Parents Know Better: Sorting on Match Effects in Primary School*

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Abstract

I study if parents select schools based on their impact on student learning over and beyond average school value-added. I compare the achievement of primary school students in London who are as good as randomly assigned to preferred institutions because school offers are centrally assigned by a deferred acceptance mechanism. I find that parents sort into schools based on unobservable attributes of the student-school match that effectively improve learning outcomes. Among students enroled at the same school, causal effects on achievement are larger if that school was ranked higher in the list of parental preferences. Results imply that parental choice may improve the efficiency of the allocation of students to schools.

JEL Codes: H75, I21, I24, I28

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1 Introduction

The benefits of expanding parental choice among public-sector schools on educational productivity are strongly debated.[1] Advocates of school choice argue that demand-side pressure incentivises schools to improve their quality (Hoxby, 2003). For this to happen, parents need to reward schools based on their causal impact on student achievement (Rothstein, 2006). However, empirical evidence suggests parents mostly respond to indicators driven by neighbourhood composition, such as test scores, reflecting peer rather than school quality (MacLeod and Urquiola, 2019).[2] By tying enrolment to intake composition rather than productivity, peer preferences weaken schools' incentives to improve (Barseghyan et al., 2019).

Parental choice may also improve school productivity if sorting across schools depends on the specific educational needs of children. In presence of complementarities between student and school inputs, parents may improve the quality of student-school match, with positive effects on learning. Sorting of workers into firms based on production complementarities has been documented in the labour market (Lamadon et al., 2021). In the context of school choice, parents' sorting would imply that they select schools based on match effect, i.e., the impact of attending the school of choice on student achievement beyond that school's value-added (VA, the average school causal impact across students). In previous studies, these two effects are not disentangled. For example, Deming et al. (2014) find positive effects of attending the first-choice school on postsecondary education only for applicants with larger gains in school VA, and conclude that there is no benefit to school choice beyond access to generally higher-quality schools. Abdulkadiroglu et al. (2020) estimate match effects in New York high schools and relate them to parental preferences in a second step.

I study whether parents select primary schools based on match effects by testing if these effects vary with the preference rank assigned to a school. Exploiting administrative records on centralised assignment to primary schools in London, I isolate quasi-experimental variation in admission to the most-preferred institutions. I leverage data on previous cohorts of students to estimate school VA, and investigate whether attending the school of choice

[1] Beyond England, studied in this paper, choice among public-sector schools is allowed in many of the largest U.S. districts (Whitehurst, 2017) and urban districts such as Amsterdam (De Haan et al., 2021), Barcelona (Calsamiglia and Guell, 2018), Paris (Fack et al., 2019), and Beijing (He, 2017).

[2] One potential explanation is that school effectiveness on short-run test scores weakly correlates with impacts on longer-run outcomes (Beuermann and Jackson, 2020). In the UK, Gibbons et al. (2013) find that school VA is capitalised in house prices as well as peer quality.

impacts student learning trajectories over and beyond the gain in school VA.

The ideal experiment would compare the learning outcomes of students randomly enrolled in otherwise identical schools in terms of VA, except for the preference rank assigned by parents. I take this idea to the data by leveraging the deferred acceptance mechanism (DA; Gale and Shapley, 1962) that matches students with school seats based on parental preferences and admission priorities. Seats in about 70% of London schools are rationed. In case of excess demand, distance to the school is used as tie-breaker between applicants with equal priority, generating catchment boundaries that vary every year depending on residence and preferences of all applicants. The uncertainty about the exact value of school catchments introduces variability in admissions of students located close to the catchment boundary.[3]

School offers are as good as randomly assigned conditional on preferences, priorities, and distance, as these inputs exhaust the possible sources of selection bias. DA maps inputs into a scalar representing the probability of receiving an offer, which I refer to as *assignment risk* (Abdulkadiroglu et al., 2021). Same-risk applicants must have different offers because school seats are rationed, generating exogenous variation in assignment. Consistently with this expectation, I show that applicants with offer from the most-preferred schools are statistically undistinguishable from applicants with the same assignment risk not receiving an offer from these schools. I use school offers to instrument enrolment conditional on assignment risk.

Comparing same-risk students by offer status conflates the match effect of most-preferred schools and the gain in school VA. I control for school VA by estimating this quantity using administrative records on previous cohorts of students at the same school. I find that enroling at the most-preferred schools increases student learning over and beyond school VA. The causal effect of enroling at the first choice relative to a school ranked lower than second on Year 2 achievement in mathematics exceeds school VA by about 0.1 standard deviations (hereafter, σ). Similar results are found for the second-choice school.

Two further sets of results support the interpretation of my findings as match effects. First, VA gains from enroling at most-preferred schools strongly predict causal effects on learning, in line with Angrist et al. (2016, 2017, 2021). The relative magnitude of match effects and of the impacts of VA gains suggests that my results are hardly driven by residual

[3]Others have considered distance-based eligibility for policy interventions (Masi, 2018) or school admission (Gorman and Walker, 2021) to study school choice and its impacts. These studies, however, have not exploited the quasi-experimental variation arising from centralised assignment.

differences in school causal effects not captured by VA estimates. Second, to hold any school input constant, I compare students assigned to the same school who ranked that school with different preferences. I find similar results to those obtained with my main research design.

Sorting on match effects is more pronounced among parents with relatively advantaged background. I find larger effects among students not eligible for subsidised lunch, living in local areas with below-median deprivation index, or with above-median achievement at school entrance. These results suggest that, even conditional on residential sorting, parents with larger resources are in the position to make the most of school choice.

I conclude that my results are likely driven by unobservable school attributes specific to the student-school match. Parental rankings exhibit remarkable variability within the same school and narrow distance cells. Regardless of socioeconomic status, parents rank schools by proximity to the place of residence and by peer quality, on average. Access to high-preforming schools is spatially segregated, with better-off parents nearly maximising both proximity and peer quality while disadvantaged parents face steeper trade-offs. The causal effects on proximity to school and on peer quality from enroling at most-preferred schools cannot fully explain the match effects I find. In addition, I show that ability rank in school-cohort (Murphy and Weinhardt, 2020) or the share of peers from the same local area enroled at the school do not fully replicate the pattern of match effects.[4]

My results provide unique evidence of parental sorting based on school match effects, implying that school choice may increase allocative efficiency.[5] Moreover, my findings imply that school effects are heterogeneous across students, questioning accountability systems based on VA models that implicitly assume homogeneous school impacts. Finally, heterogeneous effects by socioeconomic status suggest that interventions aimed at enhancing choice quality of disadvantaged parents may reduce inequality in educational outcomes.

A broad literature has investigated the impacts of attending the schools that parents prefer (e.g., Jackson, 2010; Pop-Eleches and Urquiola, 2013; Abdulkadiroglu et al., 2014; Hoekstra et al., 2018). In a meta-analysis, Beuermann and Jackson (2020) find a small

[4]Behavioural responses of parents and students are another potential mechanism. However, Pop-Eleches and Urquiola (2013) find reduced parental investment and worse peer interaction just above the school admission cut-off. Such responses would, if anything, counteract the match effects I find.

[5]Kirkeboen et al. (2016) document positive returns to sorting into the field of study in higher education. In contrast, Kline and Walters (2016), Cornelissen et al. (2018), Walters (2018), and Abdulkadiroglu et al. (2020) find no or negative selection on gains in pre-school programmes and high schools.

and statistically insignificant effect on student achievement. In contrast to these works, I isolate parental choice effect from returns driven by average school quality. Methodologically, these studies do not fully exploit school offer variation induced by centralised assignment. Moreover, this is the first study on parental choice effects in primary schools. Since early stages are crucial for student development (Chetty et al., 2011; Heckman et al., 2013), this fills an important gap in the literature.

Finally, many previous works leverage data on submitted rankings to investigate parental preference for school attributes (e.g., Hastings et al., 2009; Burgess et al., 2015; Glazerman and Dotter, 2017; Burgess et al., 2019; Ainsworth et al., 2020). I describe parental preferences accounting for the set of feasible schools, addressing recent concerns on truthfulness of reported rankings under DA (Fack et al., 2019).[6] Departing from available studies, I document substantial heterogeneity in parental rankings of the same school, and consistency of reported preferences with choice behaviour over time.

2 Institutional context

Primary education and school choice in London

Primary education in England spans seven grades, from age 5 to 11, and is organised in three phases. Students start with a reception year, which concludes the Early Year Foundation Stage (EYFS). During reception, students are assessed against several learning goals to inform teachers and parents on their readiness for Year 1. The second phase is Key Stage 1 (KS1), spanning two years. Teacher assessments in mathematics, science, and English are administered at the end of KS1. The final phase is Key Stage 2 (KS2), at the end of which students sit externally-marked standardised exams in mathematics and English. For all phases, the National Curriculum sets core knowledge and achievement objectives.

Parental choice among state-funded schools is established in England. Public-sector schools are the main provider of primary education, with less than 5% of students opting for private institutions.[7] Since the 1980s, the open enrolment policy guarantees parents the right of choosing a school for their children, as long as demand does not exceed capacity. Parents

[6] A similar idea is implemented in Burgess et al. (2015). School feasibility here is more precisely measured since it is based on school-specific boundaries obtained by replicating the centralised assignment.

[7] Author's own calculation from official 2019 data on students count by school phase and sector (link).

are required to rank up to six schools at application. Local Authorities (LAs), the English school districts, assign a seat to all students at the highest-preference school available. School funding mostly depends on enrolment count, providing incentives to attract parental demand and fill capacity.[8]

Dissemination of data on school performance sparks competition for seats at high-performing schools. School Performance Tables, published annually since 1996, collect information on academic performance (both absolute achievement and achievement growth) and on intake composition of each state-funded school. Institutions with excellent test scores are typically sought-after by parents and they easily become oversubscribed (Burgess et al., 2015).

Admission criteria to oversubscribed schools have had important impact on gentrification and urban development. When demand exceeds capacity, applicants are mostly admitted in order of proximity, generating fierce competition in the housing market to secure residence close to preferred institutions. Quality of surrounding schools is often mentioned in real estate advertising, and its impact on housing prices has been extensively documented by the economic literature (Machin, 2011; Gibbons et al., 2013; Battistin and Neri, 2017). The exact location of catchment boundaries, however, varies every year according to supply of and demand for school seats.

London is an ideal context to study school choice, with a dense supply of schools and high competition for popular institutions. Primary schools are typically small, enrolling about 50 students per cohort. Absolute achievement at KS2 outperforms the national average, driven by a dense right tail of institutions serving exceptionally performing students (almost 20% of London primary schools fall in the top decile nationally). The fraction of students missing out on their top choices is systematically the highest around the country (see Table I.1).

School assignment

Assignment to school is centrally regulated by the School Admissions Code. Applicants are admitted to their first choice as long as demand does not exceed capacity. Admission authorities must adopt and publish criteria to prioritize school applicants in case of oversubscription. National regulation leaves little discretion in setting priorities, explicitly banning a number of criteria such as selection by academic ability or interviews with parents and children. Few

[8] Primary schools have a statutory class size cap of 30 students.

specific categories of students are prioritised and, within priority groups, distance to school is used as tie-breaker. Schools are required to admit children with exceptionally disadvantaged backgrounds, constituting a very low share of students.[9] Moreover, applicants with siblings currently enroled at the school are prioritised. Finally, exceptional admission criteria are permitted to religious schools, which typically set requirements based on faith.

School districts across England make offers through a deferred acceptance mechanism (DA, Gale and Shapley, 1962), matching students to the highest-preference school with available seats. Since 2007, DA is adopted nationwide for centralised school assignment after the previously popular "Boston" mechanism was banned.[10] DA algorithms have been proven less vulnerable to strategic preference reporting (Pathak and Sonmez, 2013). As long as parents act rationally, their rankings reveal the relative preference among listed institutions (i.e., the first choice is preferred to the second choice, the second to the third, and so on, while no conclusion can be drawn on non-ranked schools; see Fack et al., 2019).

Preferences, priorities, and school capacities are mapped into offers through the student-proposing DA algorithm. Each student is initially considered by the most-preferred school. Applicants are ranked by priority and tie-breaker value and provisionally admitted up to capacity. In subsequent rounds, students who are rejected apply to the next-best school and are ranked jointly with applicants provisionally admitted up to this point. Schools retain applicants up to capacity and rejects the rest, who in turn apply to the next-best school. The algorithm stops when no rejection takes place. Some applicants may be left unassigned (3.2% in my sample, who are offered a non-ranked school with spare capacity).

All parents in the country receive a single school offer in mid-April, deemed National Offer Day. Unsatisfied parents can join the waiting list at preferred schools with the same priority enjoyed in the centralised assignment. Although parents have the right to appeal in case of irregularities, the admission outcome is rarely overturned.[11]

[9] Highest priority is given to children looked after by the LA, about the 0.5% of under 18-year old in London in 2019 (official statistics here). In addition, priority is granted to children with a statement of special education needs (0.8% in my sample). The two groups are not mutually exclusive.

[10] This mechanism prioritises applicants ranking the school first, incentivising parents to nominate a "safe" first choice. Terrier et al. (2021) investigate the effects of the ban of the Boston mechanism on school choice outcomes.

[11] Among the 688 London primary schools with appeal data in 2015 (about 40% of the total), the 95% recorded no appeal resolved in parents' favour.

3 Data

I exploit administrative data on applicants to state-funded primary schools in 2014 and 2015. Records include rank-order lists of schools submitted by parents to LAs and the school offered to each applicant as a result of the assignment mechanism. Application data are matched to the National Pupil Database (NPD), including achievement records and socioeconomic characteristics of the universe of students in primary education. I observe the postcode of residence, a granular information on residential location spanning an average of 15 properties and often corresponding to one single building in London. I compute the linear distance from each applicant's postcode to all primary schools around.[12]

Assessments at the end of KS1 (Year 2) are the outcomes considered in my empirical investigations. Students are assessed by teachers at age 7, after three years of primary school. Results are grouped in three categories, depending on student achieving below, at, or above the expected standard. Three different subjects are assessed – English, separately for reading and writing, and mathematics.[13] Although teacher assessments are not standardised, detailed guidance is issued annually by the Government and external moderation is statutory, with LAs required to moderate a sample of at least 25% of schools (Department For Education, 2017). Students sit national tests in mathematics and reading and the end of KS1, with an optional writing test, which scores are not disclosed but are meant to inform teacher assessments. Burgess and Greaves (2013) find that almost 80% of students are awarded the same achievement level in teacher assessments and standardised tests at the end of primary school (KS2), suggesting that teacher judgement is in line with externally-marked exams.[14]

To control for academic ability at entrance, I consider Early Years Foundation Stage Profile (EYFSP) teacher assessments. EYFSP assess 17 learning goals and are administered during the reception year. Similarly to KS1 assessments, EYFSP results are grouped in three categories indicating that students achieve below, at, or above expected standards. Moreover,

[12]Centroid coordinates for English postcodes are obtained from www.doogal.co.uk. Applicants with missing postcode (about 3%) are imputed distance by exploiting the information on schools ranked by parents. I assign them the median distance among applicants ranking the same school with the same preference.

[13]The outcome variable I consider weights teacher assessments mirroring the scheme used by national authorities to compute average point scores for school accountability (9, 15, and 21 points for scoring below, at, or above the expected standards, respectively).

[14]Burgess and Greaves (2013) find that non-white students are more likely to receive teacher assessments below their standardised test score level. I control for ethnicity dummies in my analysis and these do not affect the results.

I observe individual characteristics of students including gender, free lunch eligibility, special education needs, language, and ethnicity group. At the local area (LSOA) level, the income deprivation index (IDACI) measures the proportion of children in families that are considered deprived based on household income.

Sample description

I consider students who ranked at least one London primary school at application. The sample includes 199,220 applicants and 638,756 student-preference observations, with the average applicant ranking between 3 and 4 schools (Table I.1). Primary schools in London serve a mixed population, with 42% of students from white origin and 42% not speaking English at home (compared to 78% and 12% in the rest of England). Students in London are more likely eligible for free school meal than in the rest of the country. Despite the difficult context, primary schools in the capital outperform the average national institution in terms of academic achievement at Year 2, and the gap is wider than at primary school entrance.

Parents in London face striking competition for school seats. 17% of students miss out on the two most-preferred institutions, 7 percentage points (p.p.) higher than in the rest of the country. Possibly anticipating higher competition, parents in London are 15 p.p. more likely to rank three schools or more at application.[15] The 87% of parents comply with centralised school offer, 3 p.p. lower than the national average, possibly reflecting higher propensity to opt for private schools (4% enrol at private schools against 2% in the rest of England).[16]

Replication of centralised school assignment

Centralised assignment breaking ties by distance implies that, when a school is oversubscribed, applicants located further than a specific distance threshold are not admitted. This threshold, however, is not directly observed in the data since school offer also depends on parental preference and admission priorities (see Section 2). Panel A of Figure I.1 shows that, although the probability of admission markedly decreases further away from school, offer rates are not a deterministic function of distance. First, parents may rank the school differently conditional on distance, explaining why only 70% of applicants in the bottom

[15] In most English districts application form is restricted to three schools.

[16] I consider a student as enrolled at private school if not tracked into any state-funded school after application.

decile receive an offer. Consistently, 35% of parents located closest to a school rank the school less than first choice (Panel B). Second, particular categories of applicants are admitted with priority regardless of their location, partly explaining the non-negligible offer rate in the top decile of distance to school (about 0.2).

I replicate the assignment mechanism to trace catchment boundaries and identify applicants at the margin for admission. The school catchment boundary is defined as distance of the last-admitted applicant. Replication is complicated by data availability since I have no information on demographics determining admission priorities – most importantly, whether a student has siblings at the school. Students with priority, however, are partially detectable in the data. First, I construct a proxy for siblings at the school based on postcode of residence and family-specific observable characteristics, and use this proxy to replicate school assignment. Second, I adjust offer replication to residual unobserved priorities.[17] Intuitively, if an applicant with offer resides beyond the initially estimated distance threshold, she must have priority. I discuss in Appendix A how I achieve replication of school offer based on these ideas.[18]

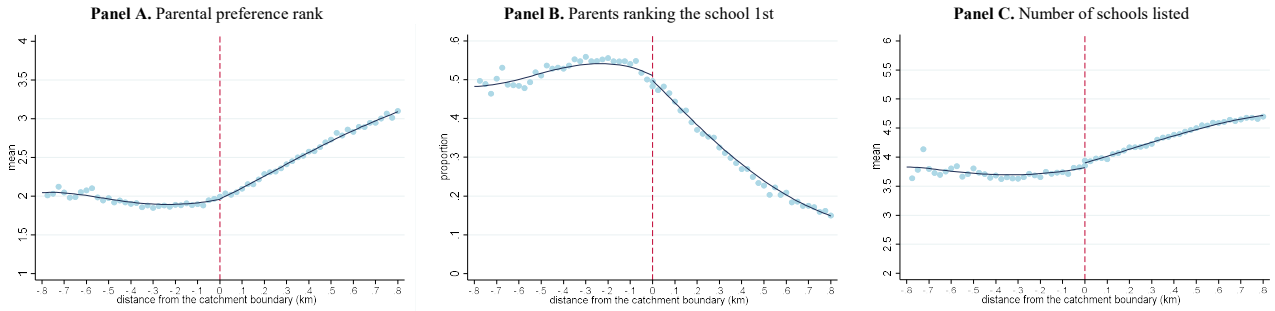
My analysis rests on the assumption that the residual measurement error in admission priorities is not correlated with potential outcomes. Although school offer is almost perfectly replicated, some students with admission priority may remain undetected. This would constitute a concern for my empirical strategy if students with unobserved priority were disproportionally located on either side of the catchment boundary. This is unlikely, however, since catchment boundaries are most likely unpredictable by parents as I document below. Moreover, the validity of my design is supported by statistical balance in a number of observable characteristics associated with potential outcomes, including lagged achievement (see Section 4).

