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Choice and consequence: Assessing mismatch at Chicago exam schools ☆

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ABSTRACT

The educational mismatch hypothesis asserts that students are hurt by affirmative action policies placing them in selective schools for which they wouldn't otherwise qualify. We evaluate mismatch in Chicago's selective public exam schools, which admit students using neighborhood-based diversity criteria as well as test scores. Regression discontinuity estimates for applicants favored by affirmative action indeed show no gains in reading and negative effects of exam school attendance on math scores and four-year college enrollment. But these results are similar for more- and less-selective schools and for applicants more and less likely to benefit from affirmative action, a pattern inconsistent with mismatch. We show that Chicago exam school effects are determined largely by the schools attended when *not* offered an exam school seat. In particular, apparent mismatch is explained by the fact that exam school admission diverts many applicants from high-performing Noble Network charter schools, where they would have done well. Consistent with these findings, exam schools reduce math scores for applicants applying from high-quality charter schools in another large urban district. Exam school applicants' previous achievement, race, and other demographic characteristics that are sometimes said to mediate student-school matching play no role in this story.

1. Introduction

The educational mismatch hypothesis predicts poor academic outcomes for the beneficiaries of diversity preferences at selective schools if the students benefiting are ill-prepared for the sort of education provided by these schools. Proposed by Sander (2004) as a cause of racial gaps in bar exam passage rates, the mismatch hypothesis has inspired a large literature on the consequences of affirmative action in college admissions (see, e.g., Sander and Taylor (2012) and Arcidiacono and Lovenheim (2016)). The debate here is more than academic: mismatch arguments were prominent in the 2016 *Fisher v. University of Texas* Supreme Court case, in which the court upheld the constitutionality of affirmative action policy at the University of Texas.[1]

Much of the mismatch debate focuses on higher education, but the same issues arise for admission to selective public high schools. Like selective colleges, selective public high schools, often called "exam schools," offer high-achieving students an educational environment with bright classmates, experienced teachers, challenging courses, and perhaps other resources beyond those available to students at mainstream public schools. Because exam schools select their students using entrance exams and course grades, criteria that may disadvantage minority applicants, affirmative action may boost minority representation in exam school student bodies.

As a consequence of broader desegregation efforts, exam schools in Boston, Chicago, and San Francisco guaranteed seats for minorities in the 1970s and 1980s. US courts have since treated the issues raised by affirmative action in exam school admissions

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[1] During the court proceedings, Justice Antonin Scalia commented: "There are those who contend that it does not benefit African-Americans to get them into the University of Texas, where they do not do well, as opposed to having them go to a less-advanced school, a slower-track school where they do well." Sander's analysis was challenged by Imbens et al. (2012) in an amicus brief.

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and higher education similarly. The landmark 1978 Bakke decision outlawed racial quotas in college admissions. Since the 1990s, exam school admissions have likewise moved to de-emphasize race.[2] But the questions of whether minority exam school applicants and the public are well-served by affirmative action remain contentious. The 2020 pandemic, which curtailed many public-school testing programs, intensified the push to experiment with schemes designed to raise minority enrollment in selective public high schools, mostly by screening less stringently.[3]

This paper assesses evidence for mismatch at Chicago's exam schools. The Chicago Public School (CPS) district currently operates 11 selective enrollment high schools, some with a long history and many distinguished graduates. Affirmative action at CPS exam schools operates by reducing admissions cutoffs for applicants from lower-income neighborhoods while raising cutoffs for applicants living in higher-income parts of the city. Our analysis begins with a fuzzy regression discontinuity (RD) design that identifies exam school effects for applicants just above and just below neighborhood-specific cutoffs. These results suggest exam school attendance reduces math scores and four-year college enrollment for applicants on the margin of acceptance.

Because affirmative action lowers the bar for many applicants, the finding of a negative exam school effect is superficially consistent with the mismatch hypothesis. To explore the mismatch hypothesis further, we develop a simple theoretical and econometric framework centered on an education production function. In this framework, achievement depends on student preparedness and a match component that decreases in the distance between preparedness and curriculum difficulty. This model suggests the achievement of less-prepared applicants should fall as a consequence of exam school enrollment, while better-prepared applicants should benefit from advanced exam school curricula.

The econometric framework developed here exploits the fact that applicants from the most disadvantaged neighborhoods face markedly lower admissions cutoffs. Like the broader population affected by affirmative action, these low-tier applicants enter exam schools with scores well below those of most admitted students. But the extent of this sort of preparedness mismatch varies across school selectivity and as a function of student background. Chicago's system for centralized exam school assignment allows us to construct instrumental variables estimates for applicants with baseline achievement around a wide range of cutoffs. These results show similar effects of enrollment at more and less selective schools and for applicants more and less likely to benefit from affirmative action, a pattern of findings inconsistent with the mismatch hypothesis. Likewise, estimates from models allowing effects to differ according to whether applicants have scores above or below those of the median student suggest forces other than mismatch lie behind negative exam school effects.[4]

Evidence against mismatch motivates our exploration of an alternative explanation for negative exam school effects; this explanation turns on attendance effects at a leading exam school counterfactual. Many of Chicago's rejected exam school applicants enroll in charter schools, some of which appear to generate impressive learning gains. By combining an RD design for exam schools with charter lottery data, we identify school attendance effects in a model with exam and charter sectors.

The leading charter school alternative is the Noble Network of charter schools, a high-performing "No Excuses"-style charter management organization. Noble is the alternative sector for only 15% of exam-school compliers, but Noble impacts are large enough for this level of diversion to account for negative exam school offer effects. A simple back-of-the-envelope calculation multiplying Noble diversion by Noble impact aligns with reduced-form exam school effects. The diversion story is further substantiated by a multi-sector model that estimates exam and Noble effects together. Results here show that, allowing for a Noble channel for exam school offer effects, exam school enrollment leaves achievement unchanged.

When exam schools do not generate gains compared to traditional public schools, and there are no mismatch effects, Noble estimates should be similar regardless of the instrument for Noble enrollment and across demographic subgroups. Across student characteristics and sources of school-offer variation, the more an exam-school applicant is diverted from Noble, the more negative the corresponding exam school effect is. In a formal overidentification test, a set of covariate-specific reduced form effects of exam school or Noble offers aligns with the first-stage effects of both offers on Noble enrollment. This pattern of homogeneous effects within sectors weighs against the importance of student-school matching as a determinant of educational outcomes.[5]

In related work, Barrow et al. (2020) report negative RD estimates of the effects of Chicago exam school offers on grades and selective college enrollment. Our analysis, by contrast, uses an instrumental variables framework to capture school enrollment (rather than offer) effects and exploits all variation arising from the CPS exam school match, thereby boosting precision. Furthermore, our model of counterfactual enrollment sectors substantiates a new explanation of negative exam school effects. Other studies of exam school effects include Abdulkadiroğlu et al. (2014) and Dobbie and Fryer (2014) in the US, Clark (2010) in the UK, and Lucas and Mbiti (2014) and Ajayi (2014) in Sub-Saharan Africa. All of these find little effect of exam school attendance on achievement or college attendance.

Finally, we report briefly on exam school effects for a sample of applicants enrolled in charter middle schools in an anonymous large urban district. This district has three exam schools and a high-performing charter sector that mostly embraces No-Excuses-style pedagogy. Exam school applicants enrolled in a charter school at the time of application typically remain in the charter sector if they fail to win an exam school seat. Consistent with the estimates for Chicago exam school applicants, exam school offers in this second district reduce achievement for this district's charter-originating exam school applicants.

The rest of the paper is organized as follows. Section 2 discusses institutional background related to Chicago's exam school sector. Section 3 describes our data, presents descriptive statistics, and discusses a set of baseline RD estimates. This section also explains the econometric methods used in the paper; these are based on Abdulkadiroğlu et al. (2017) and Abdulkadiroğlu et al. (2022). Section 4 sketches an economic model of mismatch and reports estimates by school selectivity and applicants' preparedness level motivated by mismatch theory. Section 5 isolates the leading non-traditional exam school alternatives and explains how charter

[2] The Supreme Court ruled against Seattle and Louisville's race-based public school admissions systems in 2007.

[3] Boston, for example, opted to swap the traditional exam school admissions exam for an admissions regime based on grades, state assessments, and student zip code, prioritizing those with low median household income. This appears to have boosted minority enrollment (Vaznis, 2021).

[4] The latter test is motivated by Sander (2004), who writes: "If there is a very large disparity at a school between the entering credentials of the median student and the credentials of students receiving large preferences, then the credentials gap will hurt those the preferences are intended to help. A large number of those receiving large preferences will struggle academically, receive low grades, and actually learn less in some important sense than they would have at another school where their credentials were closer to the school median."

[5] Other examples of this sort of sectoral or program-operator substitution appear in studies by Heckman et al. (2000), Kline and Walters (2016), Angrist et al. (2013), and Chabrier et al. (2016). Kirkeboen et al. (2016) explore substitution patterns across college majors.

school enrollment mediates exam school effects. Specifically, this section shows that a model with additive school sector effects, with no match effects of any kind, fits CPS data remarkably well. Results for a second large urban district are discussed briefly in Section 6. Section 7 concludes.

2. Affirmative action at Chicago exam schools

The Chicago Public School (CPS) district is the third-largest in the U.S., with more than 600 schools and roughly 400,000 students. As in many large urban districts, most CPS students are Black or Hispanic and from low-income families. CPS high school students attend neighborhood schools by default but can choose other schools and programs. Choice options include exam schools (known locally as selective enrollment schools), magnet schools, charter schools, military academies, and career academies.

In the years covered by this study, CPS operated nine exam schools.[6] Exam schools offer their students a curriculum emphasizing honors and Advanced Placement (AP) courses. Not surprisingly, exam school students are higher-achieving than most of their public school peers. Chicago's most selective exam schools, Northside and Payton, are frequently listed among the best US public high schools. In the 2016 US News & World Report ranking, for example, Northside was ranked 39th, while Payton was 41st.

Selective enrollment school applicants apply by ranking the exam schools to which they wish to be admitted. Until the 2010–2011 admissions cycle, applicants could rank up to four schools. Applicants have since been able to rank up to six schools. The selective school admissions formula assigns equal weight to entrance test results, a standardized test taken in middle school, and to letter grades earned in 7th grade. These criteria generate a *composite score* running from 0 to 900 that schools use to rank applicants. The most selective schools admit students with composite scores above about 800; the least selective admit students with scores as low as 650, typically around the 66th percentile of the applicant composite score distribution.

Chicago's school choice system grew out of a 1980 consent decree in which the district agreed to promote school integration and to increase access for Black and Hispanic students by offering diverse school choice options. Initially, the choice system considered race. In 2009, however, in response to a federal court decision vacating the original consent decree, CPS adopted an affirmative action plan for selective schools based on the characteristics of the census tracts where applicants live. This system remains in place.

Chicago's system of residential preferences has been seen as a model for race-neutral admissions at selective schools (see, e.g., Kahlenberg (2014)). Federal guidelines on acceptable alternatives to promote diversity at selective K-12 schools also resemble Chicago's plan (see OCR (2011)).[7] The Chicago system assigns each census tract a score equal to the sum of its percentile rank on five dimensions: median family income; a measure of adult educational attainment; homeownership rates; and the prevalence of single-parent households and non-native English speakers.[8] These scores are then used to assign tracts to one of four tiers so that roughly a quarter of CPS students live in each tier. Higher-income tracts in Tier 4 are on the city boundary, while the most disadvantaged, in Tier 1, are concentrated in the southern and western parts of the city center.

Since 2009, the CPS exam school admissions process has used a version of the deferred acceptance (DA) algorithm, now employed by CPS district-wide and in many other large districts. Tier-based affirmative action is implemented by allocating a share of seats using composite scores only, while the remaining seats are split equally across tiers. In 2011, for example, 70% of seats were assigned within tiers (Dur et al. (2020) detail the CPS implementation of DA further). As noted by Abdulkadiroğlu et al. (2017), the CPS system can be represented as DA without priorities by dividing each school into five sub-schools, one containing unreserved seats and the rest containing equal numbers of tier-reserved seats. DA without priorities is called serial dictatorship. An important feature of serial dictatorship (and DA in general) is that admission decisions are determined solely by a set of school-specific cutoffs. CPS's version of DA, therefore, produces distinct cutoffs for each school and tier.[9]

Fig. 1 reports Fall 2011 cutoffs for each exam school; the figure shows a common cutoff for each school computed without affirmative action, along with the tier-specific cutoffs generated by tier-based affirmative action.[10] At Northside and Payton, the most selective schools, students from Tier 4 neighborhoods needed almost 900 points (the maximum score) to get in. By contrast, cutoffs for students from Tier 1 neighborhoods are over 100 points lower. These values are indicated by the dots in the figure. The figure also shows that cutoffs generated by an admissions regime without affirmative action are near the Tier 3 cutoff of 890. Applicants from Tiers 1 and 2 with scores between the cutoff for their tier and 890 are therefore admitted because of affirmative action.

The median composite score for admitted students, also plotted in Fig. 1, provides a further point of comparison for tier-specific cutoffs. At Northside and Payton, the median for those admitted lies well above the cutoffs for Tiers 1 and 2. Applicants admitted to Northside and Payton from these more disadvantaged tiers are, therefore, especially likely to have scored below the school median. This achievement gap may lead to poor academic performance as a result of mismatch.

At Young, Jones, Lane, and Brooks, which are further down the hierarchy of exam-school selectivity, gaps between the Tier 4 and Tier 1 cutoffs are also 75–100 points. Cutoffs at schools less selective than Lane show an interesting reversal: Tier 3 applicants must clear a higher bar than applicants from (less disadvantaged) Tier 4. This reflects differences in demand for these schools across tiers. Similarly, at two less selective exam schools (King and Westinghouse), affirmative action considerations generate a cutoff for Tier 2 applicants slightly above that for Tier 4. For all but these two schools, however, applicants from the two lowest tiers face lower admissions cutoffs than do high-tier applicants.

