Parents know better: primary school choice and student achievement in London *

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Abstract

Expanding parental choice in education may increase system-wide productivity if parents select schools that are a specifically good match for their children. I investigate this hypothesis by studying the effect of attending the school of choice on student achievement in London primary schools. I exploit as good as random variation in parental preference for school arising from centralised assignment which, in case of oversubscription, awards school offer by residential distance. I replicate the algorithm used for assignment and compare students around year-specific catchment boundaries that cannot be exactly anticipated by parents. I find that attending the school of choice increases student achievement compared to an institution with lower parental preference but similar value added. Results suggest that parents pick schools that are specifically effective in increasing their children's achievement, improving the efficiency of school seats allocation.

JEL Codes: H75, I21, I24, I28

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1 Introduction

Educational policy-makers around the world are increasingly expanding parental choice in an effort to increase school productivity. Open enrolment programmes, in particular, elicit parental preferences about desired institutions and assign students to the highest school in this list with available seats.[1] Parental choice can increase system-wide productivity by sparking demand-side pressure on schools to improve (Hoxby, 2003). This requires parents to reward the most effective schools, in the sense of their causal impact on student achievement (Rothstein, 2006). It is strongly debated, however, whether parents seek effective schools or merely respond to indicators driven by neighbourhood composition, such as test scores. Empirical evidence to date suggests that parents may not value effective schools but rather reward geographical proximity and peer quality (Mizala and Urquiola, 2013; Imberman and Lovenheim, 2016; MacLeod and Urquiola, 2019; Abdulkadiroglu et al., 2020).

However, it is possible that parents select schools based on additional information, about dimensions unobserved to the analyst, considering the specific learning needs of their children. Even if peer preferences weaken the incentives for schools to exert effort, match effects may strengthen the case for school choice (Barseghyan et al., 2019). Selection on student-school match would imply that the impact of attending the school of choice on student achievement exceeds the average school value added across children. In previous studies, these two effects are not separately assessed and compared. For example, Deming et al. (2014) find positive effects of attending the preferred school on postsecondary educational outcomes only for applicants with higher gains in school value added, and conclude that choice does not improve school productivity but just redistributes seats in effective institutions. Their investigation, however, does not distinguish the impact of attending a school with higher parental preference from the average effectiveness of the attended school.

This paper investigates whether parents sort into schools where their children have a comparative advantage in achievement production. Using administrative records from centralised assignment of primary school students in London, I show substantial heterogeneity in parental rankings of a given school, even conditional on highly-valued attributes such as peer

[1] Beyond England, studied in this paper, school open enrolment policies have been implemented in many of the largest U.S. districts, serving about 8 million students (Whitehurst, 2017) and in other urban areas around the world such as Amsterdam (De Haan et al., 2018), Barcelona (Calsamiglia and Guell, 2018), Paris Fack et al., 2019) and Beijing (He, 2017).

quality and distance to residence. This possibly reflects selection of schools by parents based on the specific suitability for their children's learning needs. Leveraging quasi-experimental variation in school assignment, I investigate this hypothesis by exploring whether attending the school of choice affects student learning trajectories over and beyond school value-added.

The ideal experiment would compare the learning outcomes of students randomly enrolled in otherwise identical schools, except for the preference rank assigned by parents. I approximate random variation in preference for the school by exploiting tie-breaking embedded in centralised assignment (Abdulkadiroglu et al., 2019). School offers in London are generated by local districts through a deferred acceptance algorithm (DA, Gale and Shapley, 1962) based on parental preferences and school priorities. School seats are typically rationed, with the large majority of institutions being oversubscribed. In case of excess demand, residential distance to the school is used to distinguish between applicants with equal priority, generating catchment areas that vary year by year depending on the equilibrium allocation of school seats.

I consider applicants living at the boundary of preferred schools' catchment who have the same chance of receiving an offer. I compare students enrolled, for example, in the most preferred school to students who did not get a seat at that school because their distance scored a value marginally above the cut-off for admission. My empirical strategy addresses endogenous residential sorting around the school of choice. Previous research has demonstrated how distance-based admissions resulted in fierce competition for residential housing in the vicinity of popular schools, with parents willing to pay a substantial premium (e.g., Machin, 2011; Gibbons et al., 2013 and Battistin and Neri, 2017). This makes identification more challenging in London compared to other urban districts with lottery assignment, such as Boston (Abdulkadiroglu et al., 2011), Charlotte (Deming et al., 2014), and Denver (Abdulkadiroglu et al., 2017). The identifying assumption is that parents are not able to perfectly anticipate the catchment boundary, which depends on choices of all other candidates at the time of application. Consistent with this expectation, I show that applicants on either side of the boundary are similar in any observational respect, suggesting that school offer is as good as randomly assigned.

I adopt two empirical strategies to isolate the match effect of attending the school of choice. I show that simple outcome comparison at the admission cut-off conflates the specific achievement gain from attending the desired school and the gain in average school value added. First, I rely on baseline estimates of school value added to compare students enrolled in schools with similar effectiveness.Second, I combine exogenous variation from centralised assignment with heterogeneity in parental rankings to compare students enroled in the same school. In this comparison, I consider applicants as good as randomly assigned to the same school, ranked by their parents with different preferences.[2]

London offers a unique context to study parental choice. Primary schools are small, enrolling just about 50 students on average. High population and school density imply that the typical family faces several alternatives within short commuting distances. Parental choice is well-established and data on school performance are made widely available to ensure comparability of institutions. With high competition for seats, parents must target extremely narrow areas around a school: catchment boundaries average at about 600 meters and can be as close as 300 meters from the school. As a result, the fraction of applicants missing out on preferred schools is systematically the highest in the country.

I use administrative records on all primary school applicants in 2014-15, with about 200,000 students involved in the centralized assignment process. Data are linked to the census of all students in the state education system including socioeconomic characteristics, educational achievement, and granular information on residential location. As this information is not immediately available in the data, I identify applicants at the margin for admission by replicating the algorithm used for school seats allocation. I use the outcome of the algorithm to trace catchment area boundaries at each school and the relative position of each applicant with respect to the distance threshold.

I begin by showing that parents rank schools by proximity and peer quality, missing out on schools with substantially higher value added. Under DA assignment, submitted rankings credibly reflect parental preference over listed schools (Fack et al., 2019). Consistently with other studies (e.g., Abdulkadiroglu et al., 2020), I show that parents pick schools close to residence and with high test scores, and once these attributes are controlled for, reported preferences are orthogonal to school value added. Parents from relatively deprived areas apply to schools with substantially lower peer quality. Comparing the set of feasible schools

[2] This idea is similar to the strategy followed by Kirkeboen et al. (2016) to explore selection on comparative advantage in the choice of university major. They compare effects of entering preferred major across students ranking the same two fields with opposite preference.

across socioeconomic backgrounds, I show that this difference is mostly explained by residential sorting rather than differential preference for absolute school achievement. Across the deprivation distribution, applicants could potentially access schools with substantially higher value added. Based on attributes of ranked institutions, attending a higher-preference school is not predicted to increase student achievement.

Average patterns, however, mask substantial heterogeneity in parental rankings. I show that distance, school type and peer composition explain only 40% of the variation in preferences. This fraction increases to just 50% when accounting for unobserved school traits. Nevertheless, I provide evidence that parental rankings at application reflect robust and solid preference for schools. Analysing patterns of non-compliance with centralised assignment, I document that parents avoid lower-ranked schools than the one assigned even three years after application. Moreover, the decision of moving their children to another school responds to distance and peer quality, similarly to patterns observed at application. Heterogeneity and consistency of parental preferences motivate the investigation of specific achievement returns from attending the school of choice.

I find a positive effect on students learning from attending the school of choice conditional on school value added. Enroling at the demanded school increases achievement by $0.1 - 0.15$ standard deviations compared to a similarly effective institution with lower parental preference, corresponding to 4-7 percentage points higher probability of achieving above expectations at Year 2 (15-25%). The result is driven by achievement effects on mathematics, estimated to be larger and strongly significant. I show that estimates are substantially robust to alternative specifications and parametric choices on running variable controls.

My results have important implications for educational policies. First, they suggest parents pick schools that are specifically effective in increasing their children's achievement. This implies that parental choice may benefit school productivity by improving the quality of the student-school match. Second, my results imply that returns to school inputs are heterogeneous across students. This has important consequences for school accountability systems, often based on value added models implicitly assuming homogeneous school impacts.

To my knowledge, this paper presents the first investigation of the impact of parental choice on students learning in primary schools. As educational decisions at early stages are crucial for student development (Chetty et al., 2011; Heckman et al., 2013), this fills an important gap in the literature. A large array of studies has investigated impacts of attending high schools and colleges chosen by parents, finding at most moderate achievement effects (e.g., Pop-Eleches and Urquiola, 2013; Abdulkadiroglu et al., 2014; Hoekstra et al., 2018; Gorman and Walker, 2020). In a meta-analysis, Beuermann and Jackson (2019) find a small and statistically insignificant effect across studies in the literature. This paper uniquely isolates parental choice effect from returns driven by school effectiveness, and provides novel empirical evidence of sorting on comparative advantage in school.[3]

I build on the growing methodological literature on how to exploit centralised assignment to identify school effects. Abdulkadiroglu et al. (2017) and Abdulkadiroglu et al. (2019) show that conditioning on the ex-ante probability of admission is sufficient to eliminate selection bias from residential and application choices. I achieve identification by conditioning on assignment variables that are relevant for admission risk. To my knowledge, this is the first empirical study on school choice exploiting centralised assignment based on distance to school.[4]

Related literature leverages 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). I describe parental preferences accounting for the set of accessible schools, addressing recent concerns on truthfulness of reported rankings under DA with non-random tie-breaking (Fack et al., 2019). In line with Abdulkadiroglu et al. (2020), I link parental preferences with school effectiveness and find parents do not reward schools with higher average causal impact on achievement. In a further step, I document substantial heterogeneity in parental rankings of observationally similar schools, and consistency of reported preferences with choice behaviour over time.

The paper unfolds as follows. Section 2 presents the institutional background of parental choice in London. Section 3 describes the data and the replication of centralised school assignment. Parental preference for school attributes is explored in Section 4, while Section 5 presents the empirical strategy. Results are discussed in Section 6, and Section 7 concludes.

[3]In contrast, Kline and Walters (2016), Cornelissen et al. (2018), Walters (2018) andAbdulkadiroglu et al. (2020) find negative selection on achievement gains in pre-school programmes and high schools. Kirkeboen et al. (2016) document sorting on comparative advantage in the choice of field of study at university.

[4]Others have considered distance-based eligibility for policy interventions (Masi, 2018) or school admission (Gorman and Walker, 2020) to investigate school choice of low-income families or impacts of missing out on the most preferred school. These studies, however, have not exploited the quasi-experimental variation arising from centralised assignment.

2 Institutional background

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 primary school with a reception year, which concludes the Early Years Foundation Stage (EYFS). At the end of the reception year, 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 Year 1 and Year 2. At the end of KS1 students receive teacher assessments evaluating their achievement in mathematics, science and English, separately for reading and writing. The final phase is Key Stage 2 (KS2), from Year 3 to Year 6, at the end of which students sit externally-marked standardised exams in mathematics and English. For all these phases, the National Curriculum sets core knowledge and achievement objectives.

I consider state-funded schools, the main provider of primary education. In England, in fact, only less than 5% of students opt for private primary institutions.[5] Within state-funded schools, different type of institutions exhibit different degrees of autonomy from Local Authorities (LAs), the English school districts. Most frequent are Community schools, fully controlled and funded by the LA. Voluntary controlled and voluntary aided schools are established by private, mainly religious, organisations but are mostly funded by the LA and have limited autonomy. Finally, foundation schools and academies are the most independent state schools from the LA. Academies, similarly to US charter schools, are not bound by the National Curriculum and enjoy considerable autonomy in management.

Parental choice among state-funded schools is well-established in England. Since the 1980's, the open enrolment policy guarantees parents the right of choosing a school for their children, as long as demand does not exceed capacity. Parents are required to rank up to six schools at application, in order of preference. LAs have the statutory requirement to provide a school place to local children and assign applicants to the highest preference school available. Schools receive funding from LAs mainly based on enrolment count and are therefore incentivised to attract parental demand to fill capacity[6].

[5] Author's own calculation from official 2019 data on students count by school phase and sector, available at https://www.gov.uk/government/statistics/schools-pupils-and-their-characteristics-january-2019.

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

The wide availability of data on school performance ensures comparability across institutions and sparks competition to enter schools with high absolute achievement. Parental choice is informed by school performance tables, published annually since 1996. They collect information on academic performance, both standardised test scores and value-added measures, and on intake composition of the schools. Institutions with excellent test scores are typically sought-after by parents and they easily become oversubscribed (see Burgess et al., 2015 and Section 4 below).

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. Admission by distance has translated into 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 width of catchment areas, however, varies year by year according to supply and demand for school seats. Therefore, parents are hardly able to precisely anticipate the location of catchment area boundary.

London is an ideal context to study school choice, with a dense supply of schools and high competition for popular institutions. The 33 LAs in Greater London form the most populated urban area in Europe. Primary schools are typically small, enrolling about 50 students per cohort, implying the average family has potential access to several schools. Absolute achievement at KS2 exams is higher than the national average and this difference is driven by a dense right tail of institutions serving exceptionally performing students, as almost 20% of London primary schools fall in the top decile nationally. About 70% of schools are oversubscribed and parents must target a narrow area to obtain admission into preferred institutions, the average catchment area among oversubscribed schools is just 600 meters wide. The fraction of students missing out on their top choices is systematically the highest around the country.[7]

[7] Aggregate statistics on school admission are publicly available at https://www.gov.uk/government/collections/statistics-school-applications.

School assignment

Assignment to school is centrally regulated by the School Admissions Code. Applicants are admitted to the school listed by parents as 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 specific categories of students are typically prioritised and, within priority groups, distance to school is used as tie-breaker to assign school offer. First, schools are required to give precedence to children with particularly disadvantaged backgrounds, a situation concerning a very low share of students.[8] Second, applicants with siblings currently enrolled at the school are usually prioritised.[9] Finally, exceptional admission criteria are permitted to religious schools, which typically set requirements based on faith. All applicants outside these categories have equal priority in admission.

School districts across England assign seats through a deferred acceptance (DA, Gale and Shapley, 1962) mechanism 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. The latter, prioritising applicants who rank the school as first choice, has been proven more vulnerable to strategic preference reporting (Pathak and Sonmez, 2013). Intuitively, parents may rank a 'safe' school as first choice even if they would prefer a school where admission is less likely in order not to miss out on both institutions. DA algorithms do not suffer from this problem as school priority does not depend on parental preference. As long as parents act rationally, their ranking of schools reflects the true order of preference among listed institutions (Fack et al., 2019).