The distribution of parental rankings around the catchment boundary suggests that applicants are not able to exactly anticipate the admission cut-off. Figure 1 shows parental

[17]These mostly include religious requirements imposed by faith schools. I consider faith schools in the algorithm replication but not in estimation as the measurement error in catchment boundary is likely more serious.

[18]I replicate centralised assignment by running DA for all schools at the same time. In practice, the matching algorithm is run at the LA level and subsequently iterated up to 20 times to eliminate double offers across London LAs (Carter et al., 2020). The two procedures result in the same admission outcomes as long as 20 iterations are sufficient to sort all double admissions. In my algorithm this number of iterations is always sufficient to eliminate double offers. Consistently, the school offered to each applicant is just one.

Figure 1: Parental preference around the catchment boundary



Note. The figure plots parental rank (Panel A), the share of parents ranking the school first (Panel B), and the number of school listed (Panel C) around the catchment boundary. Preference for the school varies from 1 to 6 indicating first and sixth choice, respectively. Distance to school catchment boundary on the horizontal axis is defined by subtracting distance of the last admitted candidate to an applicant's distance to school. Negative values indicate residence within catchment. Markers represent average values in 25-meters-wide bins of distance from the boundary and the solid line is a local linear fit of underlying observations, estimated separately on either side of the cut-off. Catchment boundary is defined for oversubscribed schools not admitting by faith. The sample is restricted to applicants within 800 meters from the catchment boundary and excludes last admitted applicants, whch are used to define school catchment. Reported are average values across the two cohorts considered. See Section 3 for details.

preference for the school (Panel A, a value of 1 denotes first choice) and the share of parents ranking the school first (Panel B) are continuous around the catchment boundary. As one could expect, the figure displays decreasing parental preference with distance to school (see Section 6). The decrease accelerates, on average, starting at a slightly shorter distance than the catchment boundary, possibly suggesting that parents adjust their application behaviour based on their expectation about the cut-off realisation. However, the graph shows no discontinuity, suggesting that the exact location of the catchment boundary is, as expected, unpredictable by parents.[19]

4 Empirical framework

Parameter of interest

To fix ideas, I maintain the assumption that school offers are randomised and that compliance with these offers is perfect. I discuss below how these assumptions can be relaxed to fit the empirical context considered in this paper.

Following Abdulkadiroglu et al. (2020), student i's potential achievement at school s can be decomposed as:

$$Y_{is} = \nu_i + \alpha_s + \mu_{is}, \tag{1}$$

[19] Appendix Figure A.4, comparing catchment boundaries across the two years considered, further reinforces this expectation. Although catchment boundaries are positively correlated over time, the figure shows idiosyncratic variation that is unlikely to be anticipated by parents.

where ν_i is student ability, α_s is the school average causal effect (value-added, VA), and μ_{is} is the match effect for student i at school s. In a model where parents sort on their children's comparative advantage in the production of Y_{is} (Roy, 1951), μ_{is} is expected to increase with preference rank assigned by parents to the school.

Let r be the preference rank assigned to school s, with $r = \{1, \ldots, 6, \infty\}$ and $r = \infty$ denoting schools not ranked by the student. The school ranked r-th by student i is indexed by $s(i, r) = \{1, \ldots, S\}$. If students can receive offers only from listed institutions and Z_i denotes the preference rank for the school making an offer to student i, using equation (1) the observed outcome is:

$$Y_i = \sum_{z=1}^{6} \mathbb{1}(Z_i = z) \cdot Y_{is(i,z)} = \sum_{z=1}^{6} \mathbb{1}(Z_i = z) \cdot (\nu_i + \alpha_{s(i,z)} + \mu_{is(i,z)}). \tag{2}$$

Consider, for example, the comparison of students with offer from their first or second choice. Using equation (2), one can write:

$$E[\mu_{s(i,1)} - \mu_{s(i,2)}] = E[Y_i - \alpha_{s(i,1)} | Z_i = 1] - E[Y_i - \alpha_{s(i,2)} | Z_i = 2], \tag{3}$$

because offers are randomised. The last equation identifies the average match effect from attending the first choice relative to the second choice. The same reasoning extends to offers from schools ranked differently. The empirical analog of the right-hand side of equation (3) requires knowledge of α_s. Following Deming et al. (2014), I estimate this term by constructing the average regression-adjusted test scores growth at school s (see Appendix B for details). By using the estimated value of α_s in equation (3), I rely on the assumption that this estimate is (close to) unbiased on average. The empirical argument supporting this assumption is made in Angrist et al. (2016, 2017, 2021).

Offers from centralised assignment

School offers in London are not randomised. Under DA, school assignment depends on preferences submitted at application, admission priorities, and distance to ranked schools. Simple comparison of students by offer status is likely biased as parents may choose residence and rank schools depending on the potential outcomes of their children. At the same time, the variables considered in admission are the only potential sources of self-selection of parents into the desired schools. As long as selection from these sources is controlled for, centralised

school offers are independent from potential outcomes.

Conditional on assignment risk at school s, applicants are as good as randomly admitted to school s (Abdulkadiroglu et al., 2021). Specifically, for students with the same risk, the only variation in school offers derives from the realisation of unpredictable admission cut-offs:

$$Z_i \perp Y_{is} \,|\, \boldsymbol{p_i}, \tag{4}$$

where the vector $\boldsymbol{p_i} \equiv [p_{i1}, \ldots, p_{iS}]'$ collects assignment risk at all schools, regardless of the preference rank. Because of (4), the parameter in equation (3) is identified by a weighted average of conditional versions of the quantities discussed above:

$$E[\mu_{is(i,1)} - \mu_{is(i,2)}] = \int \Big(E[Y_i - \alpha_{s(i,1)} | Z_i = 1, \boldsymbol{p_i}] - E[Y_i - \alpha_{s(i,2)} | Z_i = 2, \boldsymbol{p_i}] \Big) \, d\boldsymbol{p_i}. \tag{5}$$

The conditional independence in (4) follows from the tie-breaking embedded in centralised assignment. In the event of oversubscription, the assignment mechanism discriminates between applicants with equal preference and priorities by using distance to school. Tie-breaking generates year-specific catchment boundaries for each oversubscribed institution depending on location, priorities and preferences of all applicants. Unless catchment boundaries are exactly anticipated by parents, centralised assignment generates uncertainty in admission outcomes conditional on the inputs considered by the matching mechanism.

The risks entering the left-hand-side term of equation (5) represent the probability of receiving an offer from the school ranked r-th conditional on assignment inputs:

$$p_{is(i,r)} \equiv P(Z_i = r | \boldsymbol{s_i}, \boldsymbol{\rho_i}, \boldsymbol{d_i}),$$

where the vector $\boldsymbol{s_i} = [s_{(i,1)}, \ldots, s_{(i,6)}]'$ collects the schools ranked by student i, and the vectors $\boldsymbol{\rho_i} = [\rho_{is(i,1)}, \ldots, \rho_{is(i,6)}]'$ and $\boldsymbol{d_i} = [d_{is(i,1)}, \ldots, d_{is(i,6)}]'$ denote student i's admission priorities and distance to ranked schools, respectively.[20] The probability $p_{is(i,r)}$ has two main building blocks. First, consider applicants with the same priority residing near the catchment boundary of school s who ranked school s at the top of their list. Intuitively, they face the same assignment risk, approximately 50% (Proposition 2 in Abdulkadiroglu et al., 2021). Second, the assignment risk of applicants ranking school s below the first choice depends on

[20] Assignment risk is scalar and coarsely distributed across applicants, addressing the empirical challenge of conditioning on the full set of assignment inputs. Preferences and priorities are too finely distributed for non-parametric type conditioning to be feasible.

the probability of admission at schools ranked higher than school s. Estimation of assignment risk in my context is detailed in Appendix C.[21]

Imperfect compliance

I address non-compliance by using school offers as instrumental variables conditional on assignment risk. Identification through IV requires additional assumptions on parental choice behaviour. First, offers need to exert a strong impact on enrolment, a condition verified by first stage results in Section 5 below.

Second, I need to assume that receiving an offer from a marginally preferred school monotonically increases the preference rank for the school where students enrol. As formalised in Appendix D, this implies that, for example, students with an offer from their second choice enrol either in their first or in their second choice. Evidence in support of such monotonicity is provided in Section 5. In particular, Panel B of Figure 6 shows that just 1% of students enrol at a school ranked with lower preference than the one offered.[22]

Under these assumptions, I identify a local average treatment effect (LATE) of attending the first-choice school. Consider the comparison between students enroled at the first relative to a lower-ranked school, where D_{i1} and Z_{i1} indicate enrolment and offer at the first choice, respectively, and let $\tilde{Y}_i \equiv Y_i - \hat{\alpha}_{s(i)}$ be the VA-adjusted achievement of student i enroled at school s. If C_i indicates school offer compliers, using Z_{i1} to instrument D_{i1} yields:

$$E[\mu_{is(i,1)} - \mu_{is(i,2)}|C_i = 1, \boldsymbol{p_i}] = \frac{E[\tilde{Y}_i|Z_{i1} = 1, \boldsymbol{p_i}] - E[\tilde{Y}_i|Z_{i1} = 0, \boldsymbol{p_i}]}{E[D_{i1}|Z_{i1} = 1, \boldsymbol{p_i}] - E[D_{i1}|Z_{i1} = 0, \boldsymbol{p_i}]} \quad , \tag{6}$$

where I assume for simplicity that students missing out on the first choice are offered the second choice.[23] I show in Appendix D that the average match effect among compliers can be obtained as weighted average of the comparison in equation (6).

[21] As expected, estimated assignment risk closely matches school offer. An application-level regression of the offer dummy on assignment risk estimates a slope coefficient of 0.995 ($\mathbb{R}^2 = 0.87$).

[22] I consider student enrolment in the reception year. The interpretation of results with school mobility between reception and Year 2 (when achievement is observed) is discussed in Appendix H.

[23] Otherwise, equation (6) identifies a similar LATE where the counterfactual is a weighted average of potential outcomes at schools ranked second or lower (see equation 14 in Appendix D). Similar comparisons can be defined for schools ranked lower than first choice.

Table 1: Balance tests

	All applicants			Applicants with risk					
				"preference" specification			"score" specification		
	1st choice offer	2nd choice offer	Joint significance (p-value)	1st choice offer	2nd choice offer	Joint significance (p-value)	1st choice offer	2nd choice offer	Joint significance (p-value)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Free school meal eligible	-0.0367***	-0.0056	105.77	-0.0163	-0.0116	1.19	-0.0111	0.0004	0.74
	(0.0031)	(0.0039)	(0.0000)	(0.0114)	(0.0107)	(0.3053)	(0.0101)	(0.0089)	(0.4767)
Female	0.0128***	0.0062	4.18	0.0258	-0.0032	1.47	0.0149	0.0063	0.46
	(0.0047)	(0.0060)	(0.0053)	(0.0175)	(0.0167)	(0.2292)	(0.0156)	(0.0138)	(0.6325)
English as additional language	0.0013	0.0042	0.32	0.0188	0.0128	0.88	0.0120	0.0163	1.00
	(0.0042)	(0.0055)	(0.7250)	(0.0150)	(0.0144)	(0.4145)	(0.0134)	(0.0119)	(0.3685)
Special Education Needs	-0.0048***	-0.0001	22.42	-0.0026	-0.0009	0.49	-0.0036	-0.0000	1.58
	(0.0009)	(0.0012)	(0.0000)	(0.0027)	(0.0031)	(0.6104)	(0.0023)	(0.0025)	(0.2065)
White	0.0786***	0.0186***	237.37	0.0364***	0.0021	4.49	0.0489***	0.0008	10.69
	(0.0042)	(0.0054)	(0.0000)	(0.0131)	(0.0124)	(0.0112)	(0.0116)	(0.0103)	(0.0000)
Black	-0.0295***	-0.0033	69.81	0.0128	0.0062	0.95	0.0120	-0.0031	1.63
	(0.0031)	(0.0040)	(0.0000)	(0.0093)	(0.0091)	(0.3865)	(0.0083)	(0.0076)	(0.1958)
Asian	0.0014	0.0005	0.12	0.0005	-0.0168*	1.89	0.0092	-0.0071	1.60
	(0.0031)	(0.0040)	(0.8905)	(0.0101)	(0.0096)	(0.1515)	(0.0089)	(0.0080)	(0.2024)
Deprivation in local area	-0.0297***	-0.0033**	549.42	-0.0011	-0.0003	0.07	-0.0037	-0.0029	1.11
	(0.0011)	(0.0015)	(0.0000)	(0.0030)	(0.0028)	(0.9322)	(0.0028)	(0.0024)	(0.3291)
% with higher education in local area	0.0221***	0.0043***	508.96	0.0176	0.0028	1.13	-0.0014	-0.0004	0.25
	(0.0009)	(0.0011)	(0.0000)	(0.0329)	(0.0019)	(0.3242)	(0.0019)	(0.0017)	(0.7826)
Baseline achievement (all subjects)	0.0991***	-0.0002	94.86	0.0195	-0.0029	0.24	0.0344	-0.0040	0.89
	(0.0094)	(0.0121)	(0.0000)	(0.0319)	(0.0273)	(0.7890)	(0.0296)	(0.0265)	(0.4118)
Baseline achievement (mathematics)	0.0818***	-0.0018	65.31	0.0096	0.0042	0.04	0.0326	-0.0177	1.21
	(0.0094)	(0.0121)	(0.0000)	(0.0345)	(0.0332)	(0.9616)	(0.0310)	(0.0273)	(0.2986)
N		181,154			23,342			23,619	
First and second choice FEs		Y			Y				
Tie-breaker controls					Y			Y	
Assignment risk (ranked schools)					Y				
Assignment risk (all schools)								Y	

Note. The table shows estimates of covariate balance by offer status. Reported are coefficients on dummies indicating offer at the first-choice (columns 1, 4, and 7) and second-choice school (columns 2, 5, and 8) from equation (7). The regression in columns (1)-(3) consider all appicants and only includes controls for cohort and first choice dummies. The regressions in columns (4)-(9) restrict the sample to students with non-determistic assignment risk at one or more ranked schools. These regressions include a local linear polynomial of distance to the catchment boundary (tie-breaker) at each ranked school. In columns (4)-(6), assignment risk is controlled by including risk at ranked schools along with first and second choice dummies. In columns (7)-(9), assignment risk is controlled by including risk at all primary schools in London. When ranked schools are less than six, the corresponding control variables are set to 0, and dummies indicating missing preferences are included. All specifications control for individual characteristics other than the one considered as dependent variable. Columns (3), (6), and (9) display F-tests of joint significance of the first and second-choice offer coeffcients in the corresponding regressions (p-values are reported in parentheses). In all other columns, robust standard errors are reported in parenthesis. See Section 4 for details. ***p<0.01. ** p<0.05. * p<0.1

Estimation

I consider students with non-deterministic assignment risk at one or more of the listed schools (i.e., students displaying $p_{is(i,r)} < 1 \, \forall r$ and $\sum_r p_{is(i,r)} > 0$). Characteristics of the 24,405 applicants at risk of assignment resemble those in the full sample (Table C.1).[24]

I start by testing covariates balance by offer status conditional on assignment risk. I

[24]Small differences include a lower likelihood of free school meal eligibility (0.14 against 0.15 in the full sample) and of not speaking English at home (0.40 against 0.42 in the full sample). This mild selection is consistent with a non-zero chance of entering oversubscribed schools.

estimate the following specification:

$$W_i = \sum_{z=1}^{2} \gamma_z \mathbb{1}(Z_i = z) + f^p(\boldsymbol{p_i}) + f^d(\boldsymbol{d_i}) + u_{1i}, \tag{7}$$

where W_i is a baseline characteristic of student i. γ_1 and γ_2 represent the effect of being admitted to the first or second choice, respectively, vis-à-vis a school ranked lower than second choice or not ranked at all (about 9% of students are in the comparison group). I consider two different specifications for assignment risk controls, $f^p(\boldsymbol{p_i})$. First, I condition on assignment risk at ranked schools only, $\sum_{r=1}^{6} p_{is(i,r)}$, and control for first and second choice dummies ("preference" specification). Second, I condition on assignment risk at each school regardless the preference rank assigned by parents, $\sum_{s=1}^{S} p_{is(i,r)}$ ("score" specification).[25] Following Abdulkadiroglu et al. (2021), $f^d(\boldsymbol{d_i})$ controls for tie-breaker value by means of a local linear polynomial of distance to the catchment boundary of each ranked school.[26] As assignment risk is estimated, $f^d(\boldsymbol{d_i})$ adjusts for residual tie-breaker effects.

Same-risk students with and without school offer are observationally similar. For example, applicants admitted to their first choice are about 4 p.p. less likely of being eligible for free school meals and display 0.1σ higher achievement at school entrance compared to peers offered their third choice or lower (column 1 of Table 1). Even among students not admitted to their first choice, those offered their second choice are more likely of white origin, and reside in less-deprived local areas (column 2). Conditional on assignment risk, these differences are substantially smaller and mostly undistinguishable from zero (columns 4-9). Despite a statistically significant imbalance remaining for white origin, the difference is less then half the uncontrolled estimate (column 1). For all other covariates, F-tests of the joint significance of γ_1 and γ_2 fail to reject the null hypothesis (columns 6 and 9), in sharp contrast with the uncontrolled specification (column 3).

Match effects are estimated through the following first-stage and second-stage equations:

$$\mathbb{1}(D_i = r) = \sum_{z=1}^{2} \pi_{rz} \mathbb{1}(Z_i = z) + f^p(\boldsymbol{p_i}) + f^d(\boldsymbol{d_i}) + u_{2i}, \qquad r = 1, 2, \tag{8}$$

[25] As assignment risk is the conditional expectation of school offer, linear controls for assignment risk are equivalent to a full set of dummies indicating each risk value (see footnote 4 in Angrist et al., 2021).