3. Data and baseline estimates of exam school enrollment effects

The CPS data analyzed here contain information on exam school applicants' application choices and admissions decisions, high school enrollment, PLAN and ACT test scores, and college enrollment from the National Student Clearinghouse (NSC). We also

[6] These are Brooks College Prep High School, Jones College Prep High School, King College Prep High School, Lane Tech High School, Lindblom Math and Science Academy, Northside College Prep High School, Payton College Prep High School, Westinghouse College Prep High School, and Whitney M. Young Magnet High School. South Shore International High School opened in 2013, and Hancock College Prep High School opened in 2015.

[7] Ellison and Pathak (2021) quantify the allocative efficiency of race-neutral affirmative action plans and compare Chicago's new plan with alternatives.

[8] Since 2010, the index has included measures of local-area school performance.

[9] To obtain DA equivalence, applicants ranking school *s* are modeled as ranking the notional school *s* with unreserved seats followed by a version of school *s* containing the seats reserved for their tier. This implementation reproduces DA with tier reserves as long as every school and tier is over-subscribed. Chicago isn't quite a serial dictatorship because a few schools and tiers are sometimes under-subscribed. Even so, rerunning the match as serial dictatorship replicates 99.7% of the assignments seen in our sample.

[10] These cutoffs are computed via a simulated match using school capacities determined by tallying the number of first-round assignments in the match. Simulated cutoffs with affirmative action, therefore, differ slightly from published cutoffs.

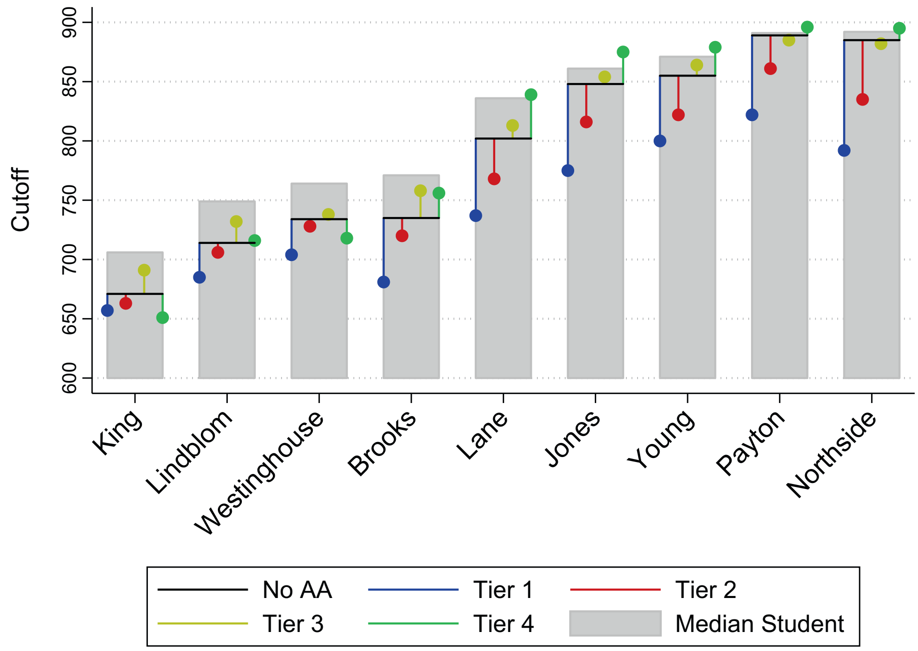

Fig. 1. Chicago Admission Cutoffs by Tier (2011).
Notes: This figure shows admission cutoffs for each tier at CPS exam schools in 2011 under a match with tier-based affirmative action. The figure also plots the cutoffs generated by a hypothetical admissions regime without the affirmative action policy. School capacities for both scenarios are the same and exclude principal discretion offers. Gray bars mark median admissions scores among those offered an exam school seat.

obtained application and admissions records from the Noble Network. These files are described briefly below, with further details in the data appendix.

Our exam school admissions files cover cohorts applying from 2009 to 2012, years that mark the beginning of tier-based affirmative action. These files record applicants' ranking of schools, their residential tier, and the school offered. Each applicant's composite admissions test score appears in the file; it's these that determine the cutoffs discussed above. Our Noble Network admissions files cover Network applicants in the same period. These files record campuses applied to, Noble lottery numbers (one for each school), and lottery outcomes (offer or waitlist). Noble files also identify applicants admitted via sibling priority. The CPS enrollment sample contains data on high school students in school during the 2009–2010 to 2015–2016 school years. These files include information on students' school and grade, as well as demographic information such as gender, race, subsidized lunch status, and special education status.

The analysis below looks first at exam school attendance effects on PLAN and ACT scores for exam school applicants enrolled in CPS at the time of application. PLAN is a preliminary ACT test, typically taken in 10th grade. ACT is a college readiness assessment taken as part of the CPS Prairie State Achievement Exams (PSAE), usually administered in 11th grade, and required of all high school students until recently. Eighth grade Illinois State Achievement Test (ISAT) scores in math and reading provide a baseline achievement control. All scores are standardized to be mean zero and have a unit standard deviation in the CPS population each year. We use the first score available for students who repeat an exam. The restriction to applicants enrolled in CPS at baseline ensures that exam school students are compared to students attending other within-district choice options and reduces loss to follow-up. We also study the impact of exam school attendance on two-year and four-year college enrollment. This information comes from National Student Clearinghouse (NSC) data obtained by CPS.

Most CPS students are nonwhite, and most are poor enough to qualify for a subsidized lunch. These and other descriptive statistics appear in columns 1–4 of Table 1, which show sample means for enrolled CPS 9th graders separately by sector (separately for traditional, exam school, charter school, and Noble students). Exam school students are less likely to be nonwhite or poor and have higher baseline scores than other CPS students. Charter students have markedly lower baseline scores than the typical CPS 9th grader and are disproportionately likely to be Black. Noble enrolls a lower proportion of Black students than other charter schools, but a higher proportion of Hispanics. Noble students also have baseline scores above those of other charter students, but these are still well below the CPS average.

Descriptive statistics for exam school *applicants* appear in columns 5–7 of Table 1. These are juxtaposed with statistics for the subset of exam school applicants affected by affirmative action. For purposes of this comparison, affected applicants are defined in two ways: statistics in column 8 are for applicants offered a seat at a more selective exam school by virtue of affirmative action (including applicants admitted to any exam school under affirmative action but not otherwise), while statistics in column 9 are for applicants offered a seat at a less selective exam school by virtue of affirmative action (including those who got no offer under affirmative action and would have been offered a seat otherwise).

Consistent with tier-based affirmative action, roughly 91% of applicants who benefit from affirmative action are from Tiers 1 and 2, while 98% of those offered a less selective school live in Tiers 3 and 4. Hispanics are over-represented among affirmative action beneficiaries, while whites are over-represented in the group of applicants offered a less selective school as a consequence of affirmative action. Those with worse offers also have baseline math and reading scores of about 0.3 standard deviations higher than those with better offers. Average baseline scores of those with a worse offer under affirmative action are similar to the baseline scores of the full sample of applicants offered an exam school seat.

Table 1
Descriptive statistics.

	CPS 9th Graders by School Sectors				Exam Applicants			Affirmative-Action-Affected Exam Applicants	
	Traditional schools (1)	Exam schools (2)	Charter schools (3)	Noble Network (4)	All exam Applicants (5)	Received an exam offer (6)	Exam and Noble Applicants (7)	AA better offer (8)	AA worse offer (9)
Tier 1	0.269	0.189	0.335	0.347	0.235	0.200	0.303	0.603	0.000
Tier 2	0.283	0.218	0.302	0.298	0.260	0.214	0.304	0.312	0.022
Tier 3	0.264	0.264	0.250	0.245	0.273	0.260	0.269	0.025	0.230
Tier 4	0.184	0.328	0.112	0.110	0.231	0.326	0.124	0.060	0.748
Black	0.440	0.350	0.573	0.463	0.418	0.287	0.387	0.338	0.168
Hispanic	0.430	0.302	0.376	0.498	0.423	0.401	0.548	0.558	0.309
White	0.075	0.211	0.020	0.015	0.096	0.188	0.025	0.048	0.353
Asian	0.033	0.085	0.013	0.011	0.048	0.099	0.031	0.043	0.130
Female	0.489	0.590	0.479	0.511	0.546	0.607	0.561	0.624	0.590
Free lunch	0.758	0.467	0.796	0.773	0.729	0.560	0.761	0.731	0.378
Reduced price lunch	0.084	0.113	0.096	0.123	0.096	0.111	0.119	0.108	0.098
Special education	0.170	0.088	0.185	0.169	0.101	0.037	0.104	0.031	0.056
Baseline math	−0.036	1.382	−0.299	−0.178	0.402	1.469	0.253	1.176	1.484
Baseline reading	−0.037	1.191	−0.243	−0.112	0.392	1.252	0.272	1.001	1.274
N	119,520	13,213	24,939	10,300	62,037	13,379	8,148	2,650	2,143

Notes: This table reports average characteristics of 9th grade CPS students and affirmative-action-affected exam school applicants. Columns 1 to 4 present descriptive statistics for CPS 9th graders attending traditional, exam, charter, and Noble Network schools between 2010 and 2013 (applying for seats in 2009-12). Columns 5 to 7 report these statistics for all exam school applicants, those that receive and offer, and those that also apply to a Noble campus. Columns 8 and 9 show the statistics for affirmative-action-affected applicants. Exam school applicants are classified into two groups by comparing exam schools offers under affirmative action and a non-affirmative action counterfactual match. Applicants can receive an offer from a more selective exam school (better offer), less selective exam school (worse offer), or the same offer because of tier-based affirmative action. School capacities for the non-affirmative-action hypothetical scenario are determined by the number of applicants who receive an offer via the school assignment match and exclude principal discretion offers. Baseline, PLAN, and ACT scores are standardized to the CPS test-taking population.

3.1. Baseline estimates

We're interested in the achievement consequences of exam school attendance for affirmative action beneficiaries and other exam school applicants. As noted above, CPS exam school assignment can be modeled as a serial dictatorship that generates school-and-tier-specific cutoffs. Denote these by $\tau_s(t)$ for school s and tier t (This notation ignores the fact that, in practice, cutoffs differ by application year as well as by tier). As detailed in Abdulkadiroğlu et al. (2017), the CPS exam school admissions process generates an RD research design that identifies the causal effects of exam school attendance by comparing applicants above and below these cutoffs. The key to our analysis is the RD-SD propensity score that controls for the local probability of school assignments. This strategy controls for the fact that this probability is non-degenerate around the admissions cutoffs of each exam school. Moreover, the centralized assignment mechanism generates additional variation at each school from the cutoff of the least selective higher-ranked school.

The CPS exam school admissions mechanism is formalized using additional notation as follows. Let $\theta_i = (\succ_i, t_i)$ denote applicant i's type, where $\succ_i$ is i's rank-order list of schools and t_i is their tier. The composite score used for admissions is denoted by r_i; this is the RD running variable. Let S_{θ_i} denote the set of schools ranked by applicants of type θ_i.

Abdulkadiroğlu et al. (2017) show that under serial dictatorship, applicant i from tier t_i obtains an offer at school s by clearing $\tau_s(t_i)$ and failing to get an offer from a school they've ranked higher than s. Individual school offer dummies, denoted D_{is}, are therefore determined by:

$$D_{is} = 1[\tau_s(t_i) \leqslant r_i < MID_s(\theta_i)] \tag{1}$$

where

$$MID_s(\theta_i) = \min_{\tilde{s} \succ_i s}\{\tau_{\tilde{s}}(t_i)\}$$

is the most forgiving (lowest) cutoff that applicant i faces in the set of cutoffs for schools that the applicant ranks ahead of s. Abdulkadiroğlu et al. (2017) call $MID_s(\theta_i)$ applicant i's *most informative disqualification.*[11]

We use the set of D_{is} as instruments for the number of years enrolled at any exam school, without (initially) distinguishing one exam school from another. The enrollment variable, denoted C_i, counts years enrolled between application and test date for achievement outcomes; for college enrollment, C_i indicates any exam school enrollment in 9th grade. The resulting over-identified 2SLS estimates use the fact that the first-stage relationship between C_i and individual D_{is} is likely to differ across schools since some offers are more attractive than others. This variation boosts the precision of 2SLS estimates relative to a just-identified model using a single any-offer dummy to instrument exam school exposure.

The 2SLS estimator deployed here mirrors that in Abdulkadiroğlu et al. (2017) in that it controls for the probability applicant i is offered a seat at exam school s, for each s. These probabilities, denoted p_{is}, are called *local propensity scores*. Under serial dictatorship, p_{is} is either 0, 1, or 0.5. Applicants have non-degenerate risk at school s, that is, a local score equal to 0.5, when they have running variable values close to either $\tau_s(t_i)$ or $MID_s(\theta_i)$, the two cutoffs determining D_{is} in (1). Applicants with running variable values between $\tau_s(t_i)$ and $MID_s(\theta_i)$ but close to neither cutoff are sure to be seated at s. All other applicants have no chance of bring seated at s.

These considerations lead to a two-equation setup that can be written:

[11] Tie-breakers are scaled to be in the unit interval, so when i prefers no school to s, we set $MID_s(\theta_i) = 1$.

$$Y_i = \beta C_i + \sum_s \lambda(p_{is}) + X_i'\delta_2 + g(t_i, r_i) + \varepsilon_i \quad (2)$$

$$C_i = \sum_s \gamma_s D_{is} + \sum_s \eta(p_{is}) + X_i'\delta_1 + h(t_i, r_i) + \nu_i, \quad (3)$$

where β is the causal effect of interest, $\eta(p_{is})$ and $\lambda(p_{is})$ are saturated propensity score control functions, and γ_s is the first-stage effect of D_{is} on exam-school exposure, C_i. Estimates of this model employ two specifications of the running variable controls, denoted $g(t_i, r_i)$ and $h(t_i, r_i)$. The first is a global quartic function, with parameters specific to each tier. This can be written:

$$g(t_i, r_i) = \sum_{p=0}^{4} \kappa_p(t_i) r_i^p, \quad (4)$$

where $\kappa_p(t_i)$ is the p^{th}-order term for tier t_i, with $h(t_i, r_i)$ defined similarly. The second uses a linear function, local to each cutoff. This can be written:

$$g(t_i, r_i) = \kappa_0(t_i) r_i + \sum_s \kappa_{1s}(t_i) \max\{0, r_i - \tau_s(t_i)\}, \quad (5)$$

where slopes change by amount κ_{1s} when the running variable crosses cutoff $\tau_s(t_i)$. Again, $h(t_i, r_i)$ is defined similarly.

