# Achievement returns to parental choice: evidence from centralised assignment in London primary schools *

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**Abstract**

Parents rank preferred public schools at application in an increasing number of districts around the world. Oversubscription implies a significant fraction of applicants is often denied the top choice. Does missing out on sought-after schools have negative consequences on pupils' achievement? This paper exploits the regression discontinuity design embedded in centralised assignment of primary school seats in London. Identification stems from sharp discontinuities in the probability of admission as function of distance to and preference for listed schools. I find that missing out on the second choice has a negative impact on pupils' achievement, while no effect is detected for the most-preferred school. Achievement losses are driven by pupils from high-income neighbourhoods and by applicants to academy schools. Results suggest affluent parents sort in areas with two effective schools available, while disadvantaged families does not benefit in entering preferred schools. I find little evidence of results being driven by differential demand for academic performance.

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# 1 Introduction

The possibility for parents to express a preference on which school to send their children have been increasing in many school districts around the world. Education reforms have expanded parental choice of state-funded school in England and Sweden since the early 1990s. Choice plans introduced more recently ask parents to rank public schools in many of the largest U.S. districts, serving about 8 million pupils (Whitehurst, 2017), and in a number of further urban areas around the world[1]. Education policies allowing parental choice are emerging as alternative to "zip code" models, where pupils are assigned to school according to the neighbourhood of residence.

Rank-order lists of preferred schools are typically used in centralised matching mechanisms to allocate school seats. Applicants are offered a place in the highest preference school with available seats and, as a consequence of oversubscription, a signficant fraction of pupils is often denied the first choice. Seats rationing at popular schools is extensively debated in media and policy discussions, often advocating for measures ensuring more pupils are admitted to sought-after schools. The question arises whether missing out on the preferred schools have negative consequences on pupil's achievement. While many studies investigate effectiveness of particular type of schools, often popular among parents,[2] empirical evaluations of district-wide school choice plans to date are limited and mostly based on admission lotteries. Considering a single U.S. district, Cullen et al. (2006), Hastings et al. (2009) and Deming et al. (2014) find attending a preferred school has modest effect on achievement, despite increasing the quality of peers.

Proponents of school choice reforms argue that, incentivising competition among schools to attract pupils, educational standards may improve if application decisions are made on the basis of school effectiveness. However, studies of school demand typically find parents do not necessarily opt for schools expected to boost pupils' achievement (e.g., Mizala and Urquiola, 2013; Walters, 2018) but value ones serving higher quality peers (Abdulkadiroglu et al., 2017b). Parental school rankings may yet reflect additional features affecting academic achievement. The aggregate impact of entering a preferred school depends on productivity

[1]E.g., Amsterdam (De Haan et al., 2018), Barcelona (Calsamiglia and Guell, 2018), Paris (Fack et al., 2019) and Beijing (He, 2017).

[2]See studies on elite schools, charters schools and academy schools reviewed in Section 2 below.

of school and peer inputs and of other factors considered by parents when listing schools.

This paper exploits centralised allocation of school seats to study the impact of missing out on preferred schools in England, where oversubscribed seats are assigned mostly based on distance to school. Leveraging the school assignment regulation, I estimate the causal effect of being offered a seat at each of the institutions named in the application form on academic achievement and on school composition and characteristics.

Admission based on proximity makes England a peculiar context to study school choice. Anticipating admission chance as a function of distance, affluent parents compete in the housing market to secure residence next to preferred schools (see, e.g., Machin, 2011). The possibility of buying higher admission chance at popular schools generates an equilibrium that fails to break the link between school and neighbourhood of residence, the key equity rationale behind school choice reforms. Rank-order lists submitted by parents are then substantially affected by characteristics of locally available schools (Burgess et al., 2015). The fact that both parental preferences and admission chances are a consequence of residential sorting constitutes a challenge for identification and interpretation of school effects.

Identification stems from discontinuities in the probability of admission arising from the deferred acceptance algorithm that matches applicants and school seats. The assignment mechanism ranks applicants (mostly) by distance and provisionally admits them up to school capacity. A single offer is generated from the highest prefrence school among ones an applicant can enter. School-year specific catchment areas result from allocation of seats depending on distance and preferences of all applicants in a school district. Oversubscription generates sharp non-linearities in school offer as function of proximity and preference rank, providing a regression discontinuity (RD) design where the running variable is an applicant's admission rank. Estimated effects have causal interpretation as long as applicants are not able to precisely manipulate their rank for admission around the threshold (Lee and Lemieux, 2010). This would require the ability of parents to coordinate location and application decisions to exactly predict the realised admission cut-off. Identifying assumption is therefore the absence of full coordination of parents in geographical sorting and school choice. I see this as a mild assumption considering the scale and complexity of individual decisions leading to the equilibrium allocation of school seats in a given district.

I exploit novel administrative data on preferences and offer of all pupils applying for a place in state-funded schools in England. School choice data are matched with the National Pupil Database, providing background and achievement data on the universe of pupils in compulsory education. I consider here admission to primary school, the phase at which parents likely make residential decision with lasting consequences on school choice. Moreover, I focus on the 33 Local Authorities in Greater London, where schools are small and typically oversubscribed. A dense urban setting with high demand for school quality is the ideal context to implement the identification strategy adopted here.

I face a number of empirical challenges in reconstructing the RD design embedded in centralised school assignment. First, administrative data do not reveal admission rank of candidates for a seat at the school, which constitutes the running variable in my econometric analysis. I measure distance of pupils from each listed school by using information on postcode of residence. This allows me to rank applicants to each institution by proximity, the main priority criterion on which school offer is granted. Second, discontinuity in school offer as a function of distance is diluted by the fact that assignment mechanism considers parental preference. For example, a pupil is at risk of admission to her second choice only if she is disqualified from her most-preferred school. To identify pupils actually competing for a place, which Abdulkadiroglu et al. (2014) refer to as "sharp sample", I replicate the deferred acceptance algorithm used to allocate school seats. Third, some applicants (mostly siblings of current students) are given priority over distance based on unobserved characteristics. I get round this problem by using the information on school offer to identify pupils with priority, as detailed in Section 3 below.

Replication of school offer closely matches actual assignment and provides sharp admission cut-offs for each school, which I pool across institutions with same parental preference rank. I present graphical analysis and regression estimates showing that marginally admitted applicants are similar in any observational respect to those who are closely denied a place. Applicants around admission thresholds are similarly likely of being eligible for free school meal, have comparable baseline assessments and live in neighbourhoods with similar average income. This evidence suggests that my analysis is based on a valid RD design, where applicants do not sort sistematically around the admission cut-offs.

Reduced-form estimates show no evidence of achievement loss in missing out on the first-choice school after three years of primary education. Academic achievement is measured as an indicator for Year 2 teacher assessments above expectations separately in mathematics, reading and writing. By contrast, I find achievement of pupils disqualified from their second choice decreases by about 0.2 of a standard deviation (hereafter, $\sigma$) across subjects. Similar magnitudes are found for pupils missing out on schools ranked third to sixth, though results are imprecise.

Heterogeneity analysis by pupils' background reveals achievement impacts are driven by applicants from relatively advantaged areas. Pupils in high-income neighbourhoods experience large achievement loss in missing the second choice school, of about 0.3-0.4$\sigma$, and smaller but statistically significant achievement decrease in writing (0.13$\sigma$) when disqualified from the first choice. By contrast, no effect of admission to each of the listed schools is detected for pupils with disadvantaged background. Results point to the conclusion that affluent parents, who have more chances to sort in the housing market, are able to choose schools that boost pupils achievement. On average, they appear to target areas where two effective schools are available. In addition, I do not find evidence of differential achievement effects by academic performance of offered school. However, greater impacts are found for applicants to academy schools, implying larger effects than those estimated in existing literature (e.g., Eyles and Machin, 2018).

Discontinuities in school characteristics around admission thresholds help interpreting achievement effects and shed light on parental preferences. Applicants missing out on each of the listed choices gain schools with higher performance and serving less disadvantaged peers, unveiling clear parental preference for peer quality. For example, fraction of schoolmates achieving above standards at final year tests and share of free school meal eligible schoolmates drop by about 0.3$\sigma$ at each admission cut-off. As they are similar across thresholds for first and lower-preference schools, these effects cannot explain achievement impacts discussed above. By contrast, school value added measures exhibit smaller discontinuities and with lower statistical significance, implying school effectiveness is not as important for parents. Once controlling for peer quality, parents do not reward effective schools, a result in line with school demand literature (e.g., Abdulkadiroglu et al., 2017b).

Heterogeneous changes in school traits at admission thresholds by pupils' background help understanding the mechanisms driving achievement effects. Potential explanations range from segregated access to effective schools to differential demand for academic quality by socioeconomic status, either due to preference or to lack of information. Though applicants in low-income areas compete for schools serving dramatically lower-peforming students, they rank schools by peer quality similary to relatively affluent pupils. More generally, no systematic difference is found in school traits discontinuities for disadvantaged applicants. Therefore, differential demand for academic performance can hardly explain heterogeneous achievement effects of parental choice I find.

To the best of my knowledge, this is the first study of school choice in England exploiting centralised assignment to state-funded schools, similarly to the strategy employed by Abdulkadiroglu et al. (2014) to evaluate elite schools in Boston and New York. This paper offers quasi-experimental evidence on achievement returns to sought-after schools improving on existing studies about England, based on selective schools in a single district (Clark, 2010 and Clark and Del Bono, 2016). Further, this paper offers exogenous evidence on parental preferences for schiik and heterogenous preference by pupils' background improving on Burgess et al. (2015), who need to assume the set of feasible schools.