In particular, preferences, priorities and school capacities are mapped into offers through the student-proposing DA algorithm. Each student initially applies to the most preferred

[8] Highest priority in school admission is given to children looked after by the LA, corresponding to the 0.5% of children under 18 years of age in London in 2019 (official counts are available at https://www.gov.uk/government/statistics/children-looked-after-in-england-including-adoption-2018-to-2019). In addition, priority is usually granted to children with a statement of special education needs, the 0.8% in my working sample. The two groups are not mutually exclusive.

[9] Burgess et al. (2019) reports that in the Millennium Cohort Study, a British longitudinal study including a detailed parental survey, the 43% of children has a school-age sibling at the time of admission. This reportedly varies substantially with family income, from 33% to 67% in bottom and top income decile, respectively.

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 in their application form and are ranked jointly with applicants provisionally admitted up to this point. School retains 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.[10]

Parents across the country receive a single school offer in mid-April, deemed National Offer Day. Parents who are unsatisfied with the assignment can join waiting list at preferred schools with the same priority, and may obtain admission if applicants with offer give up their place. Finally, parents have the right to appeal the offer decision in case of irregularities, though admission outcome is rarely overturned[11].

3 Data

I exploit administrative data on applicants to state primary schools in London in 2014 and 2015. Individual-level 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. Data on applications 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 student postcode of residence, a granular information on residential location spanning an average of 15 properties and often corresponding to one single building in London. To measure proximity to school, I compute the linear distance from each applicant's postcode to all primary schools around.[12]

Assessments at the end of KS1 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 students achieving below, at or above the expected

[10]Students disqualified from all preferred schools are assigned by the LA to an institution with spare capacity. This is happens to about 4% of applicants in my sample.

[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.

[12]I compute distance using centroids coordinates for English postcodes obtained from www.doogal.co.uk. For applicants with missing postcode (about 3%), I impute 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.

standard, corresponding to Level 2 in the National Curriculum. Three different subjects are assessed – English, separately for reading and writing, and mathematics.[13] Though 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).[14] Importantly, 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 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 broadly in line with test scores.[15] Overall, institutional details and empirical evidence suggest that KS1 assessments provide a reliable and comparable measure of students achievement. To control for academic ability at entrance, I consider Early Years Foundation Stage Profile (EYFSP) assessments. They test students against 17 learning goals and are completed at age 5 during the reception year, when students have just entered compulsory education. Similarly to KS1 assessments, EYFSP results are grouped in three categories, depending on students achieving below, at or above expected standards in each learning goal.

I observe detailed baseline characteristics of students that serve as control variables in my analysis. Individual-level records include gender, free lunch eligibility, special education needs, language and ethnicity group. Moreover, indexes computed at the local area (LSOA) level enrich the range of socioeconomic traits observed. First, the income deprivation index (IDACI) measures the proportion of children in income deprived families in the local area, and is included in administrative records. In addition, I merge NPD data with socioeconomic local area characteristics from the 2011 population Census, such as the proportion of adult residents achieving qualifications at the higher education level.

[13]Students are also tested in science, but this assessment is not very informative since the 83% of students in my sample are judged as "working at the expected standard". I do not consider this subject in my analysis.

[14]Moderation is monitored by the Standards and Testing Agency (STA) and involves a visit from an external moderator. The moderator reviews a sample of students classwork on which the assessment was based. Moderation can result in changes to teacher assessments or, in case of systematic lack of evidence on teacher judgements, in school being reported for maladministration.

[15]Burgess and Greaves (2013) also find evidence of bias in teacher assessments based on ethnicity. I observe student ethnic group and control for this variable in my analysis.

Sample selection

I consider students entering the reception year between 2014 and 2015 who ranked at least one London primary school at application. The working sample consists of 200,071 applicants and 663,240 student-preference observations, with the average applicant ranking between 3 and 4 schools. As presented in Appendix Table A.1, primary schools in London serve a population of students whose social background is strikingly mixed – about 41% are white and a similar fraction does not speak English at home, compared to 78% and 12% in the rest of England, respectively. London students are more likely to have a disadvantaged background, with higher proportion of students eligible for free lunch or with special education needs than the rest of the country. Despite this difficult context, primary schools in the capital outperform the average national institution in terms of academic achievement, with higher proportion of students achieving above expectations at Year 2 in all subjects assessed. Notably, the gap in favour of students in London is wider at Year 2 than at primary school entrance.

Primary school admission is substantially more competitive in London than in the rest of England, as reported in the bottom part of Appendix Table A.1. Parents in the capital exercise choice more actively, being 15 percentage points more likely to rank three schools or more at application[16]. Proportion of applicants admitted to the first choice is about 82% in London, around 7 percentage points lower than in the rest of the country. Parents closely comply with centralised school assignment. Take-up rate is very high, with 87% of students enrolled in the offered institution at the reception year (see Appendix Table A.1, column 1). This fraction is 3 percentage points lower than the national average (column 2), partly reflecting higher propensity to enrol at private schools among families the capital (4% of parents choose private school against 2% in the rest of England).[17]

Replication of centralised school assignment

Centralised assignment breaking ties by distance implies that, if a school is oversubscribed, no offer is granted to applicants located further than a specific threshold. Such threshold, however, is not observed as administrative data do not track the admission process. School

[16]The comparison on propensity to rank six schools is not presented as in most English districts application form is restricted to three schools.

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

assignment depends also on parental preference and school priorities, which interact with distance to determine school offer (see Section 2 above). First, I show that school offer rate is not entirely driven by distance and that admission cut-off cannot be directly inferred from available data. I then replicate the assignment mechanism to trace catchment boundaries and identify applicants at the margin for admission.

Figure 1 shows that school admission rates are not deterministic conditional on distance. The probability of receiving an offer markedly decreases with distance to school, and the figure is very similar for enrolment, represented by diamonds in Figure 1. However, rather than dropping to zero, admission rates gradually diminish, and this happens for two reasons. First, there is variability in parental ranking of the school. Regardless of distance, applicants ranking the school lower than first choice are offered a place only if they miss out on all institutions ranked with higher preference. For example, this explains why offer rate of applicants in the bottom decile of distance is far from deterministic, at about 0.7 in Figure 1.[18] Second, particular categories of applicants, as detailed in Section 2 above, are admitted with priority independently from their location. For example, this partly explains why offer rate of applicants in the top decile of distance to school is non-negligible, at about 0.2 in Figure 1.

Replication of school assignment is complicated by data availability, as I have no information on demographics that are relevant to define school priorities over applicants (see Section 2 above). Most importantly, I do not observe whether applicants have siblings currently enrolled at the school. Therefore, catchment area boundary is not identified when replicating assignment based solely on parental preference and distance. Students with priority, however, are partially detectable in the data. Intuitively, if an applicant with offer lives beyond the distance threshold estimated without considering school priorities, she must have precedence in admission. I show in the Appendix how I achieve replication of school offer based on this idea.

The main limitation in my empirical analysis is that priority status – mostly involving siblings of current students[19] – is measured with error. I can reconstruct priority only

[18]Consistent with this expectation, Panel B of Figure 1 shows that about 35% of parents located next to the school have ranked it less than first choice.

[19]In addition, I do not observe parental faith, often used to grant priority at religious schools. As the error in running variable measurement is likely more serious in this case, I do not consider religious schools in estimation.

when binding for admission, implying that students with precedence are undetected, first, at schools other than the offered one and, most importantly, when located within the catchment boundary. Unobserved priority is unlikely to constitute a major concerns for my analysis for two reasons. First, a minority of students (about 30%) are flagged with priority at the right hand side of the catchment boundary, and the distribution of priority is likely smooth at the cut-off. Second, in order to bias my results, applicants with unobserved priority would need to display differential potential outcomes conditional on parental preference and distance to the school. The fact that a number of student characteristics associated with potential outcomes, including lagged achievement, are balanced around the catchment boundary (see Section 5 below) supports the validity of my design.

4 Parental preference for schools

Ranked schools

I exploit the properties of the school assignment mechanism to infer parental preference for school attributes from application data. Under DA, parental rankings reflect true preferences over schools as long as the length of application form is unrestricted (Pathak and Sonmez, 2013). Despite parents in London cannot rank all schools at application, only a minority of them fills the six available slots, suggesting the limit is not binding (see Appendix Table A.1). Moreover, Fack et al. (2019) demonstrates that, regardless of the number of preferences allowed, parental rankings reflect the true preference order among listed school as long as they act rationally. I then infer parental preference for school by comparing attributes of listed institutions.

I describe parental preferences for geographical proximity, peer quality and school effectiveness, three of the attributes often considered in the literature (e.g., Abdulkadiroglu et al., 2020). I plot average attributes of listed schools by parental ranking controlling for feasibility of the school and number of institutions listed. I also explore differential preference for school attributes by socioeconomic status.[20] Specifically, I estimate the following regression:

[20] Hastings et al. (2009) find parents from disadvantaged contexts exhibit weaker preference for academic performance.

$$A_{is} \;=\; \gamma_1 + \sum_{s=2}^{6} \gamma_p \mathbb{1}(s = p) + X'_{is}\delta + u_{is}, \tag{1}$$

where A_{is} is the attribute of school ranked s-th by student i. The vector of controls X'_{is} includes number of preferences fixed effects, an indicator for ex-post feasibility of school ranked s-th by student i, and school attributes other than A_{is} (e.g., school value added and distance when considering peer quality). In this formulation, parameters γ_1 to γ_6 estimate average attributes of schools ranked first to sixth, conditional on controls.

Parents are surrounded by several schools at short distance from residence and they rank them in order of proximity.[21] Panel A of Figure 2 plots distance to school by parental rank using predicted values from equation (1), separately estimated for applicants with local area deprivation above or below the median. The figure shows the first choice of parents in better-off areas is on average around 800 meters from residence, and all ranked schools are located within 1.2 km. The corresponding figure in more deprived contexts is very similar, with slightly shorter distances on average. This difference could reflect either higher utility cost of travel for disadvantaged families or supply-side differences such as higher population density in worse-off neighbourhoods.

Though all parents rank schools by peer quality, those in relatively deprived areas demand institutions with lower absolute achievement. The left-hand graph in Panel B of Figure 2 plots average standardised school test scores, measured at the time of application.[22] Peer quality of the first choice ranked by relatively advantaged parents is one standard deviation (hereafter, σ) above the average, and it markedly decreases with parental rank, indicating that parents value absolute achievement. This result likely reflects the dissemination of school performance tables, published annually to inform the choice of parents, in which final year test scores are headline measures. A similar pattern is observed for worse-off parents, but peer quality of first choice is substantially lower, by about 0.8σ, and similar to the score of the sixth choice in relatively affluent areas. This stark difference likely reflects segregated access to top-scoring schools through residential sorting.

[21] Here and below, I consider applicants within 2 kilometers from ranked school, corresponding to the 90th percentile of the distance to school distribution.

[22] I measure peer quality by school average standardised test scores at final year, which I compute pooling 9 cohorts of data up to 2014. Scores are averaged across mathematics and reading.

Conditional on distance and peer quality, parents do not respond to school value added. Panel C of Figure 2 plots school value added. I follow Deming et al. (2014) and estimate a baseline measure of school value added as regression-adjusted test scores growth, averaged at the school level.[23] One σ higher school value added improves the probability of scoring above standards at Year 2 assessments by about 8 percentage points (30% of the sample average). Estimated school value added has about 17% correlation with absolute achievement, suggesting high-scoring schools are not necessarily highly effective. Parental rankings are almost orthogonal to school value added, in line with findings in other contexts (see MacLeod and Urquiola, 2019 for a review).

Parental preferences result in excess demand for schools with high peer quality. Attributes of oversubscribed schools are described in Appendix Table A.2. I define a school oversubscribed if the number of applicants missing out on any higher-preference institution exceeds capacity. In fact, these are the only candidates who would receive an offer if school capacity marginally increased. School seats are often rationed, with the 60% of institutions experiencing excess demand.[24] Oversubscribed schools have about one σ higher peer quality, suggesting strong reaction of parental demand to absolute achievement. In line with evidence from preference data, parental demand is substantially less responsive to school effectiveness. Finally, oversubscription also correlates with school type and peer composition. Religious schools are disproportionally oversubscribed likely – reflecting their relatively high test scores – while community schools are more likely to remain undersubscribed, and parental demand is concentrated on schools serving relatively advantaged students. For example, oversubscribed schools display a 10 percentage points (34%) lower share of students eligible for free lunch.[25]

[23]Specifically, I compute school average residuals from an individual-level regression of KS1 assessments on student socioeconomic characteristics (gender, ethnicity, free lunch eligibility, local area deprivation and special education needs) and baseline achievement. I use an indicator for scoring "above expected standards" as dependent variable, the same measure I employ as main outcome in my analysis below.

[24]I consider here schools oversubscribed by at least 5 seats (results are robust to this choice). Institutions oversubscribed by one seat or more are the 69%. I also note a significant fraction of schools has an impressive degree of excess demand, with the 37% of institutions oversubscribed by 20 seats or more (versus an average enrolment count of about 50).

[25]Peer composition is measured as school average socioeconomic characteristics of students enrolled across reception to Year 6 in 2014, the latest data available at the time of application.

Feasible schools

Following Ainsworth et al. (2020), I compare attributes of the school where applicants enrol (one of the top two choices for most applicants) with those at other feasible institutions. This complements the description of parental demand by outlining the supply of schools. As argued in Fack et al. (2019), parents foreseeing admission chances can 'skip the impossibile' and give up application to preferred schools located too far away. Therefore, understanding parental preference requires to account for the distribution of ex-post feasible schools faced by each applicant. I define the individual feasible school 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 the Appendix for details). Feasible set of the average applicant includes 6 schools within 2 kilometers from residence. The large majority of parents has some degree of choice, with 75% potentially accessing at least 3 schools.

Parents from different socioeconomic contexts travel very similar distances to primary school, about 600 meters. Panel A of Figure 3 depicts average distance to student's school and to the closest feasible institution by decile of local area deprivation, alongside the average in the feasible set. Parents choose schools with relatively short distance from residence among feasible ones, with 59% of applicants enroled in the closest accessible institution. On average, parents give up schools closer to residence by about 200 meters, likely trading off distance with other valued attributes. Interestingly, this difference is lower for applicants in wealthiest areas, which likely secured residence close to desired institutions.