Using either running variable control scheme, Eqs. (2) and (3) are computed in a sample of applicants with running variable values in at least one cutoff-specific bandwidth. These bandwidths are computed using the Imbens and Kalyanaraman (2012) procedure and are estimated separately for each outcome variable. Because bandwidths are computed for each cutoff, they vary by school, applicant tier, and year of application.[12] Covariate vector X_i includes race, gender, subsidized lunch status, baseline math, and baseline reading scores.

Estimates of first-stage Eq. (3) show that, on average, an exam school offer increases the probability of exam-school enrollment by 45 percentage points; exam offers increase years enrolled in an exam school in advance of taking the ACT by about 1.3 years. Admission to more selective exam schools yields higher take-up and hence a larger first stage than the offer of a seat at less selective schools. An offer from Northside, for example, increases exam school enrollment by approximately 65 percentage points. In contrast, an offer from King has an enrollment effect of only 22 percentage points. Fig. 2 shows a similar pattern of larger first-stage effects at more selective schools when the first-stage outcome is measured in years of enrollment.

Panel A of Table 2 reports 2SLS estimates of Eq. (2) computed with quartic running variable control, while Panel B shows the corresponding estimates computed with local linear control. The running variable specification matters little, though estimates in Panel A are more precise than those in Panel B. Perhaps surprisingly, the estimated effects on both PLAN and ACT math are negative and at least marginally significantly different from zero. The estimates in Panel A also suggest exam school attendance reduces four-year college enrollment. The estimated college effects with local linear control in Panel B are also negative, though not significantly different from zero. In view of the similarity of the estimates in Panels A and B, and the precision advantage seen in Panel A, the results that follow rely on quartic running variable control.

The appendix summarizes an exploration of threats to a causal interpretation of the estimates in Table 2. PLAN and ACT scores are available for 80%-82% of the sample used to compute these estimates. Appendix Table A1 shows that applicants receiving an exam offer are slightly more likely to contribute follow-up information on achievement and college than those not offered. Differential attrition is driven by the most selective exam schools. Differential attrition disappears, however, in a sample limited to the 65% of applicants coming from middle schools with the highest follow-up rates. Estimates for a balanced-attrition sample are similar to the estimates reported in the text. Appendix Table A2 presents encouraging evidence of covariate balance across exam school offer cutoffs. Despite some statistically significant imbalance in some demographic characteristics, these differences are small, and the estimates discussed below are robust to the inclusion of these covariates.[13] Manipulation tests for each exam school cutoff following Cattaneo et al. (2018), reported in Appendix Fig. A1, show no evidence of manipulation.

It's also noteworthy that the overidentification test statistics associated with the 2SLS estimates reported in Table 2 generate a decisive rejection for ACT and PLAN test scores. Rejection implies that the underlying instruments used one at a time produce statistically different IV estimates. Mismatch is a potential source of effect heterogeneity that may drive rejection. In particular, we might expect exam school effects to be lower or more negative when identified by offers to elite schools than when identified by offers from less-selective schools. The next section explores this and related sources of heterogeneous effects.

4. Assessing mismatch

4.1. Mismatch theory

In the Ellison and Pathak (2021) model of human capital production, mismatch induces a particular sort of treatment-effect heterogeneity. This heterogeneity reflects a match component that decreases in the distance between preparedness and curriculum difficulty. Suppose, in particular, that the expected outcome for a student of preparedness level a_i is described by:

$$V(a_i, q_s, \varphi_s) = f(a_i, q_s) - \zeta(a_i - \varphi_s)^2, \quad (6)$$

where q_s is school quality, φ_s is curriculum characterized by the median preparedness of the students school s serves, and ζ parameterizes the relative importance of student-curriculum matching. Let

$$a_i = x_i'\psi,$$

where x_i is a saturated description of discrete applicant characteristics coefficient vector ψ is not identically zero.

This production function yields a testable distinction between school quality effects common to all students and a set of interaction terms generated by mismatch. To see this, let the non-match-related component of human capital production be:

$$f(a_i, q_s) = q_s + a_i = q_s + x_i'\psi. \quad (7)$$

The mismatch term in (6) can now be written:

$$\zeta(x_i'\psi - \varphi_s)^2 = x_i'\omega_2 + x_i'\omega_1\varphi_s + \varrho_s, \quad (8)$$

where $x_i'\omega_2 = \zeta\psi' x_i x_i'\psi$, $\omega_1 = -2\zeta\psi$, and $\varrho_s = \zeta\varphi_s^2$.

Combining terms in (7) and (8) produces a linear-in-parameters specification of the form:

$$\begin{aligned} y_{is} &= (q_s + \varrho_s) + x_i'[\psi + \omega_2] + x_i'\omega_1\varphi_s \\ &= \mu_s + x_i'\pi + x_i'\omega_1\varphi_s, \end{aligned} \quad (9)$$

where $\mu_s = q_s + \varrho_s$ and $\pi = \psi + \omega_2$. Eq. (9) casts mismatch as a source of interactions between school selectivity (φ_s) and applicant characteristics related to preparedness (x_i), with an effect given by

[12] Estimates computed using variations on this bandwidth selection scheme are similar.

[13] The balance analysis uses the bandwidth for ACT math. The first two columns of Table A2 report F-statistics testing covariate balance for school-specific offers jointly. Columns 3–5 report coefficients and standard errors from a regression of covariates on an any-exam-school offer dummy.

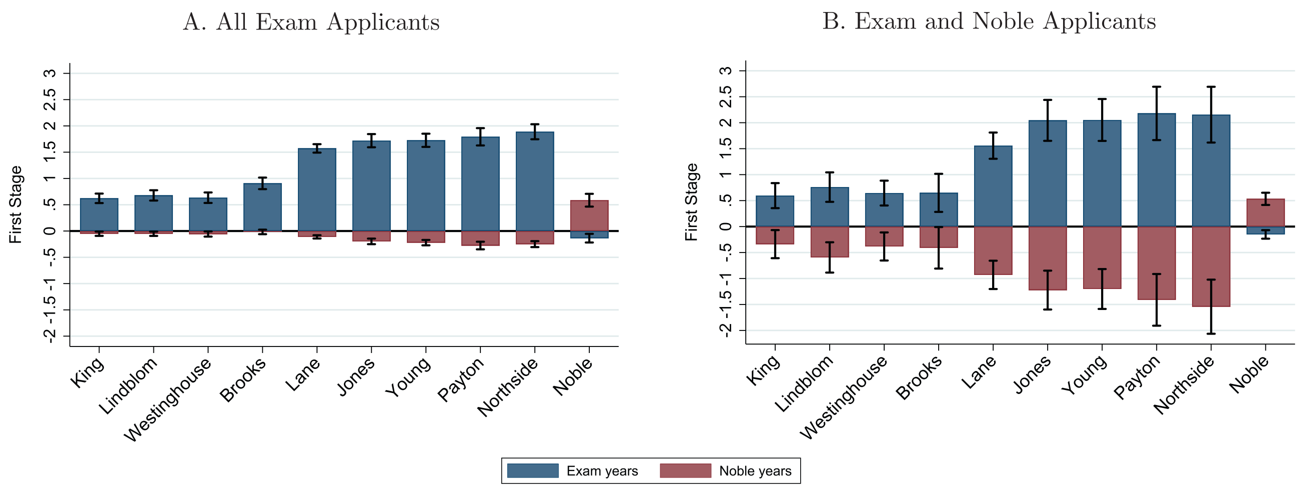

Fig. 2. Sector substitution induced by individual school offers.
Notes: This figure reports estimates of first-stage effects of individual exam school offers and the offer of a seat at an over- subscribed Noble school on years of (school-specific) exam school enrollment and years of (any) Noble school enrollment. Effects on years of exam school enrollment are plotted in blue; effects on years of Noble enrollment are plotted in red. First-stage equations include an indicator for any Noble offer at an over-subscribed campus and school-specific exam school offers, along with running variable controls and other covariates as described in the text. The sample is the same as that used to compute estimated effects on ACT math scores in Table 2. Whiskers mark 95% confidence intervals.

Table 2
2SLS estimates of exam school enrollment effects.

	PLAN		ACT		College enrollment	
	Math (1)	Reading (2)	Math (3)	Reading (4)	Any college (5)	Four-year college (6)
	A. 4th-degree polynomial					
Exam impact	−0.066**	0.013	−0.029**	−0.004	−0.006	−0.060**
	(0.027)	(0.030)	(0.014)	(0.015)	(0.017)	(0.024)
First stage F	109.6	109.5	168.7	163.8	214.2	214.6
Overid (DF=8) p-value	0.00	0.03	0.00	0.01	0.15	0.12
N	18,730	19,075	25,627	24,979	28,870	29,171
	B. Local-linear controls					
Exam impact	−0.083***	−0.037	−0.036**	0.008	−0.023	−0.047
	(0.031)	(0.035)	(0.016)	(0.018)	(0.021)	(0.029)
Non-offered mean	0.37	0.33	0.30	0.31	0.83	0.61
First stage F	72.5	72.9	109.4	110.4	129.2	130.0
Overid (DF=8) p-value	0.00	0.07	0.00	0.24	0.25	0.14
N	18,730	19,075	25,627	24,979	28,870	29,171

Notes: This table reports 2SLS estimates of exam school exposure effects for 2009-12 applicants. For test score outcomes (columns 1-4), the endogenous variable is years of enrollment between application and test date. For college outcomes (columns 5-6), the endogenous variable is 9th-grade enrollment. Panel A shows 2SLS estimates using a full set of school-specific offers as instruments, controlling for offer risk and a 4th-degree polynomial in the running variable. Panel B controls for a local-linear function in the running variable as described in the text. Additional controls include the covariates listed in Table A2. The table also shows first stage F statistics for over-identified models. Bandwidths use the formulas in Imbens and Kalyanaraman (2012). Robust standard errors are reported in parentheses; * significant at 10%; ** significant at 5%; *** significant at 1%.

ω_1. Mismatch may be especially important at elite schools and for less-prepared students.

4.2. Testing mismatch

To investigate differential effects by exam school selectivity, we distinguish the effects of enrollment at the four most selective schools (Northside, Payton, Young, and Jones) from other exam schools. The classification of schools into the elite and non-elite groups is motivated by Fig. 1, as the largest difference in the score of the median student is between Brooks and Lane. To empirically explore differences between elite and non-elite exam schools, let M_i denote years of elite exam enrollment, while L_i counts years enrolled at non-elite exam schools. The following two-endogenous-variable model identifies separate causal effects of elite and non-elite enrollment:

$$
\begin{aligned}
Y_i &= \beta_m M_i + \beta_l L_i + \sum_s \lambda_s(p_{is}) + X_i'\delta_2 + g(t_i, r_i) + \varepsilon_i, \\
M_i &= \sum_s \gamma_{ms} D_{is} + \sum_s \eta_{ms}(p_{is}) + X_i'\delta_{1m} + h_m(t_i, r_i) + \nu_{mi}, \\
L_i &= \sum_s \gamma_{ls} D_{is} + \sum_s \eta_{ls}(p_{is}) + X_i'\delta_{1l} + h_l(t_i, r_i) + \nu_{li}.
\end{aligned} \tag{10}
$$

This model is identified by using individual school offers as instruments for M_i and L_i. Coefficient β_m is the causal effect of exposure to an elite school environment while β_l is a non-elite exam school effect. First-stage effects of each instrument, denoted γ_{ms} and γ_{ls}, also differ for the two types of exam schools. Running variable controls, denoted $g(t_i, r_i)$, $h_m(t_i, r_i)$, and $h_l(t_i, r_i)$, are as described by Eq. (5).[14]

[14] Appendix Table A3 reports separate balance checks for elite and non-elite schools.

Table 3
2SLS Estimates of exam school enrollment effects by school type.

	PLAN		ACT		College enrollment	
	Math (1)	Reading (2)	Math (3)	Reading (4)	Any college (5)	Four-year college (6)
Elite exam impact	−0.067**	−0.024	−0.008	0.014	−0.013	−0.033
	(0.033)	(0.037)	(0.016)	(0.019)	(0.020)	(0.028)
Non-elite exam impact	−0.067**	0.012	−0.032**	−0.003	−0.006	−0.061**
	(0.026)	(0.029)	(0.014)	(0.015)	(0.017)	(0.024)
p-value (elite=non-elite)	0.97	0.10	0.01	0.12	0.55	0.06
F (elite exam)	184.3	181.3	297.1	268.9	471.5	451.2
F (non-elite exam)	144.5	144.0	219.1	212.4	269.1	274.3
N	18,730	19,075	25,627	24,979	28,870	29,171

Notes: This table reports 2SLS estimates of exam school exposure effects by school type, distinguishing between elite exam schools (Northside, Payton, Young, Jones, and Lane) and non-elite exam schools (King, Lindblom, Westinghouse, and Brooks) for 2009-12 applicants. For test score outcomes (columns 1-4), the endogenous variable is years of enrollment between application and test date. For college outcomes (columns 5-6), the endogenous variable is 9th-grade enrollment. The models control for a 4th-degree polynomial in the running variable and use a full set of school-specific offers as instruments, controlling for offer risk. Other controls are as described in the text. The table also shows first-stage F statistics for over-identified models and p-values for a test of subgroup effect equality. Bandwidths use the formulas in Imbens and Kalyanaraman (2012). Robust standard errors are reported in parentheses; * significant at 10%; ** significant at 5%; *** significant at 1%.