Results presented here are of potential interest for policy makers as they point to possibly unintended consequences of admission policies. Combined with the distance criterion, school choice generates an equilibrium where affluent parents sort in areas with effective schools available, while disadvantaged families reap no benefit. Achievement effects of missing out preferred schools appear not to be explained by differential school demand by socioeconomic status. Reconsidering admission priorities may, therefore, play a more decisive role in reducing educational segregation than demand-side measures such as information initiatives for parents. My results point to conclusions in line with the analysis in Noden et al. (2013), advocating for expansion of ability banding and admission lotteries as alternative admission criteria.

The remainder is structured as follows. Section 2 describes institutional context and data, offering descriptive evidence on school admission. Section 3 discusses identification challenges and the strategy adopted here along with the estimation framework. Results on achievement effects, average estimates as well as heterogenous effects, are presented in Section 4. Changes in school characteristics induced by school offer, which speak about parental preferences for school traits, are discussed in Section 5. Conclusions follow.

# 2 Background and data

### Institutional background

Compulsory education in England, from age 5 to 16, is mainly provided by public sector schools, with only about 7% of pupils choosing private education. Parental choice provisions among public schools have been present since 1988 and school league tables were introduced in the 1990s to inform parents in ranking schools. Allocation of seats is centrally regulated by the School Admissions Code, the latest edition of which came into force in late 2014. Parents rank up to six schools in the application form and children are admitted to the first choice when demand does not exceed capacity. Seats at popular schools, however, are typically rationed and admission authorities[3] must adopt and publish criteria to prioritize applicants in case of oversubscription. Discretion in setting admission criteria has been progressively reduced and offers at oversubscribed schools are mostly granted in order of proximity. The distance criterion has translated into fierce competition in the housing market to secure a place close to preferred schools. Impact on housing prices has been extensively documented by the economic literature (see, e.g., the review in Machin, 2011). In a survey of parents, around 30% of middle and upper class respondents reported having moved to areas with good schools around (Francis and Hutchings, 2013).

Primary education in England typically spans seven grades, from reception (Year 0) to Year 6. I consider here primary schools in Greater London, the largest urban area in England with about 9 million inhabitants and highest population density in the country, exceeding twelve times the national average. London primary schools serve pupils performing higher than average at final year standardised tests. Share of students exceeding expectations in mathematics is three percentage points (10%) higher than the rest of the country. This difference is driven by a fat right tail of exceptionally performing institutions, with almost 20% of London primaries performing in the top decile nationally. Scale and variety of London school market lead to a fierce competition for seats at popular schools and the fraction of pupils missing out on their top choices is systematically the highest around the country[4].

[3]Admission authorithy is the Local Authority for community and voluntary controlled schools and the school governing body for all other types of schools (e.g. religious and academy schools).

[4]Aggregate statistics on school admission are publicly available at https://www.gov.uk/government/statistics/secondary-and-primary-school-application-and-offers-2018.

To the best of my knowledge, this is the first study investigating achievement effects of admission to preferred schools in England. Impacts of school choice have been studied in a U.S. school district with admission by lotteries: Cullen et al. (2006) find little evidence of achievement benefit from school choice; Hastings et al. (2009) show positive achievement effects for applicants with preference for high-performing schools; Deming et al. (2014) find limited positive effects on postsecondary outcomes, concentrated among girls.

A related stream of literature investigates effectiveness of particular type of public schools, often popular among parents. Studies on England consider academically selective schools in a single district, finding modest effects on test scores and labor market outcomes, but positive impact on educational attainment (Clark, 2010 and Clark and Del Bono, 2016). Most related to empirical methods adopted here, Abdulkadiroglu et al. (2014) exploits centralised assignment to Boston and New York elite schools finding no achievement benefits, interpreted as absence of peer effects. The contemporaneous analysis by Dobbie and Fryer (2014) reaches parallel conclusions, while Pop-Eleches and Urquiola (2013) show significant increase in scores of a high-stake test after attending selective high-schools in Romania. Positive achievement effects are often found for particular categories of public schools enjoying substantial autonomy: charters schools in the U.S. (e.g., Abdulkadiroglu et al., 2011;Abdulkadiroglu et al., 2016 and Walters, 2018) and academy schools in England (e.g. Eyles and Machin, 2018). I report evidence on effectiveness of academies in Section 4 below.

A substantial body of literature researches determinants of parental demand for schools. Estimating preferences through a discrete choice model, Abdulkadiroglu et al. (2017b) show that, once peer quality is controlled for, parents do not value school effectiveness. In line with this result, Walters (2018) shows pupils expected to benefit more from charter schools are the least likely to apply and Abdulkadiroglu et al. (2018) find negative achievement effects of school vouchers expanding parental choice. Relatedly, Mizala and Urquiola (2013) find new information on school effectiveness does not impact choice behaviour in Chile. On the other hand, Beuermann et al. (2018) find parents prefer schools boosting scores in high-stake tests in Trinidad and Tobago. Heterogeneous preference for academic performance is studied by Hastings et al. (2009) and found to be stronger for advantaged parents. Studying school demand in England, Burgess et al. (2015) show that most of the observed difference in performance of listed schools by socio-economic status is driven by characteristics of locally available institutions. Heterogeneity in preference for autonomous schools is found by Bertoni et al. (2017), showing affluent parents exhibit stronger preference for newly established academy schools. By accounting for residential sorting in a quasi-experimental design, I show causal evidence on preferences of applicants around admission thresholds in Section 5 below.

Related to my results are studies on educational segregation arising from parental preferences. Advocates of school choice argue that, breaking the link between attended school and neighbourhood of residence, it may reduce school segregation in disadvantaged areas, which was found to have negative impacts on academic achievement and social behaviour (Billings et al., 2013). However, increased parental choice may widen educational inequality and foster segregation if preference for academic quality is heterogeneous or if parents prefer peers with similar social background (Hastings et al., 2009; Avery and Pathak, 2015).

Finally, I build on the growing mechanism design literature in education. Studying microeconomic theory of parental choice, Pathak and Sonmez (2013) measure vulnerability of assignment mechanisms to strategic behaviour., showing that deferred acceptance algorithms are less manipulable than the popular Boston mechanism. Fack et al. (2019) show that truthful preference reporting is not supported even under deferred acceptance when admission chances can be predicted and ranking additional schools is costly. Both conditions are likely met in the England context, implying submitted rank-order lists need not reflect parental preferences over the full set of schools. On the econometric theory side, Abdulkadiroglu et al. (2017a) show how to fully exploit random variation embedded in assignment lotteries to estimate school effectiveness developing an admission propensity score. The analysis is generalised to non-random assignment mechanisms, such as the one adopted in England, by Abdulkadiroglu et al. (2019).

**School assignment mechanism**

Since 2007, when the previously popular "first-preference-first" mechanism was banned, school districts across England assign seats through a deferred acceptance (DA) algorithm. The former mechanism, which prioritise applicants ranking the school first, has been proven to incentivise strategic preference reporting (Pathak and Sonmez, 2013). On the other hand, DA mechanisms have been studied for decades and are shown to generate stable matches, i.e. no applicant is eligible for schools she have ranked higher than the assigned one (see the review in Roth, 2008).

The starting point of the assignment algorithm is a ranking of schools and pupils. Schools are ranked by parents in the application form, while pupils are mostly ranked by proximity. Particular categories of applicants are given priority in admission irrespective of their distance to school. Schools are required to prioritize pupils with a statement of special education needs or which are looked after by the Local Authority, though both are residual cases. More importantly, siblings of current students are typically given priority over distance[5]. Finally, religious schools are allowed to admit pupils based on faith. Applicants with priority over distance, however, are not likely to exceed school capacity and proximity is still commonly used as tie-breaker among pupils with equal priority.

Preferences, priorities and school capacities are mapped into offer through the student-proposing DA algorithm. In the first round, each student applies to the most preferred school. Institutions rank applicants by priority and provisionally admit them up to capacity. In each of the subsequent rounds, students who are rejected in the previous stage apply to the next best school in their application form. They are ranked by priority jointly with those provisionally admitted up to this point. Once again, school retains applicants up to capacity, and the rest are rejected. The algorithm stops at the earlier round where no rejection take place. Note that this procedure imply students initially assigned at their first choice can loose admission in subsequent rounds if pupils with lower preference but higher priority apply to the school. The fact that parental preference rank is independent from admission priority is what makes this class of algorithms less vulnerable to strategic behaviour.

Primary school admission across all London districts is coordinated since 2011 through the Pan-London admission scheme, predating the institution of National Offer day in 2014. From then on, a single school offer is received in mid-April by parents throughout the country. Local authorities are required to provide a place for each applicant and pupils disqualified from all listed schools are assigned to one with spare capacity. Unsatisfied parents can join waiting list at preferred schools and have the right to appeal the decision in case of irregularities, though admission outcomes is rarely overturned.

[5]Burgess et al. (2017) reports that in the Millennium Cohort Study 43% of applicants have an older sibling at the school at the time of application for secondary school.

### Data and facts on school admission

I observe administrative data on preferences and offer of all pupils applying for a place in primary schools in England between 2014 and 2017. Individual-level records include all schools ranked in the application form (up to six) and the institution offered as a result of the assignment algorithm. Information on preferences are matched to the National Pupil Database (NPD), featuring test scores and socio-economic characteristics of the universe of pupils in primary and secondary education. Importantly, I observe also the full pupil postcode which I use to compute distance of applicants to all named schools[6].

The NPD provides examination results measuring pupils achievement at different stages of their education. I consider here Key Stage 1 (KS1) teacher assessments, completed at age 7 after three years of primary school (Year 2). Pupils are assessed against expected standards in English, separately for reading and writing, and in mathematics. To control for ability at entrance, I consider Early Years Foundation Stage Profile (EYFSP) assessments. They test pupils in 17 different areas of learning and are completed at age 5 during the reception year (Year 0), when pupils have just entered compulsory education.