Most part of the difference in peer quality across local deprivation is accounted for by residential sorting. As shown in Panel B of Figure 3, the average feasible school in wealthiest areas has about 1.2σ higher peer quality than in most deprived neighbourhoods, and schools where students enrol exhibit a comparable difference. Parents across different socioeconomic contexts similarly pick institutions with peer quality well above the average among feasible institutions. Affluent parents leave very in little in terms of peer quality, being systematically enrolled in schools with absolute achievement close to the highest available. Applicants at the top of deprivation distribution, instead, would be able to access institutions with about 0.5σ higher peer quality. This probably reflects worse trade-offs between school test scores and distance for relatively disadvantaged parents, as they enrol in school at similar distance from residence than better-off peers.

Regardless of socioeconomic background, parents miss out on accessible schools with highest value added, which would substantially increase students achievement. Students are enroled in schools with value added close to the average in the feasible set, but could potentially access institutions with about 0.8σ higher effectiveness (see Panel C of Figure 3). This corresponds to about 0.15σ potential increase in achievement at Year 2. Foregone value added moderately increases with local deprivation as the highest accessible effectiveness increases, possibly reflecting more scope to improve achievement in areas where, on average, students have lower ability at entry.

Overall, based on average value added, attending the school of choice is not predicted to increase students achievement. However, it is possible that parents select schools based on, potentially unobservable, traits that specifically increase students achievement. In fact, average patterns mask substantial heterogeneity in parental ranking of schools. First, parental choice is hardly summarised by observable school characteristics. As shown in Figure 3, distance and peer quality explain just about 40% of the variation in parental preferences. This figure does not change when adding school value added, school type and peer composition. Second, parental rankings vary substantially even conditional on unobserved school traits, as school fixed effects explain just 50% of the variation. The question arises whether such idiosyncratic choices have an impact on students learning, reflecting sorting based on specific student-school match.

5 Empirical strategy

Research design

Under DA assignment, school offers depend solely on parental preferences, school priorities and distance to school (see Section 2). Controlling for these variables is therefore sufficient to eliminate selection bias from residential sorting and application choice. With oversubscription, the distance between schools and place of residence is used as tie-breaker among applicants with equal priority. I exploit the idea that, depending on preferences and priorities, a subset of applicants is as good as randomly assigned near the distance cut-off. My identification strategy follows Abdulkadiroglu et al. (2014) and build on two steps. First, I isolate the sample of applicants for which the tie-breaker is binding for admission. Second, I compare students located around the school catchment boundary.

Consider the case of parents ranking three schools, labeled A, B and C. To fix ideas, assume that these are the only schools which are potentially accessible to parents in a certain neighbourhood. I begin by considering all applicants for which school A is the most preferred. These applicants can be grouped into those ranking B second and C third (ABC), and those ranking C second and B third (ACB). I consider applicants to school A with equal admission priority and living around the catchment boundary, regardless of how they ordered less-preferred schools at application. At the catchment boundary, the chance of receiving an offer from school A is independent from the application choice, as DA does not consider parental preferences other than those at A. Comparing students at the boundary, therefore, is sufficient to eliminate selection bias from application choice.

A similar reasoning can be applied to schools ranked by parents lower than the first choice. Consider now the case of parents raking school A as second choice. These applicants are of two possible types: those ranking B first and C third (BAC), and those ranking C first and B third (CAB). I consider applicants living around the catchment boundary of school A who have equal admission priority and live outside the catchment of their first choice, regardless how they ordered other institutions at application. In general, following Abdulkadiroglu et al. (2014), I consider sequential samples of applicants to school A: those ranking A first; those ranking A second and excluded from their first choice; those ranking A third and excluded from their first and second choice, and so on.

Within these samples, students are sharply assigned by distance, as visualised in Panel A of Figure 5. The figure depicts offer rates in 100-meters-wide bins of distance to the catchment boundary, which I employ as running variable in my analysis, and a local linear polynomial fitted to underlying observations. The left-hand graph represents equal-priority applicants to the most preferred institution, pooling cut-offs at all first-choice schools. Admission rate sharply drops at the cut-off as school capacity is reached, generating exogenous variation in assignment. A similar design is displayed for lower-ranked schools. Central and right-hand graphs in Panel A of Figure 5 plot offer rate of equal-priority applicants demanding schools with lower preference, second and third to sixth respectively, conditional on being excluded from any more-preferred school. Note that the same applicant may be located around cut-off of more than one school if excluded from the first choice. School assignment closely corresponds to enrolment at the reception year, as shown in Panel B of Figure 5.[26] The assignment mechanism implies that students excluded from the school of choice enrol to an institution with lower parental preference, as can be seen in Panel C of Figure 5 – where a parental rank of 1 indicates first choice. For example, about 70% of applicants denied the first choice are offered a seat in the second or third most preferred schools.

My research design builds on recent methodological contributions on how to leverage centralised assignment for empirical research. Abdulkadiroglu et al. (2017, 2019) argue that DA assignment embeds as good as random variation in school offer conditional on parental preferences and school priorities, labeled parental "type" to indicate that these are likely correlated with potential outcomes. However, full-type conditioning is often not feasible: in the sample I consider, for example, there are almost as many types as the number of applicants. They show, first, that conditioning on the ex-ante probability of receiving an offer is sufficient to control for parental type and, second, that the risk of admission is much coarser than type and depends on few key assignment variables. In my empirical analysis, I eliminate selection bias by conditioning on the components of parental type that are relevant for assignment risk.

Empirical framework

Following Abdulkadiroglu et al. (2020), potential outcome of student i at school ranked s-th can be written as:

$$Y_{is} = \nu_i + \alpha_s + \mu_{is}, \tag{2}$$

where ν_i is the student's general ability, α_s is the school value added, i.e. its average causal impact on achievement, and μ_{is} is the idiosyncratic match between student i and the school ranked as s-th choice. In a model where parents sort on their children's comparative advantage in achievement production (Roy, 1951), μ_{is} is expected to be positive.

Let D_{is} be a dummy variable indicating enrolment at the school ranked s-th. The outcome I observe for student i can be written as:[27]

[26] Compliance patterns are described in detail in Section 6 below.

[27] I assume here for simplicity that all students rank six schools and are admitted to one of the listed institutions. I also assume that all offers are accepted. Notation could be extended to deviate from this setting, but would get more cumbersome.

$$Y_i = Y_{i1} + \sum_{s=2}^{6} D_{is}(Y_{is} - Y_{i1}).$$

Substituting into equation (2), we obtain:

$$Y_i = \nu_i + \alpha_1 + \sum_{s=2}^{6} D_{is}(\alpha_s - \alpha_1) + \sum_{s=2}^{6} D_{is}(\mu_{is} - \mu_{i1}) + \mu_{i1}. \tag{3}$$

Let $\mathcal{B}_{i1}$ denote distance of applicant i from the the most preferred school's boundary (a similar reasoning applies to lower-ranked choices). I pool applicants at all first choices and consider the following parameter:

$$E[Y_i|\mathcal{B}_{i1} = 0^-] - E[Y_i|\mathcal{B}_{i1} = 0^+]. \tag{4}$$

This quantity represents the comparison of outcomes of students living marginally within the catchment boundary ($\mathcal{B}_{i1} = 0^-$), who are admitted at their first choice, with those located marginally outside, who are excluded ($\mathcal{B}_{i1} = 0^+$). The causal parameter identified by this comparison is the effect on student outcomes of missing out an offer from the most preferred school.

The comparison of students around cut-off for admission rests on the assumption that the catchment area boundary cannot be perfectly anticipated by parents. The admission cut-off changes over time depending on the number and parental rank of applications to the school, priorities at the school and changes in density of school-age children (e.g., because of newcomers) in the neighborhood, rendering exact sorting with respect to catchment boundary extremely unlikely. This motivates the following continuity condition at the catchment boundary:

$$E[\nu_i|\mathcal{B}_{i1} = 0^-] = E[\nu_i|\mathcal{B}_{i1} = 0^+],$$

which implies that $\mathcal{B}_{i1} = 0^-$ and $\mathcal{B}_{i1} = 0^+$ students have the same ability, on average. Furthermore, we have that:

$$E[\alpha_1|\mathcal{B}_{i1} = 0^-] = E[\alpha_1|\mathcal{B}_{i1} = 0^+].$$

Finally, I assume a third continuity condition:

$$E[\mu_{i1}|\mathcal{B}_{i1} = 0^-] = E[\mu_{i1}|\mathcal{B}_{i1} = 0^+].$$

If parents rank preferences based on expected returns to achievement, I do not expect any systematic difference in the match component at the boundary considering that applicants have ranked the same first choice.

Substituting the definition of Y_i from (3), and applying the continuity conditions, we obtain the following expression for the comparison in (4):

$$\sum_{s=2}^{6} E[D_{is}(\alpha_1 - \alpha_s)|\mathcal{B}_{i1} = 0] + \sum_{s=2}^{6} E[D_{is}(\mu_{i1} - \mu_{is})|\mathcal{B}_{i1} = 0].$$

For example, if all students missing out on the first choice were offered the second choice, the comparison at the boundary would be equal to:

$$E[\alpha_1 - \alpha_2|\mathcal{B}_{i1} = 0] + E[\mu_{i1} - \mu_{i2}|\mathcal{B}_{i1} = 0]. \tag{5}$$

I am interested here in the second element of the this equation, capturing whether parents rank schools based on specific match with their children.

I adopt two empirical strategies to isolate the parameter of interest. First, I rely on baseline estimates of school value added (see Section 4 above) and implement the comparison in equation (4) conditional on school effectiveness. In particular, I consider the sample of students for whom the school of choice and the school where enroled have similar value added. Consider again the simple case where all applicants not offered the most preferred school end up in their second choice. This comparison can be represented as:

$$E[Y_i|\mathcal{B}_{i1} = 0^-, \alpha_1 = \alpha_2] - E[Y_i|\mathcal{B}_{i1} = 0^+, \alpha_1 = \alpha_2], \tag{6}$$

and the outcome difference at the catchment boundary would identify the match effect.

Second, I combine exogenous variation from centralised assignment with heterogeneity in parental ranking of schools. This allows me to implement the comparison in equation (4) conditional on the school where applicants enrol. Two students can be quasi-experimentally assigned to the same institution if they have ranked it with different preference. Consider, for example, students 1 and 2 applying to school A respectively as first and second choice, and suppose student 1 resides just within the catchment. Suppose student 2 ranks school B as first choice. This specification compares students 1 and 2 when student 2 resides either just within catchment of school A, and is not eligible at B, or just outside of school B's catchment boundary, and is eligible at school A. As value added is a school characteristic, and the institution is held constant, this comparison is equivalent to equation (6).

Estimation and covariates balance

Consider the sample of students: (a) for which school j is the s-th listed in the application ($s = 1, \ldots, 6$), (b) without priority at school j, and (c) not admitted to any of the schools preferred to school j. Students in this sample are indexed to i, and the school ranked as s-th choice is indexed by j. Let Z_{is} be the indicator for receiving an offer from school ranked s-th. I start by testing covariates balance around the catchment boundary in the sample defined using (a), (b) and (c) above and depending on the value of s. I consider the following specification:

$$W_{is} = \pi_{0s(j)} + \pi_1 Z_{is} + f(\mathcal{B}_{is}) + u_{is}, \tag{7}$$

where W_{is} is a baseline characteristic of student i applying to the s-th choice, and $\pi_{0s(j)}$ is a full set of demanded school fixed effects. I control non-parametrically for $\mathcal{B}_{is}$, denoting distance to the catchment boundary of the s-th choice, by including a linear trend estimated separately on each side of the cut-off and by considering kernel-weighted estimates of equation (7).[28] To increase precision, I add a full set of number of schools listed fixed effects and individual socioeconomic characteristics other than W_{is}. As this specification stacks applications with different preferences, I control for parental rank fixed effects and interact parental rank with running variable controls. Standard errors are clustered at the student level.

Conditional on being assigned by distance, socioeconomic characteristics of students at the two sides of the catchment boundary are balanced. Table 1 compares uncontrolled differences in covariate means by school admission (column 1) with estimates of π_1 from equation (7) (columns 2 to 4). Applicants getting an offer for preferred schools are disproportionately less likely to be eligible for free lunch, are more likely white, live in neighbourhoods with lower deprivation and score higher at reception year assessments. Once distance to the catchment boundary is controlled for, all these differences are much smaller and not statistically different from zero. The results are in line with the idea that, conditional on offer risk, admission to school is as good as randomly assigned and provides evidence in support of the continuity conditions imposed above.

The causal effect of attending the school of choice is estimated via 2SLS by instrumenting D_{is}, a dummy indicating school enrolment at the s-th preference, with Z_{is}. The main outcome

[28] I employ a triangular kernel centered at the boundary and select optimal data-driven bandwidth following Calonico et al. (2014), separately for each outcome variable.

of interest, denoted by Y_{is}, is an indicator for scoring above expected standards at Year 2 assessments. I consider the following specification:

$$Y_{is} = \beta_{0s(j)} + \beta_1 D_{is} + f(\mathcal{B}_{is}) + \epsilon_{is}, \tag{8}$$

where notation and control variables follow equation (7). β_1 in equation (8) corresponds to the comparison in equation (4), and estimates the local average treatment effect (LATE) of attending the school of choice vis-à-vis an institution ranked with lower parental preference (see Panel C of Figure 5). It measures achievement gains from the school of choice for compliers, i.e. students who would enrol in the school only if offered a seat. LATE is a policy-relevant parameter in my context as it represents the expected impact of a marginal increase in school capacity. The corresponding first stage of equation (8) is:

$$D_{is} = \alpha_{0s(j)} + \alpha_1 Z_{is} + f(\mathcal{B}_{is}) + \eta_{is}, \tag{9}$$

where the parameter α_1 provides an estimate of the average discontinuity in school enrolment around catchment boundary, visualised in Panel B of Figure 5. Finally, estimation relies on the assumption that school offer can only shift students into the school of choice, regarded as monoticity in the IV literature (Imbens and Angrist, 1994). In my context, the presence of defiers, i.e. applicants who would enrol into the school of choice only if denied an offer, is highly unlikely and the existence of this group can be reasonably ruled out.

6 Effects of attending the school of choice

First stage estimates and parental response to assignment

The school offer instrument generates a strong first stage for enrolment into the school of choice. Estimates of α_1 in equation (9) are reported in Panel A of Table 2. The first stage is of about 65%, corresponding to the average discontinuity in school enrolment across boundaries of preferred schools (see Panel B of Figure 5). The figure shows that the largest part of non-compliance arises beyond the catchment boundary, as nearly excluded applicants find a seat in the school of choice despite not obtaining an offer in the first place. These students are "always takers", and the pattern is consistent with the possibility of joining waiting lists when denied the school of choice. Conversely, a small fraction of students do not enrol into the school of choice even if offered a seat. These are “never takers”, who prefer seeking a different state school or a private institution.

