the share of black students receiving lottery offers and the share in the applicant pool *before the mandate* to predict exogenous increases in magnet school segregation. I then utilize a panel design to assess the difference in the causal effect of receiving a randomized offer to a school that I predict will segregate before and after the mandate is enforced.

I find that racial segregation in magnet schools has deleterious effects on student outcomes. I estimate that a 10 percentage point increase in the predicted black enrollment share (i.e., more segregation) decreases middle school achievement by 0.05σ, which is in line with other estimates in the literature on the order of -0.04 to -0.07σ (Billings, Deming and Rockoff, 2014; Hanushek, Kain and Rivkin, 2009; Hoxby, 2000). Segregation also has persisting negative effects. Specifically, students who attend a more racially segregated school are less likely to enroll in college. A 10 percentage point increase in the predicted black enrollment share of a student's 6th grade school decreases college enrollment by 1 percentage point among black students.

Race-blind admissions may indirectly affect student outcomes through their impact on school resources, other peer characteristics, and teacher sorting. Indeed, I find that segregated magnets spend less money per pupil and enroll students with lower average baseline achievement scores. Further, I show that non-black students who attend more segregated magnet schools are more likely to transfer out of the district during subsequent years—thereby exacerbating segregation. Lastly, similar to Jackson (2009), I provide evidence that average teacher value-added (VA) falls in more segregated schools. Thus, the observed negative impact on academic outcomes could result from either peer racial composition directly or from changes to school spending, other peer characteristics, or teacher quality.

The main contribution of this paper is that it provides the first assessment of how recent court-ordered race-blind admissions policies impact student outcomes through their effects on racial segregation. Existing work on the recent Supreme Court rulings have focused on the implications for achieving racial balance under race-blind admissions (Ellison and Pathak, 2016), but no work has explored the impact of race-blind lottery procedures on student outcomes, school spending, and student and teacher sorting. Mounting evidence suggests that court-ordered integration mandates improved student outcomes (Guryan, 2004; Johnson, 2015) and that their removal had the opposite effect (Billings, Deming and Rockoff, 2014; Gamoran and An, 2016; Lutz, 2011; Saatcioglu, 2010).[5] Yet we have no understanding of the consequences of forcing race-blind admissions upon districts with active *voluntary* integration plans. This distinction is important because students, teachers, and administrators may respond differently to the removal of a mandate to integrate than to the imposition of a mandate that weakens voluntary integration efforts.

Why might restricting voluntary integration have different implications

[5]Other studies have focused on the impact of peer racial composition by exploiting: naturally occurring cohort- or classroom-specific variation (Hanushek, Kain and Rivkin, 2009; Hanushek and Rivkin, 2009; Hoxby, 2000; Merlino, Steinhardt and Wren-Lewis, 2019; Vigdor and Nechyba, 2007); school-switching designs where students are exposed to markedly different peer groups upon exogenously changing schools (Abulkadiroglu, Angrist and Pathak, 2014; Bergman, 2018; Dobbie and Fryer, 2014); or policies that induce large shifts in racial composition such as voluntary busing programs (Angrist and Lang, 2004; Cook, 1984), or mandated school reassignments (Hoxby and Weingarth, 2005).

than ending forced desegregation? For an illustration, consider the termination of forced busing. Lifting forced busing strengthens the tie between residential and school segregation. If parents prefer racial homophily in schools (Glazerman and Dotter, 2017; Hastings et al., 2007) then we may expect white families to remain in the district, but possibly to sort to white neighborhoods (Liebowitz and Page, 2014). Conversely, requiring race-blind admissions, which in this LUSD reduces the chance that a given white student wins a seat in a desirable magnet school, could instead cause white families to relocate to the suburbs.

Indeed, I document that magnet school racial segregation resulting from race-blind 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-blind admissions undermine this original purpose is striking. This finding contributes to the literature that studies racial sorting (see Bifulco, Ladd and Ross, 2009) and the "white flight" response to district-wide desegregation plans (see Welch and Light (1987), Reber (2005), Baum-Snow and Lutz (2011) and Liebowitz and Page (2014)). The fact that racial segregation is self-perpetuating further highlights the challenge that integration efforts face during an era of race-blind jurisprudence.

1 Institutional Background

Magnet schools were conceived as a free market means of racial integration.[6] Thus magnets provide a natural setting to explore race-blind admissions policies. 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 ed-

[6]Magnet schools still operate today, though their goals have shifted away from racial integration toward that of school choice (Rossell, 2005). However, magnet schools are still officially defined as a public school that "offers a special curriculum capable of attracting substantial numbers of students of different racial backgrounds" (20 U.S.C. §7231a, 2019).

ucation, STEM, or an International Baccalaureate program. Magnet schools in this district also differ in that they lack specified catchment boundaries, allowing them to attract enrollment district-wide—hence the term "magnet." The LUSD ran eight magnet middle schools throughout the time period of this study.

Because the demand for these magnet schools far outpaced supply, magnet seats were filled via randomized lotteries.[7] To integrate schools, the district held separate school-specific lotteries for black and non-black students. 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. Following an investigation into the district's admissions practices, the federal Office of Civil Rights required the LUSD to switch to a race-blind lottery system in 2003. The switch to race-blind admissions was not unique to this district. In anticipation of an eventual Supreme Court ruling against race-aware admissions policies, during the 2000s, many districts nationwide voluntarily shifted to considering socioeconomic status rather than race to diversify schools despite it being a less efficient substitute (Ellison and Pathak, 2016; Kahlenberg, 2007; Rossell, 2005). Similar to this LUSD, other districts abandoned attempts to diversify altogether.

2 Data

I use student-level administrative data for all sixth graders attending any LUSD public school from 1998 through 2007. Records are tracked over time for students who remain enrolled in the district. Starting in 2000, even if a student transfers out of the district, a detailed record is created that captures whether the student switched to a local charter or private school or to a

[7] A couple of LUSD magnet schools are part of a feeder system. Students who attend a feeder magnet elementary (by winning an elementary lottery) are guaranteed admission into the specified magnet middle school if desired. The majority of 6th grade seats in these schools are still filled through the lottery.

school outside of district boundaries.[8] I observe student demographics including race, gender and free or reduced price lunch eligibility (FRL), as well as statewide standardized math and reading scores for tests taken at the end of each grade.[9] Moreover, I can observe the traditional school catchment zones for each student, but do not observe specific household location.

In addition, the district 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. 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 (Dynarski, Hemelt and Hyman, 2013). 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 also merge in novel *school-level* aggregate expenditure information for every year from the state's department of education.

Finally, I observe basic demographic information for all teachers in the district. Importantly, starting in 2000, I can link students to their teachers and classmates, which allows me to calculate teacher value-added. I follow Jackson (2009) and calculate value-added using a simple test score growth model on data prior to the 2003 mandate. Specifically, I regress student test scores on teacher experience, student baseline scores and demographics, classroom-level averages of student baseline scores and demographics, indicators for whether the teacher and student share the same race or sex, and a full set of teacher, grade, and year fixed effects. The coefficients on the teacher fixed effects

[8] The first charter school opened in this district in 1998. By 2000, they comprised roughly 2 percent of district enrollment, which steadily increased to roughly 15 percent enrollment by 2007. While I can observe whether students transfer to charters generally, I do not observe any information about charter schools directly in my data.