[26] Specifically, $f^d(d_i) = \sum_{s=1}^{6} b_{is(i,r)} * [d_{is(i,r)} + d_{is(i,r)} * \mathbb{1}(d_{is(i,r)} < \tau_{s(i,r)})]$, where $\tau_{s(i,r)}$ denotes the catchment boundary of school s and $b_{is(i,r)}$ indicates whether student i resides within a bandwidth around $\tau_{s(i,r)}$. See Appendix C for details on bandwidth selection. All controls are interacted with cohort dummies.

Table 2: First stage results

	"preference" specification			"score" specification		
	Enroled at 1st choice	Enroled at 2nd choice	Enroled at 3rd choice	Enroled at 1st choice	Enroled at 2nd choice	Enroled at 3rd choice
	(1)	(2)	(3)	(4)	(5)	(6)
			Panel A. Two-choice model			
Offered 1st choice	0.6677***	-0.0152**		0.6938***	-0.0250***	
	(0.0119)	(0.0077)		(0.0108)	(0.0071)	
Offered 2nd choice	-0.0642***	0.7375***		-0.0688***	0.7509***	
	(0.0096)	(0.0088)		(0.0088)	(0.0081)	
F-test	2220.73	3662.80		3023.99	4583.04	
N	23,342	23,342		23,619	23,619	
			Panel B. Three-choice model			
Offered 1st choice	0.6442***	-0.0227***	-0.0170***	0.6674***	-0.0320***	-0.0230***
	(0.0133)	(0.0085)	(0.0062)	(0.0120)	(0.0078)	(0.0057)
Offered 2nd choice	-0.0921***	0.7289***	-0.0240***	-0.0968***	0.7435***	-0.0282***
	(0.0115)	(0.0098)	(0.0056)	(0.0103)	(0.0088)	(0.0048)
Offered 3rd choice	-0.0672***	-0.0324***	0.6785***	-0.0743***	-0.0196**	0.6873***
	(0.0136)	(0.0097)	(0.0131)	(0.0122)	(0.0086)	(0.0123)
F-test	1480.71	2395.07	1034.42	2069.96	3064.30	1227.39
N	23,342	23,342	23,342	23,619	23,619	23,619
First and second choice FEs	Y	Y	Y			
Tie-breaker controls	Y	Y	Y	Y	Y	Y
Assignment risk (ranked schools)	Y	Y	Y			
Assignment risk (all schools)				Y	Y	Y
Individual characteristics	Y	Y	Y	Y	Y	Y

Note. The table shows estimates of the impact of school offer on enrolment. Reported are coefficients on school offer indicators from equations (8). All regressions restrict the sample to students with non-determistic assignment risk at one or more ranked schools. All regressions include a local linear polynomial of distance to the catchment boundary (tie-breaker) at each ranked school. In columns (1)-(3), assignment risk is controlled by including risk at ranked schools along with first and second choice dummies. In columns (4)-(6), assignment risk is controlled by including risk at all primary schools in London. Panel B reports estimates from specifications analogue to Panel A augmented by a third-choice offer dummy, and in columns (1)-(3) these regressions include third choice dummies. When listed schools are less than six, the corresponding control variables are set to 0, and dummies indicating missing preferences are included in the controls. All regressions control for language, ethnicity, FSM eligibility, SEN, gender, deprivation index and adult education in area of residence. Reported are F-tests of joint significance of the two (Panel A) or three (Panel B) school offer indicators. Robust standard errors are reported in parenthesis. See Section 5 for details. ***p<0.01. ** p<0.05. * p<0.1

$$\tilde{Y}_i = \sum_{r=1}^{2} \beta_r \mathbb{1}(D_i = r) + f^p(\boldsymbol{p_i}) + f^d(\boldsymbol{d_i}) + u_{3i}. \tag{9}$$

where risk controls, f^p, and f^d are defined analogously to equation (7). In equation (9), β_1 and β_2 estimate the impact of enroling at the first or second choice on VA-adjusted achievement, respectively, vis-à-vis a school ranked third or lower.

5 Results

First stage

Receiving an offer from the first or the second choice steeply increases the probability of enrolling at that school. Panel A of Table 2 reports first stage estimates of school offer

coefficients from equations (8). Students offered their first choice vis-à-vis their third choice or lower are 67-69 p.p. more likely to enrol at their first choice. The corresponding estimate for the second choice is 74-75 p.p. Similar results are obtained from a model including offer from the third choice (see Panel B).

First-stage results suggest that offer take-up responds to parental preference for the school. Moderately smaller enrolment impacts of offer from the first choice relative to the second choice likely reflect the role of waiting lists at most-preferred institutions. This is visualised in Panel B of Figure A.3, showing larger enrolment rates among applicants located just beyond the catchment boundary of their first choice relative to lower-ranked schools. Moreover, all "off-diagonal" estimates in Table 2 (e.g., the impact of an offer from the second choice on first-choice enrolment) are negative, implying that an offer from a more-preferred school increases the likelihood of compliance with the centralised assignment. This is confirmed when considering uncontrolled enrolment rates (blue bars in Panel A of Figure 6). The 90% of students assigned their first choice comply with school offer, and this figure monotonically decreases when preference for the offered school decreases. Panel B shows that less than 1% of applicants enrol at a school with lower preference than the one assigned , while 2% enrol at a more-preferred school than the one offered (again, most likely through waiting lists).

Sorting on match effects

I find positive effects of attending the schools of choice above and beyond school VA. Estimates of β_1 and β_2 from equation (9) are reported in Panel A of Table 3. Columns (1)-(2) show that the effect of enroling at the first or second-choice school on student achievement in mathematics exceeds school VA by 0.12σ or 0.10σ, respectively ("preference" specification). Similar results are obtained from the "score" specification (0.10σ and 0.07σ, respectively, columns 5-6). In contrast, estimates on student achievement in English are imprecise and sometimes larger for the second choice (columns 3-4 and 7-8). Across specifications and subjects, including the third-choice school in the model supports the conclusion that parents select schools based on match effects (Panel B). Since the counterfactual here is enrolment at a school ranked fourth or lower, estimates are expected to increase relative to the two-choice model in Panel A. Indeed, estimates are larger in Panel B, although they are not statistically different. Moreover, point estimates of the third-choice coefficient are everywhere positive.

Table 3: Match effects: VA-adjusted outcomes

	"preference" specification				"score" specification			
	VA-adjusted KS1 score in mathematics		VA-adjusted KS1 score in English		VA-adjusted KS1 score in mathematics		VA-adjusted KS1 score in English	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Panel A. Two-choice model							
Enroled in 1st choice	0.1262**	0.1137**	0.0749	0.0510	0.1084**	0.0948**	0.0558	0.0404
	(0.0538)	(0.0528)	(0.0538)	(0.0523)	(0.0460)	(0.0451)	(0.0462)	(0.0451)
Enroled in 2nd choice	0.1001**	0.0958**	0.1064**	0.0979**	0.0660*	0.0638*	0.0491	0.0451
	(0.0481)	(0.0472)	(0.0482)	(0.0469)	(0.0385)	(0.0376)	(0.0386)	(0.0375)
N	22,149	22,149	22,149	22,149	22,415	22,415	22,415	22,415
	Panel B. Three-choice model							
Enroled in 1st choice	0.1390**	0.1249*	0.1178*	0.1003	0.1193**	0.0991*	0.0853	0.0672
	(0.0653)	(0.0643)	(0.0652)	(0.0636)	(0.0531)	(0.0521)	(0.0533)	(0.0521)
Enroled in 2nd choice	0.1122*	0.1022*	0.1574***	0.1475**	0.0774*	0.0683	0.0800*	0.0731
	(0.0602)	(0.0593)	(0.0604)	(0.0589)	(0.0463)	(0.0454)	(0.0463)	(0.0451)
Enroled in 3rd choice	0.0517	0.0284	0.1306*	0.1196*	0.0275	0.0108	0.0745	0.0674
	(0.0738)	(0.0727)	(0.0743)	(0.0725)	(0.0558)	(0.0547)	(0.0562)	(0.0547)
N	21,989	21,989	21,989	21,989	22,415	22,415	22,415	22,415
First and second choice FEs	Y	Y	Y	Y				
Tie-breaker controls	Y	Y	Y	Y	Y	Y	Y	Y
Assignment risk (ranked schools)	Y	Y	Y	Y				
Assignment risk (all schools)					Y	Y	Y	Y
Individual characteristics		Y		Y		Y		Y

Note. The table shows estimates of match effects at the most-preferred schools. Reported are coefficients on the school enrolment indicators from equation (9), instrumented using school offer. All regressions restrict the sample to students with non-determistic assignment risk at one or more ranked schools. All regressions include a local linear polynomial of distance to the catchment boundary (tie-breaker) at each ranked school. In columns (1)-(4), assignment risk is controlled by including risk at ranked schools along with first and second choice dummies. In columns (5)-(8), assignment risk is controlled by including risk at all primary schools in London. Dependent variable is KS1 achievement score in mathematics (columns 1-2, 5-6) or English (columns 3-4, 7-8). All outcomes are corrected for school value-added by subtracting estimated same-subject VA of the school where student enrols from achievement scores. Panel B reports estimates from specifications analogue to Panel A augmented by a third-choice offer dummy, and in columns (1)-(4) these regressions include third choice dummies. When listed schools are less than six, the corresponding control variables are set to 0, and dummies indicating missing preferences are included in the controls. Column 2, 4, 6, and 8 add controls for gender, language, ethnicity, free school meal eligiblity, special education needs, along with deprivation index and education level in the neighbourhood of residence. Robust standard errors are reported in parenthesis. See Section 5 for details. ***p<0.01. ** p<0.05. * p<0.1

An alternative approach is to focus on students with similar expected VA gains. Table 4 reports estimates from a specification that augments equation (9) with an interaction between school offers and the expected VA gain from attending most-preferred schools (see Appendix E for details). Since VA is estimated, I bin schools with similar quality to reduce noise and consider different grouping choices (quintiles, deciles, and ventiles). For example, column (2) shows that attending the first or second choice increases achievement in mathematics by 0.14σ and 0.11σ, respectively, with respect to a lower-ranked school in the same VA decile (Panel A). Results are unchanged when holding school VA quintile or ventile constant, and remarkably similar to estimates in Table 3.

Results suggest that school quality is credibly accounted for in this comparison. First, estimated VA strongly predicts causal effects. Estimates of the interaction coefficients in Table 4 are positive and strongly significant. Across subjects, these estimates decrease proportionally with the width of VA bin considered. For example, in columns (1)-(3) a one-quintile increase in the expected VA gain causes an achievement gain of 0.03-0.05σ, shrinking to 0.015-0.025σ and 0.008-0.013σ when considering deciles and ventiles, respectively. Second, school VA is effectively held constant by controlling for expected VA gain. Table I.2 reports similar estimates to Table 4 using school VA as dependent variable.The uninteracted coefficients of school offers are generally close to zero and not statistically significant, suggesting that parents do not systematically select schools with higher VA within the bins I consider.

The magnitude of estimated match effects is substantial compared to the impact of attending schools with higher average quality. For example, estimates in column (2) of Table 4 imply that attending the first-choice school increases student achievement in mathematics about as much as passing from a school in the bottom decile to one in the 7th decile of the school VA distribution. Attending the second choice has a comparable impact to passing from the bottom decile to the median of school VA in mathematics.

Alternative research design

I consider an alternative research design to reinforce the interpretation of my findings as match effects. My main design holds application choice constant, exploiting as-good-as-random variation in school offer. This design identifies the parameter of interest by exploiting the centralised assignment, but needs controlling for school VA to isolate match effects. One

Table 4: Match effects: hold VA gains constant

	"preference" specification			"score" specification		
	VA quintile	VA decile	VA ventile	VA quintile	VA decile	VA ventile
	(1)	(2)	(3)	(4)	(5)	(6)
			Panel A. KS1 score in mathematics			
Enroled in 1st choice	0.1359**	0.1357**	0.1355**	0.1144**	0.1145**	0.1144**
	(0.0535)	(0.0535)	(0.0535)	(0.0448)	(0.0448)	(0.0448)
Enroled in 2nd choice	0.1088**	0.1084**	0.1084**	0.0678*	0.0672*	0.0673*
	(0.0478)	(0.0478)	(0.0478)	(0.0382)	(0.0383)	(0.0383)
Enroled in 1st choice X	0.0269**	0.0145**	0.0075**	0.0171	0.0088	0.0046*
school value-added gain	(0.0121)	(0.0060)	(0.0030)	(0.0110)	(0.0054)	(0.0027)
Enroled in 2nd choice X	0.0515***	0.0255***	0.0128***	0.0330**	0.0157**	0.0077**
school value-added gain	(0.0163)	(0.0081)	(0.0040)	(0.0151)	(0.0075)	(0.0037)
			Panel B. KS1 score in English			
Enroled in 1st choice	0.0886*	0.0881*	0.0879	0.0618	0.0613	0.0608
	(0.0536)	(0.0536)	(0.0536)	(0.0452)	(0.0452)	(0.0452)
Enroled in 2nd choice	0.1141**	0.1139**	0.1141**	0.0507	0.0504	0.0502
	(0.0480)	(0.0481)	(0.0481)	(0.0385)	(0.0385)	(0.0385)
Enroled in 1st choice X	0.0406***	0.0206***	0.0104***	0.0224**	0.0117**	0.0061**
school value-added gain	(0.0125)	(0.0061)	(0.0031)	(0.0112)	(0.0055)	(0.0027)
Enroled in 2nd choice X	0.0418***	0.0208***	0.0108***	0.0275*	0.0134*	0.0070*
school value-added gain	(0.0159)	(0.0079)	(0.0039)	(0.0149)	(0.0073)	(0.0037)
N	22,265	22,265	22,265	22,551	22,551	22,551
First and second choice FEs	Y	Y	Y			
Tie-breaker controls				Y	Y	Y
Assignment risk (ranked schools)	Y	Y	Y			
Assignment risk (all schools)	Y	Y	Y	Y	Y	Y

Note. The table shows estimates of match effects at the most-preferred schools. Reported are estimates of school enrolment and interaction coefficients from equation (18). Estimated specifications are analogue to Panel A of Table 3, augmented with interactions of school offer coefficients with expected value-added gains. The latter are computed subtracting the average estimated VA of schools ranked third to six to the estimated VA of first or second choice. Different bins of school VA are considered to compute expected gains: quintiles in columns 1 and 4, deciles in columns 2 and 5, and ventiles in columns 3 and 6. See Section 5 for details. ***p<0.01. ** p<0.05. * p<0.1

Table 5: Alternative research design

	KS1 score in mathematics		KS1 score in English	
	(1)	(2)	(3)	(4)
	Panel A. Two-choice model			
Enroled in 1st choice	0.1489**	0.1604***	0.1138*	0.1126*
	(0.0625)	(0.0611)	(0.0632)	(0.0612)
Enroled in 2nd choice	0.1099**	0.1171**	0.1174**	0.1139**
	(0.0491)	(0.0478)	(0.0494)	(0.0478)
N	22,369	22,369	22,369	22,369
	Panel B. Three-choice model			
Enroled in 1st choice	0.2242***	0.2269***	0.2005***	0.1874**
	(0.0765)	(0.0750)	(0.0772)	(0.0749)
Enroled in 2nd choice	0.1844***	0.1828***	0.2033***	0.1879***
	(0.0631)	(0.0616)	(0.0636)	(0.0616)
Enroled in 3rd choice	0.1626**	0.1432**	0.1872***	0.1611**
	(0.0716)	(0.0701)	(0.0724)	(0.0703)
N	22,369	22,369	22,369	22,369
School where student enrols FEs	Y	Y	Y	Y
Tie-breaker controls	Y	Y	Y	Y
Assignment risk (ranked schools)	Y	Y	Y	Y
Individual characteristics		Y		Y

Note. The table shows estimates of match effects at the most-preferred schools. Reported are coefficients on the school enrolment indicators from equation (8), instrumented using school offer. All regressions restrict the sample to students with non-determistic assignment risk at one or more ranked schools. Controls include dummies indicating the school where student enrols, alongside assignment risk and a local linear polynomial of distance to the catchment boundary (tie-breaker) at each ranked school. In addition, columns (2) and (4) include gender, language, ethnicity, free school meal eligiblity, special education needs, along with deprivation index and education level in the neighbourhood of residence. Robust standard errors are reported in parenthesis. See Section 5 for details. ***p<0.01. ** p<0.05. * p<0.1

potential concern is that school quality is not fully controlled in my comparison. To eliminate this potential confounder, I compare students assigned to the same school, ranked by their parents with different preferences.

My alternative design holds school quality – and any school input – fixed, at the expense of a stronger identifying assumption. I exploit heterogeneity in application choices (see Section 6) and estimate empirical models similar to equation (9) augmented with a full set of dummies for the school where student enrols.[27] A causal interpretation of the results requires the assumption that students who are as good as randomly admitted to *different* schools have similar potential outcomes. Although this analysis holds potentially important confounders

[27]Specifically, I consider students at risk of assignment and control for assignment risk at listed school. In contrast to the main empirical model, I do not control here for a student's application set.

constant, such as residential sorting with respect to listed schools, residual selection into application choice cannot be fully ruled out.

Results are qualitatively similar to the main specifications, suggesting that differences in school quality are not driving my results. Students enroled at their first choice exhibit $0.11 - 0.16\sigma$ higher achievement than students enroled at the same school who ranked that school as third choice or lower (see Panel A of Table 5). The corresponding estimate is 0.11σ for the second choice . Estimates are remarkably similar to the main findings in Table 3 and 4, especially for mathematics.[28]

Overall, using different approaches and specifications, results suggest that students enroling at schools ranked by their parents with higher preference experience achievement gains above and beyond the impacts predicted by school VA.[29] Findings point to the presence of heterogeneity in school effectiveness, and suggest that parents sort into schools that specifically enhance achievement of their children.

Heterogeneity analysis

I find suggestive evidence that sorting on match effects is more pronounced among parents with relatively advantaged socioeconomic background. Separate estimates by student individual characteristics are presented in Table 6. Reported coefficients are obtained from specifications similar to column (6) of Table 3. For comparison, the full-sample estimates are replicated in column (1).

Estimates are larger than average, at least at the first choice, for students not eligible for free school meals (column 3), residing in low-deprivation areas (column 9), and with above-median baseline achievement (column 10), all characteristics that may reflect higher socioeconomic status. In contrast, I cannot reject null effects among students residing in high-deprivation areas or with below-median baseline achievement, while point estimates are even negative among students eligible for free school meals. Moreover, estimates are larger than average for white students (column 4), and students speaking English at home (column 6). In contrast, I cannot reject null effects among students with other ethnic or linguistic

[28] Panel B of Table 5, including enrolment at the third choice, delivers slightly larger estimates. This result is in line with stronger concerns about selection bias when comparing students with substantially different preference rank for the same school.