Analysing compliance by preference for assigned school offers further insights on parental choice. The share of parents enroled into the assigned institution at the reception year is very high on average (87%), and it strongly increases with preference for the school offered (see Appendix Figure A.4). Blue bars in Panel A shows that compliance rate exceeds 90% for students offered their most preferred school, and it drops to 50% for applicants assigned to their sixth choice. The 13% of students not complying with school offer enrols mostly at state schools not ranked at application (6%) or private institutions (4%), as shown in Panel B. A non-negligible share of students (2%) enrols at a school with higher parental preference than the one assigned, most likely through waiting lists. Conversely, almost no applicant (less than 1%) enrols in a school with lower parental preference. Overall, these patterns suggest that parents react to assignment when denied the preferred school by acting coherently with preferences expressed at application.

School mobility in the first years of primary school also depends on parental preference for assigned school. Panel A of Appendix Figure A.4 (red bars) plots the share of students moving to another school between the reception year and Year 2. School mobility rate is 18%, and it slighlty decreases with preference for school offered. Moving to another school often, but not necessarily, implies moving to a different place, as shown by the green bars,[29] and residential mobility is orthogonal to school assignment. Virtually all students who move to another institution enrol either at a state school not listed at application (12%) or to a private school (4%). The fact that a negligible share of students moves to a school with higher parental preference suggests that centralised assignment is well enforced. The fact that a negligible share of students moves to a school with lower parental preference reinforces the evidence that ranking of institutions at application represents robust preferences for schools, to which parents stick in the subsequent years. In line with evidence presented in Section 4, I show in the Appendix that school mobility is more likely when the assigned school falls short of the targeted school in terms of peer quality and proximity to residence, but it does not depend on school value added.[30]

[29]I define residential mobility as an indicator variable equal to one if a student’s home postcode changes.

[30]I refer to the Appendix for discussion on the interpretation of my estimates in presence of school mobility induced by school offer.

Effects of match with the school of choice

Attending the school of choice, on average, leads to a moderate achievement increase in mathematics with respect to institutions with lower parental preference, and does not impact assessments in English. Instrumental variable estimates of β_1 in equation (8) are presented in Panel C of Table 2. Estimated local average treatment effect on assessments in mathematics is about 0.09σ in column (1), and remains similar when adding controls for next-best choice fixed effects (i.e., the second choice when most preferred school is considered, and so on, in column 2) and neighbourhood of residence (column 3). Estimated effects on reading and writing are small and not statistically different from zero, and the same result is obtained when stacking achievement outcomes across all subjects.[31] Results are in line with findings in the literature, summarised by Beuermann and Jackson (2019), documenting a small average effect of attending the school of choice, and not statistically significant. These estimates combine average school value added and student-school match effect at the institution of choice (see Equation 5).

I find positive and significant effects of the specific match between students and the school of choice. First, Table 3 considers students with school where enroled and demanded school in the same quintile of value added (43% of applicants). First stage estimates are substantially lower for this subsample (about 40%, see Panel A), as an important part of the students with similar school value added are those who find space at the school of choice regardless school offer. Instrumental variable estimates in Panel C shows that attending the school of choice increases student achievement by about 0.15σ with respect to an institution of similar value added but with lower parental preference (significant at the 10% level). This result is driven by larger and statistically significant achievement effects in mathematics. Though not precisely estimated, match effects in English are consistently higher than average effects reported in Table 2.

Second, I estimate β_1 in equation (8) including school fixed effects. Results are presented in Table 4. Panel A shows a strong first stage of about 25%, indicating that the proportion of compliers among students ending up in the same institution is relatively low. These are students who rank the same school with different preference and enrol because of their distance to the catchment boundary of preferred schools. Instrumental variable estimates, reported

[31] The latter specification controls for subject fixed effects.

in Panel C, show a statistically significant effects of about 0.1σ, similar in magnitude to the corresponding estimates in Table 3. Once again, this is driven by a larger and strongly significant achievement effect in mathematics. Overall, the two strategies I employ give similar results, suggesting that parents select schools that are specifically effective at increasing their children's learning.

Similar results are obtained accounting for differential time of exposure to the school of choice. As described above, parents in the control group are more likely to move their children to a different school after the reception year. I then define the endogenous treatment as the number of academic years spent in the school of choice from assignment to KS1 assessment, which takes values 1 to 3. Table 5 presents estimates from specifications similar to column 2 of Table 2 (column 1), 4 (column 2), and 3 (column 3). As expected, school offer increases time spent at the school of choice by about 2 years, consistent with some students beyond the catchment boundary obtaining a seat through waiting list (see column 1 in Panel A of Table 5). Instrumental variable estimates in Panel B show that match effect on achievement from an additional year in the school of choice is $0.04 - 0.07\sigma$ (see columns 2 and 3), in line with the main findings.

A potential concern is that non-random attrition based on school offer may hinder comparability of students around the catchment boundary. Consistently with parental response to school offer described above, parents are more likely to opt for private institutions when missing out on preferred state schools, preventing the observation of achievement outcome (see Panel B of Appendix Figure A.6). However, for several reasons, it is unlikely that differential attrition explains the pattern of results in Tables 2, 3, and 4. First, follow-up rate is high (82%) even among students not assigned to their preferred schools, as shown in Appendix Table A.4.[32] Second, baseline achievement of students around the catchment boundary of ranked schools is balanced conditional on observaton of Year 2 assessments (see Table 1).[33] Third, positive estimates of the match effect with the school of choice in Tables 3 and 4 cannot be explained by differential attrition given the smaller average effects estimated in Table 2.

Estimation results are remarkably robust to different empirical specifications. Appendix

[32]I follow the presentation of columns (1) and (2) of Table 7 in Abdulkadiroglu et al. (2018).

[33]Baseline achievement is available only for students completing Year 2 assessments as the two variables are in the same data file.

Table A.5 presents estimates of achievement effects across subjects from specifications similar to those reported in Table 3, but with different parametric choices for running variable controls.[34] Coefficients are estimated restricting the sample to applicants located within 0.5, 0.8 or 1 kilometer from the catchment boundary, and controlling for quadratic (columns 1-3) or cubic (columns 4-6) polynomial controls for distance to the catchment boundary. Rsults are of similar magnitude of main findings, ranging from 0.12 to 0.24σ. Similarly, Appendix Table A.6 reports instrumental variable estimates from parametric specifications similar to those in Table 4, ranging from 0.06 to 0.12σ.

Potential mechanisms

I next explore potential mechanisms explaining the match effect between students and the school of choice. One possibility is that observable school attributes such as peer quality or class size have a specific impact on some students over and above the effect on the average children, and parents sort based on this. I can hardly test this potential explanation as several inputs vary at the same time when a student gains access to the school of choice. I consider here instead characteristics of the student-school match that could be associated with achievement production.

First, I consider distance to school. As parents rank schools by proximity (see Section 4 above), attending a higher-preference institution implies shorter distance to school and likely less commuting travel, which can benefit student achievement. Table 6 reports estimates of β_1 from equation (8), and is structured similarly to Table 5. On average, attending the school of choice reduces distance to school by about 0.65σ, corresponding to around 600 meters (column 1 of Table 6). The proximity gain is similar when holding school value added constant (columns 2 and 3). Attending the school of choice is then associated with shorter distance, and likely shorter travel to school, constituting one possible mechanism behind the match effects I find.

Second, I consider student ability rank within school-cohort. Empirical evidence increasingly suggests that students with relative high ability in the classroom experience achievement gains conditional on ability level (e.g. Murphy and Weinhardt, 2020). As shown in Table

[34]Parametric and non-parametric estimates provide mutually reinforcing specification checks, as recommended in the literature on RD design (Lee and Lemieux, 2010).

6, ability rank is not significantly impacted by attending the school of choice. I measure percentile ability rank of a student within school-cohort at the reception year using Year 0 assessments. As expected, given that parents rank schools by peer quality, estimates in Table 6 are negative, implying that attending the school of choice tends to worsen student's relative position in the classroom. However, estimates are small and not statistically different from zero, implying this mechanism cannot explain my findings.

7 Summary and conclusion

One key argument supporting the recent rapid expansion of school choice programs is that competition among schools to attract enrolment would enhance school productivity (Hoxby, 2003). A growing literature suggests, however, that parents reward schools with high peer quality rather than those effectively improving achievement, implying scarce incentives for schools to improve (MacLeod and Urquiola, 2019). This paper provides evidence on a relatively under-explored mechanism through which school choice can improve system-wide productivity. I leverage administrative records on the universe of applicants to London primary schools to study whether parents target institutions that are a better-than-average match for their children. Building on the latest developments in the econometric literature on centralised assignment, I exploit as good as random variation in school allocation by comparing students with similar risk of admission (Abdulkadiroglu et al., 2019). The effect of attending the school of choice is identified by considering applicants around the catchment boundary for which school offer is determined by distance, used as tie-breaker in the assignment.

In line with studies in the literature, I show that parents generally choose schools by peer quality and proximity to residence, but do not reward more effective institutions. In a further step, I present evidence that parental rankings are substantially heterogeneous even conditional on observed and unobserved school traits. This implies significant disagreement in school valuation and opens the door to exploring whether parents select schools based on comparative advantage in achievement production for their children.

Instrumental variable estimates reveal moderate achievement gains from attending the school of choice over and above the impact predicted by average school value added. My findings suggest, first, that returns to school inputs are heterogeneous, with important consequences for school quality measurement and accountability. Second, the results imply that parents pick schools specifically effective at improving their children's learning, increasing the efficiency of school seats allocation. This provides novel evidence on selection of comparative advantage in achievement production, and implies that school choice may be an effective policy tool to enhance productivity of the education system.

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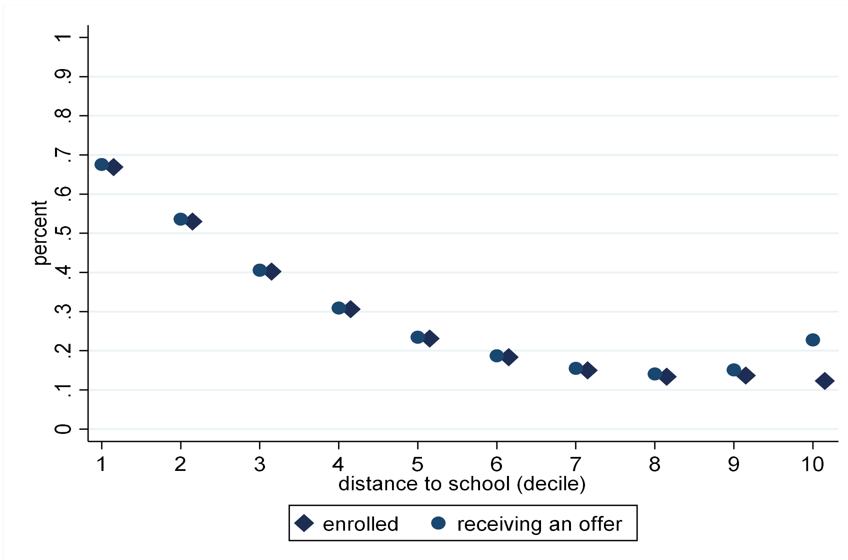
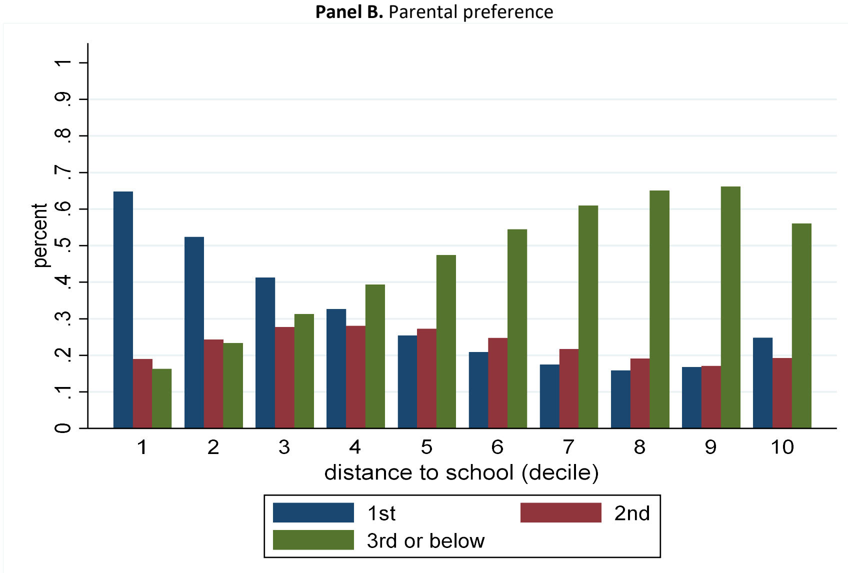
Figure 1: School admission and distance to school