I consider here applicants to London primary schools entering the reception year in 2014, the only cohort for which both preference and assessments data are currently available. The working sample consists of the 98,299 pupils applying to at least one London primary school in 2014, described in Panel A of Appendix Table A.1. London pupils are ethnically diverse, mostly from non-white origin, and the 42% of them does not speak English at home. Parents list 315,028 choices in the application form, an average of 3.2 each, nearly all of which are London primary schools (the 98.6%). Though the majority of parents exploit the possibility of expressing preferences, with almost 60% ranking at least three schools, there is substantial variation in the number of schools listed (see Appendix Figure A.1). As the decision to include a further choice in the application form is arguably not exogenous, the lenght of submitted school list will be controlled for in the empirical analysis. After admission is decided, most parents (almost 90%) take up the offered school. A small fraction of applicants, the 3.4%, is

[6] I compute linear distance between applicant and the school using centroids coordinates for English postcodes obtained from www.doogal.co.uk. When missing (3.1% of applications), I impute distance by exploiting the information on schools named in the application form. Specifically, pupils with unobserved location applying to a given school are assigned the average distance among applicants ranking that school with the same preference. Outliers in the top 5% of the distance distribution are not considered in the computation.

not retrieved in any state-funded schools and likely opted for private education.

On the supply side, the 1,708 London primary schools are small (average capacity[7] is 55) and typically oversubscribed, as shown in Panel B of Appendix Table A.1. Most schools have excess demand of 5 seats or more and the 38% is oversubscribed by at least 20 seats[8].

Figure 1 shows that the probability of receiving an offer, as expected, markedly decreases with distance to school. Reflecting high take-up rate, the figure is very similar when looking at enrollment, represented by diamonds in Figure 1. Fuzziness in admission probability over distance stems from variability in preference rank assigned by parents to the school. Appendix Figure A.3 shows that, conditional on distance, there is residual variation in school preferences and this increases when moving further from the school, likely reflecting heterogeneity in locally available choices. As the assignment mechanism considers distance and preferences, it generates variation in eligibility even at similar distance to school. In addition, fuzziness in Figure 1 originates from pupils who are given priority over distance on the basis of unobserved characteristics (see section 2 above). Most importantly, I do not observe whether applicants have an older sibling at the school. I explain how empirical analysis is framed to deal with latent priority in Section 3 below.

As result of competition for school seats, about 20% of applicants miss out on their most preferred school and, among them, about 10% are also disqualified from the second choice (see Figure A.4). Impact of missing out on preferred schools on academic achievement is explored in what follows.

# 3 Empirical strategy

I am interested in identifying the effect of receiving an offer from a school listed with a given rank in the application form. I first illustrate the challenges to identification discussing a naive approach relying on a selection-on-observables assumption. The following subsections presents the identification strategy adopted here and the estimation framework.

[7]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.2.

[8]Oversubscription is calculated after accounting for applicants eligible at more preferred schools (replication of the assignment is discussed in Section 3 below).

### Identification challenges

Let $D_{is}$ be a dummy variable equal to one if applicant $i$ is enrolled at her $s$-th choice school. Consider applicants to their $s$-th choice and compare the outcome of interest ($Y_{is}$) between pupils eventually enrolled at $s$-th choice and not, conditional on baseline characteristics $X_i'$:

$$Y_{is} = \beta^s D_{is} + X_i'\gamma + \epsilon_{is}, \qquad \forall s = 1, \ldots, 6, \tag{1}$$

where the vector of controls include background characteristics and previous assessments.

In this formulation, the coefficient $\beta^s$ identifies the effect of enrolling at the $s$-th choice for a pupil who is randomly assigned to school conditional on individual characteristics. However, likelihood of admission depends on distance to school, which determines priority, and on preference rank assigned by parents to the institution (see Section 2 for institutional details). Location of pupils is likely driven by unobservables as parents sort on the housing market to secure residence close to preferred schools. In addition, rank-order lists of named schools present substantial variability even conditional on distance to school, as shown in Figure A.3, and are likely filled on the basis of expected potential outcomes. Distance to and preference for the school are therefore omitted variables in equation (1) and OLS estimates of $\beta^s$ are most likely biased. I explain in the next subsection how exploiting the school assignment mechanism provides a more accurate design.

### Research design

Estimating causal effect of school assignment requires comparing applicants with similiar nondeterministic "risk" of admission (Abdulkadiroglu et al., 2017a). As school offer is based on distance to school and parental preferences, the ideal experiment compares applicants located at similar distance which, based on submitted choices, face an uncertain admission outcome. Whenever a school is oversubscribed, assignment mechanism generates discontinuities in the relationship of school offer with distance and preferences. I exploit these sharp non-linearitites in a RD design where the running variable is an applicant's admission rank at the school listed with a given preference. RD analysis is valid as long as applicants can not precisely manipulate their running variable to sort on the preferred side of the admission cut-off (Lee and Lemieux, 2010). Although information on past admissions are available to parents, the assignment mechanism results in school-year specific catchment areas reflecting the equilibrium allocation of school seats. Identifying assumption is then lack of full coordination of parents in residential and school choices, a plausible restriction given the scale and complexity of primary school market in London.

To retrieve the RD design embedded in centralised assignment, I follow a procedure similar to the empirical design implemented by Abdulkadiroglu et al. (2014). Admission ranks at each school, which are not directly observed in the data, are obtained sorting by distance all applicants at each school. Let $p_{ij}$ be candidate $i$'s rank at school $j$. Cut-off for admission is $c_j$, observed as the rank of the lowest-ranked admitted applicant at school $j$. RD design is implemented by centring cut-offs at all schools around zero, where running variable is

$$r_{ij} \equiv p_{ij} - c_j,$$

the rank distance of applicant $i$ to admission threshold at school $j$.

Discontinuity in the relationship between $r_{ij}$ and school offer is diluted by the fact that assignment mechanism considers parental choices. As applicants are offered the highest preference school where they are eligible, candidates at risk of admission for school $j$ are those disqualified from each school ranked better than $j$ in the application form. Sharp discontinuity in school offer at the cut-off is expected only for this group of applicants, deemed "sharp sample" by Abdulkadiroglu et al. (2014). For example, if school $j$ is applicant $i$'s second choice, pupil $i$ belongs to sharp sample at school $j$ as long as he is disqualified from his most preferred school. Identifying applicants in sharp sample requires replicating the centralised assignment mechanism, described in Section 2 above. I determine whether an applicant is eligible at a higher-preference school by implementing a deferred acceptance algorithm inputted with school capacities, parental choices and proximity of all applicants to the 1,708 London primary schools. In what follows, $r_{ij}$ is defined only for applicants in sharp sample for school $j$.

I face a further empirical challenge in that some applicants are granted priority over distance on the basis of unobserved characteristics (most importantly siblings of current students, see Section 2 above). As I am able to replicate assignment solely based on distance and preferences, estimated running variable fails to recover discontinuity in school admission in a first step (see Appedix Figure A.5). I identify latent priority by adjusting admission ranks based on actual school offer. When ranking applicants solely by proximity, I overestimate the threshold for admission as I assign by distance more seats than the actual mechanism does. Applicants receiving school offer and located further than the estimated cut-off must therefore have priority over distance and are flagged accordingly. School assignment is then repeated by ranking applicants based on retrieved priority and, within the same priority group, by distance to school. This procedure is iterated until no candidates with offer are found beyond the estimated threshold. School assignment converges after 131 iterations of DA algorithm, as shown in Panel A of Appendix Figure A.6, where plotted is the fraction of newly spotted priorities in each round. Panel B of Appendix Figure A.6, depicting errors in school assignment by iteration, shows offer is nearly perfectly replicated once the adjustment is concluded. However, a degree of measurement error in the running variable is likely to persist. First, I cannot distinguish between different layers of priority ( e.g. pupils with special needs are often prioritised with respect to siblings of current students). Second, among applicants located closer than the cut-off, I cannot distinguish those with priority. As long as such error is not sistematically related to admission cut-offs, this should not undermine the validity of my analysis.

Finally, I operate two restrictions to the estimation sample. First, as variation in school offer is required, I consider only schools oversubscribed by sharp sample applicants for at least five seats. This keeps 61% of London primary schools, representing the most popular institutions among parents. As one could expect, Table 1 shows that, on average, popular schools are significantly better performing and enrol less disadvantaged students with respect to undersubscribed institutions, consistently with results presented in Section 5 below. Regarding school type, religious schools are more popular while community schools are likely undersubscribed.

Second, I do not consider applicants to religious schools in the estimation. As such schools often prioritise candidates by proxies of faith rather than distance, measurement error in their running variable is likely to be severe[9].

Panel A of Figure 2 shows discontinuity in school offer is achieved. Plotted are average offer rates in two-units-wide bins of the running variable, separately by parental preference rank. As I consider sharp sample applicants only, second choice figure depicts candidates not

[9]As expected, applicants to religious schools are more likely found to enjoy priority over distance, 21.9% vis-à-vis 12.8% at non-religious schools.

admitted at the most-preferred school. Here and throughout, I pool applicants to schools ranked third or below in the application form as only a small fraction of applicants misses out on both first and second choice (see Appendix Figure A.4). Superimposed are local linear regression fits of individual-level observations separately estimated on either side of the admission cut-off. Smoothers are estimated weighting observations by a triangular kernel centred on admission cut-off, as in Abdulkadiroglu et al. (2014). Kernel bandwidth employed here is chosen following the data-driven procedure proposed by Calonico et al. (2014), separately for each outcome variable. The figure considers applicants ranked within 50 places from admission cut-off.

Drop in offer rate around cut-off generates discontinuity in school enrolment, with some fuzziness driven by non-compliance with centralised assignment (see Panel B of Figure 2). As could be expected, non-compliance decreases with parental preference for the school.