[9] Student-level FRL data begin in 2002. For earlier years, I use school-level FRL averages from the National Center for Education Statistics Common Core of Data. I standardize achievement measures within each subject-grade-year combination across the district. These achievement tests are comparable across the years of the study.

comprise my measure of value-added. I omit school fixed effects to make value-added estimates comparable across schools.

I limit my analysis to rising sixth-grade cohorts from 1998 to 2007 and omit students who qualify for a special education plan. This leaves the baseline analysis sample with 39,200 traditional and magnet school students. Table 1 presents descriptive information about the composition of students in my baseline sample before and after the 2003 race-blind mandate as well as by student race. Over 60 percent of the students in this district are black, roughly 75 percent qualify for free or reduced-price lunches, and 22 percent applied to a 6th grade magnet lottery.[10]

Under race-conscious lotteries, the student body differs substantially between magnet and traditional schools. Columns 2 and 3 show that under race-conscious lotteries, the share of black students attending magnet schools is 20 percentage points higher than in traditional schools. Some magnet schools lack sufficient demand from non-black families to achieve racial balance—explaining this disparity despite race-conscious lotteries. While magnets enroll a higher share of black students, a lower share of the student body qualify for free or reduced-price lunch and the average 4th grade achievement is roughly 0.3σ higher.[11] Magnet students are also mechanically more likely to have entered via a 6th grade lottery. The applicant share is not unity because a few magnet schools in the district set aside seats for feeder elementary schools as discussed earlier. Moreover, as I discuss in Section 4.3, the 2003 introduction of No Child Left Behind (NCLB) allows students to enter magnets through a NCLB-specific placement mechanism.

Under race-blind lotteries, seats for non-black students are no longer protected. Racial segregation intensifies as a result. The mandate increases the share of black magnet students by 11 percentage points and the exposure index by 7 percentage points (Massey and Denton, 1988). To give context,

[10]The district racial composition is almost entirely comprised of black and white students (cumulatively 93 percent). Lotteries were conducted separately for black and "non-black" students. I adopt this terminology.

[11]To facilitate comparisons between magnet and traditional schools, in Table 1, I standardize baseline achievement with respect to traditional school scores.

this increase in segregation is slightly larger than the immediate change in the exposure index due to the end of forced busing in Charlotte-Mecklenburg (Billings, Deming and Rockoff, 2014). Conversely, the black share enrolling in traditional schools remains stable.

Moreover during this time, average baseline achievement in magnet schools falls relative to traditional schools. Both traditional and magnet schools see increases in poverty as well. Columns 6 through 12 further disaggregate by student race. Across the characteristics, the student composition in magnet schools tends to change relatively consistently across race before and after 2003. One notable exception is that the share of magnet students who apply to a 6th grade lottery is roughly equivalent by race under race-conscious admissions. However, under race-blind lotteries a larger share of non-black students enter outside of the lottery. Some of this change is mechanical. Fewer seats are awarded to non-black students under race-blind admissions, but every student previously in a feeder elementary school is guaranteed admission. Thus, non-black students are disproportionately more likely to enter a 6th grade magnet through the feeder system after the mandate.

3 Magnet Schools Before and After Race-Blind Admissions

3.1 Lottery Empirical Framework

Students who attend a magnet school are exposed to different teachers, peers, and resources. These factors are potentially influenced by race-blind admissions, which may then impact student outcomes. In this section, I explore how the causal effect of attending a magnet school changes under race-blind admissions as well as how the school environment changes more generally. To do so, I exploit lottery variation to estimate the difference in outcomes between magnet applicants who win a lottery offer relative to those who do not.

Specifically, I estimate:

$$y_i = \pi \text{Offer}_i + \beta' X_i + \sum_j \mu_j d_{ij} + \eta_i \ , \tag{1}$$

where y_i is a given outcome for student i who applied to a 6th grade magnet school lottery.[12] Offer_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 are endogenous (de Chaisemartin and Behaghel, 2018).[13] 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, Deming and Rockoff (2014), X_i also includes quadratics in 4th grade reading and math achievement as well as indicator variables for whether the student is missing baseline achievement information. Standard errors are clustered by 6th-grade-school-by-year.

All regressions condition on risk set fixed effects d_{ij} (Angrist et al., 2016). Risk sets are unique application-portfolio-by-year-by-race combinations.[14] Their inclusion ensures that comparisons are only made between students who apply to the same set of magnet schools and thus have the same probability of winning a seat. 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

[12] For student achievement outcomes, I stack test scores across grades (six through eight for middle school and nine through twelve for high school), include grade-of-test fixed effects and two-way cluster standard errors by student and grade-by-school-by-year-of-test.

[13] de Chaisemartin and Behaghel (2018) propose a reweighting estimator that accounts for the endogeneity of "ever offers". However, because I focus mostly on reduced form estimates, I utilize "initial offers" to simplify the exposition.

[14] Because lotteries were race-specific prior to 2003, for consistency, I only use within-race variation in race-blind lotteries. Estimates are stable to removing race from risk set definitions.

on risk set fixed effects and on an indicator for whether the student received an offer in the given magnet's lottery. Table 2 presents these balance tests.[15] Lottery winners are comparable to losers across sex, baseline achievement and free or reduced-price lunch eligibility. 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.

In this article, I largely focus on the reduced form effect of magnet offers, but in some specifications, I also provide two stage least square estimates (2SLS) of the impact of enrolling in a 6th grade magnet school using the magnet lottery offer as the instrument.[16] For context, it is useful to establish the rate at which lottery offers convert into magnet enrollment. Differences in take-up before and after race-blind admissions are provided in the first row, in Columns 1 and 3 of Table 3. Receiving a lottery offer increases enrollment in magnet schools by roughly 17 and 21 percentage points before and after race-blind lotteries respectively. This difference is not statistically distinguishable from zero.

3.2 The Erosion of Magnet Schools After the Race-Blind Mandate

Magnet schools in this district generate large achievement gains relative to the traditional schools. This is visible in Table 3, which presents reduced form estimates from equation (1) in the odd columns and 2SLS estimates of magnet enrollment effects in the even columns, before and after the race-blind mandate. Under race-conscious admissions, students randomly offered

[15] 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 (roughly 7 percent of applicants) 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.

[16] Students are considered to be enrolled in a 6th grade magnet school if they appear in a magnet roster for at least one day. In the case of transfers, I consider a student's first magnet school only.

a 6th grade magnet school seat experience roughly a 0.09σ (0.49σ – 2SLS) increase in middle school standardized scores relative to students attending traditional schools. After the introduction of race-blind lotteries, these returns fall to 0.05σ (0.28σ – 2SLS), roughly a 40 percent drop. Under race-blind admissions, magnet returns are comparable to estimates in the recent literature on the order of 0.14-0.32σ (Betts et al., 2006; Bifulco, Cobb and Bell, 2009; Hastings, Neilson and Zimmerman, 2012). This pattern is mirrored in high school. Magnet middle school attendance generates large increases in high school achievement (0.43σ) and point estimates of these returns fall to 0.31σ under race-blind admissions. For both middle and high school outcomes, the differences in the returns to magnet attendance are not statistically significant. Magnet schools have little impact on the probability of being expelled and a positive, but statistically imprecise effects on high school graduation and college attendance during both time periods.