Estimates of β_m and β_l in (10), reported in Table 3, are broadly similar to the estimates from the over-identified single-effect model. Compare, for example, estimates of −0.067 and −0.067 for effects of elite and non-elite enrollment on PLAN math scores to the corresponding estimate of −0.066 in Panel A of Table 2. Interestingly, estimated effects of *non-elite* enrollment on both math ACTs and four-year college attendance are more negative than the corresponding estimates of elite effects. This runs counter to a mismatch hypothesis based on school selectivity.

Another take on the mismatch story looks at different groups of *applicants* rather than schools, distinguishing those most likely to be affected by affirmative action from others. The comparison in Table 1 shows that applicants benefiting from AA (i.e., those who receive a better offer in the AA match) reside in Tier 1 and Tier 2 neighborhoods. Along these lines, Barrow et al. (2020) argue that differences in relative preparedness, which they call "ordinal ranking", might explain heterogeneous effects by tier. Low-tier applicants admitted to exam schools see a larger fall in their position vis a vis classmates than do high-tier applicants.

Panel A of Table 4 reports estimates of a model allowing exam school effects to differ for applicants from low and high tiers. These are computed by replacing the distinction between years of enrollment at elite and non-elite schools in Eq. (10) with endogenous variables distinguishing effects of exam school exposure for applicants in Tiers 1 and 2 from effects on applicants in Tiers 3 and 4. This model is identified by instrumenting the two enrollment variables with a full set of school-specific offers interacted with tier (for a total of 36 instruments, controlling for tier main effects).

Consistent with mismatch, estimated effects of exam school exposure on ACT math scores (in column 3) are reasonably precise and considerably more negative for low-tier than for high-tier applicants. The rest of Panel A offers little support for mismatch or a relative-preparedness story, however. The estimate in column 1 for PLAN math shows only a slightly more negative effect of exam school enrollment on low-tier applicants (compare −0.055 and −0.052). Estimated effects on four-year college enrollment (reported in column 6) are also more negative for low-tier than for high-tier applicants. Still, the estimated difference in college effects by applicant type is not precise enough to be significantly different from zero.

Finally, we look at estimates of exam school effects that vary with whether applicants' baseline scores lie above those of the median among students at the exam school where they enroll. This analysis is motivated by Sander (2004)'s emphasis on relative achievement as a measure of exam school preparedness. Like the models used to compute the estimates in Table 3, this two-endogenous variables model is identified using school-specific offers as instruments. In this specification, statistically significant differences of exam effects favoring above-median applicants over below-median applicants support mismatch.

As can be seen in the bottom panel of Table 4, three of six estimated exam school effects for below-median applicants are negative and marginally significantly different from zero. At the same time, above-median students do not seem to benefit from exam school attendance. Indeed, exam school enrollment reduces reading scores more for above- than for below-median applicants, a result shown in columns 2 and 4 of the table. Therefore, these findings do not appear to support the preparedness mismatch story.

5. Diagnosing diversion

5.1. Counterfactual destinies

The estimates in Table 3 and 4 provide little support for the mismatch hypothesis as an explanation of negative exam school effects. In related work, Barrow et al. (2020) report reduced form estimates of exam school offer effects showing that exam school applicants attend schools with stronger peers and increased parental involvement and teacher satisfaction. These findings also contribute to the puzzle negative effects.

We argue here that negative exam school effects reflect *diversion* rather than mismatch. Specifically, exam school offers divert many applicants away from high-performing high schools in the Noble Network of charter schools. Charter schools are mostly autonomous, publicly-funded schools that operate under a framework known as a charter.[15] Noble is one of Chicago's most visible charter providers, enrolling 40% of Chicago's 9th-grade charter students. Founded in 1999, the Noble Network started with a single campus, Noble Street College Prep, expanding rapidly after 2006. In the years covered by our study, Noble operated eleven additional campuses. With the exception of Gary Comer College Prep, which includes a middle school, Noble runs high schools spanning grades 9–12.

Noble Network pedagogy is similar to that of other "No Excuses" charter schools, emphasizing extended instruction time, discipline and comportment, data-driven instruction, and teacher

[15] The first Illinois charter school opened in 1996. Illinois charters are typically approved by local school districts, though some are granted by the state. Charters usually last 5 years and must be certified by the Illinois State Board of Education.

Table 4
Exam school effects by tier and baseline score.

	PLAN		ACT		College enrollment	
	Math (1)	Reading (2)	Math (3)	Reading (4)	Any college (5)	Four-year college (6)
A. Effects by tier						
Low-tier applicants	−0.055**	0.037	−0.027*	−0.001	−0.002	−0.068***
	(0.026)	(0.029)	(0.014)	(0.015)	(0.018)	(0.026)
High-tier applicants	−0.052*	0.006	−0.005	−0.007	−0.006	−0.047*
	(0.029)	(0.033)	(0.015)	(0.017)	(0.017)	(0.025)
p-value (low tier = high tier)	0.90	0.19	0.06	0.61	0.79	0.29
F (low tier)	212.7	234.7	306.1	259.5	463.5	424.2
F (high tier)	90.1	78.4	140.7	130.4	168.1	150.9
N	18,730	19,075	25,627	24,979	28,870	29,171
B. Effects by baseline score (above and below the median)						
Exam enrollment	−0.084***	0.014	−0.037**	0.003	0.001	−0.047*
	(0.032)	(0.035)	(0.016)	(0.018)	(0.019)	(0.028)
Exam enrollment*above-median	0.044	−0.079**	0.004	−0.066**	0.001	0.007
	(0.035)	(0.038)	(0.026)	(0.029)	(0.011)	(0.015)
p-value (sum)	0.05	0.00	0.05	0.00	0.84	0.03
F (exam)	88.5	90.5	139.7	134.2	177.8	180.7
F (interaction)	2,870.0	2,873.7	4,192.2	3,836.4	4,743.4	4,911.7
N	18,668	19,012	25,535	24,889	28,776	29,078

Notes: This table reports 2SLS estimates of the effects of exam school exposure for 2009-12 applicants by tier and baseline ISAT score below/above median student. For test score outcomes (columns 1-4), the endogenous variable is years of enrollment between application and test date. For college outcomes (columns 5-6), the endogenous variable is 9th-grade enrollment. Panel A splits applicants by tier, distinguishing effects for applicants in tiers 1-2 and tiers 3-4. This model uses a full set of school-specific offers interacted with tier as instruments (for a total of 36), controlling for tier main effects. The running variable controls are a 4th-degree polynomial. Other controls are as used to compute the estimates in Table 2. Panel B reports estimates of exam attendance and its interaction with a dummy variable indicating that baseline scores are above those of the median student enrolled in the exam school attended. Estimates in Panel B use school-specific offers and having a baseline score above the median student enrolled in the exam school offered as instruments. The table also reports Angrist and Pischke's first-stage F statistics for models with multiple endogenous variables and p-values for a test of subgroup effect equality. Robust standard errors appear in parentheses; * significant at 10%; ** significant at 5%; *** significant at 1%.

training. Noble Network schools attract attention inside and outside Chicago. For example, the Network was one of only 12 charter management organizations to be awarded an expansion grant from the US Department of Education in 2015. In a study using admissions lotteries for 2003–2005 applicants, Davis and Heller (2019) report large positive effects of Noble attendance on college enrollment. Elsewhere, we've seen impressive achievement gains for those attending "No Excuses" charters in Boston (Abdulkadiroğlu et al., 2011; Angrist et al., 2013), Denver (Abdulkadiroğlu et al., 2017), New Orleans (Abdulkadiroğlu et al., 2016), and New York City (Dobbie and Fryer, 2013). Dobbie and Fryer (2015) also report results suggesting "No Excuses" charter attendance reduces teen pregnancy and involvement in the criminal justice system.[16]

Chicago resident students completing 8th grade can apply to as many Noble campuses as they like; Noble applicants may also receive multiple Noble offers. Seats at oversubscribed campuses are assigned in school-specific lotteries (applicants with siblings enrolled at a Noble school are automatically admitted there.) Students not receiving a lottery offer are placed on a randomly-ordered waitlist. Some Noble campuses are sometimes undersubscribed, meaning the waitlist is exhausted.

We refer to the distribution of sector enrollment for exam school applicants not offered an exam school seat as the distribution of counterfactual *destinies*. The case for Noble enrollment as a mediator of exam-school treatment effects begins by establishing the importance of Noble enrollment in the destiny distribution. The destiny distribution is defined for the offer-compliant population, that is, the set of applicants who enroll in an exam school when offered a seat but not otherwise. This distribution is most easily constructed using a single instrument indicating any exam school offer rather than school-specific offers.

The any-offer instrument exploits the fact that, under serial dictatorship, the probability applicant i from tier t_i is offered an exam school seat (somewhere) is the probability the applicant has a composite score that clears the most forgiving (that is, the lowest) cutoff for this tier. This *qualifying cutoff* is defined as:

$$q_i^* \equiv q^*(\theta_i) = \min_{\tilde{s} \in S_{\theta_i}} \{\tau_{\tilde{s}}(t_i)\}.$$

A dummy variable indicating that an exam school has offered a seat to applicant i can then be written:

$$D_i = 1[r_i \geqslant q_i^*].$$

Destinies are defined as a function of the sector in which applicant i enrolls in the year in which they take the ACT test (for example, the charter, exam, or traditional sectors). Define *potential* sector enrollment variables W_{1i} and W_{0i}, indexed by offer dummy D_i. Potential sector enrollment determines observed sector enrollment, denoted W_i, according to:

$$W_i = W_{0i} + (W_{1i} - W_{0i})D_i.$$

A dummy indicating potential exam school enrollment under alternative offer scenarios can likewise be written:

$$C_{di} = 1\{W_{di} = exam\};\ d = 0, 1,$$

where observed exam school enrollment satisfies

$$C_i = C_{0i} + (C_{1i} - C_{0i})D_i.$$

We're most interested in $E[1\{W_{0i} = j\}|C_{1i} > C_{0i}]$. This is the counterfactual destiny distribution for exam-offer compliers in the scenario where they're not offered an exam school seat. As in Abdulkadiroğlu et al. (2016), counterfactual destinies are consistently estimated by a just-identified 2SLS model with $1\{W_{0i} = j\}(1 - C_i)$ as the dependent variable and $(1 - C_i)$ as the

[16] School climate data from Illinois State Board of Education report cards (available at https://www.illinoisreportcard.com) suggest that, in comparison with Noble schools, non-Noble Chicago charters have fewer students taking advanced courses and higher student absenteeism and chronic truancy rates, among other differences. Estimates reported in Angrist et al. (2013) suggest non-"No Excuses" charters in Massachusetts are unlikely to boost achievement.

endogenous variable. These models can be described by the following two equations:

$$1\{W_i = j\}(1 - C_i) = \beta(1 - C_i) + X_i'\delta_2 + g(q_i^*, r_i) + \varepsilon_i \quad (11)$$

$$1 - C_i = \gamma D_i + X_i'\delta_1 + h(q_i^*, r_i) + \nu_i, \quad (12)$$

where D_i is the excluded instrument and parameter β is the share of compliers enrolling in sector j. Covariates, X_i, are as in the school-specific offers model, with the addition of a full set of qualifying-cut off-by-tier-by-application-year fixed effects. Running variable control is given by:

$$g(q_i^*, r_i) = \sum_{p=0}^{4} \kappa_p(q_i^*) r_i^p, \quad (13)$$

where terms denoted $\kappa_p(q_i^*); p = 1, \ldots, 4$ are quartic coefficients that depend on q_i^*. An analogous term, $h(q_i^*, r_i)$, appears in first-stage Eq. (12).[17]

The first bar plotted in Fig. 3 reveals that the most likely counterfactual destiny for exam school compliers is a traditional CPS school (including continuing and technical education (CTE) and military academies). But the charter sector is by far the most important destiny besides traditional. Another noteworthy destiny is the magnet sector. Magnet schools offer arts programs, agricultural science courses, and International Baccalaureate programs, enrolling applicants district-wide via lotteries. Cullen et al. (2006) use lotteries to estimate the achievement consequences of attendance at a Chicago magnet school in 2000, uncovering little evidence of a magnet impact; Angrist et al. (2019) replicate this null finding in the sample used here.[18] This leads us to focus on charters as the leading non-traditional alternative to exam schools.

Roughly a quarter of exam school offer-compliers enroll in a charter school when not offered an exam school seat. The outlined portion of the charter segment in the figure shows that Noble schools account for over half of the charter sector counterfactual. Column 7 in Table 1 reports descriptive statistics for exam and Noble applicants. These applicants are disproportionately likely to live in low-tier neighborhoods and to be Black or Hispanic, with baseline scores below those for all exam applicants.

Because applicants from Tier 1 and Tier 2 are disproportionately likely to benefit from affirmative action, we also report the destiny distribution for applicants from low tiers. The destiny distribution for low-tier applicants, plotted as the second bar in Fig. 3, suggests that the shares falling back to charter and Noble schools when not offered an exam school seat are similar to the corresponding shares seen in the sample as a whole.[19]

Finally, destinies are plotted for the subsample of exam school applicants who also apply to Noble schools, a group that accounts for approximately 13% of all exam school applicants. As the last column of Fig. 3 shows, Noble is the dominant counterfactual destiny for dual-sector applicants, with around 60% enrolling at a Noble campus when not offered an exam school seat.