### Estimation and placebo

I estimate effects of missing out on preferred schools by constructing nonparametric RD specifications. Discontinuities around admission threshold are isolated by controlling for local linear functions of the admission rank and separate regressions are estimated by parental preference for the school. Specifically, the estimating equations are

$$Y_{is} = \alpha + \beta^s Z_{is} + \gamma r_{is} + \delta r_{is} * Z_{is} + \sum_{k=1}^{6} \theta_k \mathbb{1}(np_i = k) + \epsilon_{is}, \quad \forall s = 1, \ldots, 6, \tag{2}$$

where $r_{is}$ is applicant $i$'s admission rank at the $s$-th preferred school. Kernel-weighted least squares estimates of (2) are considered, where choice of kernel function and bandwidth correspond to those for graphical analysis described above. In addition, all specifications control for number of preferences fixed effects, where $np_i$ is the number of schoold listed by pupil $i$ in the application form. In practice, I pool applicants to schools ranked third or below to increase precision. Pooled regressions include interactions of all control variables with cut-off dummies.

The parameter $\beta^s$ is the reduced-form effect of assignment to the $s$-th choice vis-à-vis a lower-ranked school, as applicants in the estimation sample are those disqualified from any school they prefer more. This is illustrated in Figure 3, depicting preference 8 M,./rank of the school where applicants are eventually enrolled[10]. Pupils just missing out the first and the second choice, mostly end up into the second and third choice, respectively. Discontinuity is clear in the third sub-panel as well though less marked, as third to sixth choice are jointly considered.

Estimating (2) where dependent variables are baseline individual characteristics provides an indirect test of RD design validity. Applicants with admission rank close to admission threshold should be comparable in all respects but school offer. This expectation is borne out by Figure 4. All plotted variables are standardised to have zero mean and unit variance among London primary school applicants. Panel A shows pupils around cut-offs have similar baseline achievement assessed during Year 0, when they have just entered primary education[11]. In the third sub-panel, where estimation is more noisy due to lower sample size, a small difference appears which is not found to be significant in estimation. Pupils' background is considered in subsequent panels, starting with deprivation in neighbourhood of residence depicted in Panel B of Figure 4. Deprivation index measures average family income in pupils' local area. Not only pupils are similar by neighbourhood quality around cut-offs, but they are similarly likely to speak English at home (Panel C) and to be of white ethnicity (Panel D). Paralleling graphical analysis, Table 2 reports estimates of $\beta_s$ in equation (2) where outcome variables are baseline characteristics. Controls for covariates other than the one used as dependent variable are included in columns (2), (5) and (8). Regressions in columns (3), (6) and (9) include, in addition, school of application fixed effects. Reported are estimates for gender, free school meal eligibility, special education needs and black origin in addition to variables depicted in Figure 4. Estimated coefficients are mostly not significant and close to zero in magnitude, implying that observable characteristics correlated with achievement are continuous around admission thresholds. Although a few coefficients are marginally significant, particularly baseline achievement in column (3), school offer unbalance is largely rejected when testing all covariate discontinuities at once (see bottom row of Table 2). Placebo tests are therefore reassuring that estimated effects can be attributed to school assignment. I start with reduced-form achievement effects in the next section.

[10]For applicants enrolling in none of listed schools, preference rank was coded as 7.

[11]Here and below, I use an index summing up assessments in all 17 learning areas considered. The higher the index, the higher assessed achievement is at Year 0.

# 4 Reduced-form achievement effects

### Average achievement effects

Pupils around the admission threshold do not suffer achievement losses when missing out on the first-choice school, on average. Plotted in Figure 5 is the fraction of pupils achieving above expectations at Year 2 assessments in mathematics (Panel A), reading (Panel B) and writing (Panel C). Outcome variables are standardised to have zero mean and unit variance among London primary school applicants. Superimposed is a local linear polynomial fit of individual-level dummies, separately estimated on both sides of admission threshold. Across subjects, sub-panels depicting first-choice school shows no marked discontinuities around the cut-off. Corresponding estimates are reported in columns (1)-(3) in Panel A of Table 3, showing kernel-weighted least squares fit for coefficient $\beta_s$ in equation (2). Controls for individual characteristics are included in column (2), while column (3) adds school of application fixed effects. Reported are results from separate regressions for Year 2 achievement in mathematics, reading and writing. Estimated effects are remarkably similar across specifications, showing small discontinuities and not statistically different from zero. Specifically, estimated magnitude is about 0.04 $\sigma$ for both mathematics and writing and nearly zero for reading. A minor exception is the estimated $0.06\sigma$ effect for writing in column (3), which is still just marginally stastically significant. Students just disqualified from the most preferred school are then achieving no worse than pupils who nearly get an offer from the first choice after three years of primary education.

Negative impacts on achievement emerge for pupils missing out on schools ranked second to sixth in the application form. In all three subjects assessed, Figure 5 depicts moderate discontinuities around thresholds for schools ranked second and third or below. Corresponding estimates for the second choice are reported in columns (4)-(6) in Panel A of Table 3. Estimated discontinuities are slightly larger in column (6), where identifying variation comes from pupils applying to the same school, ranging from $0.16\sigma$ for mathematics and reading to $0.19\sigma$ for writing. Effects of missing out the second choice are statistically different from zero, especially in reading and writing. Results suggest parents, on average, are able to select a couple of similarly effective schools at the top of their rank-order lists and pupils experience achievement losses if they miss out on both most-preferred choices.

Estimated effects of missing out schools ranked third or below are less precise, as reported in columns (7)-(9) in Panel A of Table 3. A statistically significant discontinuity is estimated only for mathematics, with a magnitude of $0.23\sigma$. Counterfactual assignment at this threshold is to none of listed schools, implying that allocation is decided by local authority to a school with spare seats. Results indicate parents list more effective schools at the bottom of the application form than those assigned them by the administration among institutions left undersubscribed. Evidence however is not conclusive, as statistically positive effects are estimated only in one of the three subjects assessed at Year 2.

Similar results are found when looking at achievement gains rather than absolute attainment, which is accomplished by controlling for individual ability assessed at baseline in Panel B of Table 5. This replicates the structure in Panel A adding Year 0 achievement to the covariates. Estimated effects are slightly smaller, as could be expected since a lagged measure of outcome variable is accounted for. Not surpisingly given balance in baseline achievement around the cut-offs (see Table 2), the pattern of results is confirmed when looking at achievement gains. Pupils just missing out the most-preferred school do not gain less than those who are admitted, while smaller progress from Year 0 to Year 2 is experienced by applicants missing out on lower-ranked schools. Disqualification from second choice decreases achievement gains by $0.12 - 0.15\sigma$, while missing out on bottom-ranked schools have a significant impact only in mathematics and just marginally significant.

Overall, pupils just missing out on the second choice experience achievement losses, while virtually no difference is found when looking at the most preferred school cut-off. As residential sorting of parents likely poses constraints on locally available schools, average results may mask substantial heterogeneity. Differential effects by pupil background and school characteristics are explored in the next subsections.

### Heterogeneous effects by pupil background

Heterogeneity in treatment effects is investigated by augmenting equation (2) with interactions between school offer dummy and pupil or school characteristics. I start by considering heterogeneity based on pupil's background, explored in Table 4. Reported are estimates from a+-4t5+specifications similar to columns (3), (6) and (9) of Table 3, which provide the corresponding average effects. In all heterogeneity specifications, as well as school offer dummy, control variables are also interacted with pupil characteristics. Estimates of main effects of admission to a preferred school are shown in columns (1), (3) and (5). Interaction terms estimates are reported in columns (2), (4) and (6). Panel A of Table 4 explores heterogenous effects by gender. There is generally no evidence of differential impacts of school offer for female students, as interaction terms have mixed signs and seldom differ from zero statistically. A notable exception is estimated discontinuity around cut-off for schools ranked third or below in Year 2 achievement in mathematics (see columns 5 and 6), reporting large and significant impact on male students only. Heterogenous patterns by gender unvealed here are in contrast with results in Deming et al. (2014), finding positive impact of school choice solely on girls. This might be explained by much younger age of students in my analysis, as Deming et al. (2014) consider postsecondary outcomes.

Effects of missing out on preferred schools are found to be driven by deprivation in neighbourhood of residence. Explored in Panel B of Table 4 is heterogeneity by deprivation index, measuring average family income in pupil's local area. I estimate diffrential effects for students with neighbourhood deprivation above median. Interaction terms are nearly all negative in magnitude, even when imprecisely estimated, indicating lower impacts for pupils in disadvantaged areas. In contrast with average estimates, missing out on the first choice have a significantly negative effect on Year 2 achievement in writing of about $0.13\sigma$ for pupils in developed neighbourhoods. Furthermore, missing out on the second choice in richer areas lead to a large and strongly significant decrease in achievement. Estimated magnitudes range from about $0.3\sigma$ for mathematics and reading to $0.4\sigma$ for writing. In contrast, preference for offered school does not have any impact in disadvantaged areas, as the sum of main effect and interaction estimates is mostly close to zero.

Panel C of Table 4 confirm and reinforces heterogenous effects described above. Explored are differential impacts of missing out on listed schools by pupil's baseline achievement. In particular, school offer dummy is interacted with an indicator for Year 0 assessments below median. Consistently with Panel B, missing out on the first choice has negative impact in writing ($0.1\sigma$) for high-ability students. Moreover, disqualification from the second choice generates achievement losses of $0.3 - 0.4\sigma$ for pupils achieving better at baseline. Results for schools ranked third or below are less precise, though high-ability pupils disqualified from bottom choices are found to experience statistically significant achievement losses in mathematics (see column 5 in Panel C of Table 4).