Several other magnet school inputs also deteriorate over this same time period—each presenting a possible explanation for the observed declines in magnet returns. I use the same lottery variation to show how school inputs differ under both admissions regimes. There are several important differences. First, peer compositions change. The effect of winning a lottery seat on the share of black peers more than doubles under race-blind lotteries. After the mandate, the share of black peers in a 6th grade cohort is 13.5 percentage points higher for students who attend magnet schools. This represents a substantial increase in racial segregation in this majority-minority district. Average peer socioeconomic status and baseline achievement are also slightly lower after 2002. Second, magnets spend less per pupil after the mandate. Students winning a lottery offer enjoy 5 percent higher per-pupil expenditures under race-conscious admissions and 9.6 percent lower per-pupil expenditures thereafter. Finally, teaching quality declines. Under race-conscious admissions, magnet school enrollment increases the average teacher value-added across students' 6th grade courses by 0.4σ. However, after the mandate, magnets provide no boost to teacher quality. Taken together, these channels each provide possible explanations for why achievement falls after the race-blind mandate.

The federal mandate caused substantial increases in racial segregation in LUSD magnet schools relative to traditional schools. However, it is challenging to link the observed segregation to falling magnet returns with simple pre-post comparisons that conflate any other contemporaneous policy changes.[17] As a result, in the next section, I estimate the impact of mandate-induced segregation on student outcomes and other changes in the education landscape in a panel framework by isolating exogenous variation in student racial composition.

4 The Impact of Mandate-Induced Segregation

The move to race-blind admissions directly increased racial segregation within LUSD magnet schools, but schools did not segregate uniformly. In this section, I describe how racial composition within these schools shifted in predictable ways and detail how I leverage this predicted segregation in a panel framework.

4.1 Predicting Segregation from Lottery Racial Disparities

To predict the intensity of school segregation under race-blind admissions, I exploit the fact that increases in black enrollment shares can be predicted based on how racially disparate lottery offers were before the mandate. Prior to 2003, the racial composition of seat offers did not necessarily reflect the full applicant pool, but did thereafter. Figure 1, plots the average race differential in receiving lottery offers among magnet school applicants. Black students were roughly 20 percentage points less likely to receive an offer under race-conscious admissions. The race-blind mandate eliminated this disparity. Thus the shift in racial composition for a magnet's entering cohort was a function

[17]The most notable policy change at during this period is the concurrent passage of No Child Left Behind. I discuss this policy in detail in Section 4.

of the racial disparity between lottery applicants and winners. I measure this disparity for a school by calculating the difference between the fraction of black students in the lottery pool and the fraction receiving an offer averaged over 1998 to 2002 (denoted DB).[18] Specifically,

$$(2) \qquad DB_s = \frac{1}{5}\sum_{t=1998}^{2002}\left(\frac{\sum_{i\in s,t}\mathbb{1}(\text{Black})_i}{N_{st}} - \frac{\sum_{i\in s,t}\mathbb{1}(\text{Black})_i\cdot\mathbb{1}(\text{Offer})_i}{\sum_{i\in s,t}\mathbb{1}(\text{Offer})_i}\right)$$

where N_{st} is the total number of applicants to school s during year t. Similar measures could be calculated using other baseline characteristics such as free lunch eligibility or achievement. However, because the policy change explicitly targeted race, I parameterize predicted student body changes using racial differences alone. Several magnet schools have DB values near 0, while others have values ranging up to a 0.40 difference. The average DB is 0.20 among magnet middle schools.

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 percent of pre-2003 lottery applicants were black, but the school offered only 60 percent of its seats to black students. Also consider school B, where 60 percent of the students were black in both the applicant pool and the initial offer set. Suppose that the composition of applicants remains the same after the introduction of race-blind lotteries. In that case, the composition of black students offered a seat to school A would rise to 80 percent to mirror the applicant pool, while the racial composition of lottery offers to school B would remain unchanged.

How do schools differ as a function of DB at baseline? In Table 4, I present the results from regressing the DB value for the school attended by each LUSD 6th grader on different school characteristics from 1998 to 2002, prior to the termination of race-conscious 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

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

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 make the magnets particularly appealing to black families—explaining why black students disproportionately apply. Second, high-DB schools are better schools based on measurable inputs. In Columns 2 through 5, I estimate that higher-DB schools enroll 6th grade cohorts with higher 4th grade test scores, a higher share of whom are black, employ higher value-added teachers, and spend more per-pupil. 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. These superior inputs help explain why non-black families choose to send their kids potentially long distances to attend schools located in high-minority-share neighborhoods.

4.2 Estimating Segregation Effects Using Lottery Disparities

In addition to providing random variation in magnet enrollment, lottery offers induce exogenous variation in exposure different school characteristics and provide a natural instrument for mandate-induced segregation. 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* racial lottery disparities of the magnet schools in their offer sets likely differ. Suppose I predict that magnet A will segregate more than magnet B following race-blind admissions (i.e., $DB^A > DB^B$). Even if both students prefer to attend high-DB schools 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 a measure of DB across the lottery offer set provides an additional instrument for *realized* peer racial composition.[19]

[19] In a previous version of this paper, I used the same lottery variation to also jointly estimate several dimensions of peer characteristics as well as teacher quality. I found that the impact of racial segregation was partially mediated by the corresponding changes to

I operationalize this rationale by calculating, for each student i, the school with the highest DB value among the schools in i's lottery offer set. For students receiving no offers, I assign to them a value of zero because traditional schools do not have lottery-based admissions and thereby no racial disparity in lottery offers. Specifically, for student i who was offered seats to enter the set of magnet schools $\mathbb{M}_i$, I calculate:

$$\text{Offered\_}DB_i = \text{Offer}_i * \max_{j \in \mathbb{M}_i} \{DB_j\} \tag{3}$$

where Offer_i is an indicator equal to one if the student won any magnet offer. This exogenously-determined baseline measure should highly correlate with the realized DB where student i enrolls and theoretically should predict changes in segregation induced by the mandate. $\text{Offered\_}DB_i$ varies by the set of magnet offers and is mediated by the take-up rate.

To estimate the impact of mandate-induced segregation on student achievement and postsecondary attainment, I augment equation (1) by adding the predicted segregation measure from (3). Specifically, I estimate

$$\begin{aligned} y_i =& \gamma \text{Offered\_}DB_i \times \mathbb{1}(t > 2002)_t + \rho \text{Offer}_i \times \mathbb{1}(t > 2002)_t \\ &+ \theta \text{Offered\_}DB_i + \pi \text{Offer}_i + \beta' X_i + \sum_j \mu_j d_{ij} + \eta_i \ , \end{aligned} \tag{4}$$

where terms are defined the same as in (1). Here, the coefficient of interest is γ, which provides the difference in the effect of increasing the maximum DB value among the set of magnet offers by 100 percentage points after the mandate relative to before. This reduced-form coefficient intuitively captures differences in the opportunity to attend a school that I predict will segregate, before and after the segregation occurs. This specification also controls for several other potential sources of bias. θ captures level differences in impacts of being offered high- versus low-DB schools that are constant over time (e.g., the differences documented in Table 4). Further, π absorbs the impact of winning a magnet seat under race-conscious lotteries and ρ handles any changes in magnet offer

peer baseline achievement (Cook, 2018).

effects after race-blind admissions that are common to all magnets. Standard errors are clustered by 6th grade school.

This procedure estimates multiple causal channels simultaneously, which allows for the possibility of countervailing or complementary effects. This method extends the analyses of Abulkadiroglu, Angrist and Pathak (2014), who test for peer effects in Boston and New York's selective exam schools.

Observable student characteristics balance both across lottery winners and losers as well as across offer sets with higher and lower levels of predicted segregation. The bottom half of Table 2 shows that I am unable to detect statistically significant differences for either offer measure across student sex, baseline achievement and free lunch eligibility. This supports the assumption that lottery offers and offered DB are as good as randomly assigned to students.