A graphical exam school RD in this final subsample hints at the diversion story. Fig. 4 plots exam school enrollment and ACT math scores against the exam school admissions composite centered at applicants' qualifying cutoffs (positive values indicate qualification for admission), after regression-adjusting for the qualifying cutoff. Sector substitution in dual-sector applicant group is pronounced:

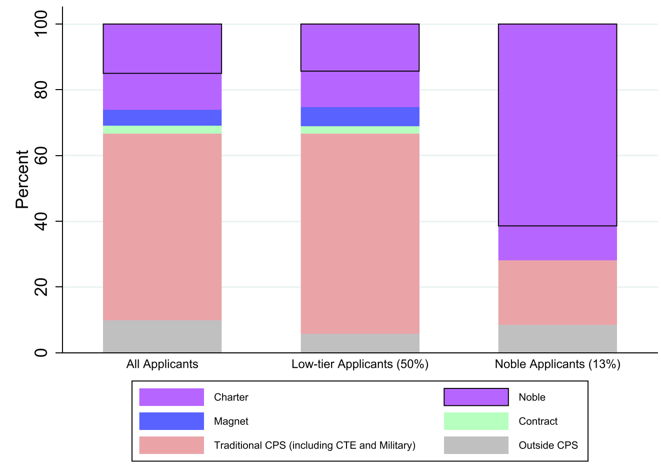

Fig. 3. Enrollment densities.
Notes: This figure shows the enrollment destinies of exam school compliers when not offered an exam school seat. Enrollment compliers are applicants who attend an exam school when offered a seat but not otherwise. The 1st bar plots destinies for all rejected exam school applicants. The 2nd bar plots destinies for rejected low-tier applicants. The 3rd bar plots destinies for rejected exam school applicants who also applied to a Noble school. Destinies are estimated as in Abdulkadiroglu et al. (2014). Enrollment rates are measured in the fall of the year of the ACT math test. A student who does not take ACT math is counted as "Outside CPS".

the drop in Noble enrollment at the qualifying cutoff equals about half of the increase in exam school enrollment. The right-hand side of this figure also shows a marked decrease in ACT math scores at the exam school cutoff.

5.2. Multi-sector models

A multi-sector model of education production unifies key features of Fig. 2. This model instruments Noble enrollment, denoted N_i, as well as exam school enrollment, C_i. Our Noble offer instrument, D_{in}, indicates applicants receiving a first-round offer at any oversubscribed Noble campus. As a benchmark, 2SLS estimates computed using D_{in} to instrument N_i in the sample of Noble applicants (ignoring exam school enrollment) suggest Noble enrollment boosts both PLAN and ACT scores markedly, while also increasing college enrollment.[20]

The diversion hypothesis asserts that exam school offers reduce Noble enrollment and that, after accounting for the benefits of this, negative exam school effects disappear. This story is substantiated and tested with the aid of the following two-sector model:

$$Y_i = \beta_c C_i + \beta_n N_i + \sum_s \lambda_s(p_{is}) + X_i'\delta_2 + g(t_i, r_i) + \varepsilon_i \quad (14)$$

$$C_i = \sum_s \gamma_{cs} D_{is} + \gamma_{cn} D_{in} + \sum_s \eta_{cs}(p_{is}) + X_i'\delta_{1c} + h_c(t_i, r_i) + \nu_{ci} \quad (15)$$

$$N_i = \sum_s \gamma_{ns} D_{is} + \gamma_{nn} D_{in} + \sum_s \eta_{ns}(p_{is}) + X_i'\delta_{1n} + h_n(t_i, r_i) + \nu_{ni}, \quad (16)$$

where γ's denote first-stage coefficients for individual exam school offers and an offer from any over-subscribed Noble campus, and the two β's are causal effects of exam school and Noble charter school enrollment. As in Eq. (10), this model includes propensity score and running variable controls, and a vector of covariates, X_i. The covariate vector here includes dummies indicating sets of Noble

[17] Because qualifying cutoffs are assumed to be unique, coefficients κ_p in this model vary freely with the school that determines q_i^* and applicants' tier and application year.

[18] In the period covered by our sample, CPS had four magnet high schools: Chicago High School for Agricultural Sciences, Clark Academic Prep, Curie Metropolitan, and Von Steuben Metropolitan.

[19] "Outside CPS" in Fig. 3 refers to enrollment in the test year. Some of those missing when not offered a seat return, while some of those offered a seat exit later, leading to the balanced attrition rates by offer status seen in Table A1.

[20] These estimates appear in Appendix Table A4. Models generating Noble effects include controls for the set of Noble schools to which applicants apply. Appendix Tables A1 and A2 report statistics related to attrition and balance for the Noble lottery research design. Panel C of Table A1 shows small differential attrition for the Noble offer instrument (around 2.3 pp). However, Noble estimates in the full sample are similar to those in a balanced attrition sample, suggesting that this attrition differential is immaterial.

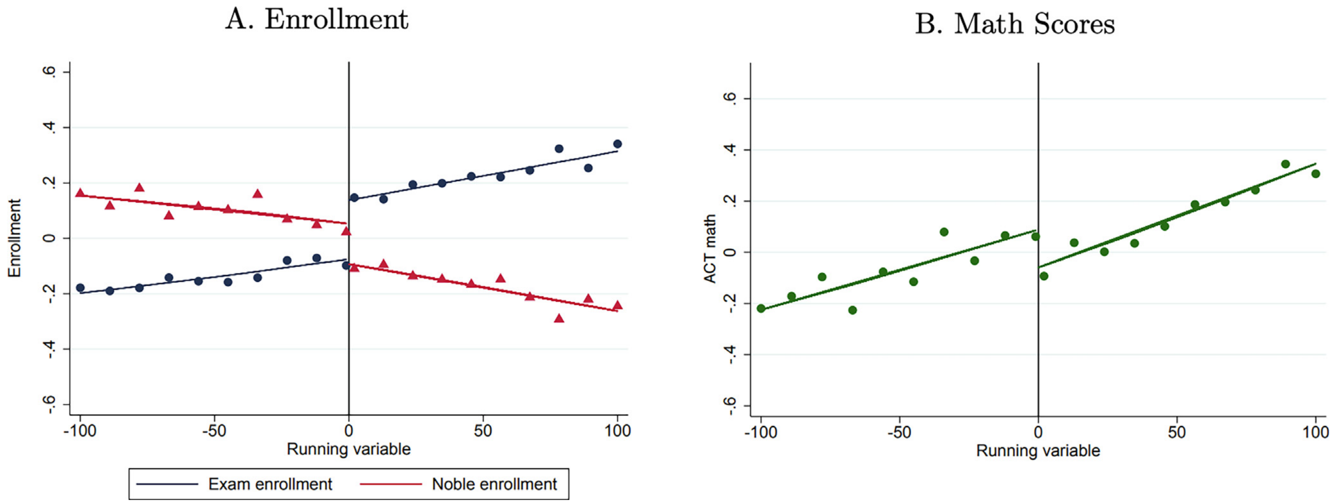

Fig. 4. Effects at qualifying cutoffs for exam and noble applicants.
Notes: The left side of this figure plots enrollment rates at exam and Noble schools against the centered and regression- adjusted exam school running variable (that is, the tie-breaker used in the exam school match). Applicants who clear their qualifying cutoff are offered an exam school seat. The right side plots ACT math scores against the same x-variable. Plotted points are averages in 10-unit windows; lines in the plots are estimated conditional mean functions smoothed using local linear regression (LLR). All variables are plotted after partialling out saturated qualifying-cutoff-by-tier-by-application-year fixed effects.

campuses applied to and the other controls used to compute the estimates in Table 2. Running variable controls (denoted by functions $h_c(t_i, r_i)$, $h_n(t_i, r_i)$, and $g(t_i, r_i)$ in the system above) are parameterized with a quartic function, as for the RD estimates in Panel A of Table 2.

Multi-sector first stage estimates, plotted in Fig. 2, show that the offer of a seat in each sector boosts enrollment in the sector offered, while reducing enrollment elsewhere. For instance, the estimates of γ_{cs} on the left side of the figure (plotted in blue and computed in the sample of all exam school applicants) suggest that an exam sector offer increases exam years enrolled from 0.5 to 2 years, with larger take-up at more selective schools. At the same time, the offer of a seat at an over-subscribed Noble school increases Noble years by a little over 0.5 years (this estimate of γ_{nn} is plotted in red). In the sample limited to exam school applicants who also applied to a Noble school (for which estimates appear in Panel B), many school-specific offer effects on exam school enrollment exceed the corresponding estimates computed in the wider sample.

The estimated cross-sector effects in Fig. 2 (i.e., estimates of γ_{cn} in (15) and γ_{ns} in (16)) document considerable substitution across sectors. The offer of an exam school seat at Payton, for example, reduces Noble enrollment by around 0.2 years in the exam-applicant sample. Estimated effects of exam school offers on Noble enrollment are even larger (i.e., more negative) in the sample limited to applicants to both sectors. In this sample, a Payton offers pulls Noble exposure down by over 1.25 years. At the same time, Noble offers reduce exam school enrollment similarly in the two samples; estimates of γ_{cn} show a decline of just over 0.2 years.

As a benchmark for estimates of the two-sector model, odd-numbered columns in Panel A of Table 5 repeat the 2SLS estimates shown in Panel A of Table 2, while odd columns in Panel B show the corresponding estimates computed in the sample of exam school applicants who also applied to a Noble school. Consistent with the large discontinuity in math scores seen in Fig. 4, estimates from the narrower sample are considerably larger and more negative than the corresponding estimates in Panel A. In the narrower sample, for example, each year of exam school enrollment reduces ACT math scores by 0.216. Estimates in Panel B also show large exam-school-induced reductions in four-year college enrollment for exam and Noble applicants.

2SLS estimates of β_c in Eq. (14), reported in even-numbered columns in Table 5, are small and not significantly different from zero. In view of the large, significant, and positive estimates of β_n shown in these columns, this finding suggests that negative effects of exam school exposure on ACT math scores and four-year college-going are explained by the combination of large gains from Noble enrollment and exam-induced substitution away from the Noble sector. The estimated effect of Noble enrollment on ACT scores ranges from 0.111 to 0.288 in Panel A, while the associated exam school estimates are small and not significantly different from zero.

Estimated effects on four-year college enrollment, reported in column 8, show a similar pattern (as do estimated effects on PLAN scores, not shown in the table). These results are broadly consistent with Barrow et al. (2020), which shows that CPS exam school qualification reduces grades and the likelihood exam school applicants attend a selective college. Our analysis extends and explains these earlier findings: significant negative exam school enrollment effects on college enrollment are closer to zero and not significantly different from zero once we account for Noble enrollment. The college enrollment boost from Noble enrollment appears to be around 18 percentage points, a striking gain. The fact that exam offers draw students away from Noble causes many of those offered a seat to forego this gain.[21]

While Noble schools are the counterfactual for 15% of exam-school compliers (the first bar of Fig. 3), the estimates suggest that the large positive impacts of Noble and the diversion from exam schools offers account for the negative exam effects on math scores and college outcomes. Multiplying the 15% share of compliers attending Noble by the ACT math effect of 0.376 in Table A4 results in an impact of −0.056, which is in the ballpark of the exam effect on ACT math of −0.029 in the sample of all exam applicants shown in column 1 of Panel A, Table 5. A parallel calculation for four-year college enrollment gives 0.15*(-0.225)=-0.034, which is also in the ballpark of the −0.060 seen in column 7 of Panel A, Table 5.

[21] Appendix Table A5 is a version of Table 5 estimated on the balanced attrition sample described in Panel C of Table A1. The results for the balanced attrition sample are in line with those in Table 5. If anything, the substitution pattern for four-year college-going is more substantial in this sample. An alternative Table 5-like strategy simply drops Noble applicants. This likewise yields small estimated exam school effects, not significantly different from zero, with standard errors similar to those shown in Panel A of Table 5.

Table 5
2SLS estimates of multiple sector effects.

	ACT				College enrollment			
	Math		Reading		Any college		Four-year college	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
A. All exam applicants								
Exam impact	-0.029**	-0.003	-0.004	0.006	-0.006	0.003	-0.060**	-0.041
	(0.014)	(0.014)	(0.015)	(0.016)	(0.017)	(0.018)	(0.024)	(0.026)
Noble impact		0.288***		0.111**		0.074		0.184**
		(0.045)		(0.048)		(0.055)		(0.081)
p-value (exam = Noble)		0.00		0.02		0.17		0.00
F (exam)	168.7	157.6	163.8	155.1	214.3	200.6	214.6	197.4
F (Noble)		16.3		17.1		21.0		18.1
N	25,627	25,627	24,979	24,979	28,870	28,870	29,171	29,171
B. Exam and Noble applicants								
Exam impact	-0.216***	0.032	-0.021	0.078	-0.024	0.026	-0.207***	-0.117
	(0.042)	(0.059)	(0.044)	(0.065)	(0.041)	(0.067)	(0.061)	(0.098)
Noble impact		0.385***		0.152**		0.083		0.182*
		(0.064)		(0.065)		(0.069)		(0.101)
p-value (exam = Noble)		0.00		0.10		0.23		0.00
F (exam)	20.4	10.1	21.8	11.7	26.4	15.4	29.3	15.5
F (Noble)		6.2		7.4		10.2		9.7
N	3,641	3,641	3,572	3,572	4,217	4,217	4,246	4,246

Notes: This table reports 2SLS estimates of the effects of exam and Noble exposure for 2009-12 applicants, estimated in multi-sector models. For ACT scores (columns 1-4), the endogenous variable is years of enrollment between application and test date. For college outcomes (columns 5-8), the endogenous variable is 9th-grade enrollment. Estimates use school-specific offer dummies as instruments. The running variable controls are a 4th-degree polynomial. Other controls are as used to compute the estimates in Table 2. The table also reports Angrist and Pischke's first-stage F statistics for models with multiple endogenous variables and p-values for sector-effect equality tests. Robust standard errors appear in parentheses; * significant at 10%; ** significant at 5%; *** significant at 1%.

5.3. Mismatch vs. diversion in theory and data

The last piece of evidence favoring diversion over mismatch is based on the observation that mismatch disappears when the interaction term ω_1 in Eq. (9) equals zero, since this requires $\zeta = 0$ in (6). This leads to a test of the claim that subgroup variation in the *Noble* first stage is sufficient to explain heterogeneous *exam school* offer effects, with no other interactions needed. The test is implemented via 2SLS estimation of an empirical version of Eq. (9):

$$Y_i = \sum_s A_{is}\mu_s + X_i'\pi + \sum_s A_{is}X_i'\delta_s + \varepsilon_i, \tag{17}$$

where A_{is} indicates enrollment at *any* school s (exam or otherwise, so A_{is} might also indicate enrollment at a Noble school) with quality effect μ_s. Coefficient δ_s parameterizes student-school interaction terms analogous to $x_i'\omega_1\varphi_s$ in theoretical Eq. (9). In principle, Eq. (17) can be identified by using offer dummies D_{is} and interactions $D_{is}X_i$ as instruments for A_{is} and $A_{is}X_i$.