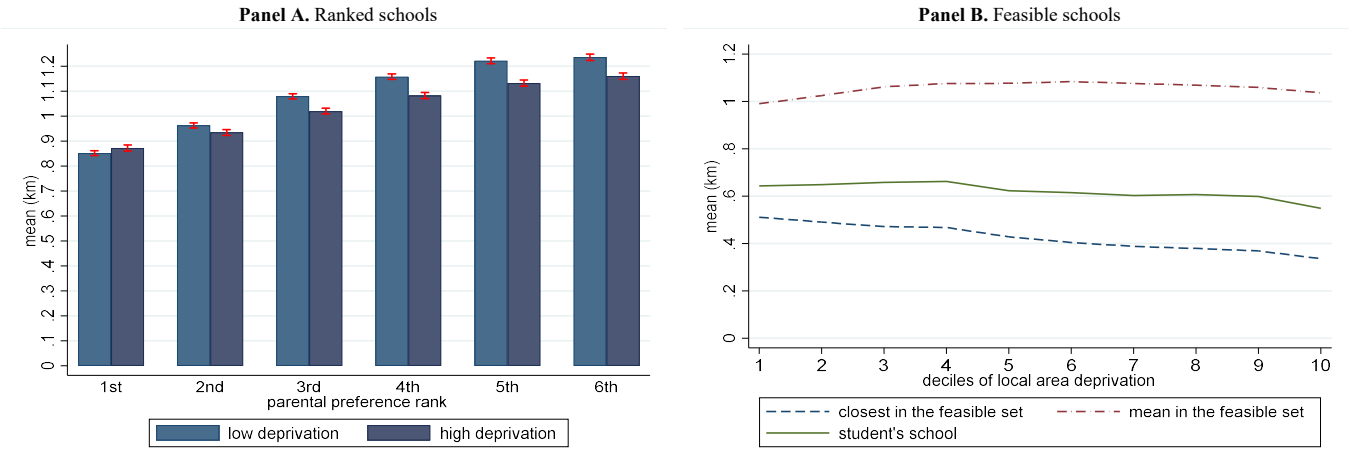
[29] See Appendix F for additional robustness checks.

Table 6: Heterogeneous match effects

		Free school meal eligible		White		Speaking English at home		Deprivation in local area		Baseline achievement	
	All	Yes	No	Yes	No	Yes	No	Above median	Below median	Above median	Below median
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
Panel A. VA-adjusted KS1 score in mathematics											
Enroled in 1st choice	0.0948**	-0.1968*	0.1176**	0.1967***	0.0346	0.1674***	0.0754	0.0412	0.1693**	0.2010***	0.0357
	(0.0451)	(0.1194)	(0.0488)	(0.0723)	(0.0584)	(0.0581)	(0.0725)	(0.0632)	(0.0667)	(0.0538)	(0.0616)
Enroled in 2nd choice	0.0638*	-0.0661	0.0852**	0.0576	0.0941*	0.0465	0.1268**	0.0501	0.0574	0.0635	0.0880*
	(0.0376)	(0.1036)	(0.0406)	(0.0622)	(0.0482)	(0.0496)	(0.0586)	(0.0550)	(0.0525)	(0.0459)	(0.0503)
Panel B. VA-adjusted KS1 score in English											
Enroled in 1st choice	0.0404	-0.2176*	0.0620	0.1408*	-0.0363	0.1272**	-0.0125	-0.0212	0.1188*	0.1016*	0.0234
	(0.0451)	(0.1204)	(0.0486)	(0.0749)	(0.0571)	(0.0594)	(0.0705)	(0.0628)	(0.0668)	(0.0527)	(0.0614)
Enroled in 2nd choice	0.0451	-0.0268	0.0592	0.0550	0.0474	0.0606	0.0398	0.0261	0.0311	0.0306	0.0932*
	(0.0375)	(0.1061)	(0.0403)	(0.0635)	(0.0471)	(0.0498)	(0.0576)	(0.0543)	(0.0528)	(0.0446)	(0.0495)
N	22,415	3,185	19,230	9,060	13,355	13,432	8,983	10,231	12,184	10,639	11,649
Tie-breaker controls	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Assignment risk (all schools)	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Individual characteristics	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y

Note. The table shows estimates of match effects at the most-preferred schools separately by student observable characteristics. Column (1) reports estimates in column (6) and (8) in Panel A of Table 3. All other columns report estimates from similar specifications on different subsamples: student eligible or not eligible for free school meals (columns 2-3), students of white or other ethnic origin (columns 4-5), students speaking English or any other language at home (columns 6-7), students residing in local areas with above- or below-median deprivation (columns 8-9), or students with Year 0 achievement score above or below median (columns 10-11). Robust standard errors are reported in parenthesis. See Section 5 for details. ***p<0.01. ** p<0.05. * p<0.1

Figure 2: Parental preferences and distance to school



Note. The figure plots parental preference rank against distance to school (Panel A) and distance to the school where student enrols compared to average or lowest distance in the feasible choice set (Panel B). Bars in Panel A plot predicted values from equation (10), where prefrence rank dummies are interacted with an indicator variable equal to one if deprivation index in the LSOA of residence is above the median. Controls include dummies for quintiles of peer quality and school value added. Superimposed in red are 95% confidence intervals of predicted values. In all panels, distance to school is measured in kilometers and computed as linear distance between student postcode and school postcode centroids. Schools further than 2 kilometers from residence (the 90th percentile) are not considered. Deprivation index is based on average income in the LSOA of residence. See Section 6 for details.

background. Interestingly, effects in mentioned sub-groups are marginally significant in English as well (Panel B), despite the corresponding average estimates are statistically equal to zero. I note, however, that most estimates by sub-groups are not statistically different from each other.

Overall, results possibly suggest that parents with more resources are in the position to better exploit school choice and improve match quality for their children.

6 Parental preference for schools

The results presented in Section 5 imply that parents select schools based on match effects, i.e., the school's specific effectiveness for their own children. In this section, I describe parental preferences for school attributes and discuss potential links with my results.

Parental preferences and school attributes

I start by using submitted rankings to describe parental preferences for geographical proximity, peer quality and school effectiveness. First, I plot average attributes of listed institutions by parental preference rank, conditional on feasibility and number of schools listed. Specifically, I plot estimates of λ_1 to λ_6 from the following application-level regression:

$$A_{is(i,r)} = \sum_{p=1}^{6} \lambda_r \mathbb{1}(r = p) + X'_{is(i,r)}\zeta + u_{is}, \tag{10}$$

where $A_{is(i,r)}$ is an attribute of school s ranked r-th by student i. The parameter λ_r estimates the average level of attributes at schools ranked as r-th choice conditional on controls.[30]

Second, following Ainsworth et al. (2020), I compare the school where student enrols with other feasible institutions. This complements the description of parental preferences by considering the supply side, and provides insights on which attributes parents maximise. I define the individual feasible set as the collection of schools, to which the student may or may not have applied, which would have been accessible based on distance (see Appendix G). Feasible set of the median applicant includes 6 schools within 2 kilometers (km) from residence and 75% of parents could potentially access at least 4 schools.

Parents generally rank schools in order of proximity.[31] Panel A of Figure 2 plots distance to school by parental preference rank from equation (10), separately for students residing in areas with deprivation above or below the median.[32] The estimated distance to the first-choice school in better-off areas is around 850 meters, and all ranked schools are located within 1.2 km from residence. Similar distance to the first choice in high-deprivation neighourhoods suggests that parents with lower socioeconomic status have similar willingness to travel.

Parents nearly minimise distance among available alternatives. Panel B of Figure 2 compares distance to the school where student enrols against the closest feasible institution and the average feasible distance by decile of local area deprivation (Ainsworth et al., 2020). Students with different socioeconomic status travel very similar distances to primary school, about 600 meters.[33] Parents in areas with higher deprivation face schools closer to the place of residence, likely reflecting higher population density. The 51% of applicants enrol in the closest accessible institution (see columns 8-10 of Table I.3). On average, parents "leave on the table" schools closer to the place of residence by 220 meters, possibly trading off

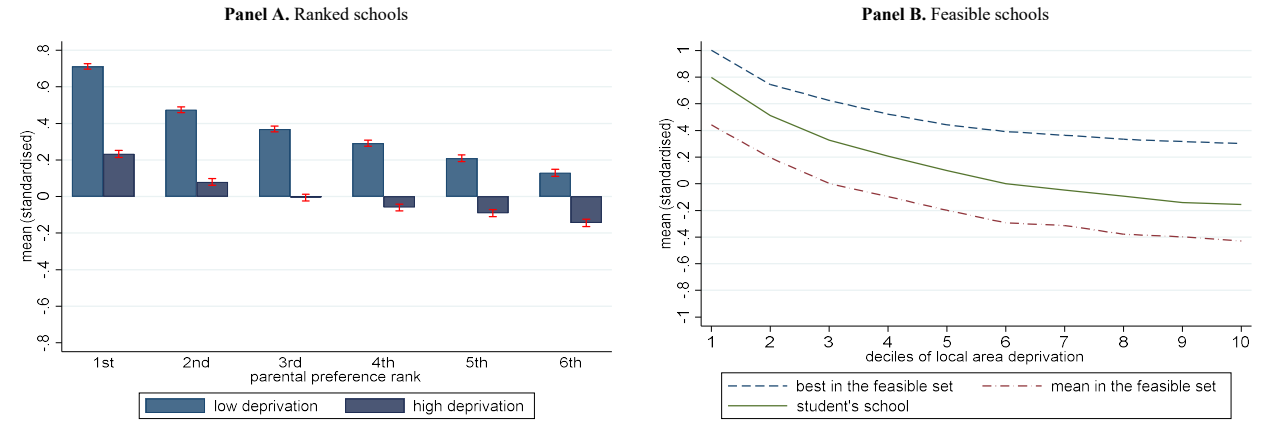
[30] Besides dummies for number of schools listed and ex-post feasibility of the school, the vector $X'_{is(i,r)}$ includes attributes other than $A_{is(i,r)}$ (e.g., school VA and distance when considering peer quality).

[31] In this section, I consider applicants residing within 2 km from the school (the 90$^{\text{th}}$ percentile).

[32] Hastings et al. (2009) find different school preferences by socioeconomic status. Heterogeneity is explored here by interacting preference rank dummies in equation (10) with an indicator for above-median deprivation in the area of residence.

[33] This differs from distance values in Panel A since the latter are regression-adjusted estimates.

Figure 3: Parental preferences and peer quality



Note. The figure plots parental preference rank against peer quality (Panel A) and peer quality at the school where student enrols compared to average or highest peer quality in the feasible choice set (Panel B). Bars in Panel A plot predicted values from equation (10), where prefrence rank dummies are interacted with an indicator variable equal to one if deprivation index in the LSOA of residence is above the median. Controls include dummies for quintiles of school value added and distance to school. Superimposed in red are 95% confidence intervals of predicted values. In all panels, peer quality is measured by school-level final year test scores, averaged across 2007-2016 cohorts and across mathematics and English. Deprivation index is based on average income in the LSOA of residence. See Section 6 for details.

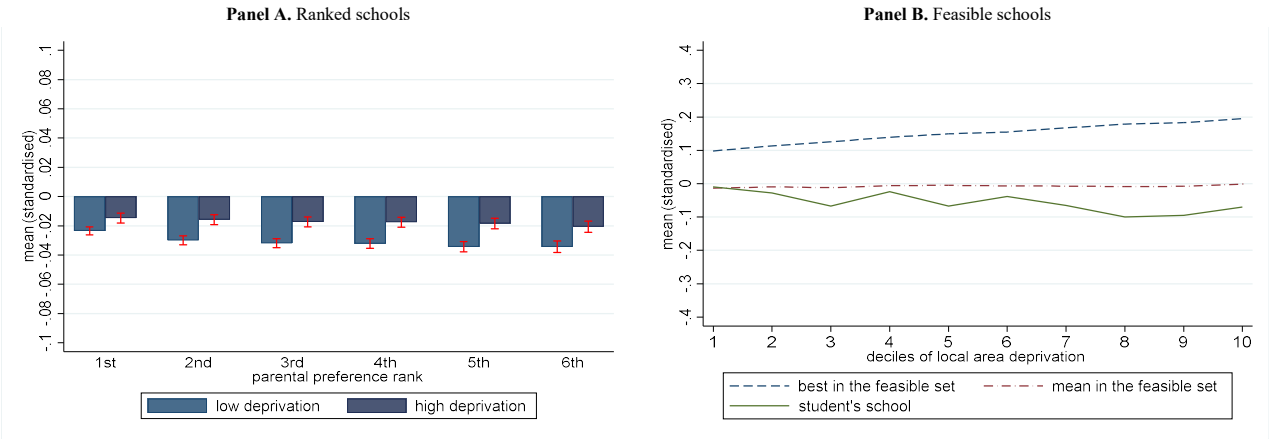
proximity with other valued attributes. Interestingly, this figure is lower for applicants in low-deprivation areas, who likely choose their residence close to desired institutions.

All parents rank schools by peer quality, on average, and access to high-performing institutions is spatially segregated. Panel A of Figure 3 plots standardised final test scores at the school by parental preference rank from equation (10).[34] Estimated peer quality at the first choice in low-deprivation areas is 0.7σ above the average, decreasing to 0.2σ in worse-off neighbourhoods (similar to peer quality at the fifth choice in low-deprivation areas). Regardless of socioeconomic status, peer quality markedly decreases with preference rank. Most of the gap in peer quality by local deprivation is explained by segregation of high-performing schools. Parents residing in areas with deprivation in the bottom decile face 0.8σ higher feasible peer quality than parents in the top decile, and the gap for schools where students enrol is about 1σ (see Panel B). Parents in areas with below-median deprivation leave little "on the table" in terms of peer quality, 0.27σ, compared to 0.43σ in areas with above-median deprivation (columns 5-7 of Table I.3). This gap may reflect steeper trade-offs between school test scores and distance for disadvantaged parents.

Conditional on distance and peer quality, parents do not respond to school VA. Panel A of Figure 4 shows almost no association between school VA and parental preference rank, in line with findings in other contexts (MacLeod and Urquiola, 2019). Panel B shows that,

[34] I measure peer quality using KS2 test scores from 2006 to 2016 averaged across mathematics and reading.

Figure 4: Parental preferences and school VA



Note. The figure plots parental preference rank against estimated school value-added (Panel A) and value-added at the school where student enrols compared to average or highest value-added in the feasible choice set (Panel B). Bars in Panel A plot predicted values from equation (10), where prefrence rank dummies are interacted with an indicator variable equal to one if deprivation index in the LSOA of residence is above the median. Controls include dummies for quintiles of peer quality and distance to school. Superimposed in red are 95% confidence intervals of predicted values. In all panels, valu- added is estimated by regression-adjusted test scores growth at the school and averaged across subjects (see Appendix B). Deprivation index is based on average income in the LSOA of residence. See Section 6 for details.

regardless of socioeconomic status, students enrol in schools with lower VA than the average feasible institution. Students could potentially access schools with 0.4σ larger VA (columns 2-4 of Table I.3), implying that achievement could substantially increase under alternative allocations if returns to school were homogeneous. However, findings presented in Section 5 suggest that school effects are heterogeneous, offering a potential explanation for the weak correlation between parental preferences and average school VA.

Parental preferences lead to rationing of seats at schools with high peer quality. Attributes of oversubscribed schools are described in Table I.4. I define a school as oversubscribed if the number of applicants missing out on any more-preferred institution exceeds capacity. School seats are rationed in the 58% of institutions.[35] Mirroring evidence from preference data, oversubscribed schools have markedly higher peer quality, 0.65σ in mathematics and 0.77σ in English, while the difference in VA is substantially narrower (0.16σ and 0.2σ, respectively).

Potential mediating outcomes

Although parental preferences generate substantial gains in peer quality and geographical proximity from entering highest-ranked schools, these attributes do not explain my results. Students enroling at their first choice are exposed to 0.57σ higher peer quality compared to

[35] I consider schools oversubscribed by at least 5 seats. The 66% of schools are oversubscribed by one seat or more and the 45% by 20 seats or more.

peers with the same assignment risk who end up at their third choice or lower. This can be seen in Panel A of Table 7 (column 1), reporting peer quality estimates from specifications analogue to Table 6. The gain is substantially lower (0.15σ) for students enroling at their second choice, consistently with the strong association between peer quality and parental preference discussed above. Heterogeneous effects in Table 6do not follow the pattern of results in Panel A of Table 7. For example, students eligible for free school meal exhibit larger-than-average peer quality gains (column 2), despite zero or negative match effects. Similar conclusions hold for distance to school. Students enroling at their first or second choice exhibit shorter home-to-school distances compared to peers with the same assignment risk who end up at their third choice or lower (Panel B). Reflecting preferences for geographical proximity, gains are similar across different socioeconomic backgrounds (columns 2-7), failing to replicate the pattern of results in Table 6.

I next consider two further observable attributes that are specific to the student-school match. First, students may benefit from schoolmates that are already in their network, which I proxy using peers residing in the same local area. Students enroling at their first or second choice significantly increase the share of neighbours attending the same school compared to peers with the same assignment risk who end up at their third choice or lower (Panel C). Gains in the share of peers from the same neighbourhood are similar, for example, by achievement score at entrance (columns 6-7), failing to replicate the pattern of results in Table 6. Finally, academic rank in the class has an independent effect on student achievement (Murphy and Weinhardt, 2020). However, Panel D shows that parental preferences are not related to the percentile achievement rank within school-cohort, on average. Students eligible for free school meals who enrol at their top two choices experience a substantial decrease in achievement rank (column 2), potentially explaining zero-to-negative match effects in this subgroup. Gains in ability rank, however, exhibit little difference by other student characteristics associated with match effects (columns 4-7).