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, 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.

4.3 Full 6th Grade Cohort Analysis

The specification in (4) is empirically demanding. Estimates are often noisy because these specifications restrict attention to magnet applicants and control for risk sets and direct magnet offer effects. As a result, to enhance precision, I complement these analyses with a more parsimonious panel specification that does not attempt to handle endogeneity in a student's decision to enroll in a magnet school. Instead this design leverages the same predicted segregation measure from (2), but utilizes information on all 6th students across the district. In addition to enhanced precision, another benefit relative to using lottery variation directly is that this approach arguably provides a more holistic assessment of the impacts of race-blind admissions. In addition to considering implications of segregation for 6th grade magnet applicants, these analyses include observations from students entering through feeder schools or later through the NCLB placement mechanism. Moreover, these estimates are benchmarked against all traditional school 6th graders, not solely on magnet applicants. Because both methods have their relative merits and drawbacks, I present both specifications throughout the paper.

Specifically, for these models I regress:

$$y_i = \beta DB_s \times \mathbb{1}(t > 2002)_t + \gamma_1' X_i + \phi_s + \theta_t + \nu_i \tag{5}$$

where y_i is the given outcome for student i who attends 6th grade school s during year t (i.e., the year following the lottery).[20][21] X_i is a vector of pre-lottery demographics identical to equation (1).[22] ϕ_s and θ_t are school and year fixed effects.[23] Standard errors are clustered by school.

[20] 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.

[21] For student achievement outcomes, I augment (5) by stacking middle school (resp. high school) test scores for students across grades 6, 7, and 8 (resp. 10 and 11), adding controls for grade-of-test fixed effects, and two-way clustering standard errors by student and grade-by-school-of-test. Here, school s and year t index the student's school and year where/when they first attended 6th grade.

[22] Estimates of ρ are not substantively different if the X_i controls are excluded.

[23] Despite not contributing identifying variation, it is important to include traditional

Importantly, the school fixed effects account for any direct impacts of attending a high-DB school on academic outcomes such as those documented in Table 4. Moreover, the empirical strategy can also account for contemporaneous policy changes. If other policy changes do not differentially affect schools across various DB levels then these potential confounders will be captured by the year fixed effects.

The coefficient of interest β gives the difference in the change in the outcome between magnets with high and low DB values under race-blind relative to race-conscious admissions. To better model effect dynamics, some models feature a trend-break specification, where I additionally control for $DB \times (t - 2002)$ and $DB \times (t - 2002) \times \mathbb{1}(t > 2002)$. These terms respectively capture differences between high- and low-DB schools in pre-trends and post-treatment trend breaks.

Magnet Applications as a Function of DB—A key assumption underlying this empirical strategy is that the composition of magnet applicants remains relatively stable as a function of DB across the implementation of the mandate. I test this assumption in Table 5 where I estimate a version of equation (5) on the sample of 6th grade magnet school applicants. Not surprisingly, as black students have improved odds of gaining enrollment to magnet schools, the black share of applicants increases after the mandate. I estimate that the share of black applicants for a school with a 10 percentage point larger racial lottery disparity increases by 1.2 percentage points (1.6 percent) after the mandate. While this estimate is statistically significant, it is an economically small change. Perhaps more importantly, it appears that the baseline achievement composition is relatively stable across the introduction of the mandate. As a result, I conclude that changes in the composition of the magnet applicant pools are not a first-order concern to interpreting the results.

Identifying Assumptions—Models such as (5) that include group and time fixed effects to compare outcomes among groups with varying exposure to a treatment require additional assumptions to identify meaningful treatment ef-

schools in the analysis to help identify year fixed effects—particularly for subsequent event study estimates (Borusyak and Jaravel, 2017; de Chaisemartin and D'Haultfœuille, 2019).

fects. Here the treatment is assigned at the school-by-post-2002 level. While all schools are fully untreated prior to 2003, no schools become fully treated afterward. de Chaisemartin and D'Haultfœuille (2018, 2019) show that under common trends, treatment monotonicity, and stable treatment effect assumptions, fuzzy two-way fixed effects models estimate a weighted sum of local average treatment effects (LATEs) among the units whose treatment status changes over time, i.e., among the "switchers." By further assuming that treatment effects are homogeneous across groups and time, they show that this weighted sum identifies the LATE for the switchers in the population. In this section, I discuss the common trends assumption in detail and provide evidence in support of the remaining assumptions in Online Appendix A.

Common trends requires that unobserved determinants of student outcomes are not trending differentially by schools with varying DB levels. This is the same assumption that is required for sharp panel designs. While the school fixed effects account for the fixed differences between high- and low-DB schools, they do not account for possible trend differences.

Because the minority share in a school's surrounding neighborhood appears to strongly predict DB values (see Table 4), 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 changes than the nearby traditional schools, which enroll students using catchment zones. Thus, I argue that while high- and low-DB schools differ in initial levels, these differences do not necessarily imply trend differences in the unobserved determinants of the outcomes (Kahn-Lang and Lang, 2018).

I empirically test the common trends assumption in an event study framework. To begin, I assess the relationship between DB levels and 6th grade black student enrollment shares by regressing an analog of equation (5) where

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

$$\text{(6)} \qquad \text{Fraction Black}_i = \sum_{\substack{t\,=\,1998;\\ t\neq 2002}}^{2007} \rho_t DB_s \cdot \mathbb{1}(\text{Year} = t)_t + \gamma_1' X_i + \theta_t + \phi_s + \nu_i \ ,$$

where variables are defined as in equation (5). 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, then the share of black students enrolled in both types of schools should trend similarly prior to the 2003 policy change. 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.

Event study estimates support the common trends assumption in my setting. Figure 2a displays estimates of ρ_t from (6). 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 4.5 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 Figure 2 present estimates of the same regression, but for several important dimensions of peer and teacher composition and student outcomes. Figure 2b presents estimates for the leave-one-out average of baseline achievement (i.e., the average over 4th grade math and reading scores) among peer 6th graders within the student's enrolled school. As expected, peer baseline scores begin to fall after race-blind lotteries are implemented.[24] Fig-

[24] 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 the following section for details.

ure 2d shows a similar event study for average 6th grade teacher value-added across the classes attended by the given student. After race-blind lotteries were instituted, the value-added composition of 6th grade teachers steadily declined in high-DB relative to low-DB schools. The remaining panels foreshadow future results. I observe a steady decrease in school-level per-pupil expenditures. Non-black students who attend more segregated 6th grade schools under race-blind admissions are more likely to subsequently transfer out of the district. Further, student middle school achievement and college enrollment rates fell in high-DB relative to low-DB schools after 2003.[25] However, black students in more segregated schools are less likely to be expelled. In general, the absence of pre-trends across this set of peer, teacher, and school characteristics as well as own outcomes supports the common trends assumption. See Online Appendix B for event studies for the other outcomes explored in this paper.

No Child Left Behind—In 2002, the No Child Left Behind (NCLB) Act was signed into law as an update to the Elementary and Secondary Education Act of 1965. Because NCLB and race-blind lotteries were contemporaneously implemented, 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 implications for my empirical strategy.

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.[26]

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

[25]Unfortunately, the district did not provide middle school assessment information for 2003 and 2004.