When matching matters little, school quality effects are the same for all students, and therefore captured by μ_s. In this scenario, $\delta_s = 0$ and the reduced form associated with (17) becomes:

$$E[Y_i|X_i, D_{is}] = \sum_s E[A_{is}|X_i, D_{is}]\mu_s + X_i'\pi. \tag{18}$$

Suppose also that Noble enrollment is the *only* source of heterogeneity in education production. Although a strong claim, the multi-sector estimates discussed in the previous section suggest this hypothesis is worth exploring. To that end, let Z_i be any instrument that changes Noble enrollment. Eq. (18) then simplifies to:

$$E[Y_i|X_i, Z_i] = \mu_0 + E[N_i|X_i, Z_i]\mu_n + X_i'\pi, \tag{19}$$

where μ_n is a Noble sector effect on Y_i.

Finally, let $\rho_Z(X_i)$ and $\phi_Z(X_i)$ denote the reduced form and the first stage for a Noble sector effect identified by instrument Z_i, in a sample stratified on X_i. Differencing by Z_i conditional on X_i, we obtain:

$$\rho_Z(X_i) = (E[N_i|X_i, Z_i = 1] - E[N_i|X_i, Z_i = 0])\mu_n = \phi_Z(X_i)\mu_n, \tag{20}$$

a strong and potentially falsifiable proportionality restriction.

Eq. (19) implies that the reduced form effect of instrument Z_i for *each* X_i and *any* offer instrument, Z_i, is driven by the corresponding covariate-specific Noble first stage coefficient associated with Z_i. In other words, the covariate-specific reduced form effect of *any* school offer (exam or Noble) should be proportional to the corresponding covariate-specific first stage effect of this offer on Noble enrollment. When these restrictions hold, the slope of the line linking offer-status differences in outcomes to the corresponding differences in average years enrolled at Noble equals μ_n. Moreover, this line runs through the origin: instruments that leave Noble enrollment unchanged should leave outcomes unchanged.

5.4. Covariate heterogeneity explained

The proportionality hypothesis embodied in Eq. (20) generates two families of testable restrictions. The first says that IV estimates using exam school offers as instruments for Noble enrollment should generate 2SLS estimates of Noble enrollment effects similar to estimates generated using Noble offer dummies as instruments for Noble enrollment. The second says that, for a given instrument, IV estimates across covariate-defined subgroups should be equal.

As a point of reference, Panel A in Table 6 reports just-identified IV estimates computed using a Noble offer dummy to instrument Noble enrollment in the sample of applicants applying to both Noble and exam schools. The estimated Noble effects seen here are similar to those for the sample of all Noble applicants reported in Appendix Table A4. For example, estimated effects on math scores ranging from about 0.34 to 0.39. The estimated effect on PLAN reading is just about as large at around 0.26. The same sample and identification strategy also suggests Noble generates large gains in four-year college enrollment.

Estimates using an *any exam school offer* to instrument *Noble* enrollment appear, with standard errors, in the second pair of rows in Table 6. These are mostly larger than the corresponding estimates computed using a Noble offer instrument. Not surprisingly, since the first stage is smaller, they're also less precise. At the same time, estimates using alternative offer instruments are qualitatively similar to each other in that both show large and statistically significant gains from Noble attendance. These alternative IV esti-

Table 6
Noble effects identified by offer instruments and covariate interactions.

	PLAN		ACT		College outcomes	
	Math (1)	Reading (2)	Math (3)	Reading (4)	Any college (5)	Four-year college (6)
A. Offer instruments without covariates						
Noble lottery offer						
Noble impact	0.387***	0.264**	0.336***	0.077	0.075	0.183*
	(0.089)	(0.104)	(0.050)	(0.062)	(0.064)	(0.094)
Exam offer (clears qualifying cutoff)						
Noble impact	0.615***	0.542**	0.463***	0.106	0.185	0.445**
	(0.175)	(0.212)	(0.109)	(0.123)	(0.123)	(0.180)
Both offers						
Noble impact	0.453***	0.380***	0.369***	0.089	0.102*	0.268**
	(0.079)	(0.096)	(0.045)	(0.058)	(0.061)	(0.085)
First stage F	39.6	42.3	45.9	41.3	60.2	57.1
Overid (DF=1) p-value	0.346	0.255	0.347	0.795	0.513	0.173
N	2,407	2,491	3,488	3,397	4,053	4,126
B. Offer instruments with covariate interactions						
Noble lottery offer						
Noble impact	0.370***	0.227**	0.327***	0.086	0.068	0.186**
	(0.083)	(0.096)	(0.048)	(0.059)	(0.061)	(0.088)
First stage F	6.6	7.3	8.2	7.0	10.1	9.6
Overid (DF=9) p-value	0.332	0.445	0.994	0.853	0.144	0.713
Exam offer (LIML)						
Noble impact	0.573***	0.497**	0.438***	0.094	0.176	0.456*
	(0.153)	(0.202)	(0.111)	(0.123)	(0.121)	(0.241)
First stage F	3.7	3.8	3.3	3.4	4.0	3.8
Overid (DF=9) p-value	0.698	0.341	0.451	0.513	0.655	0.169
Both offers						
Noble impact	0.437***	0.326***	0.364***	0.098*	0.093*	0.245**
	(0.072)	(0.086)	(0.042)	(0.053)	(0.056)	(0.080)
First stage F	4.8	5.3	5.4	4.9	6.9	6.6
Overid (DF=19) p-value	0.717	0.294	0.889	0.807	0.349	0.375
N	2,407	2,491	3,488	3,397	4,053	4,126

Notes: This table reports alternative IV estimates of the effects of Noble exposure on 2009-12 exam school applicants. The sample includes applicants who applied to exam schools and Noble schools. Estimates in Panel A are computed by instrumenting Noble exposure with a dummy for the type of offer indicated in the column heading. For the estimates in Panel B, the instrument list includes the type of offer indicated plus interactions with covariates (lunch status, race, gender, baseline test scores, and tier dummies). Running variable controls are a 4th-degree polynomial. Other controls are as used to compute the estimates in Table 2. The table also reports first-stage F-statistics and over-identification test p-values. Robust standard errors are reported in parentheses; * significant at 10%; ** significant at 5%; *** significant at 1%.

mates are also consistent with one another in showing large ACT math gains, with less evidence of an effect on ACT reading. The estimates also consistently show large gains in four-year college attendance with less evidence of increased any-college enrollment.

As can be seen at the bottom of Panel A in Table 6, 2SLS estimates using both Noble and exam school offers to instrument Noble attendance are a little more precise than the just-identified estimates computed using Noble offers alone. The overidentification test statistic associated with a 2SLS procedure that uses both Noble and exam offer dummies as instruments gives a formal test of the equality of IV estimates computed using the offer dummies as instruments one at a time (see, e.g., Angrist and Pischke (2009)). The p-values for the overidentification tests associated with these estimates are no smaller than 0.17 and mostly quite a bit larger.

The overidentification test statistic associated with a 2SLS procedure that uses offer dummies plus offer-covariate interactions as instruments implicitly tests the equality of IV estimates computed separately for covariate subgroups. This test, therefore, evaluates the second family of restrictions implied by Eq. (20). This test is first implemented by instrumenting Noble enrollment with D_{in} and nine D_{in}-covariate interactions, in a model which controls for covariate main effects (as well as the other controls used to compute the estimates in Tables 2–5). With 9 covariate interactions and an offer main effect in the instrument list, the resulting overidentification test has 9 degrees of freedom. The covariates generating interactions are dummies for free and reduced-price lunch eligibility, Black and Hispanic race, gender, low baseline test scores, and three tier dummies.

2SLS estimates using Noble offers and offer-covariate interactions as instruments for Noble enrollment are much like those using a Noble offer dummy alone. Compare, for example, the estimate of approximately 0.327 for ACT math in the first row in Panel B of Table 6 with the corresponding estimate of 0.336 at the top of the table. Estimates for the remaining five outcomes are similarly close. The overidentification test statistics associated with these estimates provide little evidence of differences in impact across covariate subgroups.

Panel B of Table 6 also shows estimates computed using an instrument list that interacts exam offers with covariate subgroups. Because the first-stage F statistics here are low enough to raise concerns about finite-sample bias, this part of the table reports LIML estimates rather than 2SLS (LIML is relatively robust in models with many weak instruments; see, e.g., Angrist and Pischke (2009)). The LIML estimates of over-identified models reported in Panel B are again close to those generated by a single exam offer dummy. (LIML bias mitigation is not a free lunch: two of the LIML estimates in the table are less precise than the corresponding just-identified estimate). The overidentification p-values again show little evidence of heterogeneous effects.

Finally, the bottom of Table 6 reports results from a 2SLS procedure that combines both offer dummies, plus the associated sets of covariate interactions, for a total of 20 instruments. As with the two-offer estimates at the bottom of Panel A, the estimates computed by interacting both offer dummies with covariates mostly lie between the corresponding 2SLS estimates computed using covariate interactions with exam offer or Noble offer dummies alone. The overidentification test statistics for this model (with

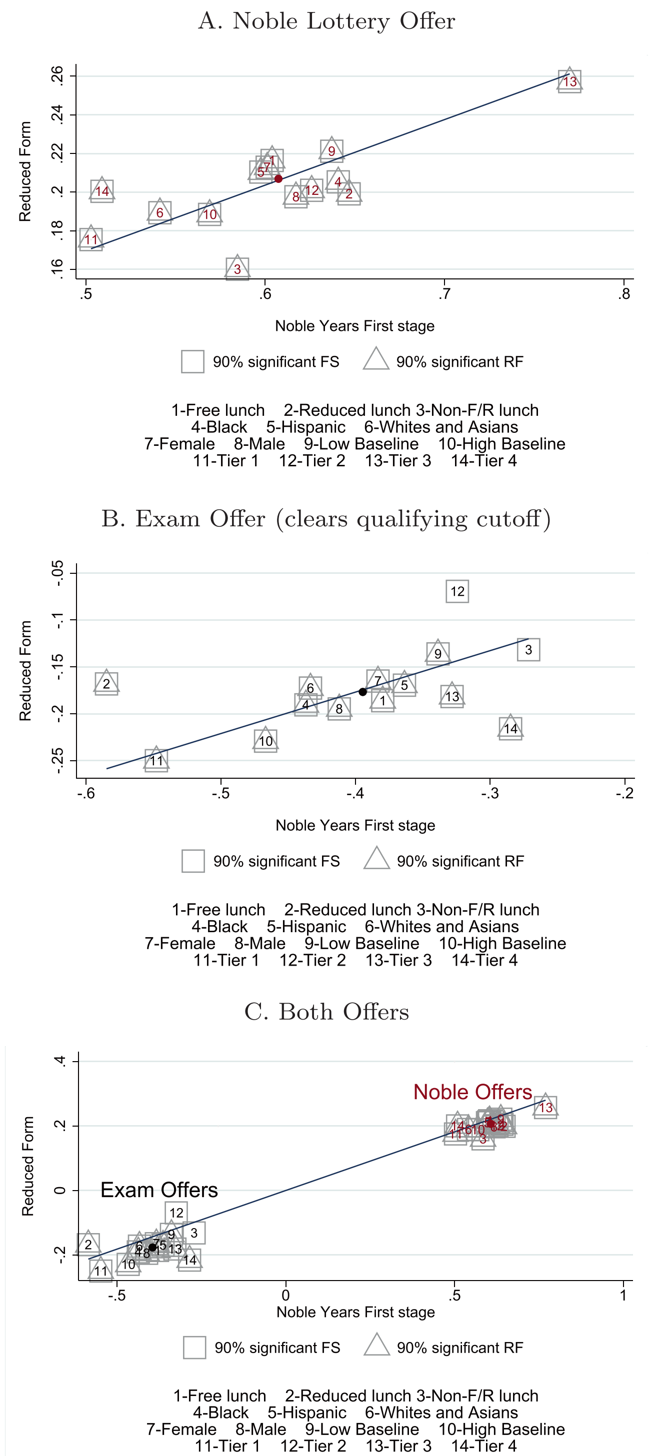

Fig. 5. Covariate VIV for the effects of noble enrollment on ACT Math.
Notes: This is a visual instrumental variable (VIV) plot of reduced form effects of exam school and Noble offers on ACT math scores against the corresponding first stage effects of exam school and Noble offers on Noble years enrolled, separately for a set of 14 covariate-defined groups. Exam offer effects are plotted in black; Noble offer effects are plotted in red. Panel A plots Noble offer effects and interactions only; Panel B plots exam offer effects and interactions only; Panel C plots both. Covariate-specific estimates are computed one at a time in the relevant subsamples and labeled from 1–14. The slope of the line through these estimates is 0.34 in Panel A, 0.44 in Panel B, and 0.35 in Panel C. Fitted lines are forced to pass through the origin, as implied by the proportionality restriction described in the text. Estimates for all applicants are plotted with dots; these are omitted from the VIV slope calculation.

19 degrees of freedom) offer little evidence against the homogeneity restrictions described by Eq. (20).

The remarkable homogeneity documented in Table 6 is highlighted by Fig. 5, which presents a graphical representation of 2SLS estimates computed with two offer types and covariate interactions included in the instrument list. Specifically, the figure plots reduced form estimates for ACT math against the corresponding first stage estimates for Noble exposure, conditional on each covariate, and constructed using both exam and Noble offer instruments. The model used to construct the points in the figure estimates all covariate-specific reduced-form and first-stage interactions at once. (The appendix to Angrist et al. (2022) details methods and formulas for this style of visual IV (VIV)). Red numbers in the figure mark estimates computed using a Noble offer instrument, while black numbers label those computed using the any-exam-school offer dummy. For example, the two points labeled with "1" show first stage and reduced-form estimates for applicants who qualify for a free lunch. Solid dots in the figure plot estimates using any-offer instruments across all covariate cells.[22]

Across covariate cells, the Noble lottery instrument generates large positive first stage increases in Noble enrollment and a corresponding set of positive reduced form estimates. This can be seen in Panel A of Fig. 5. At the same time, Panel B of the figure shows that first-stage effects of an exam offer on Noble enrollment is mostly negative, as are the corresponding reduced form estimates. Finally, in Panel C, the points from both research designs fall roughly on a straight line with a slope equal to about 0.35.[23]

Overall, the empirical evidence in Table 6 and Fig. 5 favors the diversion over the mismatch hypothesis. The 2SLS estimates of Noble attendance on students' outcomes are similar across different demographic groups and when computed using different types of offer variation. Estimated Noble effects computed using school-specific exam school offers for Noble enrollment (not shown) are likewise similar to those reported here.