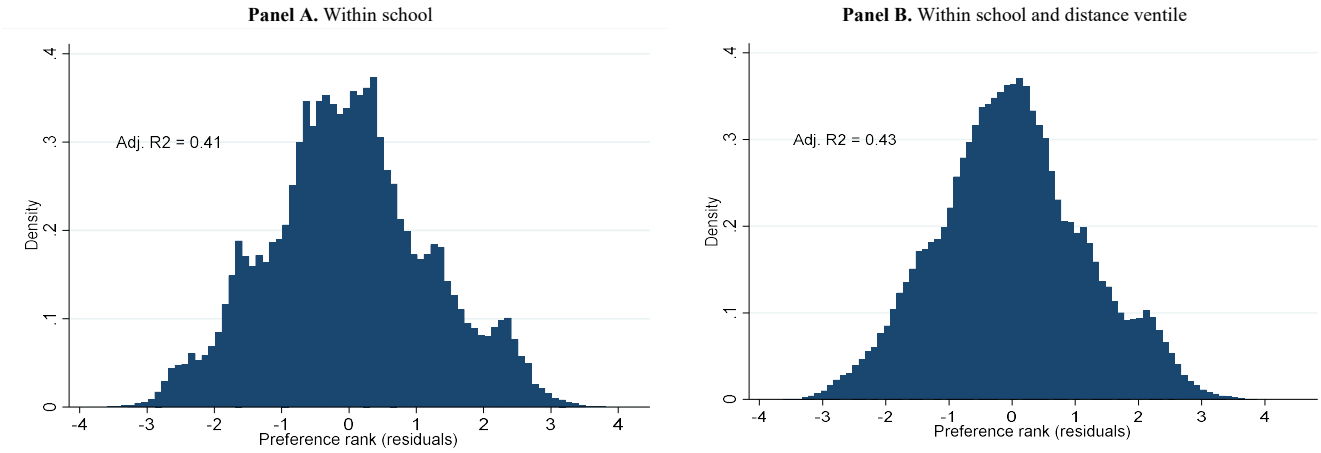
Overall, no observable school attribute can fully explain match effects unveiled in Section 5. In the next subsection, I show that unobservable and student-specific school attributes explain a substantial share of parental preferences.

Table 7: Mediating outcomes

		Free school meal eligible		Deprivation in local area		Achievement score at Year 0	
	All	Yes	No	Above median	Below median	Above median	Below median
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
				Panel A. Peer quality			
Enroled in 1st choice	0.5722***	0.7335***	0.5434***	0.4851***	0.6300***	0.6484***	0.4919***
	(0.0316)	(0.0788)	(0.0343)	(0.0430)	(0.0464)	(0.0446)	(0.0458)
Enroled in 2nd choice	0.1524***	0.3295***	0.1306***	0.1220***	0.1709***	0.2010***	0.0891**
	(0.0252)	(0.0626)	(0.0271)	(0.0347)	(0.0365)	(0.0365)	(0.0366)
N	18,122	2,647	15,475	8,657	9,465	8,060	9,153
				Panel B. Distance to school			
Enroled in 1st choice	-0.4131***	-0.3578***	-0.4143***	-0.3461***	-0.4523***	-0.4006***	-0.3949***
	(0.0198)	(0.0465)	(0.0211)	(0.0261)	(0.0292)	(0.0269)	(0.0298)
Enroled in 2nd choice	-0.2780***	-0.1973***	-0.2897***	-0.2261***	-0.3174***	-0.2886***	-0.2603***
	(0.0166)	(0.0400)	(0.0178)	(0.0231)	(0.0234)	(0.0240)	(0.0239)
N	23,350	3,226	20,124	10,543	12,807	10,600	11,572
				Panel C. % of neighbours enroled			
Enroled in 1st choice	0.1031***	0.0864***	0.1041***	0.0694***	0.1281***	0.1024***	0.0966***
	(0.0058)	(0.0129)	(0.0064)	(0.0064)	(0.0099)	(0.0087)	(0.0081)
Enroled in 2nd choice	0.0642***	0.0471***	0.0652***	0.0431***	0.0829***	0.0739***	0.0561***
	(0.0045)	(0.0108)	(0.0049)	(0.0054)	(0.0072)	(0.0068)	(0.0063)
N	22,433	3,108	19,325	10,221	12,212	10,138	11,182
				Panel D. Ability rank in school-cohort			
Enroled in 1st choice	0.0030	-0.1108***	0.0148	-0.0178	0.0239	0.0169	-0.0021
	(0.0129)	(0.0323)	(0.0140)	(0.0176)	(0.0195)	(0.0110)	(0.0078)
Enroled in 2nd choice	-0.0005	-0.0805***	0.0092	-0.0066	-0.0014	0.0151	0.0025
	(0.0107)	(0.0281)	(0.0117)	(0.0152)	(0.0154)	(0.0094)	(0.0064)
N	22,315	3,176	19,139	10,185	12,130	10,667	11,648
Tie-breaker controls	Y	Y	Y	Y	Y	Y	Y
Assignment risk (all schools)	Y	Y	Y	Y	Y	Y	Y
Individual characteristics	Y	Y	Y	Y	Y	Y	Y

Note. The table shows estimates of the effect of entering most-preferred institutions on the characteristics of the school where students enrol. Reported are coefficients obtained from specifications similar to Table 6. The dependent variables are: peer quality (Panel A), measured by school-level final year test scores, averaged across 2007-2016 cohorts and across mathematics and English; distance to school (Panel B), measured in kilometers and computed as linear distance between student postcode and school postcode centroids; the share of students residing in the same local area (LSOA) who are enroled at the school (Panel C); student's percentile rank in the school-cohort distribution of achievement score at entrance (Panel D). Robust standard errors are reported in parenthesis. See Section 6 for details. ***p<0.01. ** p<0.05. * p<0.1

Figure 5: Variability in parental preferences



Note. The figure plots residual parental preference for schools after controlling for dummies indicating the number of schools listed, ex-post feasibility of the school, and school dummies (Panel A); or additionally including dummies for distance to school ventile (Panel B). The dependent variable is parental preference rank, ranging from 1 to 6. The adjusted R-squared index of the regression is reported in the top left. See Section 6 for details.

Heterogeneous parental preferences

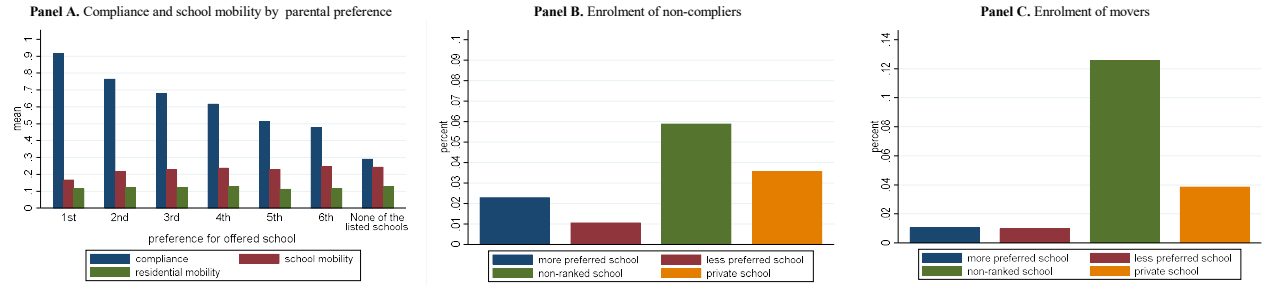
Average patterns mask substantial disagreement in parental ranking of schools. Within-school dispersion is plotted in Figure 5, reporting the distribution of residual preference ranks from a regression on school dummies, ex-post feasibility, and number of ranked schools. Panel A shows that residuals exhibit remarkable variability, with an adjusted R-squared index of just 0.41. A substantial share of parents assigns preference ranks one or two positions away from the average for the same school. Including dummies for distance-to-school ventiles only slightly increases the adjusted R-squared (0.43, Panel B).[36] Therefore, even conditional on distance, a substantial share of variation in parental preferences is explained by unobservable attributes that are specific to the student-school match.

Nevertheless, parental choice behaviour over time is consistent with preferences submitted at application. The likelihood of moving children to a different institution by Year 2 decreases with preference for the offered school (Panel A of Figure 6, red bars). Residential mobility, in contrast, is roughly orthogonal to school assignment (green bars).[37] Panel C

[36]Similar results are obtained when considering also non-ranked feasible schools (available upon request). In this case, controlling for distance to school generates a larger increase in explained variability ($\mathbb{R}^2 = 0.5$), suggesting that distance is strongly associated with the decision of ranking a school.

[37]I define residential mobility as an indicator variable equal to one if a student's home postcode changes from Year 0 to Year 2. This result suggests that parents who are willing to move their residence to secure a school place do so before applying.

Figure 6: Compliance with assignment and mobility



Note. The figure plots compliance with school offer and school and residential mobility by parental preference. Panel A plots compliance, school mobility and residential mobility rates by parental rank for school offered. Panel B plots the share of students who do not comply with school offer by preference for the school where they enrol at the reception year. Panel C plot the share of students who change school with respect to the reception year by preference for the school where they enrol at Year 2. Residential mobility is defined as changing home postcode with respect to the previous academic year. See Section 6 for details.

shows that only 1% of students move to a school ranked with lower preference, suggesting that parents consistently avoid less-preferred schools even after initial enrolment. A similar fraction of students move to a school with higher parental preference, suggesting that centralised assignment is successfully enforced. In addition, I show in Appendix H that the decision to move to a different school responds to peer quality but not to school VA, in line with preferences submitted at application.

In conclusion, although parents generally prefer schools with higher peer quality and closer to the place of residence, their rankings exhibit substantial variability. Nevertheless, preferences expressed at application predict school choice behaviour in subsequent years. Therefore, parental preferences appear to to reflect solid tastes for available schools, and yet to a large extent they are student-specific. Findings presented in Section 5 suggest that such heterogeneity is linked to unobservable school attributes that increase student achievement.

7 Summary and conclusion

Expanding parental choice can be viewed as a zero-sum game if school quality is homogeneous across students or if parental choices are unrelated to student achievement. On the other hand, it may increase system-wide school productivity if students have specific educational needs, and parents select schools on such basis. I have investigated this hypothesis in the context of primary school choice in London, a dense urban area featuring fierce competition for seats at popular schools. Identification is challenged by the possibility for parents to increase their chance of entering desired schools through residential sorting. I show that

centralised assignment breaking ties by distance can be used to isolate exogenous variation in admission, building on methods proposed by Abdulkadiroglu et al. (2021).

I show that the impact of enroling at most-preferred schools exceeds the effect predicted by average school VA. Findings imply that returns to school are heterogeneous, and that parents leverage this heterogeneity to improve quality of the student-school match. Consistently, I document striking heterogeneity in parental preference for a given school, even conditional on residential distance. Since the information on specific educational needs of students is likely private, expanding parental choice may improve the efficiency of school seats allocation.

My results are in line with surveys of parents documenting that a school's local reputation or particular needs of children are more important to parents than distance or peer quality (Francis and Hutchings 2013; Montacute and Cullinane 2018). Moreover, parents with relatively advantaged background more likely report using multiple information sources, consistently with the results of my heterogeneity analysis. A potentially important source of match effects, that could not be assessed with the data at hand, is the allocation to specific teachers. Ahn et al. (2021) find that teacher effectiveness depends on the individual characteristics of their students.

I finally note that my results refer to students without certainty of obtaining an offer. I find that parents who could not buy their way into the preferred schools through residential sorting select schools that are specifically effective for their own children. Among these parents, effects are larger for families with a relatively advantaged socioeconomic background. Although the external validity to other parents cannot be assessed by my research design, the results possibly suggest that motivated and resourced parents who set their residence next to a particular school may make productivity-enhancing choices as I find for marginal families. This may be an interesting direction for future work.

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Appendix (for on-line publication only)

A School assignment replication

I replicate centralised school assignment by running a student-proposing DA algorithm starting from data on parental preferences, distance to school and school capacity.[1]

I start by constructing a proxy for siblings at the school, which constitutes the main source of unobserved admission priority. My proxy is based on postcode of residence (in my sample, there are about 4.5 students per postcode on average) and on family-specific observable characteristics.[2] I flag an applicant as supposed sibling if a student living in the same postcode with exactly the same family-specific observables is enroled at the school of choice at the time of application. This variable provides an upper bound to the number of siblings at the school, which I adjust by ignoring (the very few) implausibly high values.[3] As a result, the 32% of first-choice applications, and 13% of all applications, are from students with supposed siblings at the school.

Several pieces of evidence suggest this measure provides a reasonable proxy to admission priority. First, one could expect strong incentives for parents to send their children at the same school. The 85% of applicants with potential siblings in just one school, indeed, rank it as first choice. Second, applicants with supposed siblings are extremely likely to be admitted. The 97% of applicants with supposed siblings obtain an offer from the most preferred school (compared to the 82% on average). Third, when the catchment boundary is estimated only based on distance, the 94% of students with a supposed sibling at their first choice who reside beyond the admission cut-off obtain an offer (compared to 66% on average). Fourth, if ranking schools involves an effort cost (Fack et al., 2019), one can expect parents with siblings at the school, who are extremely likely to obtain an offer, to express fewer preferences. The number of schools listed by supposed siblings is on average about 2.5, against 3.5 for all other applicants. Finally, the share of applicants with at least one sibling in primary school, 37%, is broadly in line with available statistics.[4]

[1] I proxy school capacity with the number of offers issued. This is a lower bound of the real capacity if a school is not oversubscribed. The distribution of school capacity looks as expected, with spikes around multiples of 30 (the statutory class size cap, see Figure A.1).

[2] I use free school meal eligibility, ethnic group, and language spoken at home, which depend on parental socioeconomic background. I observe a granular measure of ethnicity spanning 17 different groups.

[3] Specifically, I do not assign priority to students with a supposed sibling at more than two different schools (0.66% of applicants) or with more than four supposed siblings in total (3.34%). These numbers likely reflect postcode density rather than siblings at the school.

[4] 43% of sixth-grade students born in 2000-02 have a siblings in secondary school (Burgess et al., 2017).

Assignment replication proceeds in two steps. First, I replicate centralised assignment based on distance to school, parental preference, and the admission priority proxy for siblings at the school discussed above. Within priority groups, I rank applicants to a school in ascending order of distance and iteratively eliminate candidates who are eligible at schools ranked with higher preference. Catchment boundary is defined at each oversubscribed school as the distance of the last admitted applicant.[5] As some applicants with priority may remain undetected, this is not sufficient to fully replicate school offer. Panel B of Figure A.2 shows that predicted offer is wrong for almost 8% of applicants. This first step, however, provides useful information to complete the replication.

Second, I rely on the observation of the centrally assigned school offer and exploit the idea that, if an applicant located beyond the initially estimated catchment boundary receives school offer, she must have been admitted with priority. The catchment boundary estimated in the first step of my algorithm is an upper bound of the true threshold as some candidates admitted with priority remain undetected. Therefore, any school offer granted to applicants located beyond the initially estimated threshold reveals priority in admission. I flag these applicants and re-attempt the replication of school assignment by admitting them first. This procedure is iterated until no applicant with offer is found beyond the estimated catchment boundary.

In details, my algorithm works as follows.

1. Rank all applicants, regardless of their preference, by priority group and, within priority group, in ascending order of distance to school. Each student is ranked at up to 6 schools, depending on the number of schools listed. I start with two priority groups: applicants with supposed siblings, and all other students.

2. All applicants ranked within school capacity are eligible for admission at the school. If eligible at one school, the applicant is dropped from the list at all schools ranked with lower preference. This is executed sequentially preference by preference as follows.

   (a) Consider first-choice school. If an applicant is eligible, drop the applicant from the queue at schools ranked second to sixth.

   (b) Re-rank applicants at all schools considering only those retained after step (a).

[5] Catchment boundary is not defined for undersubscribed schools and religious schools.

(c) Repeat (a) and (b) analogously for second to fifth choice. In particular, if an applicant is eligible at the s-th choice, drop the applicant from the queue at all schools ranked lower than s. Retained applicants are re-ranked.

3. Repeat step 2 until no more applicant is dropped from the admission list. Assignment converges in at most 13 iterations.

4. Assign priority to applicants who are admitted to school according to administrative records but who would not receive offer based on steps 1-3.

5. Repeat steps 1-4 until no more applicants with priority are detected. The algorithm converges in less than 60 iterations.

   Steps 1-3 replicate the DA algorithm used by school districts to assign applicants to school seats. Steps 4 and 5 correct the replication by detecting applicants with unobserved priority. In each iteration, at the end of step 4, I store dummies indicating admission priority and correspondence between actual and replicated school offer. I also keep track of median catchment area boundary, defined as distance to school of the last applicant admitted. The algorithm is executed separately for both cohorts, and data are stacked across cohorts.

Algorithm performance after 60 iterations is illustrated in Figure A.2. Convergence is shown in Panel A, plotting the fraction of applicants with priority identified in each iteration. This fraction monotonically decreases to zero at an increasingly slow pace, and is virtually flat from iteration 45. At the last iteration, 0.00005% of applicants are flagged with unobserved priority. In total, about 9.5% of applications enjoy unobserved priority and these are disproportionally found at faith schools, as expected.[6] Panel B of Figure A.2, depicting errors in school assignment by iteration, shows my final assignment almost perfectly corresponds to the true school offer. At the last iteration, just 0.0002% of applicants are wrongly assigned and the correlation of predicted offer with the true offer is 99.9995%. School offer discontinuously drops at the estimated boundaries of preferred schools, as shown in Panel A of Figure A.3.

[6] At the median non-religious school, 7% of applicants have unobserved priority. This can reflect undetected siblings or, more likely, other less frequent priorities such as children of staff (commonly granted just to tenured teachers, and often subject to shortage in particular subjects).

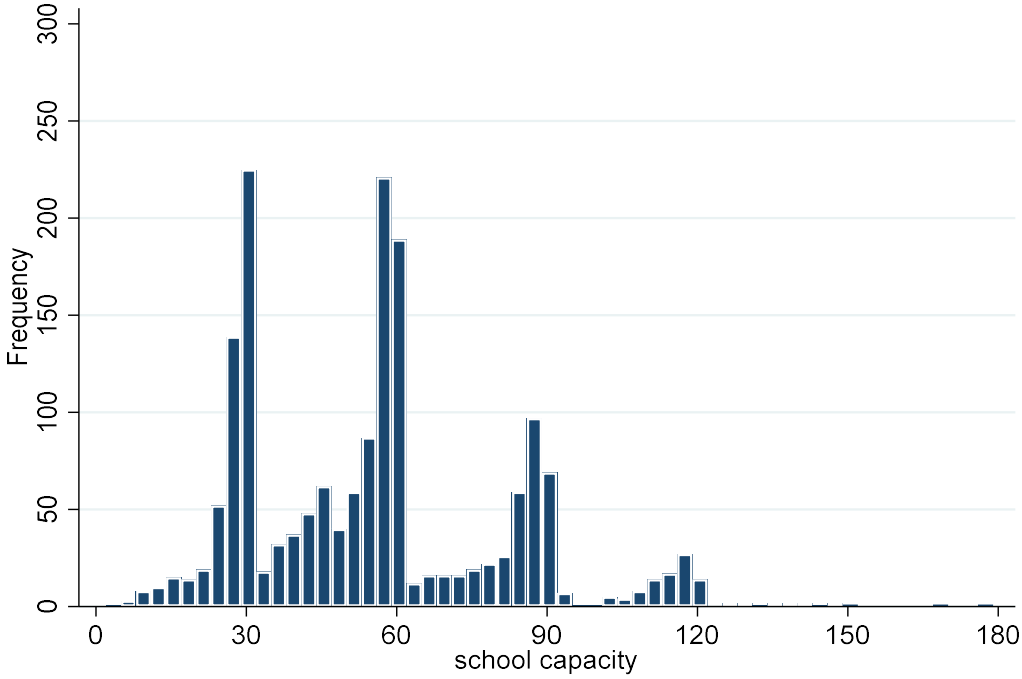
Panel B shows a similar, fuzzier, pattern for school enrolment, as expected.[7] Consistent with the idea that catchment boundary is initially overestimated, Panel C of Figure A.2 shows that median distance threshold decreases roughly monotonically as the algorithm is iterated. When assignment replication is concluded, the estimated median catchment boundary at oversubscribed schools is about 470 meters.[8]

Catchment boundary is stable over time on average, but the change in distance threshold exhibits remarkable dispersion. Among the 705 non-faith schools that are oversubscribed in both years 2014 and 2015, the catchment boundary shrinks by 10 meters on average. For half of institutions the year-on-year change is within 200 meters, and it is within 500 meters for the 80% of schools. Figure A.4 depicts catchment boundary in the two years considered, focusing on the 576 schools with distance thresholds within 1 km in both periods. Markers are weighted proportionally on enrolment count. Despite most schools lie around the 45-degree line, benchmarking an unchanged boundary, a substantial fraction of schools exhibit 2016 boundaries sensibly above or below the 2015 counterpart. These changes, caused by fluctuations in application and residential choices across cohorts, have no predictable direction and are unlikely to be anticipated by parents.