As high-ability students are arguably more likely to be found in high-income neighbourhoods, the two bottom panels of Table 6 are pointing in the same direction. Findings suggest that relatively affluent parents sort in areas where two effective schools are available, and that their pupils suffer notable achievement losses if not admitted in one of the two most preferred institutions. By contrast, missing out on named schools does not impact achievement of pupils from disadvantaged areas, regardless the preference rank. There are several potential explanations for this result. On the demand side, less affluent parents may have weaker preference for academic achievement or may lack information on school quality. On the supply side, parents in disadvantaged areas may be segregated away from effective schools, possibly too distant to be accessible. I discuss some evidence on these channels in Section 5 below.

**Heterogeneous effects by school quality**

Achievement effects of admission at preferred schools may simply depend on quality gradient between listed institutions. I explore heterogenous effects by school characteristics in Table 6. Following a similar structure than Table 4, it reports estimates of differential effects by school absolute test scores (Panel A), by Ofsted rating[12] (Panel B) or between academies and all other schools (Panel C).

No systematic difference in achievement effects are found by peers' ability, measured by baseline share of students exceeding expectations at Year 6 standardised tests. Interaction estimates in Panel A of Table 6 have mixed sign, are generally small and never statistically significant. Undetectable achievement effects of higher-quality peers is in line with findings, e.g., by Pop-Eleches and Urquiola (2013), Abdulkadiroglu et al. (2014) and Dobbie and Fryer (2014). Similarly, I do not find evidence of higher achievement effects of entering a school deemed outstanding by Ofsted inspectors. Though most interaction estimates in Panel B of Table 6 are positive in magnitude, none is significantly different from zero.

Finally, I find that achievement impact of missing out on preferred choices are substantially larger for applicants to academy schools. Estimates of interaction terms in Panel C of

[12]The Office for Standards in Education, Children's Services and Skills (Ofsted), is a UK government department inspecting schools on a regular basis. Inspection outcomes are summarised in a four-tier rating ranging from "outstanding" to "inadequate". 32% of schools in the estimation sample are deemed outstanding.

Table 6 are mostly positive in magnitude and are statistically significant for the second-choice school. In particular, while missing out on a non-academy second choice has no statistically significant impact on Year 2 achievement, estimated effects are about $0.6-0.7\sigma$ for applicants to acadamies (obtained summing up estimates in columns 3 and 4). While point estimates are remarkably high (for example, Eyles and Machin, 2018 find a positive achievement effect of about$0.12\sigma$ for secondary academies), confidence intervals are quite large arguably due to sample size. Academy school applicants are just about 14% of the total across preference ranks, as depicted in Appendix Figure A.7.

Overall, I find no evidence of heterogeneous effects of missing out on preferred schools by observable school performance. However, results suggest academy schools are particularly effective in boosting pupils' achievement.

# 5 Parental preferences for school

### Average preferences for school

To interpret achievement impacts of missing out on preferred schools, discussed in Section 4 above, I illustrate how school characteristics change around admission cut-offs. Features of the school where applicants end up to enrol are mediating outcomes for achievement effects. In addition, RD analysis of school characteristics sheds light on parental preferences. As applicants around the thresholds differ only by preference rank for the school where they enrol (see Figure 3), jump in observable school traits reflect parental taste to the extent that school are ranked in the true order of preferences. Fack et al. (2019) proove that, within submitted rank-order lists, preference ranks truthfully represent parental preferences, unless applicants play dominated strategy. Discontinuities in school characteristics around admission cut-offs are then interpreted as parental preferences in what follows.

Parents clearly prefer primary schools with high peer quality. This can be seen in Table 6, reporting estimates of $\beta_s$ in equation (2) where outcome variables are school characteristics. Results are from specifications similar to ones reported in columns (3), (6) and (9) of Table 3. Students missing out on first choice end up in schools with $0.31\sigma$ lower performance (column 1 of Table 6), measured by baseline share of schoolmates achieving above expectations in mathematics at Year 6 standardised tests. As reported in columns (2) and (3), a similar loss is experienced by applicants disqualified from second-choice school, while estimate is slightly smaller and imprecise for bottom-ranked schools. A similar pattern holds when looking at school performance in reading, confirming that parents clearly rank schools by absolute test scores. Fallback schools for applicants to first and second choices are similarly less likely to be judged outstanding by Ofsted, an estimated $0.4\sigma$ and $0.3\sigma$ respectively. Furthermore, share of free lunch eligible schoolmates and average deprivation in their local area both increase substantially when missing out on each of the named schools, reinforcing finding that parents look at school composition. However, as they are similar across cut-offs for each of the named school, jumps in peer quality cannot explain achievement effects unveiled above. Undetectable achievement loss in missing out on the first choice is found despite a marked decrease in absolute performance and peer socio-economic status.

Parents also exhibit preference for close schools, in line with school demand literature (e.g. Burgess et al., 2015; Abdulkadiroglu et al., 2017b; Fack et al., 2019). Applicants just refused by their first choice end up enrolling in schools located almost $0.6\sigma$ further from home[13]. Moderate increase in distance to school are experienced also by pupils missing out on second and bottom-ranked choices, for an estimated $0.2\sigma$ and $0.33\sigma$ respectively.

Finally, parental preference for school quality, measured by value added indicators in performance tables, appears less marked. Estimated discontinuities of progress measures in Table 6 are smaller compared to peer quality variables and have weaker statistical significance. Furthermore, in regression estimates not reported here, discontinuity in school effectiveness is statistically zero around all cut-offs when controlling for peer test scores. Finding that parents reward peer quality rather than school effectiveness is in line with a large body of literature (e.g., Mizala and Urquiola, 2013; Abdulkadiroglu et al., 2014; Abdulkadiroglu et al., 2017b).

### Heterogeneity by pupil background

To drill deeper into heteregenous achievement effects discussed in Section 4 above, I investigate heterogeneity in school characteristics discontinuities by pupil's background. Depicted in Figure 6 are absolute test scores (Panel A) and value added indicators (Panel B) of the school for applicants around admission thresholds for listed institutions, separately by de-

[13]Outliers in the top 5% of home to school distance distribution are not considered here.

privation in area of residence. Panel A shows pupils from disadvantaged background apply to schools serving strikingly lower-achieving students. Applicants from disadvantaged neighbourhoods just admitted in their first choice enrol into a school performing at the London average, while the figure is about $0.5\sigma$ higher for peers in developed areas. Furthermore, pupils just entering schools ranked third to sixth in relatively affluent neighbourhoods enrol in schools with higher average performance than first choices of peers from disadvantaged background. Despite marked segregation in access to high-performing schools, discontinuities in peer quality around cut-offs for first and second choice are similar regardless neighbourhood quality. Regression estimates in Table 7 confirm no significant differences in parental preference for peer quality by deprivation. Reported in columns (1) and (3) are impacts on school characteristics of missing out on first and second choice respectively in relatively rich areas. Additional effects for pupils from disadvantaged background, reported in columns (2) and (4), are small and statistically undistinguishable from zero. To sum up, relatively disadvantaged parents, though facing choice between substantially lower-quality schools, rank institutions by performance in their application form similarly to what affluent parents do.

Larger differences, statistically significant for reading, emerge for schools ranked third to sixth. Applicants from disadvantaged background experience smaller losses in school peformance in missing out on bottom-ranked institutions, as can be seen in column (6) of Table 7. Overall effect on school performance for disadvantaged pupils, obtained by adding up estimates in columns (5) and (6), is nearly null. Interpreted together with results in columns (2) and (4) this finding suggests that, though preference for academic performance is similar across pupils with different backgrounds, applicants from disadvantaged areas face limited choice of good schools beyond those ranked in the top two places of the application form.

Panel B of Table 7, considering school value added in mathematics, tells a similar story. As progress measures control for achievement at Year 2, dramatic differences in school performance level by deprivation shown in Panel A are not detected here. Considering discontinuities around cut-offs, impacts on school quality of missing out first and second choices are similar across different backgrounds, implying similar observed preference for school effectiveness. Result is confirmed by regression estimates in columns (2) and (4) of Table 7. Smaller gradient for disadvantaged pupils estimated in column (6) may reflect limited choices available as argued for school test scores above. Moreover, estimates on further school characteristics imply parents from deprived areas exhibit similar preferences for peer composition and distance to school than peers with better background across cut-offs for each listed school.

Overall, parents appear to prefer close primary schools and institutions with higher peer quality, while school effectiveness seems less important. Once different quality of available schools is controlled for, parents from disadvantaged neighbourhood do not exhibit different choice behaviour than relatively affluent peers. Results imply that differential preference for academic performance can hardly explain heterogenous achievement effects of school choice. It seems rather more plausible that results on attainment are driven by segregated access to effective schools.

# 6 Summary and conclusion

A robust trend in education policy reforms around the world allow parents to rank a number of preferred public schools at application, as opposed to assignment at nearest school based on residence. However, achievement effects of enrolment to a school with higher parental preference are currently underexplored, with district-wide evaluation available for a single U.S. locality only (Cullen et al., 2006; Hastings et al., 2009; Deming et al., 2014). This paper investigated achievement effect of missing out on preferred institutions among London primary school applicants. I find students from advantaged background perform worse in teacher assessments administered after three years of education when denied a place in the two most-preferred schools. On the other hand, no returns to parental choice are detected for students with lower socioeconomic status. Admission by distance to school, fostering competition in the housing market, is likely central in interpreting the results. Possible explanations can be formulated as supply-side mechanisms, with access to effective schools prevented to worse-off pupils because of residential sorting. Alternatively, demand-side explanations, with weaker demand for academic performance among disadvantaged parents, may either derive from different preferences or fom lack of information on quality of available schools. I show that academic quality of listed schools is dramatically lower for disadvantaged pupils. Despite this fact, I find similar gradient in peer quality and further traits of preferred schools between applicants from different backgrounds. There is therefore little evidence that results are driven by weaker demand for academic performance. Results are then consistent with an equilibrium in which affluent parents succesfully sort around effective schools while more disadvantaged ones are segregated away from achievement-boosting institutions. Breaking the link between school and neighbourhood quality would likely require to reconsider the proximity criterion.