6. Exam schools elsewhere

How general is the Chicago diversion story? In a study of Boston and New York City exam schools, Abdulkadiroğlu et al. (2014) find that the offer of an exam school seat reduces math scores by around -0.05σ for applicant cohorts applying in the late 1990s and 2000s. Consistent with mismatch, negative offer effects on math are markedly larger for Black and Hispanic applicants in both cities (-0.09σ in Boston and -0.08σ in New York). Like Chicago, however, Boston and New York also have large charter sectors, so diversion may play a role in generating negative effects in these cities too.

Evidence on this point comes from an analysis of exam schools in an anonymous large urban district (LUD). As in Chicago, most LUD students are nonwhite. LUD exam schools likewise admit students using a centralized DA-based assignment scheme similar to that used in the CPS match. LUD also has a robust charter sector, with many schools employing pedagogical practices characteristic of the Noble network. This motivates a comparison of exam school effects estimated for all LUD applicants with effects estimated for those enrolled in one of the district's charter middle schools at the time of application. Most applicants in the latter group are destined for charter schools when not offered an exam school seat.

The analysis sample for this investigation includes applicants who applied for an exam school seat from 2003 to 2015. Our LUD research design exploits centralized seat assignment using a 2SLS setup analogous to that described by Eqs. (2) and (3) for CPS. LUD outcome variables are scores on statewide math and Eng-

Table 7
Exam school effects in a large urban district.

	All Applicants		Charter-enrolled Applicants	
	Math (1)	English (2)	Math (3)	English (4)
	A. Scores after one year			
Exam impact	-0.042**	-0.024	-0.223***	0.060
	(0.020)	(0.024)	(0.077)	(0.104)
Control mean	0.597	0.559	0.824	0.751
First stage F	594.1	462.0	37.4	29.6
N	16,900	16,904	2,745	2,750
	B. Scores for all available years			
Exam impact	-0.025	-0.008	-0.158***	-0.005
	(0.016)	(0.017)	(0.058)	(0.062)
Control mean	0.573	0.548	0.754	0.728
First stage F	452.5	428.5	34.9	35.0
N	25,616	25,618	3,861	3,863

Notes: This table reports 2SLS estimates of the effects of years of exam school exposure for 2003-15 applicants to exam schools in a large urban district. Charter-enrolled applicants were enrolled in charter schools at the time of application and are likely to attend charters when not offered an exam school seat. Outcomes are test scores on a statewide assessment, standardized to the district mean and to have unit standard deviation. All students were tested at the end of the first post-application school year; some were also tested in later grades. The estimates use a full set of school-specific offers as instruments, controlling for offer risk. Models also include local linear running variable controls (with coefficients varying by school and year), demographic variables, and baseline test scores. The bandwidth calculation parallels that used to compute the estimates in Tables 2-4. Standard errors clustered at the applicant level are shown in parentheses; * significant at 10%; ** significant at 5%; *** significant at 1%.

lish assessments, standardized to have mean zero and unit standard deviation in the district. While 11% of LUD's exam-offer compliers land in a charter school when not offered an exam school seat, the proportion with a charter destiny reaches 63% when the sample is limited to applicants enrolled in a charter school at the time they applied.

2SLS estimates for the sample of all LUD exam school applicants suggest exam school attendance reduces math scores by a statistically significant -0.042σ in the first year after application. This estimate appears at the top of the first column of Table 7. As can be seen in Panel B in the table, the negative math effect shrinks to -0.025 when scores on tests taken later are added to the sample. Consistent with CPS results, estimated exam school effects on English scores (reported in column 2) are small and not significantly different from zero. On balance, the picture painted by columns 1–2 in Table 7 is remarkably consistent with that seen for CPS in Table 2.

In contrast with the small significant decline in math scores estimated for all applicants, 2SLS estimates of exam school effects for the sample of charter-originating LUD applicants show a sharp reduction in math achievement. The estimated effect on math scores, reported in column 3 of Table 7, ranges from -0.22 standard deviations in the first post-application year to -0.16 per year enrolled when later years are taken into account. These results mirror those for Chicago (reported in Table 5), showing that negative exam school effects in CPS are much larger for applicants who also apply to Noble. Table 7, therefore, bolsters the case for diversion as the key driver of negative exam school effects.

7. Summary and conclusions

Diversity concerns have long been center stage in the debate over selective public schools (see, e.g., The Boston Globe (2016) and New York Times (2017)). The mismatch hypothesis is a touchstone in this debate. It stands to reason that academic preparedness might mediate exam school effects. Our findings show, however, that as far as achievement and four-year college enrollment go, exam school impact is driven by alternative schooling options rather than by measures of applicant preparedness. More generally, we find little in the way of school-specific match effects of any kind. Remarkably, a constant-effects model rationalizes the reduced form effects of being offered a seat at both exam schools and charter schools across a wide range of covariate subgroups.

Chicago is not unique among large urban districts in featuring important exam school and charter school sectors. It's noteworthy, therefore, that data from another large urban district reflect the same sort of consequential sector substitution among exam school applicants originating in the district's charter schools. These results suggest that policies focused solely on increasing selective school diversity, rather than school quality in the form of causal value-added, are likely to yield few learning gains for disadvantaged groups.

Data availability

The authors do not have permission to share data.

Declaration of Competing Interest

The authors declare that they have no known competing financial interests or personal relationships that could have appeared to influence the work reported in this paper.

Appendix A. Appendix: additional figures and tables

See Fig. A1 and Tables A1, A2, A3, A4, A5.

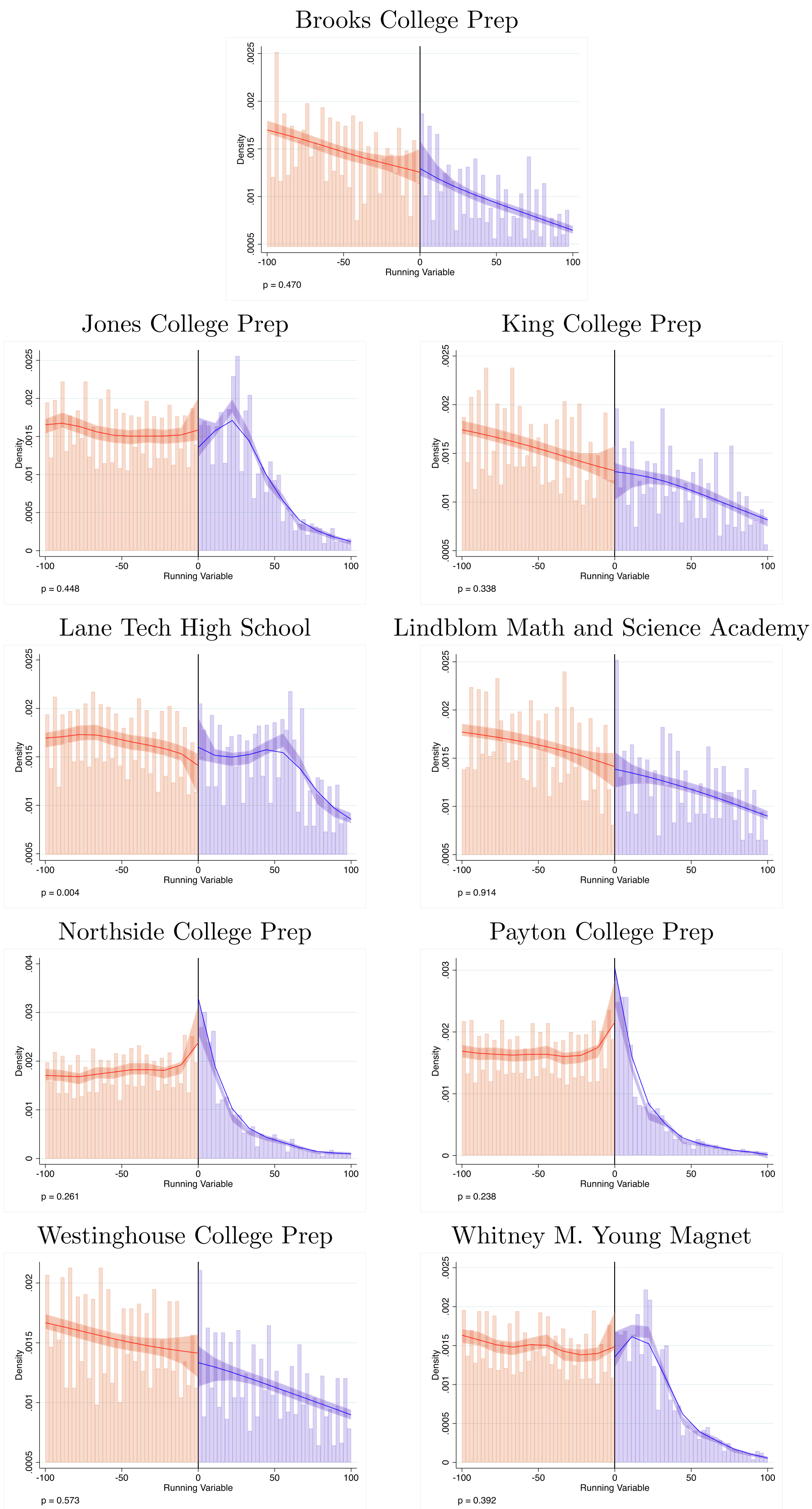

Fig. A1. Running variable density tests.
Notes: This figure shows density tests around the cutoff of each school following Cattaneo et al. (2018). The note of each plot reports the p-value of no manipulation around each particular cutoff.

Table A1
Differential attrition.

	PLAN		ACT		College	
	Math	Reading	Math	Reading	Any college	Four-year college
	(1)	(2)	(3)	(4)	(5)	(6)
			A. Any exam offer			
Clears qualifying cutoff	0.024**	0.026***	0.033***	0.030***	0.034***	0.032***
	(0.010)	(0.010)	(0.009)	(0.009)	(0.008)	(0.009)
Mean	0.83	0.83	0.80	0.80	0.78	0.78
N	20,288	21,125	28,788	27,792	34,354	35,099
			B. Elite and non-elite exam offers			
Elite exam offer	0.088***	0.088***	0.091***	0.095***	0.079***	0.078***
	(0.017)	(0.017)	(0.015)	(0.015)	(0.014)	(0.014)
Non-elite exam offer	0.033***	0.032***	0.042***	0.040***	0.036***	0.037***
	(0.010)	(0.010)	(0.009)	(0.009)	(0.009)	(0.009)
Mean	0.82	0.82	0.80	0.80	0.77	0.76
N	22,080	22,483	30,655	29,835	35,620	36,088
			C. Elite and non-elite exam offers (balanced sample)			
Elite exam offer	0.034*	0.030	0.023	0.029*	0.017	0.018
	(0.020)	(0.020)	(0.018)	(0.018)	(0.017)	(0.017)
Non-elite exam offer	0.013	0.010	0.016	0.013	0.013	0.009
	(0.012)	(0.012)	(0.010)	(0.010)	(0.010)	(0.010)
Mean	0.84	0.84	0.83	0.83	0.79	0.79
N	15,503	15,804	21,438	20,871	24,827	25,110
			D. Noble lottery offers			
Noble lottery offer	0.018	0.019	0.022**	0.022**	0.023**	0.023**
	(0.012)	(0.012)	(0.011)	(0.011)	(0.011)	(0.011)
Mean	0.86	0.86	0.83	0.83	0.81	0.81
N	5,616	5,616	7,965	7,965	7,965	7,965

Notes: This table reports estimates of the effects of offer receipt on follow-up data availability for 2009-12 applicants. For the estimates in Panel A-C, controls are as used to compute the estimates in Table 2 and a 4th-degree polynomial in the running variable. For the estimates in Panel D, controls are as used to compute the estimates in Table A4. Robust standard errors are shown in parentheses; * significant at 10%; **significant at 5%; *** significant at 1%.

Table A2
Covariate balance for exam and noble network research designs.

	School-specific Exam Offers		Any Exam Offer			Noble Offer (Over-subscribed)	
				Offer effect			
	Local-linear controls p-value	4th-degree polynomial p-value	Mean	Local-linear controls	4th-degree polynomial	Mean	Offer effect
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Black	0.860	0.516	0.454	0.007*	0.009***	0.433	0.003
				(0.003)	(0.003)		(0.003)
Hispanic	0.996	0.841	0.419	0.007*	0.008***	0.505	0.004
				(0.003)	(0.003)		(0.003)
White	0.868	0.706	0.071	0.007*	0.000	0.021	0.002
				(0.003)	(0.003)		(0.003)
Asian	0.372	0.504	0.044	0.003	-0.001	0.031	0.003
				(0.003)	(0.003)		(0.003)
Female	0.026	0.020	0.575	0.014	0.024**	0.585	-0.012
				(0.014)	(0.012)		(0.017)
Free/reduced price lunch	0.833	0.522	0.831	0.008	0.030***	0.883	0.016
				(0.010)	(0.010)		(0.011)
Baseline math	0.919	0.039	0.609	0.006	0.025	0.249	0.012
				(0.017)	(0.016)		(0.022)
Baseline reading	0.514	0.016	0.578	-0.015	-0.014	0.270	-0.019
				(0.018)	(0.010)		(0.020)
N	25,627		23,968			6,703	

Notes: This table reports estimates of offer effects on covariates for 2009-12 applicants to exam schools (in columns 1-5) and Noble campuses (in columns 6-7). Columns 1-2 show p-values of F statistics for school-specific offers; columns 3-5 report exam applicant means and any-exam offer effects; columns 6-7 show statistics for Noble offer effects. Sample sizes reported in columns 1 and 3 show the number of exam applicants in the bandwidth for ACT math for which baseline scores are available. Estimates in column 6 likewise use Noble applicants with baseline scores. Controls are as used to compute the estimates in Table 2 for columns 2-5 and as used to compute the estimates in Table A3 for column 6. Robust standard errors are reported in parentheses; * significant at 10%; ** significant at 5%; *** significant at 1%.