[7] The figures consider only applicants with marginal priority, that is the same admission priority of the last-admitted applicant. In addition, for schools ranked second or lower, it considers only applicants who are not offered a more-preferred school. This selection yields the sub-group of applicants for which offer is sharply determined by the catchment boundary.

[8] A minor concern is that catchment boundary is measured with error if the last admitted applicant enjoys unobserved admission priority. In that case, the correct cut-off is the distance of the applicant located immediately closer to the school. Density of applicants in London implies that such error is at most very small. Nevertheless, catchment boundary is constant for all applicants to a given school and measurement error cancels out when comparing students around the cut-off.

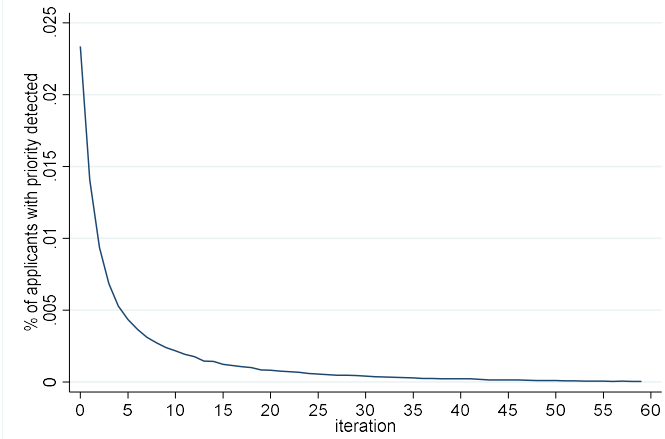
Figure A.1: School capacity



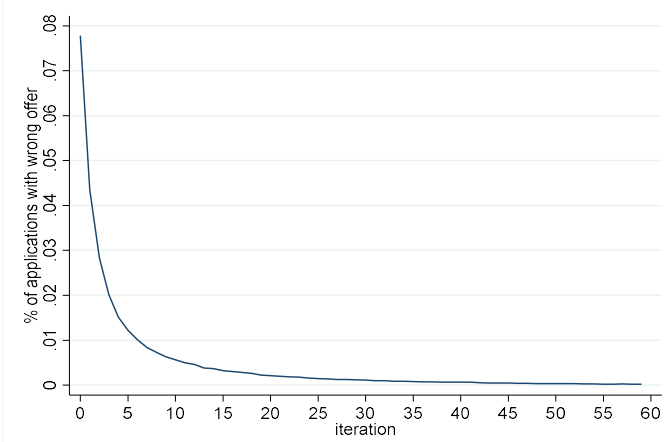
Note. The figure plots the distribution of school capacity in London primary schools. Capacity is approximated by the number of offers issued. Bars represent frequency counts in three-units-wide bins, computed using one observation per school-year. See Section 3 and Appendix A for details.

Figure A.2: Replication of school assignment

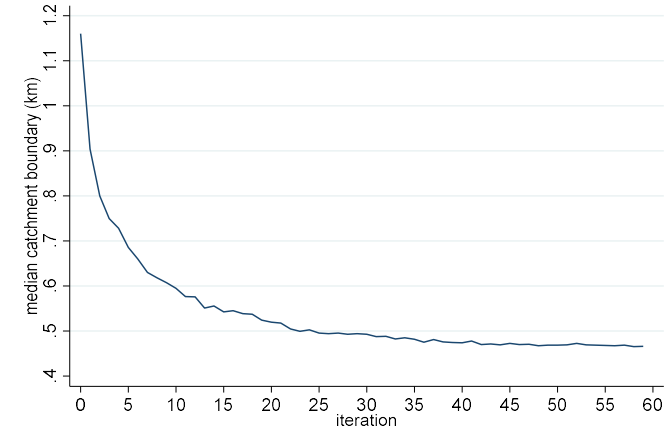
Panel A. Unobserved priorities



Panel B. Error in school offer replication

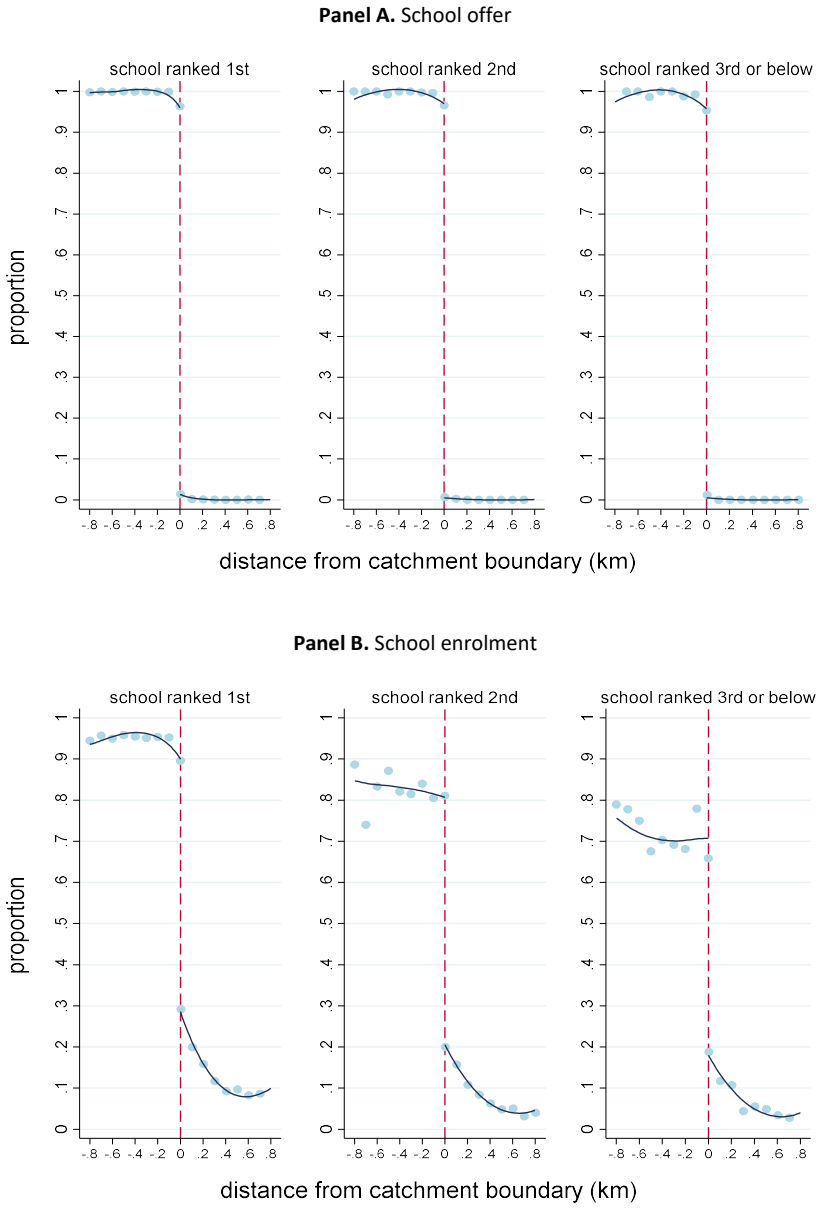


Panel C. Estimated catchment area boundary



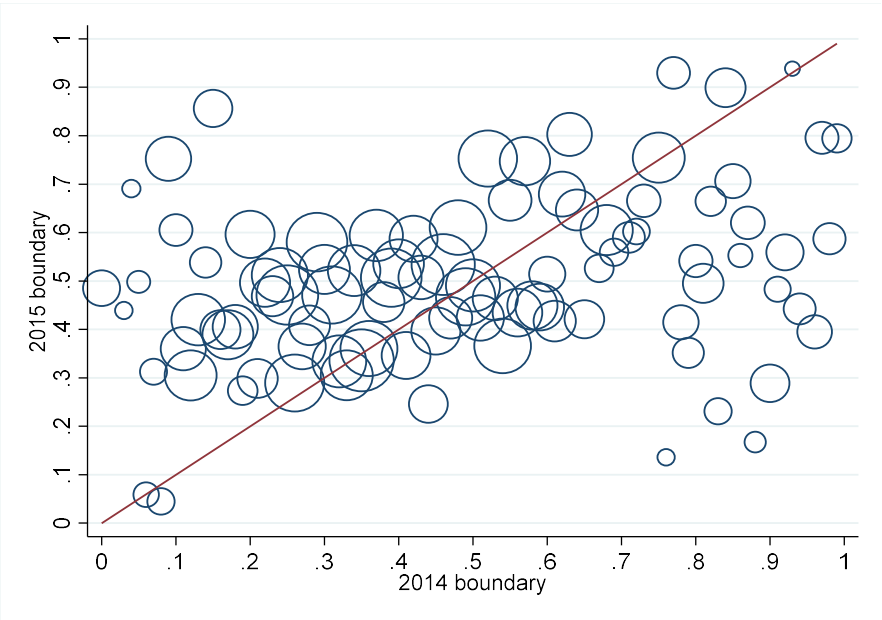
Note. The figure plots the fraction of applicants with admission priority detected (Panel A), the fraction of applicants with wrong predicted offer (Panel B), and median estimated catchment boundary (Panel C) by iteration of the school assignment replication algorithm. The sample includes all applicants to at least one London primary school in 2014 or 2015. Applicants are ranked by sibling status (proxied as detailed in Appendix A) and proximity in iteration 0. Applicants with offer beyond estimated boundary are then flagged as enjoying priority. Subsequent iterations rank pupils by priority as retrieved in the previous round, sibling status, and proximity. Reported are average figures across the two cohorts considered. See Section 3 and Appendix A for details.

Figure A.3: Tie-breaking



Note. The figure depicts school offer (Panel A) and enrolment (Panel B) around catchment boundary for schools ranked first, second and third or below at application. Enrolment is measured at the reception year. Catchment boundary is defined only for oversubscribed non-faith schools. Distance to school catchment boundary is represented on the horizontal axis and defined subtracting distance of the last admitted candidate to an applicant's distance to school. Negative values indicate residence within catchment. Markers represent average values in 100-meters-wide bins of distance from the boundary and the solid line is a local linear fit of underlying observations, estimated separately on either side of the cut-off. The sample is restricted to applicants within 800 meters from the catchment boundary and to applicants at risk of admission at the school, i.e. those with marginal admission priority and not eligible at any school ranked higher. See Section 3 and the Appendix for details.

Figure A.4: Year-on-year change in catchment boundaries



Note. The figure plots estimated catchment area boundary of oversubscribed, non-faith state schools in 2014 and 2015. Boundaries are traced by replicating the centralised assignment mechanism for all state primary schools in London. Reported is average 2016 boundary as function of 10-meter-wide bins of 2015 boundary. Markers show one observation per school, with size proportional to enrolment count. The 45-degree line, indicating unchanged catchment boundary, is reported in red. Sample is restricted to the 576 schools with catchment boundary within 1Km in both years and issuing at least 30 offers (attracting the 46% of appications, and the 74% among oversubscribed schools). See Appendix A for details.

B Estimation of school VA

Estimates of school VA are employed in my empirical analysis to hold school quality constant. Following Deming et al. (2014), I compute school average residuals from a student-level regression of standardised KS1 assessments on observable characteristics and baseline achievement. Specifically, I consider previous cohorts of achievement data and estimate the following regression model for student i enroled at school s in Year 2 at time t:

$$Y_{ist} = \eta_o + X_i'\eta_1 + \eta_2 Y_{is,t-2} + \delta_t + \epsilon_{ijt}, \tag{11}$$

where the vector of controls X_i' includes dummies for gender, language, ethnicity, free lunch eligibility, special education needs and local area of residence (MSOA), and δ_t are year dummies. I additionally control for lagged achievement using EYFSP assessments.[9] In this model, estimates of η_1 and η_2 proxy the impact of individual ability on student achievement (ν_i in equation 1). I estimate school VA as institution-level average of the residuals from this regression ($\hat{\epsilon}_{ijt}$):

$$\hat{\alpha}_s = \frac{\sum_t \sum_i \hat{\epsilon}_{ist}}{\sum_t N_{st}},$$

where N_{st} is the number of students enroled at school s. By comparing equation (11) with (1), it can be seen that regression residuals capture both school VA and average match effects at the school:

$$\hat{\alpha}_s = \alpha_s + \bar{\mu}_s,$$

where $\bar{\mu}_s = 1/N \sum_i \mu_{is}$. VA estimates are comparable across institutions only if the school average match effect is constant. Under the hypothesis that match effects are an increasing function of parental preference, this requires the average rank assigned by parents of enroled students to be constant across schools. Variation in the latter quantity is likely limited in my empirical analysis, as only oversubscribed schools contribute to the identification. Nonetheless, with large differences in school popularity among parents, my estimates would constitute lower bounds of the true match effects as school VA would be inflated at schools with higher average parental preference. Results from my alternative design exploiting within-school vari-

[9] Specifically, I average results over the 17 learning goals assessed. As EYFSP are administered during the reception year, they could be influenced by school quality. However, they are meant to inform teachers and parents on student educational needs, and schools are not held accountable on the basis of EYFSP results. They represent therefore a good proxy of student academic ability at school entrance. I consider the 3 cohorts up to 2014 due to availability of lagged achievement.

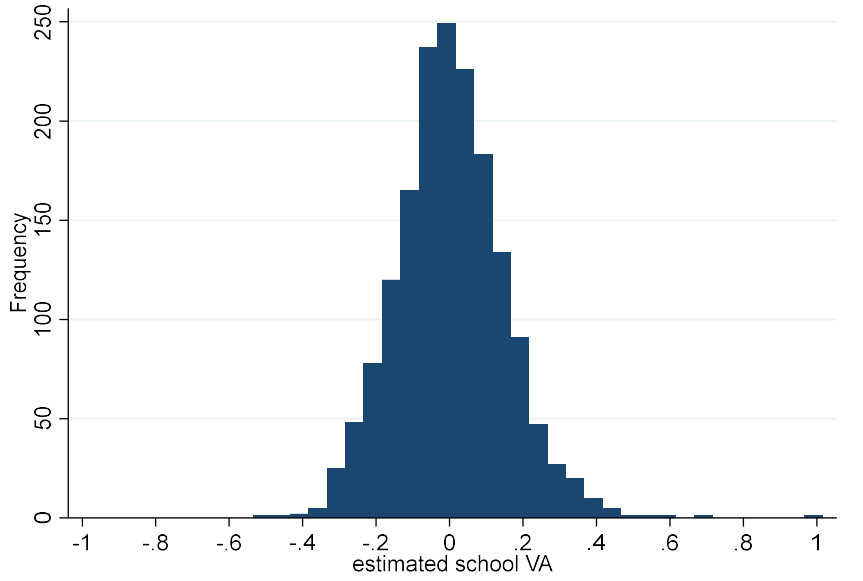
ation are similar to the main findings (see Section 5), suggesting that, in practice, differences in average match effects across schools do not drive my results.

One σ higher VA improves the probability of scoring above standards at Year 2 by about 5.5 p.p. (18% of the sample average), corresponding to 0.15σ higher achievement. The magnitude of school VA dispersion is comparable to what found in related studies (Angrist et al., 2017; Abdulkadiroglu et al., 2020; Angrist et al., 2021). The distribution of school VA is plotted in Figure B.1. Estimated school quality has about 17% correlation with absolute achievement, suggesting high-performing schools are not necessarily highly effective.

VA is estimated separately by subject – mathematics, reading, and writing – and the latter two estimates are averaged to obtain school VA in English. To estimate match effects in equations (9) and (18), I consider school VA in the same subject of the outcome variable considered. When a given preference is not expressed (e.g., the fourth choice for students ranking only three schools), I estimate expected VA gains in equation (18) by considering VA of the closest ex-post feasible school that the student has not ranked.[10]

[10]See below for details on the construction of feasible choice set.

Figure B.1: Estimated school VA



Note. This figure plots the distribution of estimated school value-added. Reported are frequency counts in 0.05-wide bins using one observation per school. Value-added is estimated at baseline using KS1 scores in 2014-2016. Student-level regressions use dummies for scoring above the expected standards at KS1 as dependent variables and control for language, ethnicity, free school meal eligibility, specialeducation needs, gender, dummies for the MSOA of residence, and achievement at the reception year (EYFSP). The outcome is standardised to have zero mean and unit variance by year. Value-added is computed as school-level residuals from the regression, separately by subject. Plotted is average value-added across subjects. See Appendix B for details.

C Estimation of assignment risk

I use information on preferences, priorities, and distance for all applications to estimate their risk of receiving an offer. Following Abdulkadiroglu et al. (2021), I proceed in three steps by considering the centralised assignment inputs one at a time.

Assignment risk depends, first, on admission priority. The key priority group to assess admission chance at a given school is the one of the last admitted student, defined as *marginal priority*. Applicants with higher than marginal priority (e.g., siblings of current students) receive an offer with certainty, while applicants with lower than marginal priority are never admitted. Assignment risk, therefore, is non-degenerate only for applicants with marginal priority. Based on priority status obtained in Section 3, students in my sample have marginal priority in 48% of applications.[11]

Conditional on priority, assignment risk depends on the value of the tie-breaker. Applicants located sufficiently close to the catchment boundaries face equal and non-degenerate chance of admission. On the contrary, applicants located comfortably within the boundary receive an offer with certainty, while those located well beyond the boundary are never admitted. To approximate this idea, applicants are grouped based on a narrow distance bandwidth around the cut-off.[12] Let τ_s be the catchment boundary at school ranked s-th and let δ denote the optimal bandwidth. Applicants can be partitioned in three groups based on their chance of admission:

- student i with marginal priority at the s-th choice is *conditionally seated* if $d_{is} \in [\tau_s - \delta, \tau_s + \delta]$,
- is *always seated* if her priority is higher than marginal or $d_{is} < (\tau_s - \delta)$,[13]
- is *never seated* otherwise.