Panel A. School admission

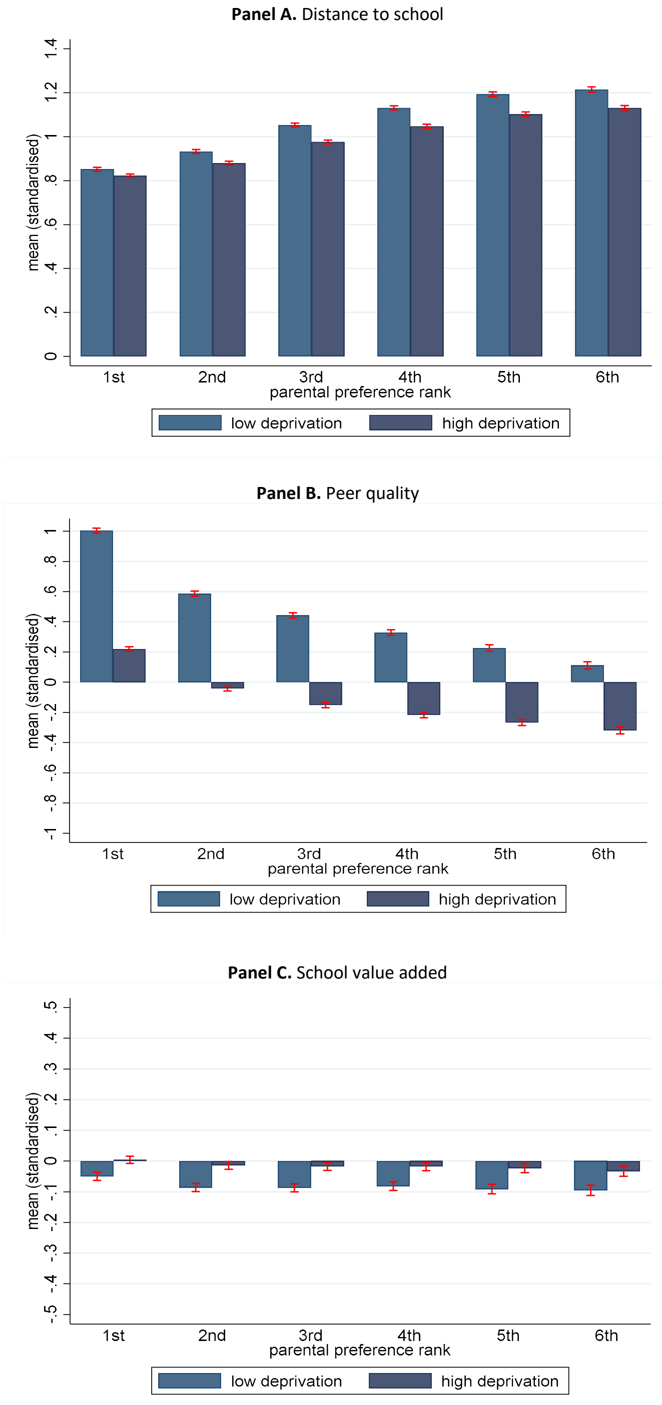


Panel B. Parental preference



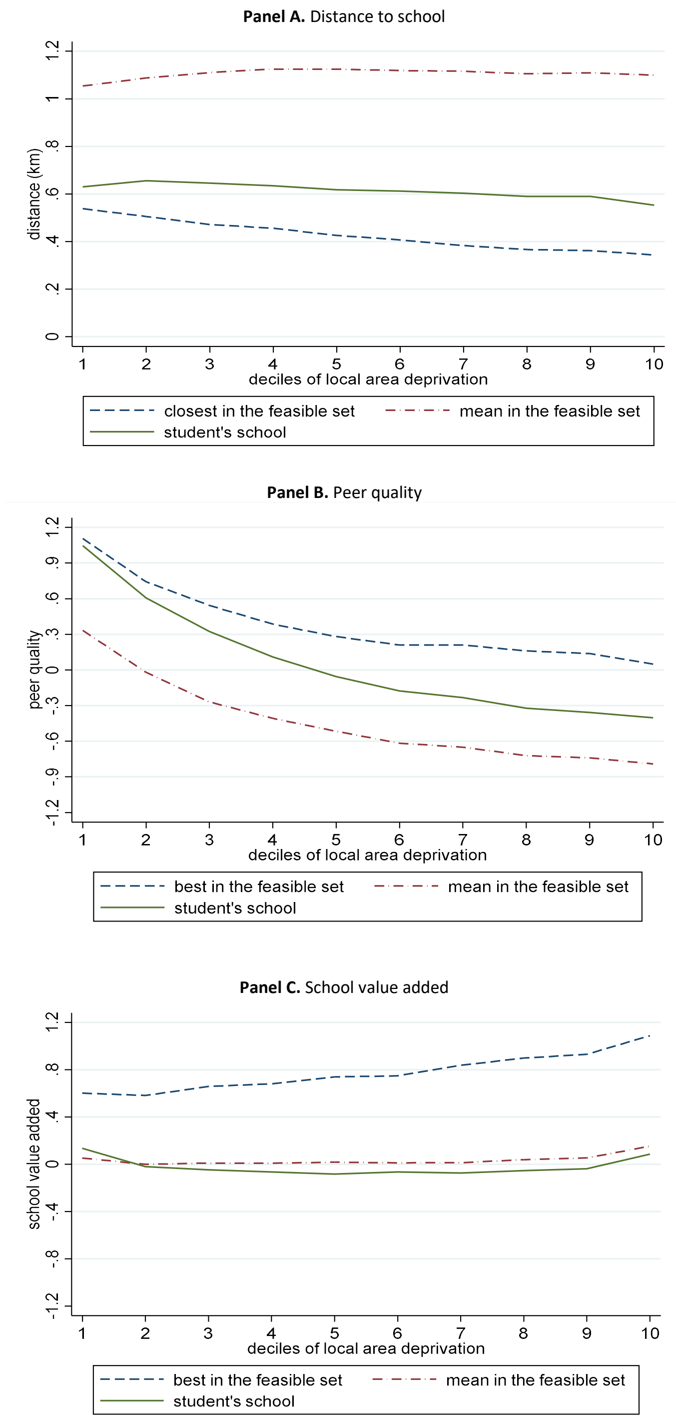
Note. The figure depicts school offer and enrolment rate and and parental preferennce for the school by distance, conditional on applying for the school. Offer is reported by markers in Panel A, while diamonds represent enrolment measured at the reception year. Bars in Panel B represent the share of 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.

Figure 2: Parental rankings and school attributes



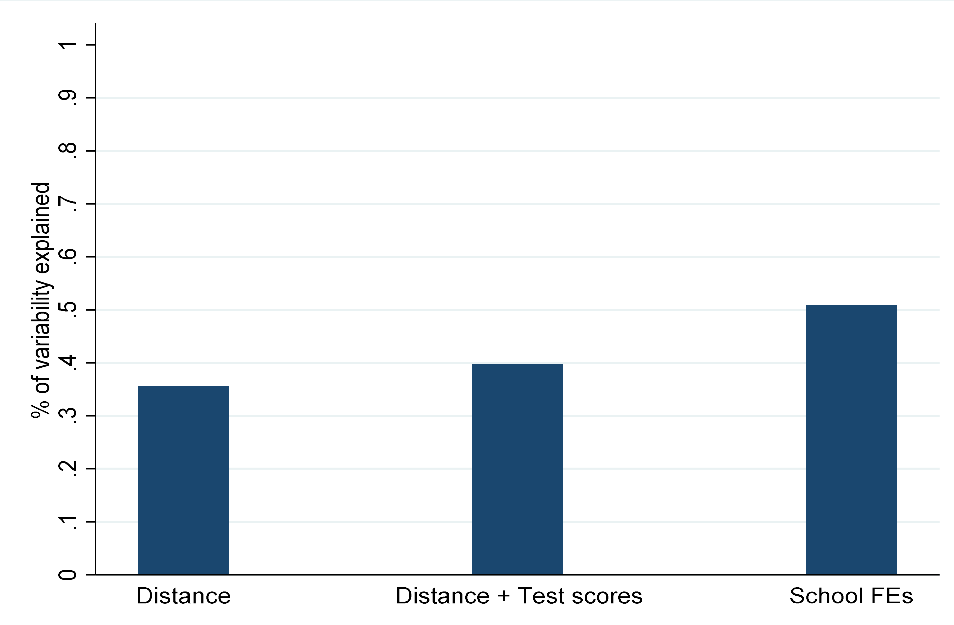
Note. The figure depicts average school attributes by parental rank estimated from equation (1). Bars plot predicted values from OLS regressions controlling for school feasibility and n. of schools listed, separately for students with local deprivation above or below the median. Controls also include dummies for quintile groups of school attributes other than the one considered, e.g. distance and peer quality when considering value added. Superimposed in red are 95% confidence intervals of predicted values. Panel A plots distance to school in kilometers computed as linear distance between student postcode and school postcode centroids. Panel B plots peer quality measured by school-level final year test scores averaged across subjects. Panel C plots school value added, estimated by regression-adjusted test scores growth at the school and averaged across subjects. Peer quality and value added are standardised among primary schools in London. Deprivation index is based on average income in the LSOA of residence. See Section 4 for details.

Figure 3: Attributes of feasible schools



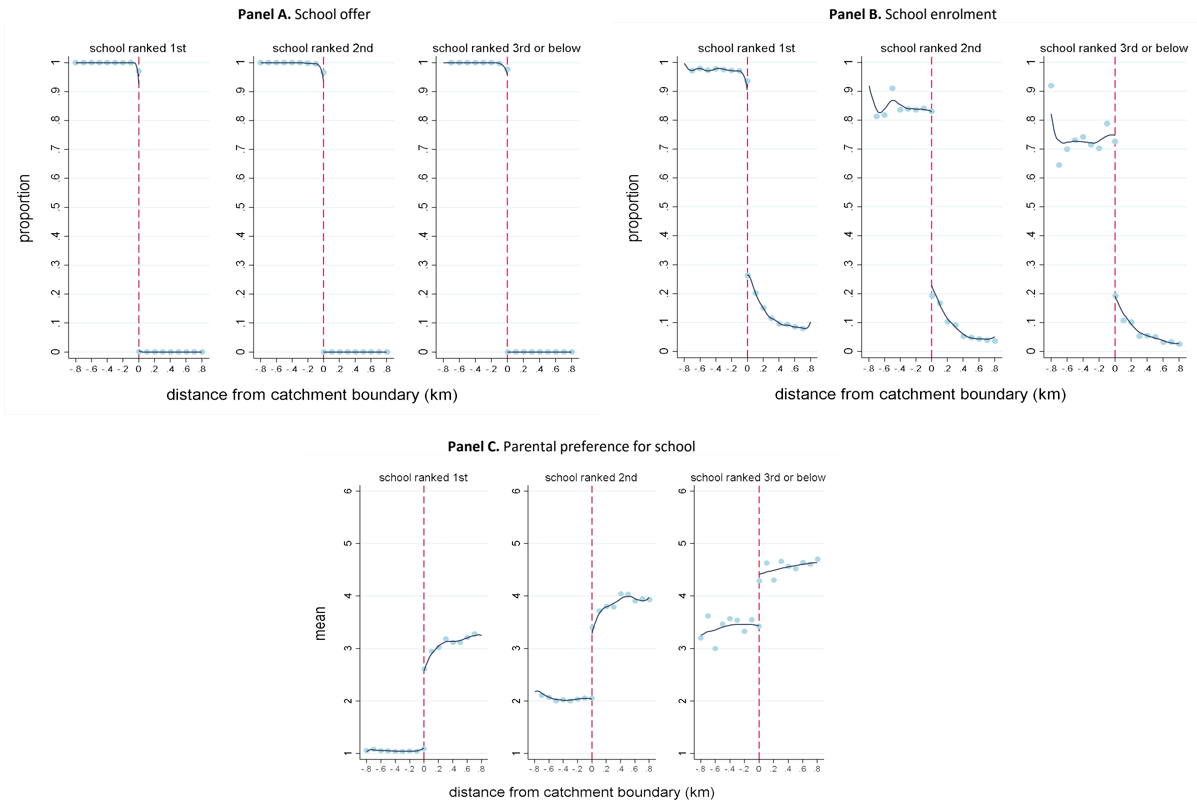
Note. The figure depicts average attributes of feasible schools by decile of deprivation index. Solid lines represent the school where an applicant enrols, dashed lines represent the best feasible school based on the attribute considered, and dashed and dotted lines represent the average attribute among feasible schools. Panel A plots distance to school, in kilometers. Distance to school is computed as linear distance between student postcode and school postcode centroids. Panel B and C plot peer quality and school value added, respectively, standardised among primary schools in London. Peer quality is measured by school-level final year test scores, while value added is estimated by regression-adjusted test scores growth at the school. Deprivation index is based on average income in the LSOA of residence. See Section 4 for details.

Figure 4: Variability in parental rankings



Note. The figure depicts explained variability in parental preferences by school characteristics. Plotted is the adjusted R-squared index from OLS regressions of parental preference for the school on distance deciles, peer quality deciles or school fixed effects. I consider all schools ranked by parents together with other institutions in individual choice set (these are ex-post feasible schools where parents did not apply). Parental rank of non-listed schools is coded to 7. Regressions further controls for ex-post feasibility and n. of preferences expressed. See Section 4 for details.

Figure 5: Research design



Note. The figure depicts school offer (Panel A), enrolment (Panel B), and parental rank (Panel C) around catchment boundary for schools ranked first, second and third or below at application. Enrolment is measured at the reception year. Preference for the school varies from 1 to 6 indicating first and sixth choice, respectively. Where an applicant is enrolled in none of listed schools, parental rank is coded to 7. 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 catchment 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 catchment boundary and to applicants at risk of admission at the school, i.e. those with no priority and not eligible at any school ranked higher. See Section 5 for details.

Table 1: Estimated covariate discontinuities

	Uncontrolled	Discontinuity at the boundary		
	(1)	(2)	(3)	(4)
Free school meal eligible	-0.0133***	-0.0149*	-0.0139	-0.0140
	(0.0014)	(0.0084)	(0.0085)	(0.0086)
Female	0.0037*	0.0051	0.0106	0.0121
	(0.0021)	(0.0137)	(0.0139)	(0.0142)
Special Education Needs	-0.0017***	0.0018	0.0016	0.0016
	(0.0004)	(0.0019)	(0.0019)	(0.0021)
White	0.0193***	0.0120	0.0114	0.0045
	(0.0020)	(0.0118)	(0.0119)	(0.0121)
Black	-0.0162***	0.0177**	0.0146*	0.0132*
	(0.0015)	(0.0076)	(0.0077)	(0.0078)
Asian	0.0116***	0.0018	0.0063	0.0058
	(0.0016)	(0.0079)	(0.0082)	(0.0083)
English as additional language	0.0057***	0.0093	0.0181	0.0238*
	(0.0020)	(0.0125)	(0.0125)	(0.0126)
Deprivation in area of residence (LSOA)	-0.0160***	-0.0029	-0.0010	-0.0001
	(0.0006)	(0.0027)	(0.0027)	(0.0023)
% of population with higher education (LSOA)	0.0094***	-0.0016	-0.0019	-0.0016
	(0.0006)	(0.0015)	(0.0015)	(0.0012)
Achievement at Year 0	0.0287***	-0.0049	-0.0053	-0.0002
	(0.0043)	(0.0135)	(0.0136)	(0.0140)
N (Free school meal eligible)	361,880	42,127	41,702	41,593
Parental rank and n. of preferences FEs	Y	Y	Y	Y
Running variable LLP controls		Y	Y	Y
School of application FEs		Y	Y	Y
Next-best school FEs			Y	Y
Area of residence (MSOA) FEs				Y