Table A3
Covariate balance for exam research designs by school type.

	Elite Exam Offers		Non-elite Exam Offers	
	Local-linear controls p-value (1)	4th-degree polynomial p-value (2)	Local-linear controls p-value (3)	4th-degree polynomial p-value (4)
Black	0.864	0.374	0.624	0.493
Hispanic	0.892	0.375	0.982	0.993
White	0.684	0.626	0.656	0.435
Asian	0.630	0.781	0.185	0.295
Female	0.142	0.203	0.032	0.019
Free/reduced price lunch	0.548	0.659	0.845	0.483
Baseline math	0.826	0.107	0.765	0.097
Baseline reading	0.444	0.001	0.441	0.900
N	25,627		25,627	

Notes: This table reports estimates of offer effects on covariates for 2009-12 applicants to exam schools. Columns 1-2 show p-values of F statistics for school-specific offers of elite schools; and columns 3-4 for non-elite schools.

Table A4
Noble network lottery estimates.

	PLAN		ACT		College	
	Math (1)	Reading (2)	Math (3)	Reading (4)	Any college (5)	Four-year college (6)
			A. First Stage			
Noble offer	0.353***	0.352***	0.490***	0.490***	0.177***	0.177***
	(0.031)	(0.031)	(0.040)	(0.040)	(0.014)	(0.014)
			B. Reduced Form			
Noble offer	0.135***	0.062**	0.184***	0.074***	0.020**	0.040***
	(0.022)	(0.024)	(0.021)	(0.020)	(0.010)	(0.013)
Mean control	0.20	0.10	0.29	0.08	0.81	0.62
N	7,209	7,208	9,819	9,816	9,573	9,573
			C. 2SLS			
Noble impact	0.382***	0.177***	0.376***	0.151***	0.114**	0.225***
	(0.059)	(0.069)	(0.036)	(0.039)	(0.058)	(0.073)
F	129.0	128.6	151.9	151.9	171.5	171.5
N	7,209	7,208	9,819	9,816	9,573	9,573

Notes: This table reports estimates of Noble exposure effects on test scores and college outcomes for 2009-12 Noble applicants, including those who did not apply to an exam school. For test scores (columns 1-4), the endogenous variable is years of enrollment between application and test date. For college outcomes (columns 5-6), the endogenous variable is 9th-grade enrollment. Panel A shows first-stage estimates. Panel B shows reduced-form effects of an over-subscribed Noble offer. Panel C shows 2SLS estimates using an oversubscribed Noble offer as an instrument for Noble exposure, controlling for Noble risk sets and the baseline covariates listed in Table A2. The table also reports the first stage F statistic for over-identified models. Robust standard errors appear in parentheses; * significant at 10%; ** significant at 5%; *** significant at 1%.

Table A5
2SLS estimates of sector effects (balanced attrition sample).

	ACT				College enrollment			
	Math		Reading		Any college		Four-year college	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
			A. All exam applicants					
Exam impact	-0.022	0.010	0.001	0.011	-0.034*	-0.023	-0.086***	-0.055*
	(0.016)	(0.017)	(0.018)	(0.019)	(0.020)	(0.021)	(0.029)	(0.031)
Noble impact		0.332***		0.087		0.099		0.301***
		(0.052)		(0.058)		(0.062)		(0.093)
p-value (exam = Noble)		0.00		0.17		0.04		0.00
F (exam)	116.2	106.8	112.4	105.1	149.5	138.6	149.2	135.9
F (Noble)		11.4		11.5		15.7		13.8
N	18,131	18,131	17,663	17,663	20,332	20,332	20,534	20,534
			B. Exam and Noble applicants					
Exam impact	-0.254***	0.079	-0.010	0.141	0.004	0.066	-0.164**	0.017
	(0.054)	(0.084)	(0.058)	(0.100)	(0.052)	(0.085)	(0.079)	(0.132)
Noble impact		0.439***		0.181**		0.092		0.264**
		(0.079)		(0.089)		(0.077)		(0.115)
p-value (exam = Noble)		0.00		0.49		0.65		0.00
F (exam)	12.1	4.8	11.8	5.0	16.3	8.5	16.4	7.8
F (Noble)		4.0		4.3		7.7		7.0
N	2,453	2,453	2,413	2,413	2,841	2,841	2,849	2,849

Notes: This table reports 2SLS estimates of the effects of exam and Noble exposure for 2009-12 applicants analogous to those reported in Table 5, but restricting the sample to the 65% of applicants coming from middle schools with the highest follow-up rates. For ACT scores (columns 1-4), the endogenous variable is years of enrollment between application and test date. For college outcomes (columns 5-8), the endogenous variable is 9th-grade enrollment. The instrument list is a set of school-specific offer dummies. Running variable controls are 4th-degree polynomials. Other controls are as used to compute the estimates in Table 5. The table also reports Angrist and Pischke's first-stage F statistics for models with multiple endogenous variables and the p-value for sector-effect equality tests. Robust standard errors appear in parentheses; * significant at 10%; ** significant at 5%; *** significant at 1%.

Appendix B. Chicago data

This appendix describes the data and the procedures used for the analysis in Chicago. The administrative data sets used in this project are the following:

(i) Noble application files provided by the Noble Network.
(ii) Exam application files provided by Chicago Public Schools (CPS).

CPS is also the source for:

(i) Enrollment files with student demographics and school attendance information.
(ii) PLAN and ACT scores from the Prairie State Achievement Examination (PSAE).
(iii) National Student Clearinghouse (NSC) data on college attendance.

B.1. Data sources

B.1.1. Exam school application data

We use exam application files between 2009 and 2012. Eligible applicants enroll at an exam school in the fall of the following year of each application cycle. For example, 2009-applicants would enroll during the 2010–2011 school year. The exam school application files contain a record for each applicant with an application ID and CPS ID numbers, students' demographics such as gender, race, special education status, address and tier, preferences over exam schools, and the composite score for admission. Each record also includes the school where the student receives an offer (if any). The RDD in the paper uses cutoffs for this period published on the CPS website: http://cps.edu/AccessAndEnrollment/Documents/SEHS_CutoffScores.pdf. We exclude duplicate observations and applicants who were missing the CPS ID number from the analysis.[24]

B.1.2. Noble network application data

We use over-subscribed lotteries to identify Noble effects. Table B1 lists the Noble schools operating for the 2009–2012 application cycles with oversubscribed lotteries. The application files to the Noble Network have a record for each applicant with the year of application, the CPS ID number, demographics such as gender and race, and the set of Noble schools the student applies to. Each record also has the lottery number for oversubscribed schools, whether the applicant receives a Noble offer and where, and whether it is a lottery offer or a sibling offer. We have Noble files for applicants between 1998 and 2015, but our analysis only includes 2009–2012 applicants.

B.1.3. CPS enrollment data

The CPS enrollment file spans school years 2007–2008 through 2015–2016. We use this file to define years of enrollment at an exam or a Noble school. Each record contains a snapshot at the start of the school year (October) for each student enrolled in Chicago Public Schools. The data has a unique student identifier (the CPS ID), the student's grade and school, and demographic information. We transform the enrollment file into a wide-format layout. We also identify sending schools using the school where the student enrolled in the year of application to exam or Noble.

[24] The proportion of applicants missing the CPS ID was 11.4% in 2009, 10.9% in 2010, 11.2% in 2011, and 0.0% in 2012.

B.1.4. Test scores

The PLAN test score file spans the 2010–2014 school years. The file includes scores for four subjects: English, Math, Reading, and Science, the grade and the year in which the test was taken (primarily 10th grade), and the student's school. We standardize scores among CPS test-takers by year.

The ACT scores come from the Prairie State Achievement Examination (PSAE) files from 2010 to 2016. PSAE is a two-day standardized test taken by all High School Juniors in the State of Illinois through 2014. Until 2014, on the first day, students took the ACT and were evaluated in four subjects: Math, Reading, Science, and Writing. In 2015 and 2016, the test was only the first day of the ACT. The files have the grade and the year of the test. Scores are standardized among all CPS test-takers by year.

B.1.5. NSC data

For college enrollment outcomes, CPS provided us with data from the National Student Clearinghouse (NSC). Starting in 2014, the CPS submits a list of students to NSC yearly to obtain their post-secondary enrollment information, which is updated multiple times a year. We use CPS NSC search results from 2014 to 2018. These files include the student's CPS ID, the institution(s) attended by the student, whether the institution attended is 2-year or 4-year, the start and end dates of each enrollment period, and the status of enrollment (full-time, half-time). We compile all of the search results and remove duplicate observations based on the year of the enrollment start date and enrollment length. For our college enrollment outcomes, we create dummy variables for whether a student ever attended a 2-year, 4-year, or any type of post-secondary institution. (see Table B1).

B.2. Merging files

We merge the data using the enrollment file as the master file. We use the CPS ID to combine this data with the application files for each school sector. Here, we restrict our sample to applicants to either an exam or a Noble school.

We then merge this data with our set of outcomes: PLAN, ACT scores, and college attendance. Table A1 shows merge (follow-up) rates for each outcome, along with estimates of differential attrition around the qualifying cutoff. t. We calculate years of exposure at each sector (exam or Noble) using the number of years enrolled in an exam or a Noble school between the year of application and the year in which the student took the test. For college attendance, we use enrollment at an exam or a Noble school at any point in our study period.

Appendix C. Data anonymous large urban district

This appendix describes the data and the procedures used for the analysis in the second large urban district. The Public Schools Office is the source of the application files for exam schools. The State provided us with the enrollment files and statewide assessment data.

C.1. Exam school application file

The exam school application file contains a record for each student with an application ID and student ID numbers, demographics such as gender, race and date of birth, the year and grade of application, preferences over exam schools, and the admissions composite score. Each record also includes the school where the student receives an offer (if any). This data set covers students with application years from 1995–2017. The analysis sample includes applicants from 2004–2016 for which both baseline and outcome

Table B1
Noble network school lotteries.

School	Year of application	Number of applicants	Total offers	Oversubscribed School Offers	
				Sibling offers	Lottery offers
	(1)	(2)	(3)	(4)	(5)
Noble Street College Prep	2009	840	372	41	331
	2010	831	385	41	344
	2011	798	388	42	346
	2012	809	408	58	350
Pritzker College Prep	2010	571	399	66	333
	2011	500	332	71	261
Gary Comer College Prep	2010	443	285	24	261
UIC College Prep	2009	1,128	314	37	277
	2010	1,242	334	44	290
	2011	1,443	398	67	331
	2012	1,482	433	62	371
Chicago Bulls College Prep	2011	1,001	752	48	704
Muchin College Prep	2010	793	291	41	250
	2011	793	355	55	300
	2012	918	391	67	324

Notes: This table shows the list of Noble Network campuses that run an oversubscribed lottery. Column 1 displays the name of the school. Column 2 shows application year; column 3 shows the total number of applicants per year; column 4 shows the total number of offers to enroll in a particular school. Column 5 shows the total number of offers due to sibling priority and column 6 shows the number of offers generated by lotteries. The sum of the number of observations in columns 5 and 6 equals the number of observations in columns 4.

Table C1
Merging application, enrollment, and outcomes files.

Year	Missing ID	Match Enrollment	Match Outcomes
	(1)	(2)	(3)
2004	10.9%	82.3%	92.4%
2005	10.4%	82.8%	93.7%
2006	7.2%	84.1%	95.8%
2007	8.2%	83.2%	95.3%
2008	5.4%	82.7%	94.3%
2009	5.6%	83.1%	95.0%
2010	4.8%	84.2%	95.1%
2011	4.9%	83.9%	95.9%
2012	3.7%	84.9%	96.0%
2013	3.3%	85.2%	96.2%
2014	3.1%	85.1%	94.1%
2015	5.0%	85.2%	94.1%
2016	5.3%	85.0%	95.7%
2017	3.3%	88.8%	93.5%

Notes: This table reports the matching rates between application, enrollment, and outcome files for the large urban district. The first column shows the percentage of applicants in the application file missing the State identifier. Columns 2 and 3 show the percentage of students merged with the enrollment and outcomes files.

test scores are available. Students enroll in the fall of the year of application. Duplicate observations and applicants missing the application ID number are excluded from the analysis.

C.2. Enrollment file

The State enrollment file spans school years 2001–02 through 2017–18. Each record contains a snapshot at the start and end of the school year for each student enrolled in public schools. The variables include a unique student ID, the student's grade, and school, and demographic information such as sex, race, low-income, and special education status (SPED), status. We use the end of the year variables to define the student's school before the application. The start of the enrollment file determines the school of the student after the application. We compute the grade and exam school years attended for a given year.

C.3. Outcome files: statewide assessment test

The test score file spans school years from 2002 to 2018. It includes scores for English and math, the grade, and the year of the test. We standardize scores among test-takers by year and grade.

C.4. Merging files

We merge these three data sets with the State identifier. Students who could not be found in the State file for any year before applying to exam schools were dropped from the analysis. Column 1 of Table C1 shows for each year the proportion of applicants who are missing this ID. The master file is the application data to exam schools. We join this file with:

- The enrollment file of high school students from 2004 to 2017. Column 2 of Table C1 shows the matching rate between the application and the enrollment files by year. On average, approximately 85% of the applicants are in the enrollment file.
- The outcome file covers 2002 to 2018. Column 3 of Table C1 shows the matching rate between the application and the outcome files by year. On average, we have test scores for approximately 94% of the exam applicants.

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