[11] Admission priority is likely observed with significant error for applicants at faith schools (see Section 3). Therefore, I do not exploit faith schools in estimation. Specifically, I assign to faith school applications higher- or lower-than-marginal priority depending on offer status so that they do not generate assignment risk.

[12] I use catchment boundaries obtained in Appendix A and select the optimal data-driven bandwidth proposed by Calonico et al. (2014). Following Abdulkadiroglu et al. (2021), I select the minimum bandwidths across outcomes (achievement in mathematics and English) separately for each cohort, obtaining values of 333 and 243 meters in 2015 and 2016, respectively.

[13] Note that all applicants to undersubscribed schools are always seated as $\tau_s = \infty$.

Students in my sample are conditionally seated in about 12% of applications.

Finally, assignment risk depends on parental preferences. In particular, an applicant who would be admitted based on priority and distance, will not receive an offer if she is eligible at more-preferred institutions. I compute for each application the number of schools ranked higher than s where a student is conditionally (B^c_{is}) or always seated (B^a_{is}) based on the definition above.

The assignment risk of student i at the s-th-choice school is:[14]

- $p_{is} = 0$ if $B^a_{is} > 0$ or i is never seated,
- $p_{is} = 0.5^{B^c_{is}}$ if i is always seated,
- $p_{is} = 0.5^{(1+B^c_{is})}$ if i is conditionally seated.

[14]This formula derives as a particular case of Theorem 1 in Abdulkadiroglu et al. (2021) where the *most informative disqualification* is zero. In my context, indeed, tie-breakers are school-specific at all institutions.

Table C.1: Descriptive statistics (estimation sample)

	All applicants		Applicants at risk	
	Mean	SD	Mean	SD
	(1)	(2)	(3)	(4)
Baseline Characteristics				
FSM eligible	0.1516	0.3587	0.1395	0.3465
Not speaking English at home	0.4184	0.4933	0.4007	0.4900
White	0.4167	0.4930	0.4092	0.4917
Asian	0.1946	0.3959	0.2035	0.4026
Black	0.1617	0.3681	0.1266	0.3325
Special education needs	0.0077	0.0873	0.0083	0.0908
Female	0.4901	0.4999	0.4907	0.4999
Deprivation index (LSOA)	0.2886	0.1631	0.2763	0.1623
% with higher education (LSOA)	0.3391	0.1300	0.3682	0.1352
School choice variables				
N. of schools listed	3.2069	1.8733	3.6527	1.8250
Offered 1st choice	0.8280	0.3773	0.6791	0.4668
Enroled at offered school at reception year	0.8649	0.3419	0.8824	0.3222
N	190,493		24,045	

Note. This table shows descriptive statistics about applicants to at least one mainstream state-funded primary school in Greater London in 2014 and 2015. Columns (1)-(2), report averages and standard deviations for all applicants. Columns (3)-(4) consider applicants with non-deterministic assignment risk at one or more listed schools. All statistics are conditional on non-missing observations. See Section 5 and Appendix C for details.

D Imperfect compliance

I start here by maintaining the assumption that offers are randomly assigned, and relax this hypothesis below. I also maintain the assumption that students enrol to one of the ranked schools throughout this Appendix. The notation can be extended to include enrolment at non-ranked schools using the same reasoning.

Let $D_i = \{1, \ldots, 6\}$ be the preference rank for the school where student i enrols. With imperfect compliance, this might differ from the preference rank for the offered school, Z_i. The observed outcome for student i is:

$$Y_i = \sum_{d=1}^{6} \mathbb{1}(D_i = d) \cdot Y_{is(i,d)}$$

Let $D_i(z)$ denote potential enrolment as a function of school offer, representing the preference rank for the school where student i enrols if she receives an offer from her z-th choice. Let $\tilde{Y}_i \equiv Y_i - \hat{\alpha}_{s(i)}$ denote the VA-adjusted achievement of student i enroled at school s (see Appendix B for details on estimation of school VA). The comparison in the left-hand-side of equation (3) yields:

$$\begin{aligned} E \quad & [\tilde{Y}_i | Z_i = 1] - E[\tilde{Y}_i | Z_i = 2] = \\ = \quad & E[\sum_{d=1}^{6} \mathbb{1}(D_i(1) = d) \cdot \tilde{Y}_{is(i,d)} | Z_i = 1] - E[\sum_{d=1}^{6} \mathbb{1}(D_i(2) = d) \cdot \tilde{Y}_{is(i,d)} | Z_i = 2] = \\ = \quad & E[\sum_{d=1}^{6} \mathbb{1}(D_i(1) = d) \cdot \tilde{Y}_{is(i,d)} - \sum_{d=1}^{6} \mathbb{1}(D_i(2) = d) \cdot \tilde{Y}_{is(i,d)}], \end{aligned} \tag{12}$$

where the latter equality uses random assignment of offers.

A plausible monotonicity assumption is imposed to identify the parameter of interest. Assume that students do not enrol at a school with lower parental preference than the one they are offered, $D_i(z) \leq z \, \forall z = 1, \ldots, 6$. This assumption implies $D_i(1) = 1$ and $D_i(2) = \{1, 2\}$. Let $C_i \equiv \prod_{z=1}^{6} \mathbb{1}(D_i(z) = z)$ denote compliance with school offer. The expression in (12) equals:

$$\begin{aligned} E[\tilde{Y}_{is(i,1)} - \tilde{Y}_{is(i,2)} | C_i = 1] \cdot P(C_i = 1) \quad & = \\ = E[\mu_{is(i,1)} - \mu_{is(i,2)} | C_i = 1] \cdot P(C_i = 1) \quad & , \end{aligned} \tag{13}$$

where the latter equality uses random assignment of offers. To rescale the reduced form comparison by the proportion of compliers, I consider 2SLS models where enrolment is in-

strumented by school offer. Let $D_{i1} \equiv \mathbb{1}(D_i = 1)$ and $Z_{i1} \equiv \mathbb{1}(Z_i = 1)$ be dummy variables indicating enrolment at and offer from the first choice, respectively. Instrumenting D_{i1} with Z_{i1} implements the following comparison:

$$\frac{E[\tilde{Y}_i|Z_{i1}=1] - E[\tilde{Y}_i|Z_{i1}=0]}{E[D_{i1}|Z_{i1}=1] - E[D_{i1}|Z_{i1}=0]} \quad = \quad E[\mu_{is(i,1)} - \sum_{z=2}^{6} \mathbb{1}(Z_i = z) \cdot \mu_{is(i,z)}|C_i = 1]. \quad (14)$$

Under the simplifying assumption that students missing out on the first choice are offered the second choice, the last equation identifies the average match effect of enroling at the first choice relative to the second choice for school offer compliers. Similar comparisons can be defined for schools ranked less than first choice.

Although offers are not randomised, they are as-good-as random conditional on assignment risk. The average match effect among compliers is identified by a weighted average of conditional versions of the comparison in equation (14):

$$\int \frac{E[\tilde{Y}_i|Z_{i1}=1, \boldsymbol{p_i}] - E[\tilde{Y}_i|Z_{i1}=0, \boldsymbol{p_i}]}{E[D_{i1}|Z_{i1}=1, \boldsymbol{p_i}] - E[D_{i1}|Z_{i1}=0, \boldsymbol{p_i}]} \cdot d(\boldsymbol{p_i}) \quad = \quad E[\mu_{is(i,1)} - \mu_{is(i,2)}|C_i = 1]. \quad (15)$$

E Alternative approach to hold VA constant

Consider comparing un-adjusted achievement of students with offer from their first or second choice. Using equation (2):

$$E[Y_i|Z_i = 1] - E[Y_i|Z_i = 2] \quad = \quad E[\alpha_{s(i,1)} - \alpha_{s(i,2)}] + E[\mu_{s(i,1)} - \mu_{s(i,2)}], \quad (16)$$

where I assume that school offers are randomised. The comparison in equation (16) conflates the match effect from attending the first relative to the second choice ($\mu_{is(i,1)} - \mu_{is(i,2)}$) and the gain in school VA ($\alpha_{s(i,1)} - \alpha_{s(i,2)}$). The parameter of interest is the last term of this equation, representing the average match effect from attending the first relative to the second choice.

One possible strategy to hold school VA constant is to adjust observed outcomes by school VA, as in equation (3). An alternative strategy is to focus on applicants whose counterfactual school has the same quality:

$$E[Y_i|Z_i = 1, \alpha_{s(i,1)} = \alpha_{s(i,2)}] - E[Y_i|Z_i = 2, \alpha_{s(i,1)} = \alpha_{s(i,2)}] = E[\mu_{s(i,1)} - \mu_{s(i,2)}]. \quad (17)$$

To implement this comparison, I consider un-adjusted achievement and augment equation (9) by adding an interaction of school enrolment with the expected VA gain. Let $\hat{\alpha}_{s(i,r)}$ be

the estimated VA of the school ranked r-th by student i. First, I compute the average VA of schools ranked lower than second choice, $\bar{\hat{\alpha}}_i = (\sum_{r=3}^{6} \hat{\alpha}_{s(i,r)})/4$. Expected VA gains from entering the top two choices are then obtained as $(\hat{\alpha}_{is(i,r)} - \bar{\hat{\alpha}}_i)$, for $r = 1, 2$. Finally, match effects are estimated by θ_1 and θ_2 in:

$$Y_{is(i,r)} = \sum_{r=1}^{2} \theta_r D_{is(i,r)} + \sum_{r=1}^{2} \psi_r D_{is(i,r)} \times (\hat{\alpha}_{s(i,r)} - \bar{\hat{\alpha}}_i) + f^p(\boldsymbol{p_i}) + f^d(\boldsymbol{d_i}) + u_{4i}, \tag{18}$$

with analogue first stage equations. Estimates of θ_1 and θ_2 corresponds to the comparison in equation (17).

F Robustness checks

I consider two further exercises to test the robustness of my results. First, an important empirical choice in my approach is the bandwidth used to estimate assignment risk (see Appendix C). Estimates of match effects on achievement in mathematics under different bandwidth choices are reported in Table F.1. Columns (1)-(3) estimate match effects by using VA-adjusted outcomes (similarly to Table 3), while columns (4)-(6) hold expected VA gains constant (similarly to Table 4). The optimal data-driven bandwidths chosen for my main analysis are 333 meters and 243 meters in 2015 and 2016, respectively. Columns (1) and (4) consider a unique bandwidth equal to 300 meters across cohorts. This is close to the baseline bandwidth on average, as reflected in the similar sample size. As expected, results in columns (1) and (4) of in Table F.1 are similar to estimates in column 2 of Table 3 and Table 4, respectively. The baseline bandwidth is increased by 25% in columns (2) and (5) of Table F.1, increasing the sample size proportionally. Results are somewhat smaller, but very similar to the baseline estimates. Finally, the baseline bandwidth is decreased by 25% in columns (3) and (6), decreasing the sample size proportionally. Results are somewhat larger, but again very similar to the baseline estimates.

Second, I test the sensitivity of my results to the exclusion of local authorities where unobserved admission priorities are more likely. I replicate centralised assignment by using school offer to correct for unobserved priorities (see Appendix A). In columns (1) and (3) of Table F.2, I exclude from estimation the four districts granting admission priority to students residing within pre-defined catchment areas. In columns (2) and (4), I exclude the

five districts breaking ties by walking distance rather than straight-line distance.[15] Both these institutional settings may generate larger errors in the estimation of assignment risk. Nonetheless, estimates are similar to baseline findings, suggesting that different admission arrangements do not affect my results.

[15]In summer 2021, the local authorities of Barnet, Brent, Hillingdon, and Redbridge prioritise students using pre-defined catchment areas; the local authorities of Tower Hamlets, Hounslow, Newham, Redbridge and Richmond Upon Thames break ties by walking distance.

Table F.1: Robustness to the choice of bandwidth

	VA-corrected KS1 score in mathematics			KS1 score in mathematics		
	bw = 300m	bw = 1.25Xbaseline	bw = 0.75Xbaseline	bw = 300m	bw = 1.25Xbaseline	bw = 0.75Xbaseline
	(1)	(2)	(3)	(4)	(5)	(6)
			Panel A. Two-choice model			
Enroled in 1st choice	0.1252**	0.1095**	0.1295**	0.1336***	0.1169**	0.1387**
	(0.0522)	(0.0472)	(0.0611)	(0.0518)	(0.0469)	(0.0608)
Enroled in 2nd choice	0.1017**	0.0756*	0.1020*	0.1059**	0.0783*	0.1056*
	(0.0465)	(0.0420)	(0.0561)	(0.0463)	(0.0417)	(0.0559)
Enroled in 1st choice X school value-added gain				0.0164***	0.0194***	0.0176***
				(0.0058)	(0.0053)	(0.0067)
Enroled in 2nd choice X school value-added gain				0.0198**	0.0172**	0.0234**
				(0.0078)	(0.0070)	(0.0092)
			Panel B. Three-choice model			
Enroled in 1st choice	0.1586**	0.1085*	0.1587**	0.1750***	0.1250**	0.1801**
	(0.0643)	(0.0570)	(0.0769)	(0.0641)	(0.0568)	(0.0767)
Enroled in 2nd choice	0.1334**	0.0714	0.1336*	0.1389**	0.0785	0.1463**
	(0.0588)	(0.0518)	(0.0717)	(0.0586)	(0.0515)	(0.0714)
Enroled in 3rd choice	0.0745	-0.0309	0.0847	0.0836	-0.0215	0.1091
	(0.0722)	(0.0639)	(0.0896)	(0.0716)	(0.0634)	(0.0893)
Enroled in 1st choice X school value-added gain				0.0151**	0.0168***	0.0159**
				(0.0063)	(0.0057)	(0.0074)
Enroled in 2nd choice X school value-added gain				0.0225***	0.0180**	0.0242**
				(0.0082)	(0.0075)	(0.0099)
Enroled in 3rd choice X school value-added gain				-0.0057	0.0099	-0.0253
				(0.0160)	(0.0146)	(0.0200)
N	22,943	25,909	17,877	23,079	26,059	17,965
First and second choice FEs						
Tie-breaker controls	Y	Y	Y	Y	Y	Y
Assignment risk (ranked schools)	Y	Y	Y	Y	Y	Y
Assignment risk (all schools)	Y	Y	Y	Y	Y	Y
Individual characteristics	Y	Y	Y	Y	Y	Y

Note. The table explores robustness of estimated match effects at the most-preferred schools by bandwidth choice. Columns (1)-(3) report estimates from specifications analogue to column (2) of Table 3. Columns (4)-(6) report estimates from specifications analogue to column (2) of Table 4. Bandwidth chosen to estimate assignment risk is 300 meters for all cohorts in columns (1) and (4); it is equal to the baseline bandwidth (333 and 243 in 2015 and 2016, respectively) increased by 25% in columns (2) and (5); it is equal to the baseline bandwidth decreased by 25% in columns (3) and (6). See Appendix E for details. ***p<0.01. ** p<0.05. * p<0.1

Table F.2: Robustness to unobserved admission priorities

	VA-corrected KS1 score in mathematics		KS1 score in mathematics	
	LAs not using pre-defined catchment	LAs using straight-line distance	LAs not using pre-defined catchment	LAs using straight-line distance
	(1)	(2)	(3)	(4)
	Panel A. Two-choice model			
Enroled in 1st choice	0.0987*	0.1275**	0.1068*	0.1351**
	(0.0566)	(0.0583)	(0.0562)	(0.0579)
Enroled in 2nd choice	0.0866*	0.0652	0.0918*	0.0716
	(0.0516)	(0.0521)	(0.0512)	(0.0517)
Enroled in 1st choice X school value-added gain			0.0153**	0.0169***
			(0.0064)	(0.0064)
Enroled in 2nd choice X school value-added gain			0.0229***	0.0325***
			(0.0086)	(0.0086)
	Panel B. Three-choice model			
Enroled in 1st choice	0.1209*	0.1316*	0.1345*	0.1472**
	(0.0696)	(0.0705)	(0.0693)	(0.0702)
Enroled in 2nd choice	0.1190*	0.0718	0.1272*	0.0807
	(0.0653)	(0.0648)	(0.0650)	(0.0645)
Enroled in 3rd choice	0.0570	0.0042	0.0712	0.0232
	(0.0796)	(0.0800)	(0.0791)	(0.0795)
Enroled in 1st choice X school value-added gain			0.0142**	0.0157**
			(0.0069)	(0.0069)
Enroled in 2nd choice X school value-added gain			0.0234**	0.0338***
			(0.0092)	(0.0091)
Enroled in 3rd choice X school value-added gain			0.0065	0.0139
			(0.0174)	(0.0175)
N	19,037	18,565	19,127	18,673
First and second choice FEs	Y	Y	Y	Y
Tie-breaker controls	Y	Y	Y	Y
Assignment risk (ranked schools)	Y	Y	Y	Y
Individual characteristics	Y	Y	Y	Y

Note. The table explores robustness of estimated match effects at the most-preferred schools by admission priorities. Columns (1)-(2) report estimates from specifications analogue to column (2) of Table 3. Columns (3)-(4) report estimates from specifications analogue to column (2) of Table 4. Columns (1) and (3) exclude from estimation the four local authorities prioritising students living in pre-defined catchment areas. Columns (2) and (4) exclude from estimation the four local authorities breaking ties by walking, rather than straight-line, distance. See Appendix E for details. ***p<0.01. ** p<0.05. * p<0.1

G Construction of individual feasible school set

I define the individual feasible school set exploiting school catchment boundaries obtained from replication of centralised school assignment (see Section 3 and Appendix A). I compute linear distance between student postcode and all primary schools, including those not ranked by parents. Specifically, I pair each student with all schools ranked by at least one applicant residing in the same school district. This mild restriction ensures computational feasibility, as there are about 200,000 applicants and 1,750 schools in my sample. I view this as a natural assumption since 93.2% of students enrol in the LA of residence.

I define a school as ex-post feasible if the student resides within the catchment boundary or if the school remained undersubscribed. I exclude religious schools from choice set since I do not accurately observe ex-post feasibility for these institutions. Moreover, even when applicants reside beyond the catchment boundary, a school is considered feasible if the student has a currently enrolled sibling (proxied as detailed in Appendix A) or the student is offered a seat. A school is included in the individual choice set if located within 2 km from student residence, corresponding to the 90th percentile of distance to school. The individual feasible school set is defined as the collection of ex-post feasible schools.