Note. This table shows estimates of covariate balance around catchment boundary. Column (1) reports OLS estimates of mean difference in baseline characteristics by school offer status, conditional on parental rank and n. of schools listed. Columns (2) to (4) restrict the sample to applicants with no admission priority and who cannot enter any institution listed with higher preference. Reported are estimates of offer balance from equation (2), where controls include a local linear polynomial of distance to the catchment boundary, estimated separately on each side of the cut-off. Observations are weighted by a triangular kernel with optimal data-driven bandwidth estimated following Calonico et al. (2014) separately for each outcome variable. Number of observations reported refer to regressions of free school meal eligibility. Specifications in columns (2) to (4) include school of application fixed effects, column (3) adds fixed effects for the next-best school listed by parents and column (4) add neighbourhood of residence (MSOA) fixed effects. All specifications control for individual characteristics other than the one considered as dependent variable. Standard errors are clustered at the individual level and reported in parenthesis. See Section 5 for details. ***p<0.01. ** p<0.05. * p<0.1

Table 2: Effects of attending the school of choice

	All		
	(1)	(2)	(3)
Panel A. First Stage			
Enrolled in the school of choice	0.6299***	0.6337***	0.6268***
	(0.0180)	(0.0182)	(0.0188)
N	14,733	14,392	14,319
F-statistics	1227.81	1218.63	1110.14
Panel B. Reduced form estimates			
All subjects	0.0201	0.0264	0.0226
	(0.0191)	(0.0193)	(0.0194)
Mathematics	0.0583***	0.0650***	0.0567**
	(0.0216)	(0.0221)	(0.0224)
Reading	-0.0069	0.0025	-0.0004
	(0.0240)	(0.0245)	(0.0250)
Writing	0.0101	0.0136	0.0174
	(0.0250)	(0.0256)	(0.0260)
Panel C. 2SLS estimates			
All subjects	0.0298	0.0390	0.0336
	(0.0284)	(0.0285)	(0.0289)
Mathematics	0.0858***	0.0951***	0.0835**
	(0.0318)	(0.0323)	(0.0330)
Reading	-0.0103	0.0038	-0.0006
	(0.0360)	(0.0366)	(0.0375)
Writing	0.0152	0.0203	0.0262
	(0.0376)	(0.0382)	(0.0391)
N (All subjects)	122,046	120,963	120,780
Parental rank FEs	Y	Y	Y
School of application FEs	Y	Y	Y
Next-best school FEs		Y	Y
Area of residence (MSOA) FEs			Y

Note. This table shows estimates of the effect of attending the school of choice on student learning. Sample is restricted to applicants to the first choice and applicants to lower ranked schools conditional on missing out on all more-preferred institutions. Reported in Panel A are first stage coefficients on school offer estimated from equation (7). Reported in Panel B are reduced form estimates of school offer coefficient, while 2SLS estimates of the school enrolment coefficient from equation (3), instrumented using school offer status, are reported in Panel C. Dependent variable is an indicator for scoring above standards at Year 2 assessments by subject. Reported are also coefficients from a specifications stacking all subjects and controlling for subject fixed effects. In all regressions, controls include a local linear polynomial of distance to the catchment boundary, estimated separately on each side of the cut-off. Observations are weighted by a triangular kernel with optimal data-driven bandwidth estimated following Calonico et al. (2014). Specifications in columns (1) to (3) include school of application fixed effects, column (2) adds fixed effects for the next-best school listed by parents and column (3) add neighbourhood of residence (MSOA) fixed effects. Standard errors are clustered at the individual level and reported in parenthesis. See Section 6 for details. ***p<0.01. ** p<0.05. * p<0.1

Table 3: Effects of attending the school of choice conditional on school value added

	(1)	(2)	(3)
	Panel A. First Stage		
School enrolment at Year 2	0.3854***	0.3911***	0.3819***
	(0.0200)	(0.0196)	(0.0195)
N	21,306	20,962	20,884
F-statistics	371.94	399.87	383.43
	Panel B. Reduced form estimates		
All subjects	0.0566*	0.0582*	0.0511*
	(0.0298)	(0.0307)	(0.0305)
Mathematics	0.1021***	0.1092***	0.0893**
	(0.0325)	(0.0340)	(0.0348)
Reading	0.0067	0.0097	-0.0001
	(0.0329)	(0.0341)	(0.0344)
Writing	0.0471	0.0427	0.0436
	(0.0380)	(0.0398)	(0.0402)
	Panel C. 2SLS estimates		
All subjects	0.1431*	0.1458*	0.1293*
	(0.0756)	(0.0770)	(0.0773)
Mathematics	0.2525***	0.2677***	0.2216**
	(0.0809)	(0.0839)	(0.0865)
Reading	0.0388	0.0380	0.0160
	(0.0518)	(0.0525)	(0.0789)
Writing	0.1200	0.1076	0.1113
	(0.0968)	(0.1006)	(0.1028)
N (All subjects)	85,296	84,441	84,318
School of application FEs	Y	Y	Y
Parental rank FEs	Y	Y	Y
Next-best school FEs		Y	Y
Area of residence (MSOA) FEs			Y

Note. This table shows estimates of the effect of attending the school of choice on student learning from specifications similar to Table 2, restricting the sample to students enroled in schools with similar value added than the school of choice. Students considered here are those with school where enroled and demanded school in the same quintile of value added. See Section 6 for details. ***p<0.01. ** p<0.05. * p<0.1

Table 4: Effects of attending the school of choice conditional on enrolment

	(1)	(2)	(3)
	Panel A. First Stage		
School enrolment at Year 2	0.3439***	0.3441***	0.3314***
	(0.0215)	(0.0214)	(0.0218)
N	14,010	13,636	13,550
F-statistics	256.85	259.60	232.12
	Panel B. Reduced form estimates		
All subjects	0.0487**	0.0512**	0.0492**
	(0.0200)	(0.0203)	(0.0203)
Mathematics	0.0937***	0.0985***	0.0912***
	(0.0230)	(0.0237)	(0.0241)
Reading	0.0189	0.0198	0.0194
	(0.0257)	(0.0265)	(0.0268)
Writing	0.0306	0.0341	0.0401
	(0.0268)	(0.0278)	(0.0279)
	Panel C. 2SLS estimates		
All subjects	0.0948**	0.0987**	0.0950**
	(0.0390)	(0.0393)	(0.0392)
Mathematics	0.1785***	0.1857***	0.1721***
	(0.0439)	(0.0448)	(0.0455)
Reading	0.0374	0.0388	0.0380
	(0.0509)	(0.0518)	(0.0525)
Writing	0.0606	0.0668	0.0786
	(0.0532)	(0.0544)	(0.0548)
N	122,043	120,960	120,777
School of application FEs	Y	Y	Y
Parental rank FEs	Y	Y	Y
School where enroled FEs	Y	Y	Y
Next-best school FEs		Y	Y
Area of residence (MSOA) FEs			Y

Note. This table shows estimates of the effect of attending the school of choice on student learning from specifications similar to Table 2, adding a full set of school of enrolment fixed effects. See Section 6 for details. ***p<0.01. ** p<0.05. * p<0.1

Table 5: Effects of an additional year in the school of choice

	All		Hold schol value added constant
	(1)	(2)	(3)
	Panel A. First stage		
Enrolled at school of choice (reception year)	1.7073***	0.8073***	0.9198***
	(0.0507)	(0.0473)	(0.0567)
N	16,185	15,424	19,511
F-statistics	1133.63	291.08	263.21
	Panel B. 2SLS estimates		
All subjects	0.0148	0.0415**	0.0740**
	(0.0106)	(0.0166)	(0.0376)
Mathematics	0.0356***	0.0779***	0.1496***
	(0.0121)	(0.0189)	(0.0427)
Reading	0.0020	0.0166	0.0193
	(0.0136)	(0.0219)	(0.0451)
Writing	0.0077	0.0281	0.0484
	(0.0143)	(0.0230)	(0.0450)
N (All subjects)	120,963	120,960	84,441
Parental rank FEs	Y	Y	Y
School of application FEs	Y	Y	Y
School where enroled FEs		Y	
Next-best school FEs	Y	Y	Y

Note. The table shows the effects of spendng one additional year at the school of choice on student achievement. Endogenous treatment is a categorical variable representing the number of years a student is enrolled at the school of choice, from 0 to 3. Achievement is observed after three years of primary education. Columns (1), (2) and (3) report estimates from specifications similar to column (2) of Tables 2, 3 and 4, respectively. See Section 6 for details. ***p<0.01. ** p<0.05. * p<0.1

Table 6: Potential mechanisms

	All		Hold schol value added constant
	(1)	(2)	(3)
Distance to school (s.d.)	-0.6339***	-0.5039***	-0.7797***
	(0.0395)	(0.0507)	(0.0690)
Percentile ability rank in school-cohort	-0.0097	-0.0042	-0.0206
	(0.0077)	(0.0111)	(0.0190)
Parental rank FEs	Y	Y	Y
School of application FEs	Y	Y	Y
School where enroled FEs		Y	
Next-best school FEs	Y	Y	Y

Note. The table shows the effects of attending the school of choice on characteristics of pupil-school match. Columns (1), (2) and (3) report estimates from specifications similar to column (2) of Tables 2, 3 and 4, respectively. Distance to school is measured as linear distance between student postcode of residence and school postcode, and standardised to have zero mean and unit variance in the working sample. Ability rank is measured as percentile rank in Year 0 assessments in school-cohort following Murphy and Weinhardt (2020). See Section 6 for details. ***p<0.01. ** p<0.05. * p<0.1

Appendix

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.[35]

First, I replicate centralised assignment based solely on distance to school and parental preference. I rank applicants to a school in ascending order of distance and iteratively eliminate candidates who are eligible at schools ranked with higher preference. Without observing priorities, this is not sufficient to replicate school offer. As shown in Appendix Figure A.2, catchment boundary estimated solely based on distance fails to retrieve the discontinuity in school offer embedded in centralised assignment. 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 catchment boundary estimated solely based on distance receives school offer, she must have been admitted with priority. In the first step, catchment boundaries are overestimated as admission priority is ignored. The distance to school of last admitted applicant is an upper bound of the true threshold as some candidates are admitted with priority. Therefore, any school offer granted to applicants located beyond the initially estimated threshold reveals priority in admission. These applicants are flagged and replication of school assignment is re-attempted by admitting them first. The procedure is iterated until no applicant with offer is found beyond the estimated threshold.

In details, the algorithm I set up works as follows.

1. Rank all applicants, regardless 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. As it is unobserved, all students start in the same priority group.

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

[35] 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), as shown in Appendix Figure A.1.

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).

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

3. Repeat step 2 until no more applicants are dropped. Assignment converges in at most 15 iterations.

4. Assign priority to applicants who are admitted to school according to administrative records but who are ranked beyond school capacity after steps 1-3.

5. Repeat steps 1-4 until no more applicants with priority are detected. The algorithm converges in 131 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 admitted because of school priority. At 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.

Convergence is shown in Panel A of Appendix Figure A.3, plotting the fraction of applicants with priority identified in each iteration, and showing this monotonically decreases to zero. Panel B of Appendix Figure A.3, depicting errors in school assignment by iteration, shows my assignment almost perfectly corresponds to actual school offer when the procedure is concluded. Consistent with the idea that catchment boundary is overestimated when ignoring priority, Panel C of Appendix Figure A.3 shows that median distance threshold monotonically decreases as applicants with priority are detected.

In an effort to validate the priority measure produced by my algorithm, I compare it with a proxy for siblings at the school, constituting the main source of unobserved priority in my context. I consider all students enroled at the school of choice at the time of application and compute the number of students located in the same postcode of a given applicant.[36] This is an upper bound of the number of siblings at the school, but it arguably provides an interesting proxy given the granularity of postcodes in London (the 80% of applicants' school-postcode combinations is not matched by any currently enrolled student).[37] Moreover, among students located beyond the catchment boundary of the first choice, about 80% of those with a schoolmate in the same postcode receive an offer, suggesting that my proxy effectively captures admission priority. Appendix Figure A.7 shows that, as expected, the share of students estimated to have a sibling at the school varies smoothly around the catchment boundary, suggesting unobserved priority is not a serious concern for my analysis. The share of applicants with admission priority according to my algorithm, which are detected only beyond catchment, is in line with the sibling proxy. As expected, the former fraction is generally higher than the latter, reflecting that the sibling proxy is likely to be an upper bound of the true unobservable measure.

Construction of individual feasible school set

I define the individual feasible school set exploiting school catchment boundaries I obtained from replication of centralised school assignment (see Section 3 above). I compute linear distance between student postcode and all schools around, 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 100,000 applicants and 1,700 schools in my sample.

I define a school as ex-post feasible if the student is located within catchment 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. Admission is often loosely related to distance as religious schools are allowed to prioritise applicants based on faith criteria. Non-religious undersubscribed schools are included in the individual choice set if they are located within 2 km from student postcode, corresponding to the 90th percentile of distance to school.

[36] I keep school-postcode cells with at most two students, as higher counts are more likely to reflect densely populated postcodes rather than potential siblings at the school.

[37] Residential mobility across postcodes is a potential source of error in this proxy. However, it concerns a small fraction of students, as show in Appendix Figure A.4, which I discuss further below.

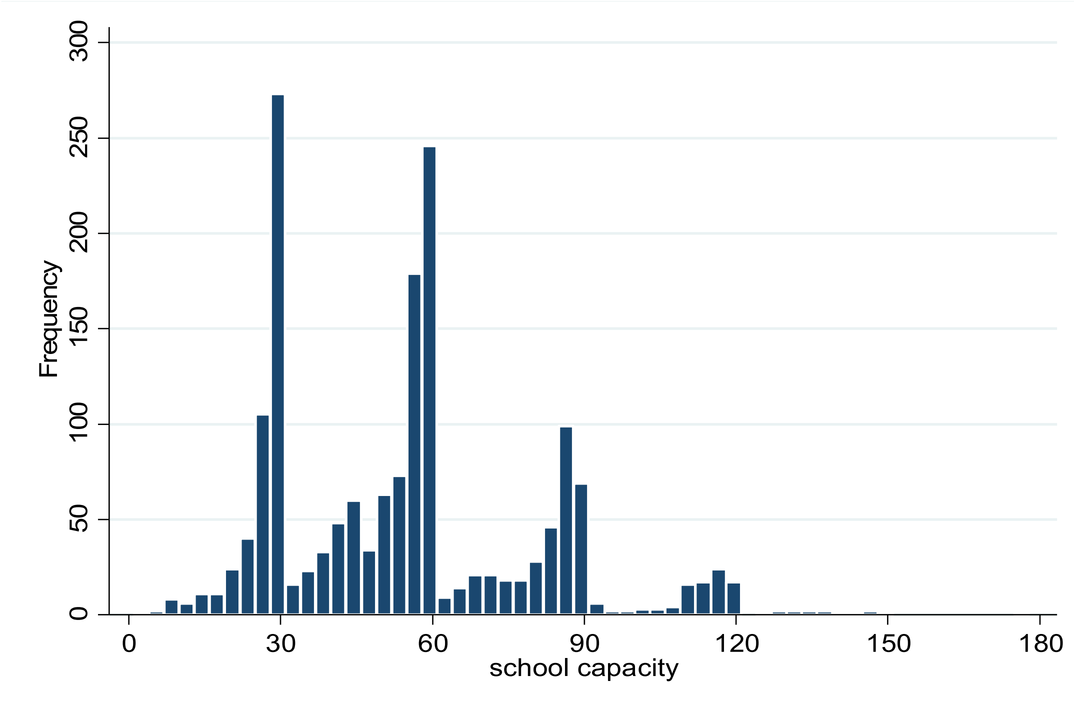
The individual feasible school is defined as the collection of ex-post feasible schools.