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

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 potentially complicates my analysis as it may directly alter the composition of entering magnet school cohorts as a function of DB. If students who were offered NCLB seats differentially sorted into schools by DB levels, then my empirical strategy will be identified off variation from the NCLB mechanism in addition to the introduction of race-blind lotteries. In Online Appendix Table C.1, I show that the composition of students who accepted NCLB magnet seats differs by the school's DB value.[27] While the NCLB mechanism was no more likely to provide seats to black students in high-DB schools, NCLB seats in high-DB schools were 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 (5). However, because only 10-15 percent 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.

In addition to potentially influencing student composition, NCLB may directly impact school personnel and funding. Schools that systematically fail to make Adequate Yearly Progress (AYP) by not hitting statewide achievement targets experience increasingly harsh sanctions. These sanctions can directly impact the education landscape (see for example, Reback, Rockoff and Schwartz, 2014). However, I estimate that high-DB schools are *less* likely

[27] 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.

to miss AYP targets from 2003-2007.[28] Because my main findings suggest that the education landscape declines in more segregated high-DB schools, these estimates may actually understate the true effect of mandate-induced segregation.

5 Segregation and the Educational Landscape

5.1 Peer and Teacher Characteristics

How did race-blind admissions impact peer and teacher characteristics? I address this question in Table 6, which presents estimates of the effect of predicted black enrollment shares (i.e., DB) on the composition of other peer and teacher attributes from equation (5) in Panel I and (4) in Panel II. Schools I predict to have a 10 percentage point increase in black enrollment shares due to race-blind admissions (i.e., $DB = 0.1$) experience an average increase of 4.5 percentage points. 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. Similarly, Panel II shows that students receiving an offer set with a 10 percentage point higher maximum DB value attend schools with a 2.8 percentage point higher black enrollment share.

The increased proportion of black students entering magnet schools also (somewhat mechanically) shifts the student composition along other dimensions. In Panel I, I find that under race-blind admissions, schools I predict to have larger black inflows also enroll students who are more likely to be free-lunch eligible and have lower baseline achievement. Lottery estimates in Panel II corroborate these findings.

The racial segregation stemming from race-blind lotteries also reduces per-pupil school-wide expenditures. Column 4 shows that a 10 percentage point

[28] I regress whether a school's AYP target is missed in the given year on DB and school fixed effects. A school with a 10 percentage point larger DB is roughly 13 percent less likely to miss AYP.

increase in predicted black enrollment shares decreases per-pupil spending by 4.5 percent for the full cohort analysis and by 0.8 percent for the estimate from lottery variation.

Racial segregation also impacts the pool of teachers. For each sixth grader, I calculate their exposure to various teacher characteristics by taking averages across the set of courses the student attends. Students who attend increasingly segregated schools take courses from less experienced and lower value-added teachers. I estimate that following the mandate, schools with a 10 percentage point higher DB value employ teachers with 0.3 fewer years of experience and a 0.05σ (n.s.) lower value-added on average. Figure 2d shows that the average teacher value added steadily declines for schools predicted to segregate following the adoption of race-blind admissions. As a result, the trend break specification is more appropriate. I estimate that after race-blind admissions, teacher value-added falls by roughly 0.035σ each year. Again, estimates using lottery variation mirror these results. For the lottery specification, scaling the reduced-form effect by the first-stage estimate of 0.28 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 0.23σ. This estimate is similar in magnitude to estimates of a 0.13 to 0.22σ decrease due to the end of forced busing in Charlotte-Mecklenburg (Jackson, 2009). Teacher sorting is also consistent with Johnston (2019) who elicits teacher preferences on an array of school characteristics and finds that teachers prefer classes with a lower share of low income students and a higher share of high-achievers.

5.2 Student Attrition

Magnet schools were originally created to discourage non-minority families from moving out of urban areas with the intent to send their children to 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. In this section, I test whether segregation causes students to leave the school district and whether these responses are heterogeneous by 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 of long-run outcomes.

Table 7 provides OLS estimates of the impact of predicted changes in black enrollment shares on an array of attrition outcomes. The outcome in Column 1 is whether the student is missing postsecondary outcome information, which are matched on 12th grade cohorts. 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 for the lottery specification. Separate estimates for non-black and black students in Panels B and C reveal that this increase is mostly driven by non-black attrition. Non-black students who are offered a seat to a 6th grade school that I predict will enroll a 10 percentage point higher share of black students are roughly 8 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 the destination of these student transfers. Nearly all of the non-black student attrition is accounted for by student transfers to other school districts within the state. Non-black students who attend a school with a 10 percentage point higher DB value are 4 percentage points (9 percent) more likely to transfer to another in-state school district after the mandate. The effect is even larger using lottery variation with an estimated 25 percent increase. This is consistent with previous work documenting the relationship between desegregation and "white flight" (Baum-Snow and Lutz, 2011; Reber, 2005) as well as family preferences for racial homophily (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.[29] Finally, black students in more segregated schools are far less likely to leave the district due to being truant or expelled. Together these results are consistent with the idea that school segregation resulting from race-blind admissions undermines the ability of LUSD magnet schools to prevent "white flight."

5.3 Achievement and Postsecondary Attainment

Finally, I explore whether the racial segregation stemming from race-blind admissions ultimately impacts student achievement and postsecondary attainment. Racial segregation decreases short-run achievement for black students. As seen in Column 1, I estimate that a black student who is randomly offered a seat to a magnet that is predicted to experience a 10 percentage point increase in its black enrollment share performs 0.05σ lower on 6th through 8th grade standardized tests after race-blind lotteries are implemented. Scaling by the effect of predicted on realized black enrollment shares, the estimate is roughly 0.19σ. This estimate is larger than the previous 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, Deming and Rockoff, 2014; Hanushek, Kain and Rivkin, 2009). For non-black students, point estimates disagree between the two methods and there is too much noise to detect a clear relationship. There is also not a clear relationship between 6th grade segregation and high school achievement. I estimate a marginally significant negative effect of the predicted share of black peers on high school achievement using lottery variation that is not corroborated using the full 6th grade cohort.

While segregation had a negative influence on student achievement for black students, these potential short-term losses do not guarantee longer-term penalties to important education milestones such as high school graduation and postsecondary attainment. However, in Columns 3 and 4, I show that the negative consequences of racial segregation are still visible across longer-run 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

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

graduation rates by 1.2 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 (5) among the set of non-black students who have missing postsecondary outcomes. I estimate that racial segregation made the composition of non-black attriters higher-achieving. A school with a 10 percentage point higher DB value lost students with an average of 0.047σ ($p < 0.01$) 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.[30] While it is possible that peer racial composition had negative long-run consequences on non-black student outcomes, 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.

School segregation negatively impacted postsecondary attainment for black students. Black students who attend a school with a 10 percentage point higher DB value after the mandate are 1.1 percentage points less likely to enroll in a postsecondary institution 6 months after graduating high school. The magnitude of this effect is comparable to exposing a student from 6th through 12th grade to teachers having roughly 0.2σ lower value-added (Chetty, Friedman and Rockoff, 2014).[31] Together, these results provide evidence that the

[30]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 had 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.

[31]Chetty, Friedman and Rockoff (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 0.82 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.

racial segregation due to race-blind admissions negatively impacts education production in both the short- and long-run.