By considering school characteristics around cut-offs for admission at higher-preference institutions, this paper offers quasi-experimental evidence on parental tastes for school. I find clear preferences for peer quality, measured by school performance in standardised tests or by school intake composition. On the other hand, value-added measures proxying school effectiveness appear to be less important, as studies on school demand often find (e.g., Mizala and Urquiola, 2013; Abdulkadiroglu et al., 2017b). Result that preference for academic quality does not differ substantially by pupil background is in line with findings in Burgess et al. (2015).

Finally, I find no achievement effects around the first-choice threshold despite marked changes in peer quality. Moreover, achievement effects of disqualification from preferred schools are not found to depend on observable indicators of school perfomance, e.g. absolute test scores and inspection ratings. These results consistent with modest or null peer effects, at least in first years of primary education, in line with conclusions in Pop-Eleches and Urquiola (2013) and Abdulkadiroglu et al. (2014).

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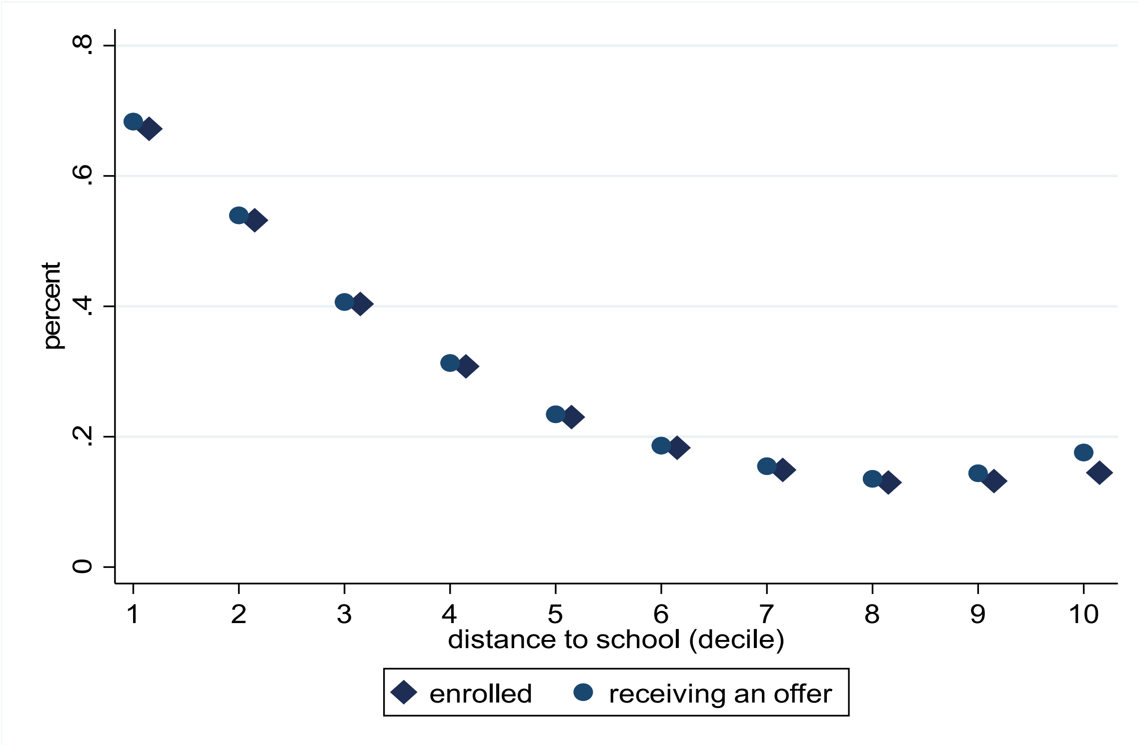
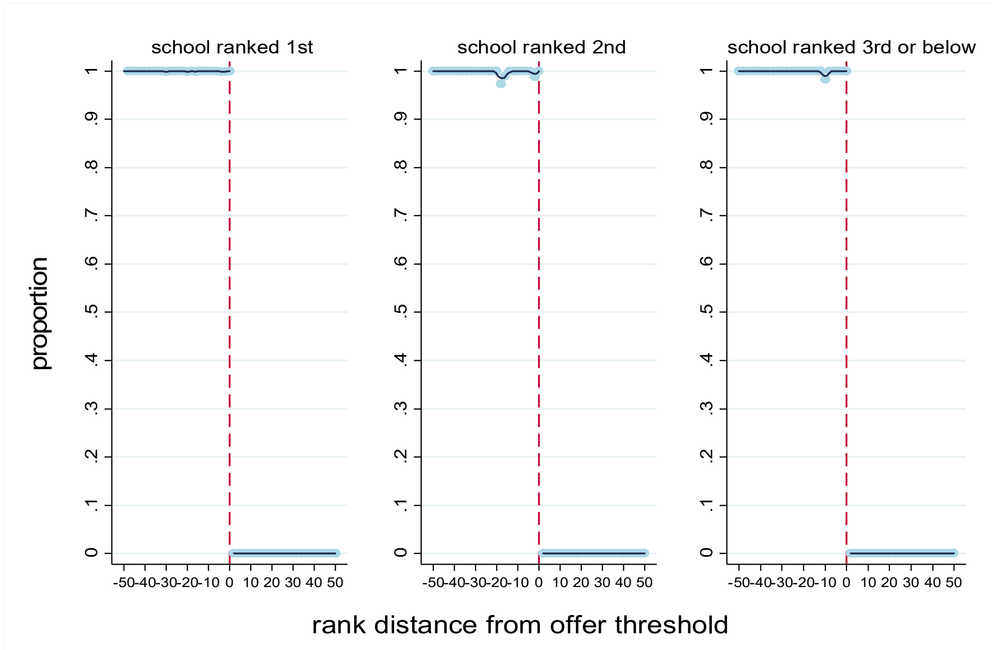
Figure 1: Admission by distance to school



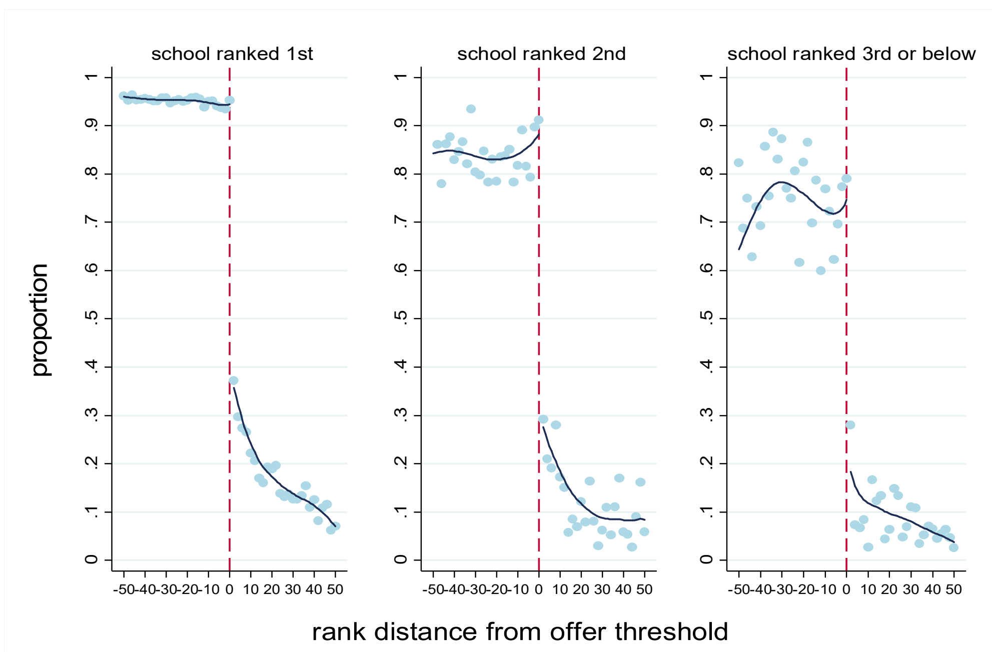

**Note.** The figure depicts school offer and enrollment likelihood by distance to school conditional on having applied for a seat. Offer is reported by markers, while diamonds represent enrollment. Distance bins are deciles of within-school distribution of applicants. Outliers in the top 5% of the aggregate distance distribution are excluded. See Section 2 for details.