Parental choice and school mobility

Parents move their children to a different school after reception year based on peer quality rather than school value added, consistently with application behaviour described in Section 4 above. Panel A of Appendix Table A.3 presents estimates of linear regressions of school mobility on school attributes for the sample of students at risk of admission. One σ higher difference in peer quality between offered and desired school is associated with 6-7 percentage point higher likelihood of moving to another school after reception year. This difference persists, substantially unchanged, when controlling for school choice covariates as well as individual socioeconomic characteristics (see columns 2 and 3). On the contrary, the estimated coefficient on school value added is much lower, about 1 percentage point. Residential mobility, likely involving larger costs, is almost unrelated to relative attributes of school offered and the school of choice (see Panel B of Appendix Table A.3). Given the evidence on residential sorting discussed in Section 4 above, this result suggests that parents who are willing to move their residence secure location close to desired schools before the assignment takes place.

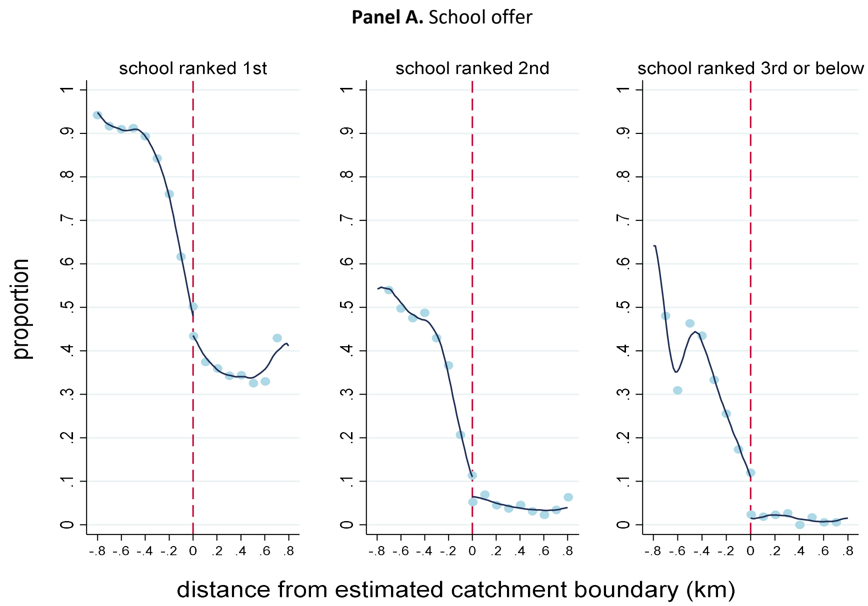
School mobility response to centralised assignment implies that 2SLS estimates of β_1 in equation (8) capture a combination of initial enrolment and school mobility induced by school offer. Students located just beyond the catchment boundary are about 10 percentage points more likely to move to another school after reception year, as shown in Panel A of Appendix Figure A.6. As achievement is measured in Year 2, the relationship between school offer, initial enrolment (denoted by D_0), and enrolment at Year 2 (denoted byD_1), is represented by the directed acyclic graph (DAG, Abadie and Cattaneo, 2018) in Appendix Figure A.5. School offer, as good as randomly assigned around the catchment boundary, affects achievement only through initial enrolment. The latter, however, leads to the outcome of interest combining two different channels: the direct impact of school where the student initially enrols and the indirect impact of increased school mobility based on initial enrolment. To account for increased school mobility in the control group, I additionally present estimation results defining treatment as number of years spent in the school of choice in Section 6 above.

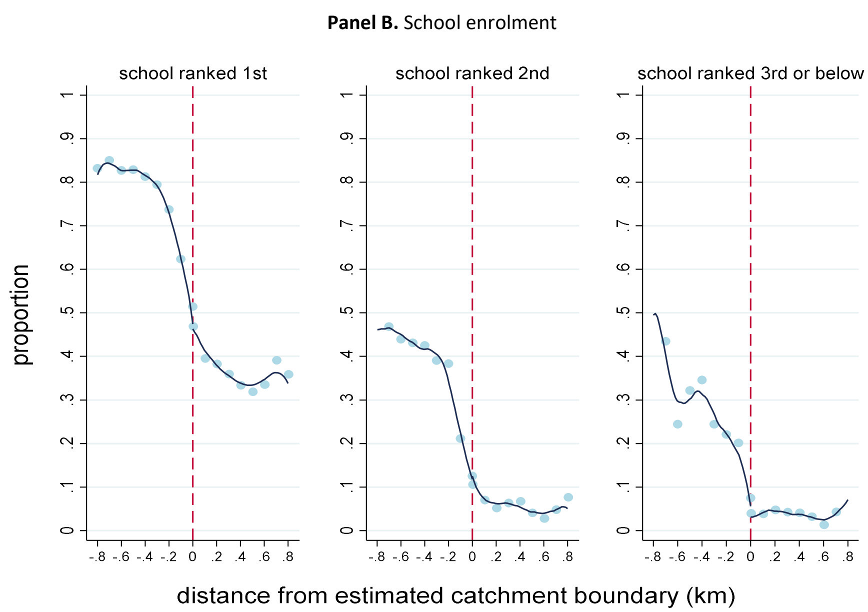
Figure A.1: School capacity



Note. The figure depicts 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. See Section 3 for details.

Figure A.2: School assignment solely based on distance

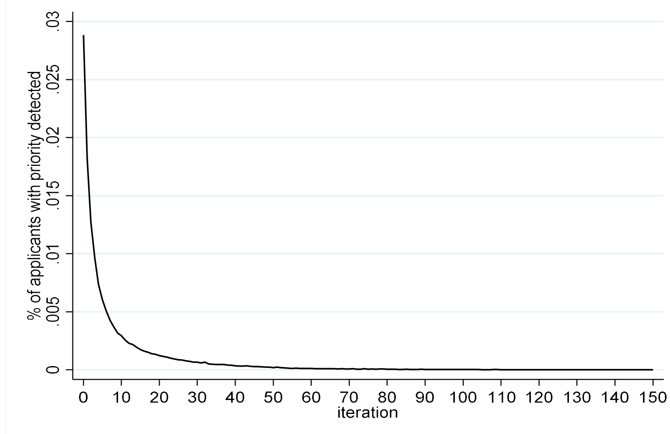




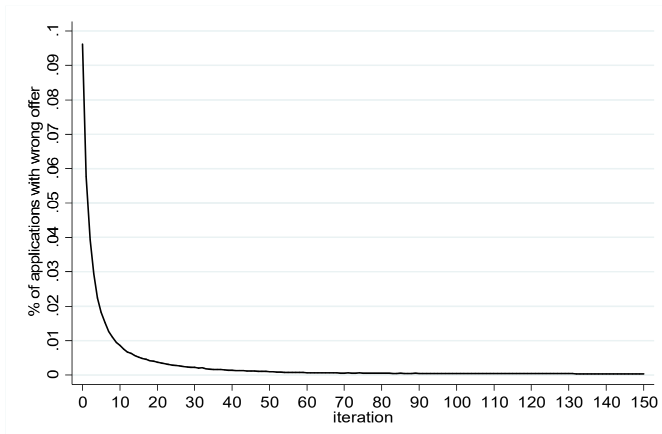
Note. The figure depicts school offer (Panel A) and enrolment (Panel B) around catchment boundary estimated by ranking applicants solely by distance to school. Sub-panel graphs group schools ranked first, second and third or below at application. School enrolment is measured at Year 2, when achievement is assessed. Distance to estimated 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 estimated catchment. Markers represent average values in 100-meters-wide bins of the running variable and 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 catchment boundary and to applicants at risk of admission at the school, i.e. those who can not enter any institution listed with higher preference. See Section 5 for details.

Figure A.3: Replication of school assignment

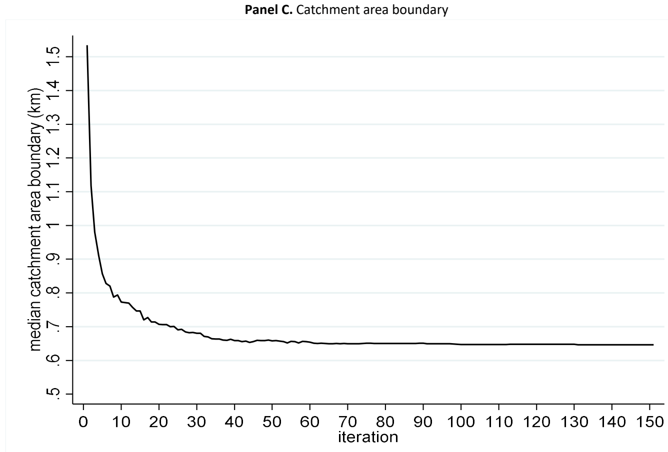
Panel A. Priority over distance



Panel B. Error in school offer

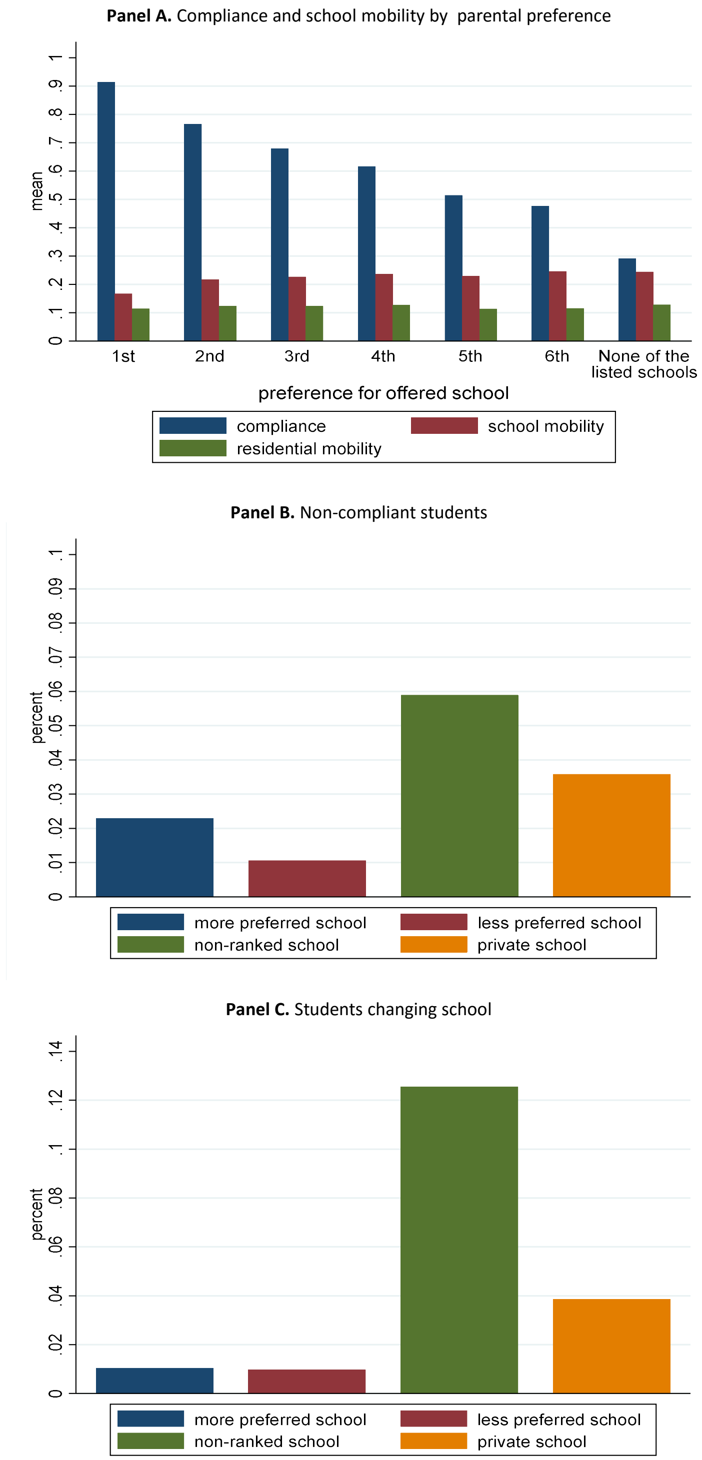


Panel C. Catchment area boundary



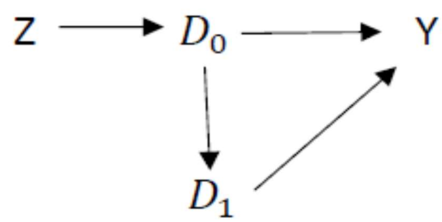
Note. The figure depicts the fraction of applicants with admission priority detected (Panel A), the fraction of applicants with wrong predicted offer (Panel B), and median catchment area boundary (Panel C) by iteration of the school assignment replication. School assignment mechanism is replicated based on school capacity, parental preference and distance to school. Applicants are ranked solely by proximity in iteration 0 and those with offer beyond estimated boundary are flagged as enjoying priority. Subsequent iterations rank pupils by priority as retrieved in the previous round and, conditional on priority, by distance to school. Assignment converges in 131 iterations, after which no more applicants are found to enjoy priority. See Section 3 and the Appendix for details.

Figure A.4: Compliance with assignment and mobility



Note. The figure depicts compliance and mobility patterns 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. See Section 6 for details.