6 Conclusion

The changing legal landscape over the past several decades has increasingly weakened desegregation efforts across the United States. In recent years, the Supreme Court ruled that school assignment policies that explicitly use race in admissions decisions are unconstitutional (*Parents Involved in Community Schools v. Seattle School District No. 1* – 2007). Moreover, the Trump administration recently 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). Given this context, it is not surprising that the United States education system has grown increasingly racially *de facto* segregated since the end of court-ordered desegregation (Clotfelter, Ladd and Vigdor, 2008; Clotfelter, Vigdor and Ladd, 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, Deming and Rockoff, 2014; Guryan, 2004; Johnson, 2015; Lutz, 2011), we understand very little about the more recent rulings that require race-blind admissions. I study a related federal mandate that forced an urban school district to no longer integrate its magnet schools through race-conscious admissions lotteries.

I show that lottery estimates of magnet value-added fall after the federal mandate. I then turn attention to exploring the most plausible mechanism—the substantial increase in magnet school racial segregation. Mandate-induced segregation has several meaningful impacts on the magnet school landscape. Segregation causes magnets to spend less per pupil and 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. Moreover, such "white flight" could lessen the tax base and support for public education in the future. Ultimately, the change in peer racial composition driven by race-blind admissions decreases student achievement and postsecondary outcomes.

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-blind mechanisms, but they also note that race-blind 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 composition of the student body.

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Table 1: Average 6th Grade Student Characteristics across Traditional and Magnet Schools Surrounding Race-Blind Lotteries

	TPS & Magnets (1998-2007)	Race-Conscious Lotteries (1998-2002)				Race-Blind Lotteries (2003-2007)			
		TPS		Magnets		TPS		Magnets	
	Pooled (1)	Pooled (2)		Pooled (3)		Pooled (4)		Pooled (5)	
Black	0.623	0.564		0.762		0.588		0.870	
FRL	0.748	0.704		0.596		0.779		0.707	
4th Grade Math	0.050	0.002		0.236		0.004		0.172	
4th Grade Read	0.062	0.003		0.305		0.005		0.200	
Applied to Magnet	0.222	0.071		0.853		0.087		0.727	
6th Graders/School (Avg.)	188	214		166		173		151	
# of Schools	26	18		7		18		8	
Observations	39,200	17,531		4,230		13,774		3,665	
		Non-Black (6)	Black (7)	Non-Black (8)	Black (9)	Non-Black (10)	Black (11)	Non-Black (11)	Black (12)
FRL		0.573	0.792	0.399	0.652	0.672	0.854	0.501	0.738
4th Grade Math		0.321	-0.240	0.821	0.064	0.335	-0.228	0.773	0.080
4th Grade Read		0.275	-0.207	0.848	0.130	0.265	-0.176	0.663	0.130
Applied to Magnet		0.037	0.098	0.832	0.859	0.055	0.109	0.613	0.744
Observations		7,636	9,895	1,008	3,222	5,675	8,099	478	3,187

Notes: This table provides the averages of student characteristics in 6th grade across traditional public schools (TPS) and magnets before and after the termination of racial lotteries. The lower-half of the table reports these averages separately for non-black and black students. *Applied to Magnet* gives the share of students who applied to a 6th grade magnet school. Special education students are excluded from these samples. Observation counts provide the number of students specified in each column. To facilitate comparisons between magnets and traditional schools, baseline achievement is standardized based on traditional public school scores.

Table 2: Lottery Offer Balance Tests

	Female (1)	Baseline Reading (2)	Baseline Math (3)	FRL (4)
Initial Offer Gap	-0.006	0.009	0.028	-0.011
	(0.015)	(0.026)	(0.025)	(0.025)
Combined p-value:	0.610			
Initial Offer Gap	0.002	0.006	0.034	-0.018
	(0.017)	(0.029)	(0.028)	(0.028)
Offered DB	-0.078	0.030	-0.055	0.055
	(0.067)	(0.115)	(0.111)	(0.101)
Combined p-value:	0.657			

Notes: $N = 5,890$. The sample includes students with non-missing baseline characteristics who are in the magnet lottery sample as detailed in section 3.1. I seperately regress each student demographic on an initial magnet seat offer indicator as well as a full set of risk-set fixed effects. The second regression presented in the bottom rows adds the maximum DB among the set of magnet school offers as an additional regressor. Heteroskedasticity-robust standard errors are reported in parentheses. p-value tests the hypothesis that all coefficients on the initial magnet offer indicator (and the offered DB) are zero.

Table 3: The Impacts of Magnet Offers

	Race-Conscious Lotteries (1998-2002)		Race-Blind Lotteries (2003-2007)	
	Magnet Offer (1)	Magnet Enroll (2)	Magnet Offer (3)	Magnet Enroll (4)
First Stage				
Enroll in Magnet	0.171***		0.213***	
	(0.048)		(0.034)	
Student Composition				
Black	0.010	0.058	0.029***	0.135***
	(0.011)	(0.060)	(0.011)	(0.041)
FRL-Eligible	-0.016	-0.094*	-0.016***	-0.075***
	(0.011)	(0.055)	(0.006)	(0.026)
Baseline Scores	0.041*	0.238**	0.049***	0.202***
	(0.024)	(0.105)	(0.019)	(0.076)
Teacher Composition				
Black	0.008	0.052	-0.013	-0.062*
	(0.009)	(0.061)	(0.008)	(0.036)
Experience	0.083	0.528	0.331**	1.550**
	(0.126)	(0.666)	(0.159)	(0.715)
Value-Added	0.069**	0.402***	0.010	0.048
	(0.027)	(0.118)	(0.020)	(0.090)
School Spending				
Log of PP Exp.	0.009*	0.054	-0.020***	-0.096***
	(0.005)	(0.033)	(0.006)	(0.030)
Attrition				
Missing 6th Grade	0.005	0.033	-0.005	-0.025
	(0.014)	(0.087)	(0.008)	(0.037)
Missing NSC	-0.030	-0.184	-0.036	-0.168
	(0.024)	(0.144)	(0.025)	(0.114)
Leave LUSD	-0.016	-0.102	-0.049**	-0.228***
	(0.021)	(0.123)	(0.019)	(0.082)
Student Outcomes				
MS Achievement	0.085***	0.487***	0.051***	0.277***
	(0.024)	(0.175)	(0.018)	(0.097)
HS Achievement	0.062*	0.430*	0.056***	0.313***
	(0.035)	(0.252)	(0.021)	(0.111)
Expelled	0.004	0.028	-0.000	-0.001
	(0.006)	(0.036)	(0.008)	(0.037)
HS Grad	0.025	0.148	0.031	0.160
	(0.021)	(0.122)	(0.024)	(0.132)
Attend College	0.024	0.184	0.023	0.112
	(0.029)	(0.228)	(0.031)	(0.147)

Notes: *, **, and *** denote statistical significance at the 10, 5, and 1 percent levels, respectively. Regressions follow equation (1). Columns 2 and 4 provide

Table 4: 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	Log of School PP Expenditures
	(1)	(2)	(3)	(4)	(5)
DB	1.068**	0.832**	0.510	0.868***	0.350**
	(0.478)	(0.330)	(0.328)	(0.305)	(0.145)
N	19,065	16,587	22,151	12,662	22,217

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 1998-2002. 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 5: Changes to the Composition of Magnet Applicants by DB

	Black	Female	4th Grade Achievement
	(1)	(2)	(3)
$DB * \mathbb{1}(\text{Post}'02)$	0.126**	-0.068	0.061
	(0.054)	(0.062)	(0.211)
Outcome Mean	0.799	0.544	0.489
Observations	17,374	17,447	13,588

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 interaction between a post 2002 indicator and 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 1998-2002. I additionally control for year-of-application and school-of-application fixed effects. Standard errors are clustered by school. Fourth grade achievement is the average of 4th grade math and reading scores.