Figure 2: RD design

**Panel A**. Offers at listed schools

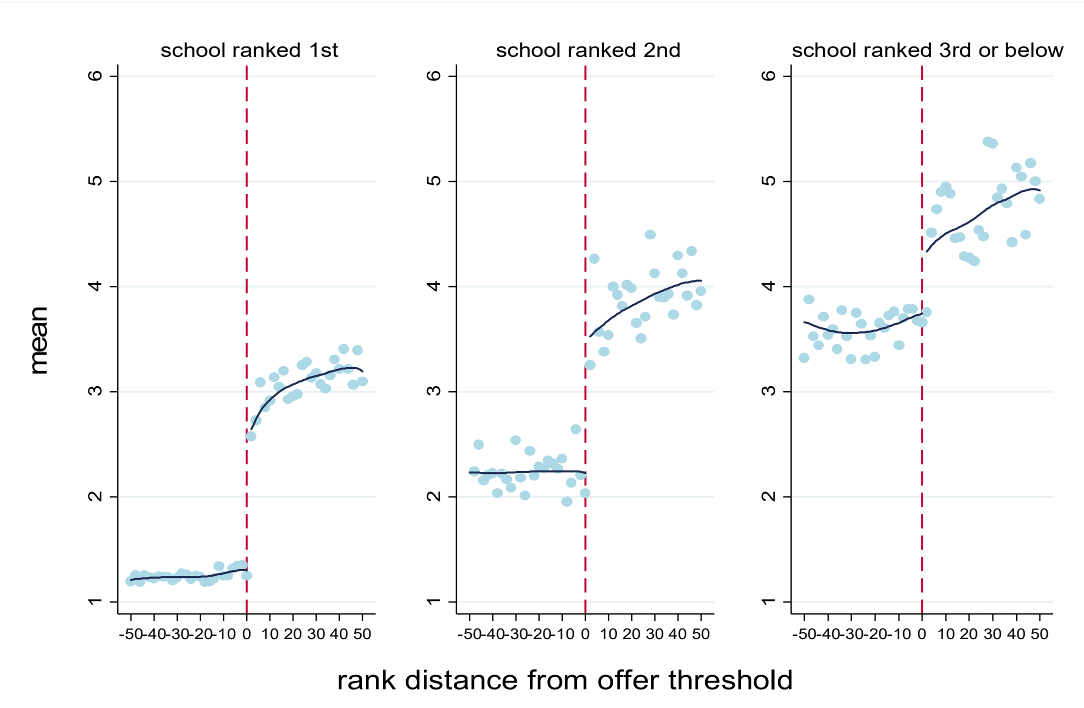



**Panel B**. Enrollment at listed schools



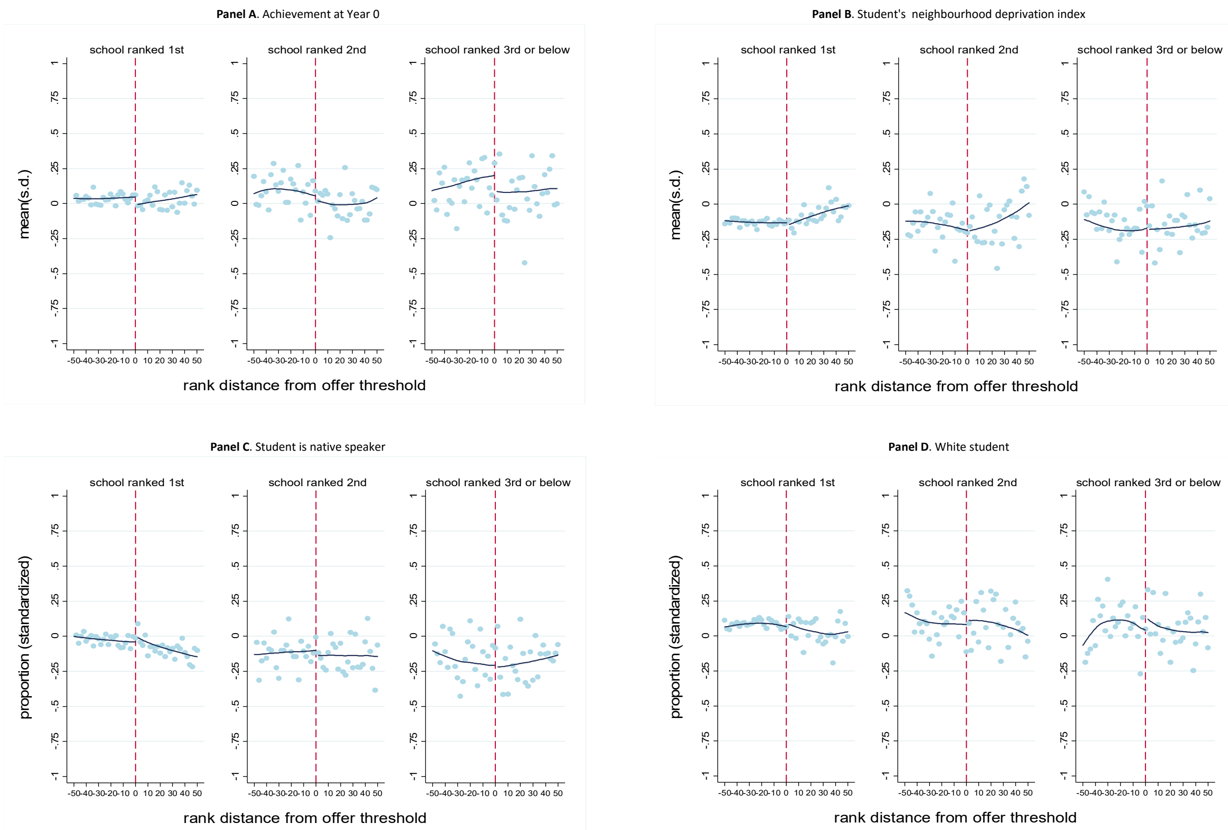

**Note.** The figure depicts offer rate around admission threshold for schools ranked first, second and third or below in the application form, when applicants are ranked solely by distance to school. Admission ranks are centred around zero so that negative values represent eligible students and applicant with rank zero is the last admitted. Markers represent average values in two-units-wide bins of admission rank and solid line is a local linear fit of underlying observations estimated separately on either side of the cut-off. Depicted are candidates who can not enter any school they prefer more. These are identified by replicating school assignment based on school capacities, parental preferences, distance to school and actual school offer. See Section 3 for details.

Figure 3: Preference for school where enrolled



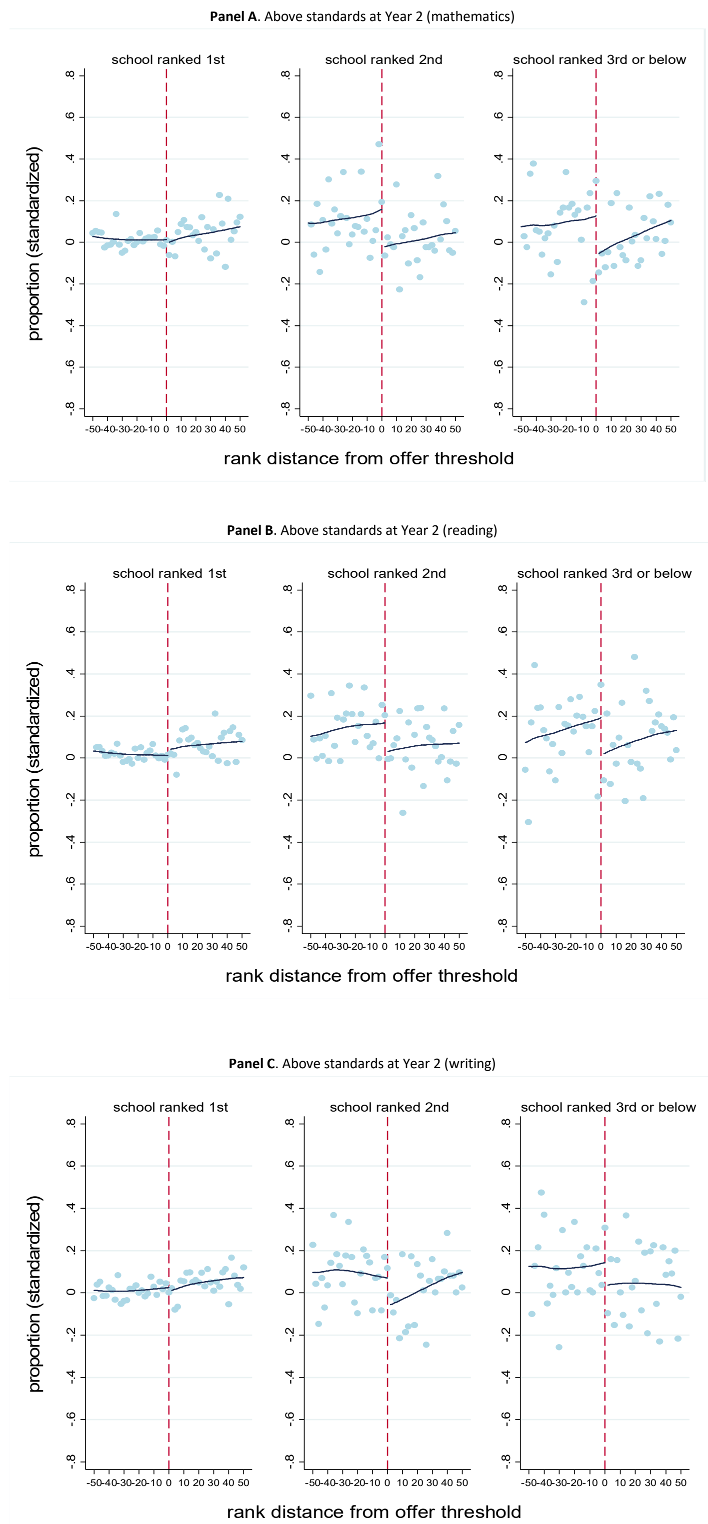

**Note.** The figure depicts preference rank for school where applicants are enrolled around admission threshold for schools ranked first, second and third or below in the application form. A preference rank of one indicates the first-choice school, while a school ranked sixth is the lowest preferred among those listed. For pupils not enrolled in any of listed schools, preference rank is coded as 7. Admission rank are centred around zero so that negative values represent eligible students and applicant with rank zero is the last admitted. Markers represent average values in two-units-wide bins of admission rank and solid line is a local linear fit of underlying observations estimated separately on either side of the cut-off. Admission rank are obtained replicating school assignment using information on school capacities, parental preferences distance to school and actual school offer. See Section 3 for details.

Figure 4: Covariates balance around admission cut-offs




**Note.** The figure depicts baseline individual characteristics around admission threshold for schools ranked first, second and third or below in the application form. Panel A and Panel B show proportion of pupils above standards in mathematics at Year 0 and deprivation index in pupils' local area , respectively. Panel C and Panel D shows the proportion of pupils speaking English at home and of white pupils, respectively. All variables are standardised to have zero mean and unit variance in the sample. Markers represent average values in two-units-wide bins of admission rank. Solid line is a local linear polynomial fit of underlying observations estimated separately on either side of the cut-off. See Section 3 for details.