H Parental choice and school mobility

I have shown in Section 6 that the decision to change school after reception year is associated with parental preference for the offered school (see Figure 6). I further show here that school mobility responds to peer quality rather than school VA, consistently with parental preferences for schools. Panel A of Table H.2 presents estimates from regressions of school mobility on relative attributes of the most-preferred school and the school where student enrols. When a student's school falls short of the first choice in terms of peer quality by 1σ, the likelihood of moving to another school by Year 2 increases by 5-6 p.p. Controlling for school choice covariates or individual characteristics barely affects the estimates (columns 2 and 3). On the contrary, the estimated coefficient on school VA is much lower, at 0.8 p.p. in column 1, and it decreases when including further controls (columns 2 and 3). Residential mobility, likely involving larger costs, is almost unrelated to peer quality and VA (Panel B).

School mobility is more likely when students are assigned a school closer to the place of

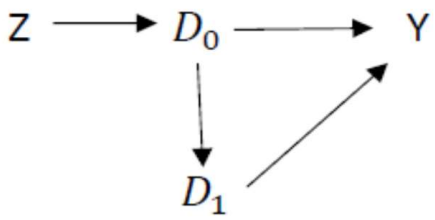
residence than their first choice. I estimate that the likelihood of moving to a different school increases by 1.2 p.p when a distance to school is 1km lower with respect to the first choice (Panel A of Table H.2). Although even smaller, the effect of distance to school on residential mobility is also statistically significant (Panel B). Findings suggest that parents willing to travel further distances to their first choice face particularly unsatisfactory schools at close proximity from residence, and are slightly more likely to move to different schools (or even local areas) when assigned to the latter.

Figure 6 suggests that students with offer from the most-preferred schools are less likely to move to a different institution after the reception year. Table H.2 reports estimates from specifications analogue to Table 3 where school mobility indicator is the dependent variable. Panel A shows that, consistently across different specifications, students offered their first choice are 9 p.p. less likely to move to a different school with respect to peers offered their third choice or lower, decreasing to 5 p.p. for the second choice (mobility rate for students without offer from the two most-preferred choices is about 0.2).

School mobility response to centralised assignment is unlikely to affect the interpretation of my results. Student achievement is observed at Year 2, the third year of primay education (see Section 3). Figure H.1 illustrates the relationship between student achievement (Y) and parental preference for the offered school (Z), the school where students initially enrols (D_0), and the school where student enrols at Year 2 (D_1) in a directed acyclic graph (DAG, Abadie and Cattaneo, 2018). The figure considers students with the same assignment risk so that school offer is good as randomly assigned (see Section 4) and affects student achievement only through initial enrolment. In turn, initial enrolment may prompt parental response in the form of increased school mobility, impacting later enrolment. If school mobility has an impact on student achievement, the effect of initial enrolment combines two different channels: the direct impact of the school where the student initially enrols and the indirect impact of school mobility. A potential concern is that school mobility may constitute an alternative explanation of my results. I argue that my findings are not driven by school mobility for several reasons. First, almost no student moves to more-preferred schools than the one offered after the reception year (Figure 6, Panel C), implying that school mobility does not weaken the association of centralised offers with parental preference for the school where student enrols. Second, school mobility is unrelated to school VA (Table H.1, Panel

A). Third, school mobility response to centralised offers is a parental decision. If, consistently with my results, this tends to improve the student-school match, my estimates would possibly be pushed downward.

Figure H.1: Offer, enrolment and outcome in a DAG



Note. The relationship between instrument, treatment and outcome in a directed acyclic graph. The graph includes parental preference rank for the school offered (Z), the school where student enrols at the reception year (D0), and the school where student enrols at Year 2 (D1); and student achievement (Y). See Appendix H for details.

Table H.1: School mobility and school attributes

	(1)	(2)	(3)
First choice VS student's school:			
	Panel A. Dep. Var.: School mobility		
Peer quality difference	0.0595***	0.0599***	0.0479***
	(0.0027)	(0.0032)	(0.0036)
School value added difference	0.0083***	0.0059**	0.0035
	(0.0026)	(0.0029)	(0.0030)
Distance difference	0.0125***	0.0126***	0.0124***
	(0.0011)	(0.0012)	(0.0012)
N	127,887	107,893	103,176
	Panel B. Dep. Var.: Residential mobility		
Peer quality difference	0.0001	0.0017	-0.0009
	(0.0020)	(0.0024)	(0.0028)
School value added difference	0.0038*	0.0014	0.0013
	(0.0020)	(0.0023)	(0.0024)
Distance difference	0.0061***	0.0060***	0.0060***
	(0.0009)	(0.0010)	(0.0010)
N	124,501	114,299	111,243
School choice controls		Y	Y
Individual characteristics			Y

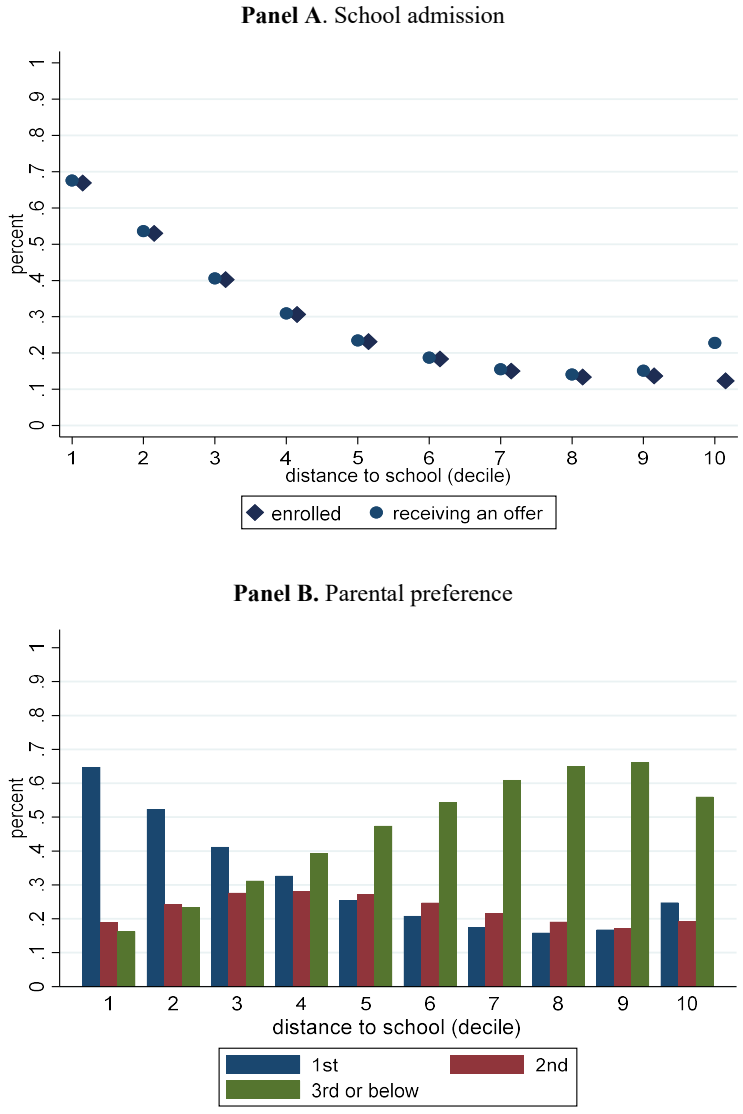
Note. The table shows correlation between school mobility and school attributes. Sample is restricted to first-choice applications. Reported in Panel A are estimates from linear regressions of school mobility indicator, equal to one if a student moves to another school between reception year and Year 2. Dependent variable in Panel B is an indicator variable equal to 1 if a students moves residence (observed as home postcode). Independent variables are the differences between characteristics of the first choice and the school where student enrols. School value-added is estimated by regression-adjusted test scores growth at the school and averaged across subjects (see Appendix B). Peer quality is measured by school-level final year test scores, averaged across 2007-2014 cohorts and across mathematics and English. Distance, measured in kilometers and computed as linear distance between student postcode and school postcode centroids (schools further than 2 kilometers from residence, the 90th percentile, are not considered).Control variables include year dummies. Column (2) adds dummies for n. of schools listed and ex-post feasibility of the school. Column (3) adds individual socioeconomic characteristics: gender, free lunch eligibility, special education needs, ethnicity, language, deprivation index and level of education in the area of residence. Robust standard errors are reported in parentheses. See Appendix G for details. *** p<0.01, ** p<0.05, * p<0.1

Table H.2: School mobility by offer status

	"preference" specification		"score" specification	
	(1)	(2)	(3)	(4)
	Panel A. Two-choice model			
Enroled in 1st choice	-0.0903***	-0.0906***	-0.0914***	-0.0911***
	(0.0139)	(0.0138)	(0.0125)	(0.0125)
Enroled in 2nd choice	-0.0570***	-0.0572***	-0.0506***	-0.0508***
	(0.0133)	(0.0133)	(0.0112)	(0.0112)
N	23,342	23,342	23,619	23,619
	Panel B. Three-choice model			
Enroled in 1st choice	-0.1150***	-0.1148***	-0.1053***	-0.1048***
	(0.0158)	(0.0158)	(0.0138)	(0.0138)
Enroled in 2nd choice	-0.0770***	-0.0768***	-0.0653***	-0.0653***
	(0.0157)	(0.0157)	(0.0128)	(0.0128)
Enroled in 3rd choice	-0.0602***	-0.0596***	-0.0391**	-0.0387**
	(0.0187)	(0.0187)	(0.0153)	(0.0153)
N	23,187	23,187	23,619	23,619
First and second choice FEs	Y	Y		
Tie-breaker controls	Y	Y	Y	Y
Assignment risk (ranked schools)	Y	Y		
Assignment risk (all schools)			Y	Y
Individual characteristics		Y		Y

The table shows the effect of school offer on school mobility. The dependent variable is an indicator equal to one if student moves to a different school than in reception year by the end of Year 2. Reported are estimated coefficients on first, second, or third choice offer from specifications analogue to Table 3. Robust standard errors are reported in parentheses. See Appendix G for details. *** p<0.01, ** p<0.05, * p<0.1

Figure I.1: School admission and distance to school



Note. The figure plots school offer and enrolment rates, and parental preference assigned to the school, by distance to residence. The sample includes all applicants to at least one London primary school in 2014 or 2015. Offer is reported by markers in Panel A, while diamonds represent enrolment measured at the reception year. Bars in Panel B represent the share of parents ranking the school first, second and third or below. Distance bins are deciles of within-school distribution of applicants. Outliers in the top 5% of the aggregate distance distribution are excluded. See Section 3 for details.

I Additional tables and figures

Table I.1: Descriptive statistics (working sample)

	London	Rest of England	Difference (1 - 2)
	(1)	(2)	(3)
Baseline Characteristics			
FSM eligible	0.1517	0.1362	0.0155***
Not speaking English at home	0.4185	0.1199	0.2986***
White	0.4165	0.7765	-0.3600***
Asian	0.1946	0.0776	0.1170***
Black	0.1618	0.0223	0.1395***
Special education needs	0.0079	0.0064	0.0015***
Female	0.4900	0.4896	0.0004
Exceeding expectations at Year 0: mathematics	0.1330	0.1250	0.0080***
Achievement outcomes			
Exceeding expectations at Year 2: mathematics	0.2667	0.2145	0.0522***
Exceeding expectations at Year 2: reading	0.3023	0.2626	0.0397***
Exceeding expectations at Year 2: writing	0.2032	0.1602	0.0430***
School choice variables			
N. of schools listed	3.2069	--	
Ranked 1 choice	0.2684	0.3801	-0.1117***
Ranked at least 3 choices	0.5759	0.4343	0.1416***
Ranked 6 choices	0.2132	--	
Offered 1st choice	0.8283	0.8944	-0.0662***
Offered one of the top three choices	0.9422	0.9684	-0.0261***
Offered one of ranked choices	0.9684	--	
Enroled at offered school at reception year	0.8692	0.8975	-0.0283***
Not enroled at state schools at reception year	0.0372	0.0193	0.0179***
N	199,220	1,035,825	1,235,045

Note. This table shows descriptive statistics about applicants to any mainstream state-funded primary school in England (column 1) or to at least one primary school in Greater London (column 2) in 2014 and 2015. Columns (1) and (2) report averages computed using one observation per student, column (3) reports the mean difference between (1) and (2). All statistics are conditional on non-missing observations. See Section 3 for details. *** p<0.01, ** p<0.05, * p<0.1

Table I.2: Impacts on school VA

	"preference" specification			"score" specification		
	VA quintile	VA decile	VA ventile	VA quintile	VA decile	VA ventile
	(1)	(2)	(3)	(4)	(5)	(6)
			Panel A. School VA in mathematics			
Enroled in 1st choice	0.0073	0.0074*	0.0075*	0.0051	0.0057	0.0060
	(0.0045)	(0.0044)	(0.0044)	(0.0043)	(0.0042)	(0.0042)
Enroled in 2nd choice	0.0037	0.0029	0.0032	-0.0020	-0.0024	-0.0020
	(0.0041)	(0.0039)	(0.0039)	(0.0036)	(0.0035)	(0.0035)
Enroled in 1st choice X	0.0922***	0.0472***	0.0236***	0.0643***	0.0328***	0.0164***
school value-added gain	(0.0011)	(0.0005)	(0.0003)	(0.0011)	(0.0006)	(0.0003)
Enroled in 2nd choice X	0.0952***	0.0485***	0.0244***	0.0684***	0.0346***	0.0173***
school value-added gain	(0.0013)	(0.0006)	(0.0003)	(0.0013)	(0.0007)	(0.0003)
			Panel B. School VA in English			
Enroled in 1st choice	0.0124***	0.0112**	0.0107**	0.0062	0.0047	0.0038
	(0.0045)	(0.0043)	(0.0043)	(0.0042)	(0.0041)	(0.0041)
Enroled in 2nd choice	0.0075*	0.0066*	0.0070*	0.0011	0.0000	-0.0002
	(0.0039)	(0.0038)	(0.0037)	(0.0034)	(0.0033)	(0.0033)
Enroled in 1st choice X	0.0859***	0.0433***	0.0218***	0.0609***	0.0305***	0.0153***
school value-added gain	(0.0011)	(0.0005)	(0.0003)	(0.0011)	(0.0005)	(0.0003)
Enroled in 2nd choice X	0.0868***	0.0442***	0.0222***	0.0622***	0.0314***	0.0158***
school value-added gain	(0.0011)	(0.0005)	(0.0003)	(0.0012)	(0.0006)	(0.0003)
N	22,896	22,896	22,896	23,154	23,154	23,154
First and second choice FEs	Y	Y	Y			
Tie-breaker controls				Y	Y	Y
Assignment risk (ranked schools)	Y	Y	Y			
Assignment risk (all schools)	Y	Y	Y	Y	Y	Y
Individual characteristics	Y	Y	Y	Y	Y	Y

Note. The table shows estimates of the impact of enroling at most-preferred schools on value-added of the school where student enrols. All specifications are analogue to Table 4, where the dependent variables are school value-added in mathematics (Panel A) or English (Panel B). Different bins of school VA are considered to compute expected gains: quintiles in columns (1) and (4), deciles in columns (2) and (5), and ventiles in columns (3) and (6). See Section 5 for details. ***p<0.01. ** p<0.05. * p<0.1

Table I.3: School where student enrols VS other feasible schools

	N. of feasible schools	School value-added (standardised)			Peer quality (standardised)			Distance (km)		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
		% in best feasible school	Mean percentile rank in feasible set	Left "on the table"	% in best feasible school	Mean percentile rank in feasible set	Left "on the table"	% in best feasible school	Mean percentile rank in feasible set	Left "on the table"
All students	6.84	23.37	0.434	0.131	41.37	0.578	0.340	50.57	0.591	0.223
Deprivation in local area above median	8.63	17.02	0.450	0.189	30.97	0.576	0.434	45.27	0.553	0.253
Deprivation in local area below median	5.43	29.46	0.419	0.090	52.40	0.579	0.269	55.58	0.629	0.193

Note. The table compares the school where student enrols to other feasible schools. Column (1) reports the number of schools in the feasible set. Columns (2), (5), and (8) report the share of students enroling in the best feasible school according to a given attribute (the highest peer qualty or value-added, the lowest distance). Columns (3), (6), and (9) report the mean percentile rank of the school where student enrols in the feasible set, where feasible schools are ranked according to a given attribute (100 = school with highest peer qualty or value-added, or with the lowest distance). Columns (4), (7), and (10) report the difference between the school where student enrols and the best feasible school according to a given attribute. Columns (2)-(4) consider school value-added, estimated by regression-adjusted test scores growth at the school and averaged across subjects (see Appendix B). Columns (5)-(7) consider peer quality, measured by school-level final year test scores, averaged across 2007-2016 cohorts and across mathematics and English. Columns (8)-(10) consider distance, measured in kilometers and computed as linear distance between student postcode and school postcode centroids (schools further than 2 kilometers from residence, the 90th percentile, are not considered). Reported are averages among all students, or separately for those residing in a LSOA with deprivation above or below median. See Section 6 for details.

Table I.4: Oversubscribed schools

	Popular schools	Not popular schools	Difference (1-2)
	(1)	(2)	(3)
Peer quality			
Sixth grade mathematics score	0.3313	-0.3182	0.6495***
Sixth grade reading score	0.3838	-0.3901	0.7738***
School quality			
School value added in mathematics	0.0652	-0.0949	0.1601***
School value added in reading	0.0815	-0.1209	0.2024***
School type			
Religious school	0.2178	0.1378	0.0799***
Academy school	0.1378	0.1739	-0.0361***
Community school	0.5405	0.6089	-0.0685***
Peer composition			
% FSM eligible students	0.1822	0.2789	-0.1057***
% white students	0.4813	0.3625	0.1188***
Income deprivation in student loca area (LSOA)	0.3140	0.4010	-0.0870***
N	2039	1386	3425
N (schools)	1180	865	2045

Note. This table shows characteristics of London primary schools by oversubscription status in 2014 and 2015. Columns (1) and (2) report average characteristics of oversubscribed and undersubscribed schools respectively, while mean difference is reported in column (3). Observations are at the school-year level. A school is coded as oversubscribed in a given year if applicants missing out on any higher-preference school exceed capacity by at least 5 seats. Peer quality is measured by school-level final year test scores across 2007-2016 cohorts, and standardised to have zero mean and unit variance. School value-added is estimated by regression-adjusted test scores growth at the school (see Appendix B). A school is defined as religious if it admits by faith. Peer composition variables are computed as average characteristic among a school's intake across grades 0-6 in 2014. Deprivation index is based on average income in the LSOA of residence. See Section 6 for details. ***p<0.01. ** p<0.05. * p<0.1