Table 6: Effect of Predicted Peer Racial Composition on Teacher and Peer Characteristics (1998-2007)

	Peer Characteristics				Teacher Characteristics		
	Fraction Black	Fraction FRL	Peer Baseline Achievement	Log of Per-Pupil Spending	Fraction Black	Average Exper.	Average Value-Added
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel I. *Full 6th Grade Cohorts*							
Observed $DB \times \mathbb{1}(Post\ 2002)$	0.451***	0.205	-0.229**	-0.446***	0.002	-2.993*	-0.472
	(0.109)	(0.144)	(0.116)	(0.087)	(0.063)	(1.670)	(0.304)
Outcome Mean	0.621	0.701	0.089	2.034	0.190	9.419	0.076
Sum of Negative Weights	-0.001	-0.001	-0.001	-0.001	-0.001	-0.001	-0.001
Observations	39,630	39,550	39,300	39,630	30,043	30,043	24,698
Trend Break Specification							
$DB \times (t - 2002)$	0.014	-0.012	0.013	0.035	-0.015	1.368**	-0.090
	(0.026)	(0.028)	(0.059)	(0.070)	(0.036)	(0.581)	(0.185)
$DB \times \mathbb{1}(t > 2002)$	0.470***	0.191	-0.024	-0.092	0.135***	-1.426	0.900***
	(0.090)	(0.162)	(0.144)	(0.160)	(0.044)	(2.196)	(0.212)
$DB \times (t - 2002) \times \mathbb{1}(t > 2002)$	-0.030	0.025	-0.092	-0.182**	-0.025	-2.384*	-0.346***
	(0.036)	(0.032)	(0.075)	(0.082)	(0.057)	(1.270)	(0.122)
Panel II. *Lottery Offers to Magnet Applicants*							
Offered $DB \times \mathbb{1}(Post\ 2002)$	0.284**	0.364**	-0.350	-0.080***	-0.036	-3.080*	-0.681**
	(0.137)	(0.180)	(0.218)	(0.029)	(0.068)	(1.750)	(0.265)
Observations	6,710	6,717	6,096	5,782	5,080	5,080	4,321

Notes: *, **, and *** denote statistical significance at the 10, 5, and 1 percent levels, respectively. Specifications follow (5) and (4).

Table 7: Effect of Predicted Peer Racial Composition on Attrition (1998-2007)

	Missing NSC (1)	Withdrawal Destination/Reason: Outside LUSD (2)	Private (3)	Other (4)	Expelled (5)
Panel A: *Pooled Sample*					
Full 6th Grade Cohorts					
Observed $DB \times \mathbb{1}(Post\ 2002)$	-0.017	0.127**	-0.100***	0.028	-0.054*
	(0.072)	(0.056)	(0.031)	(0.046)	(0.028)
Outcome Mean	0.559	0.404	0.027	0.140	0.040
Lottery Offers to Magnet Applicants					
Offered $DB \times \mathbb{1}(Post\ 2002)$	0.195*	0.115	0.019	-0.076	-0.056*
	(0.117)	(0.131)	(0.035)	(0.070)	(0.030)
Panel B: *Subgroups — Non-Black Students*					
Full 6th Grade Cohorts					
Observed $DB \times \mathbb{1}(Post\ 2002)$	0.230	0.362***	-0.239*	0.027	0.028
	(0.143)	(0.088)	(0.123)	(0.153)	(0.026)
Lottery Offers to Magnet Applicants					
Offered $DB \times \mathbb{1}(Post\ 2002)$	0.803	1.033**	-0.253**	0.339	-0.057
	(0.534)	(0.480)	(0.105)	(0.478)	(0.139)
Panel C: *Subgroups — Black Students*					
Full 6th Grade Cohorts					
Observed $DB \times \mathbb{1}(Post\ 2002)$	-0.075	0.068	-0.065***	0.018	-0.081***
	(0.083)	(0.086)	(0.016)	(0.030)	(0.028)
Lottery Offers to Magnet Applicants					
Offered $DB \times \mathbb{1}(Post\ 2002)$	0.150	0.069	0.035	-0.101*	-0.057**
	(0.121)	(0.146)	(0.041)	(0.054)	(0.027)

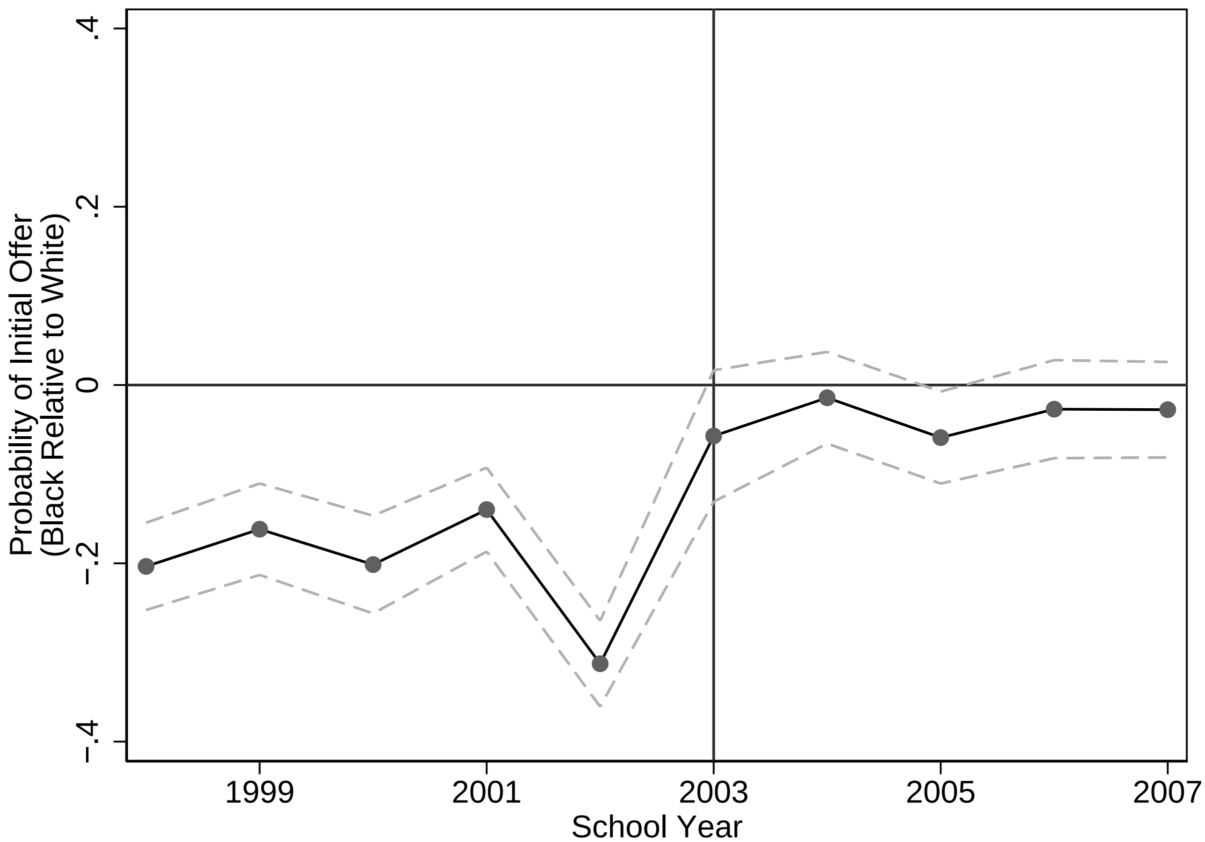
Notes: N=5,095. *, **, and *** denote statistical significance at the 10, 5, and 1 percent levels, respectively. Specifications follow (5) and (4). Sum of the negative weights is -0.001, the total of the negative weights as calculated by the procedure set forth in (de Chaisemartin and D'Haultfœuille, 2019).