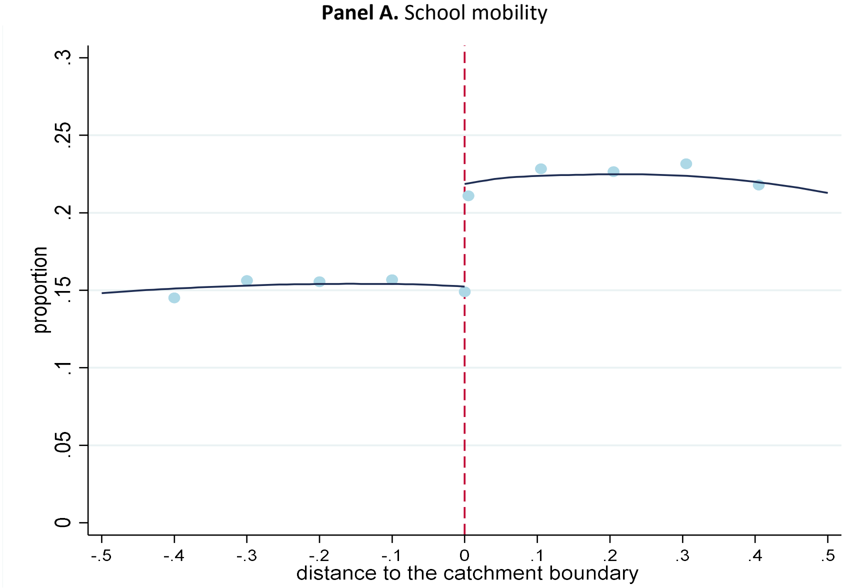
Figure A.5: Offer, enrolment and outcome in a DAG



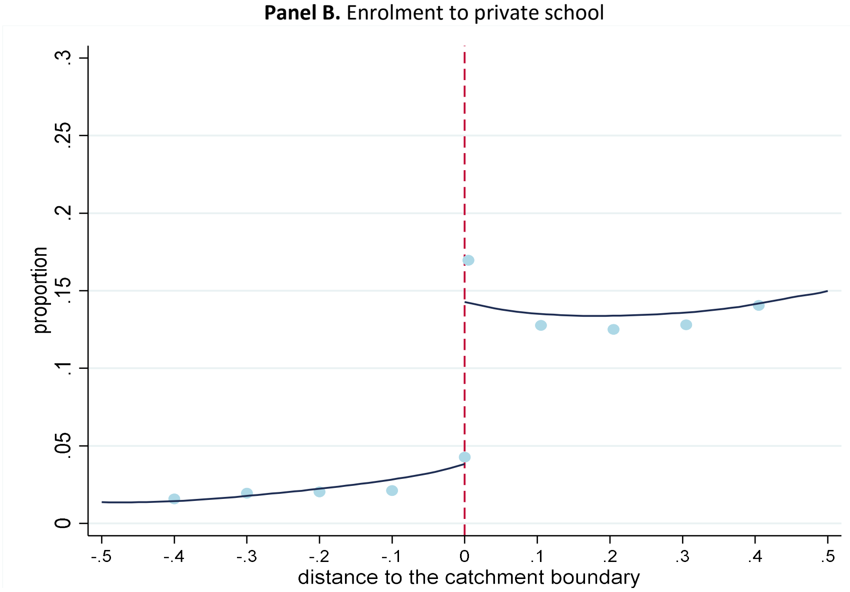
Note. The relationship between instrument, treatment and outcome in a directed acyclic graph. The graph includes initial school enrolment, D0; enrolment at Year 2, D1; and the achievement outcome, Y. See section 6 for details.

Figure A.6: School mobility and private school enrolment around the catchment boundary

Panel A. School mobility

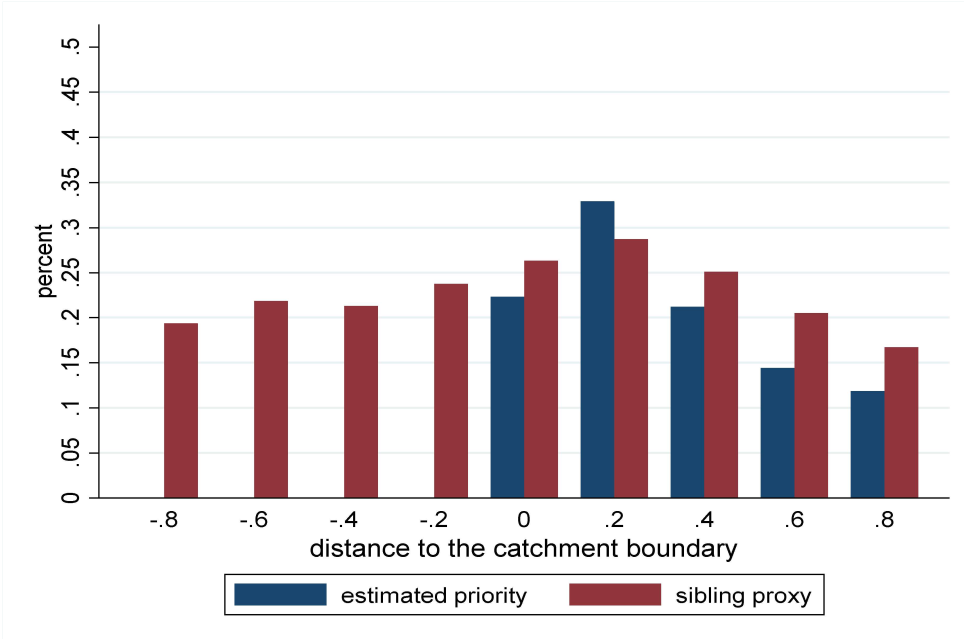


Panel B. Enrolment to private school



Note. The figure depicts school mobility (Panel A) and enrolment to private school (Panel B) around catchment boundary for schools of choice, pooling institution ranked first to sixth at application. School mobility is an indicator variable equal to one if a student enrols in a different school in Year 2 with respect to the reception year. Enrolment to private school is an indicator variable equal to one if an applicant is not observed in any state school in the reception year. 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 catchment 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 500 meters from catchment boundary and to applicants at risk of admission at the school, i.e. those with no priority and not eligible at any school ranked higher. See Section 6 for details.

Figure A.7: Estimated priority and proxy for siblings at the school of choice



Note. The figure depicts the share of applicants estimated to enjoy admission priority (blue bars) or to have a sibling at the school of choice (red bars) as a function of distance to the catchment boundary. Reported are averages in 200-meters-wide bins of distance from catchment boundary for students living within 800 meteres from the cut-off. Admission priority is proxied by receving a school offer while leaving outside the catchment area. Having a sibling at the school is proxied by the number of students at the school of choice living at the same postcode at the time of application. I exclude from the sibling proxy student-postocode combinations with more than 2 potential schoolmates (about 20%). See the Appendix for details.

Table A.1: Descriptive statistics

	London	Rest of England	Difference (1 - 2)
	(1)	(2)	(3)
Baseline Characteristics			
FSM eligible	0.1527	0.1362	0.0165***
Not speaking English at home	0.4211	0.1199	0.3012***
White	0.4138	0.7765	-0.3627***
Asian	0.1955	0.0776	0.1179***
Black	0.1629	0.0223	0.1406***
Special education needs	0.0080	0.0064	0.0015***
Female	0.4899	0.4896	0.0003
Exceeding expectations at Year 0: mathematics	0.1325	0.1250	0.0075***
Achievement outcomes			
Exceeding expectations at Year 2: mathematics	0.2666	0.2145	0.0521***
Exceeding expectations at Year 2: reading	0.3019	0.2626	0.0393***
Exceeding expectations at Year 2: writing	0.2029	0.1602	0.0428***
School choice variables			
N. of schools listed	3.2069	--	
Ranked 1 choice	0.2709	0.3801	-0.1092***
Ranked at least 3 choices	0.5728	0.4343	0.1386***
Ranked 6 choices	0.2140	--	
Offered 1st choice	0.8276	0.8944	-0.0669***
Offered one of the top three choices	0.9427	0.9684	-0.0257***
Offered one of ranked choices	0.9687	--	
Enroled at offered school at reception year	0.8717	0.8975	-0.0258***
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. Columns (1) and (2) report averages computed using one observation per pupil, 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 A.2: Oversubscribed schools

	Popular schools	Not popular schools	Difference (1-2)
	(1)	(2)	(3)
Peer quality			
Sixth grade mathematics score	0.3651	-0.5877	0.9529***
Sixth grade reading score	0.3634	-0.6027	0.9661***
School effectiveness			
School value added in mathematics	0.0640	-0.1018	0.1657***
School value added in reading	0.0840	-0.1337	0.2177***
School type			
Religious school	0.2184	0.1292	0.0893***
Academy school	0.1406	0.1930	-0.0525***
Community school	0.5394	0.6067	-0.0673***
Peer composition			
% FSM eligible students	0.1843	0.2882	-0.1039***
% white students	0.4767	0.3654	0.1025***
Income deprivation in student loca area (LSOA)	0.3159	0.4012	-0.0853***
N	1053	689	1742

Note. This table shows characteristics of London primary schools by oversubscription status in 2014. Column (1) and column (2) report means for oversubscribed and undersubscribed schools respectively, while mean difference is reported in column (3). A school is coded as oversubscribed 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, while value added is estimated by regression-adjusted test scores growth at the school. Both measures are computed at baseline considering previous cohorts and are standardised to have zero mean and unit variance in the working sample. 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 4 for details. *******p<0.01. ** p<0.05. * p<0.1

Table A.3: School mobility and school attributes

	(1)	(2)	(3)
	Panel A. School mobility		
Peer quality difference	0.0739***	0.0667***	0.0694***
	(0.00379)	(0.00400)	(0.00396)
Distance difference	-0.0468***	-0.0207***	-0.0249***
	(0.00671)	(0.00698)	(0.00665)
School value added difference	0.00673*	0.00807**	0.0103***
	(0.00366)	(0.00369)	(0.00355)
N	63,080	61,145	58,079
	Panel B. Residential mobility		
Peer quality difference	0.00420	0.00406	0.00333
	(0.00288)	(0.00310)	(0.00324)
Distance difference	-0.00196	-0.00128	-0.00145
	(0.00504)	(0.00547)	(0.00554)
School value added difference	-0.00145	-0.000527	-0.00170
	(0.00287)	(0.00289)	(0.00291)
N	61,540	59,693	57,906
School choice controls		Y	Y
Individual characteristics			Y

Note. This table shows correlation between school mobility and school attributes. Sample is restricted to applicants to the first choice and applicants to lower ranked schools conditional on missing out on all more-preferred institutions. 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 variable are difference between characteristics of the school of choice and of the school offered. Peer quality and school value added are standardised to have zero mean and unit variance across London primary schools. Distance is measured in kilometers. Control variables include level of school characterstics. Column (2) adds n. of preferences expressed, preference for the school, preference for the school offered, ex-post feasibility of the school. Column (3) adds individual socioeconomic characteristics: gender, free lunch eligibility, special education needs, ethnicity, language, deprivation in area of residence and baseline achievement. Standard errors are clustered at the student level and reported in parentheses. See Section 6 for details.

*** p<0.01, ** p<0.05, * p<0.1

Table A.4: Differential attrition

	Non-offered follow-up rate (1)	Offer differential (2)
First choice	0.7950***	0.1451***
	(0.0093)	(0.0092)
Second choice	0.8036***	0.0865***
	(0.0271)	(0.0244)
Third choice or lower	0.8287***	0.0309
	(0.0349)	(0.0304)
All choices	0.8121***	0.1224***
	(0.0081)	(0.0078)
N (all choices)	48,587	

Note. This table shows differential follow-up rates by offer status. Reported are estimates from linear regressions of follow-up indicator on school offer dummy. Dependent variable is an indicator equal to 1 if a student's KS1 achievement is observed. Sample is restricted to applicants to the first choice and applicants to lower ranked schools conditional on missing out on all more-preferred institutions. Column (1) reports coefficients on the intercept term, representing average follow-up rate among non-offered students. Column (2) reports coefficients on the school offer variable. Results are presented by parental preference for the school and on average across all ranked schools ("all choices"). In all regressions, controls include a local linear polynomial of distance to the catchment boundary, estimated separately on each side of the cut-off. Observations are weighted by a triangular kernel with optimal data-driven bandwidth estimated following Calonico et al. (2014). Standard errors are clustered at the student level and reported in parentheses. See Section 6 for details.

Table A.5: Alternative empirical specifications 1

	quadratic r.v. controls			cubic r.v. controls		
	(1)	(2)	(3)	(4)	(5)	(6)
Within 500m from boundary	0.1821*	0.2139**	0.2091**	0.1691	0.2317*	0.2201*
	(0.0966)	(0.0995)	(0.1009)	(0.1161)	(0.1187)	(0.1212)
Within 800m from boundary	0.1464**	0.1481**	0.1202	0.2078**	0.2397**	0.2281**
	(0.0745)	(0.0752)	(0.0763)	(0.1010)	(0.1021)	(0.1036)
Within 1 km from boundary	0.1305**	0.1318**	0.1137*	0.1856**	0.2099**	0.1938**
	(0.0653)	(0.0661)	(0.0669)	(0.0925)	(0.0933)	(0.0954)
School of application FEs	Y	Y	Y	Y	Y	Y
Parental rank FEs	Y	Y	Y	Y	Y	Y
Next-best school FEs		Y	Y		Y	Y
Area of residence (MSOA) FEs			Y			Y

Note. This table shows 2SLS estimates of the effect of attending the school of choice on student learning from specifications similar to Table 3. Applicants are considered only if residing within a given distance from catchment boundary, indicated in rows. Independent variables include quadratic (columns 1-3) or cubic (columns 4-6) polynomial controls of distance to the catchment boundary. See Section 6 for details. ***p<0.01. ** p<0.05. * p<0.1

Table A.6: Alternative empirical specifications 2

	quadratic r.v. controls			cubic r.v. controls		
	(1)	(2)	(3)	(4)	(5)	(6)
Within 800m from boundary	0.1028**	0.1251***	0.1276***	0.0854	0.1215**	0.1233**
	(0.0458)	(0.0464)	(0.0464)	(0.0559)	(0.0566)	(0.0566)
Within 1 km from boundary	0.0819**	0.0944***	0.0950***	0.0921**	0.1207***	0.1186***
	(0.0354)	(0.0358)	(0.0356)	(0.0452)	(0.0457)	(0.0455)
Within 1.2 km from boundary	0.0659**	0.0782**	0.0806**	0.0944**	0.1104***	0.1174***
	(0.0315)	(0.0319)	(0.0317)	(0.0410)	(0.0414)	(0.0412)
School of application FEs	Y	Y	Y	Y	Y	Y
Parental rank FEs	Y	Y	Y	Y	Y	Y
School where enroled FEs	Y	Y	Y	Y	Y	Y
Next-best school FEs		Y	Y		Y	Y
Area of residence (MSOA) FEs			Y			Y

Note. This table shows 2SLS estimates of the effect of attending the school of choice on student learning from specifications similar to Table 4. Applicants are considered only if residing within a given distance from catchment boundary, indicated in rows. Independent variables include quadratic (columns 1-3) or cubic (columns 4-6) polynomial controls of distance to the catchment boundary. See Section 6 for details. ***p<0.01. ** p<0.05. * p<0.1