Table 8: Effect of Predicted Peer Racial Composition on Student Outcomes

	Achievement			
	Middle School	High School	Grad. HS	College in 6 mo.
	(1)	(2)	(3)	(4)
Panel A: *Pooled Sample*				
Full 6th Grade Cohorts				
Observed $DB \times \mathbb{1}(Post\ 2002)$	-0.267	0.083	-0.121***	-0.105*
	(0.176)	(0.092)	(0.046)	(0.058)
Outcome Mean	0.064	0.333	0.643	0.468
Lottery Offers to Magnet Applicants				
Offered $DB \times \mathbb{1}(Post\ 2002)$	-0.307	-0.296	-0.149	-0.026
	(0.196)	(0.217)	(0.118)	(0.092)
Outcome Mean	0.190	0.071	0.747	0.571
Panel B: *Subgroups — Non-Black Students*				
Full 6th Grade Cohorts				
Observed $DB \times \mathbb{1}(Post\ 2002)$	-0.200	0.380	-0.545***	-0.083
	(0.161)	(0.238)	(0.082)	(0.159)
Lottery Offers to Magnet Applicants				
Offered $DB \times \mathbb{1}(Post\ 2002)$	0.716	1.003	-0.930*	-0.884***
	(0.595)	(0.774)	(0.516)	(0.324)
Panel C: *Subgroups — Black Students*				
Full 6th Grade Cohorts				
Observed $DB \times \mathbb{1}(Post\ 2002)$	-0.212	0.120	0.035	-0.111*
	(0.200)	(0.095)	(0.044)	(0.062)
Lottery Offers to Magnet Applicants				
Offered $DB \times \mathbb{1}(Post\ 2002)$	-0.450**	-0.381*	-0.091	0.033
	(0.222)	(0.225)	(0.118)	(0.094)

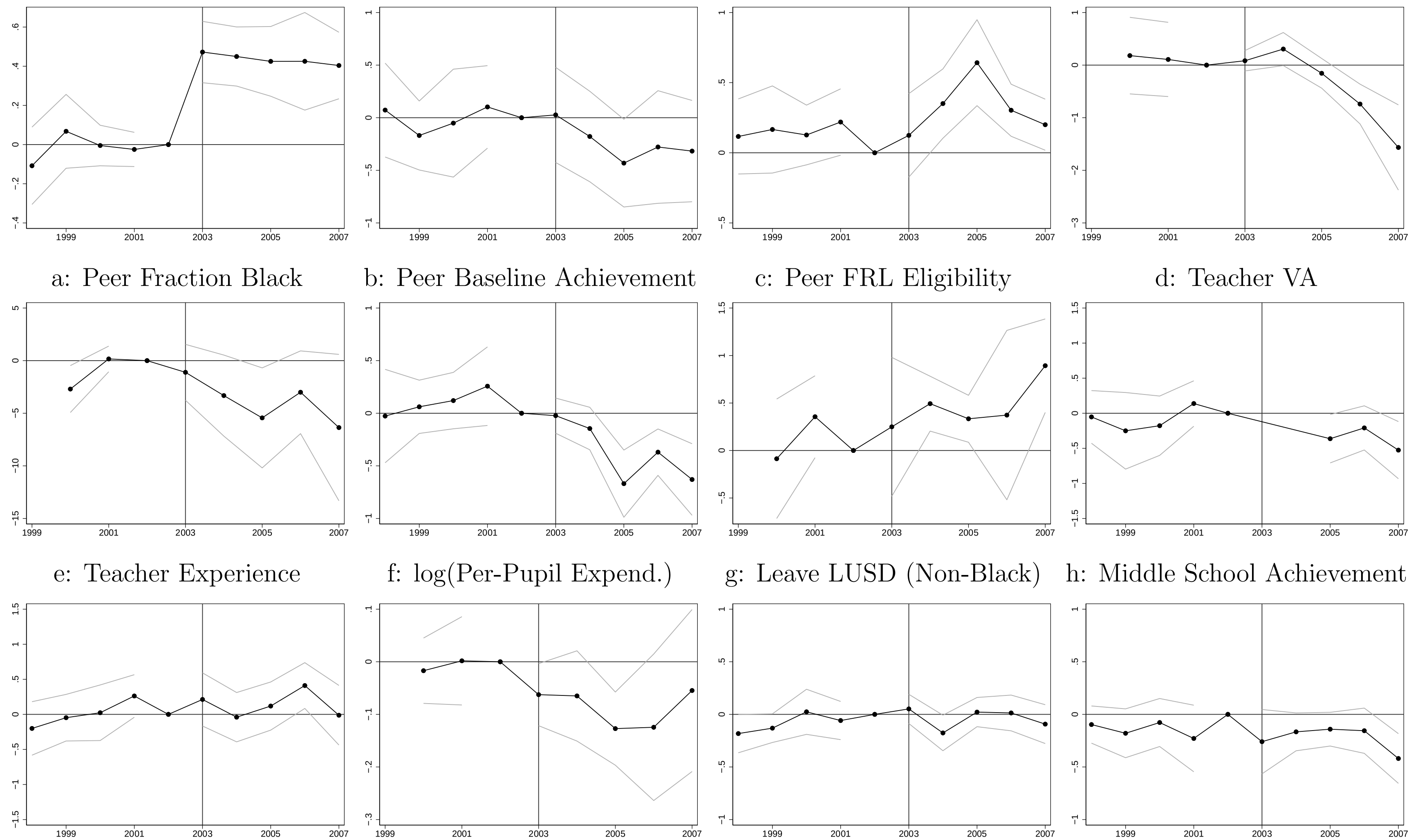
Notes: *, **, and *** denote statistical significance at the 10, 5, and 1 percent levels, respectively. Specifications follow (5) and (4). Sum of the negative weights are (-0.002), as calculated by the procedure set forth in (de Chaisemartin and D'Haultfœuille, 2019).

Figure 1: Racial Differences in Magnet Seat Offer Rates



Notes: The figure presents the results from a regression of magnet seat offers on an indicator for whether the student is black interacted with year fixed effects and a full set of lottery risk-set fixed effects. The vertical line denotes the first year that race-blind lotteries were implemented.

Figure 2: Trends in Various Peer and Teacher Characteristics by Lottery Racial Disparity (DB)



a: Peer Fraction Black

b: Peer Baseline Achievement

c: Peer FRL Eligibility

d: Teacher VA

e: Teacher Experience

f: log(Per-Pupil Expend.)

g: Leave LUSD (Non-Black)

h: Middle School Achievement

i: HS Achieve. (Black)

j: Expelled (Black)

k: HS Graduate (Black)

l: College in 6 mo. (Black)

Notes: Each figure presents the effect of enrolling in a magnet school with a 100 percentage point larger DB relative to 2002 on the given outcome. Regressions are estimated using (6) as explained in Section 4. The reference line in 2003 denotes the first year the LUSD implemented the race-blind lottery system.