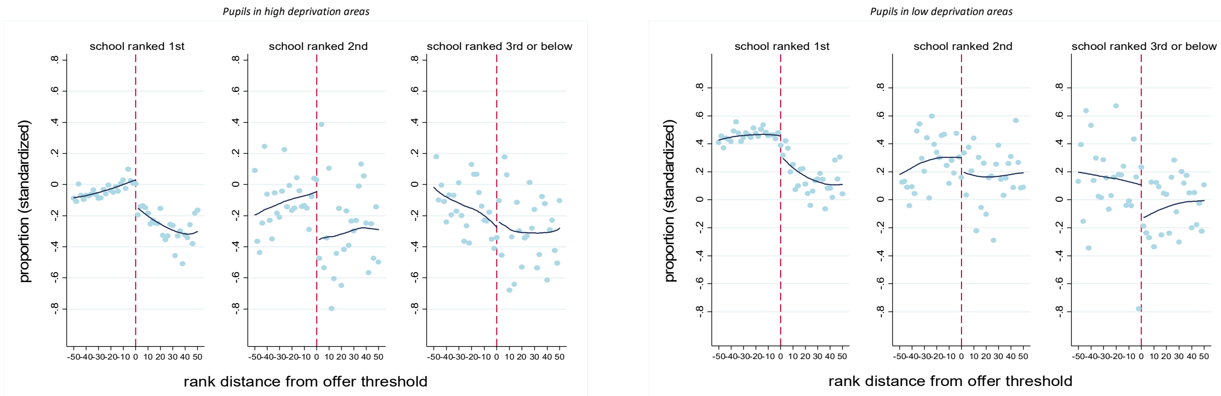
Figure 5: Year 2 assessments around admission cut-offs



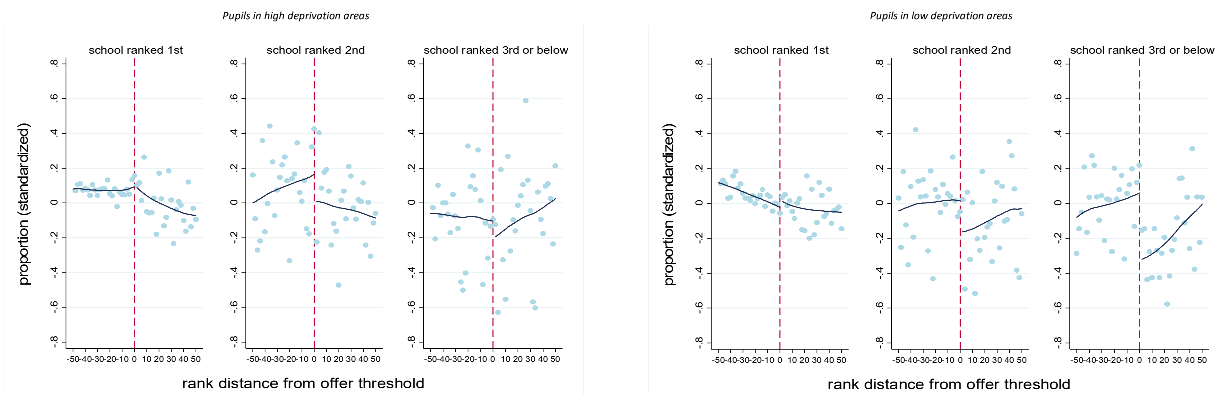

**Note.** The figure depicts achievement at Year 2 around admission threshold for schools ranked first, second and third or below in the application form. Reported is the share of pupils above expected standards (level 4) in mathematics (Panel A), reading (Panel B) and writing (Panel C). All outcomes are standardized to have zero mean and unit variance among 2014 London primary school applicants. Markers represent average values in two-units-wide bins of admission rank. Solid line is a local linear polynomial fit of underlying observations estimated separately on either side of the cut-off. See Section 4 for details.

## Figure 6: School performance around admission cut-offs

**Panel A.** Share of students above standards at Year 6 (mathematics)




**Panel B.** Share of students making expected Year 2 - Year 6 progress (mathematics)




**Note.** The figure depicts baseline characteristics of school where pupil is enrolled around admission thresholds for schools ranked first, second and third or below in the application form. Figures are separated by deprivation in pupil's local area. Depicted in Panel A is the proportion of pupils achieving above expectation (level 5) in mathematics at Year 6 test scores. Depicted in Panel B is the share of pupils progressing at least as expected (two levels) between Year 2 and Year 6. All outcomes are standardized to have zero mean and unit variance among 2014 London primary school applicants. Markers represent average values in two-units-wide bins of admission rank and a local linear polynomial fit of underlying observations estimated separately on either side of the cut-off. See Section 5 for details.

Table 1: Oversubscribed primary schools

| | Popular schools | Not popular schools | Difference |
|---|---|---|---|
| | (1) | (2) | (3) |
| *School performance* | | | |
| % above standards at Year 6 (mathematics) | 0.2499 | -0.4094 | 0.6593*** |
| % above standards at Year 6 (reading) | 0.3080 | -0.5050 | 0.8127*** |
| % making expected Year 2 - Year 6 progress (mathematics) | 0.0821 | -0.1341 | 0.2162*** |
| % making expected Year 2 - Year 6 progress (reading) | 0.1420 | -0.2329 | 0.3749*** |
| *School type* | | | |
| Religious school | 0.0901 | -0.1412 | 0.2313*** |
| Academy school | -0.0423 | 0.0663 | -0.1086** |
| Community school | -0.0663 | 0.1038 | -0.1701*** |
| *School composition* | | | |
| % FSM eligible pupils | -0.1711 | 0.2631 | -0.4342*** |
| % native speakers | -0.1717 | 0.2776 | -0.4493*** |
| % white pupils | 0.1320 | -0.2029 | 0.3349*** |
| Students' deprivation | -0.1604 | 0.2465 | -0.4069*** |
| N | 1042 | 667 | 1709 |

**Note.** This table shows characteristics of London primary schools by oversubscription status in 2014. Column (1) and column (2) consider oversubscribed and undersubscribed school respectively, while the difference is reported in column (3). A school is coded as oversubscribed if applicants at risk of admission (see Section 3 for details) exceed capacity by at least 5 seats. A student is above expectations at final year standardised tests if attained level 5 or above. Expected progress is two levels gain from Year 2 to Year 6. Deprivation is measured by an index based on average income in neighbourhood of residence. All variables are standardised to have zero mean and unit variance in the sample. See Section 3 for details. ***p<0.01. ** p<0.05. * p<0.1

## Table 2: Estimated covariate discontinuities

| | School ranked 1st | | | School ranked 2nd | | | Schools ranked 3rd or below | | |
|---|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) |
| Baseline achievement | 0.0626* | 0.0474 | 0.0695** | -0.0746 | -0.0574 | -0.0037 | 0.0732 | 0.0347 | 0.0928 |
| | (0.0330) | (0.0320) | (0.0296) | (0.0759) | (0.0726) | (0.0899) | (0.0890) | (0.0829) | (0.104) |
| Deprivation | 0.0002 | 0.0160 | 0.0030 | 0.1090 | 0.0872 | 0.0365 | 0.0511 | 0.0704 | -0.0312 |
| | (0.0425) | (0.0416) | (0.0236) | (0.0784) | (0.0798) | (0.0532) | (0.101) | (0.102) | (0.0903) |
| Native Speaker | -0.0449 | -0.0333 | -0.00832 | 0.0500 | 0.0227 | 0.0776 | 0.0383 | 0.0276 | 0.0345 |
| | (0.0354) | (0.0347) | (0.0294) | (0.0761) | (0.0774) | (0.0834) | (0.0847) | (0.0842) | (0.0946) |
| White | 0.0115 | -0.0119 | 0.00706 | -0.0251 | -0.000537 | 0.0478 | -0.1305 | -0.1321 | -0.0935 |
| | (0.0439) | (0.0415) | (0.0385) | (0.0762) | (0.0775) | (0.0800) | (0.122) | (0.123) | (0.160) |
| Free school meal eligilble | -0.0431 | -0.0405 | -0.0459 | 0.0265 | 0.0103 | 0.0861 | -0.0659 | -0.0539 | -0.0568 |
| | (0.0371) | (0.0383) | (0.0371) | (0.0608) | (0.0626) | (0.0675) | (0.0937) | (0.0959) | (0.110) |
| Black | 0.0131 | 0.0205 | 0.0242 | -0.0723 | -0.0758 | -0.00371 | 0.1101 | 0.1379* | 0.1640* |
| | (0.0328) | (0.0322) | (0.0276) | (0.0690) | (0.0710) | (0.0816) | (0.0790) | (0.0801) | (0.0917) |
| Special education needs | 0.0146 | 0.0240 | 0.0395 | 0.00516 | -0.00701 | -0.00595 | -0.2592* | -0.2024 | -0.0783 |
| | (0.0265) | (0.0265) | (0.0276) | (0.0484) | (0.0519) | (0.0761) | (0.142) | (0.136) | (0.120) |
| Female | 0.0388 | 0.0348 | 0.0371 | -0.0953 | -0.0637 | 0.0233 | -0.0644 | -0.0696 | -0.0796 |
| | (0.0365) | (0.0373) | (0.0368) | (0.0785) | (0.0796) | (0.0887) | (0.0999) | (0.103) | (0.136) |
| Joint test: p-value | 0.2232 | 0.3368 | 0.2387 | 0.6052 | 0.7074 | 0.9704 | 0.3784 | 0.3885 | 0.3388 |
| N. of preferences | X | X | X | X | X | X | X | X | X |
| Individual controls | | X | X | | X | X | | X | X |
| School of application FEs | | | X | | | X | | | X |

**Note.** The table reports regression estimates of school offer balance around admission threshold for schools ranked first (columns 1-3), second (columns 4-6) and third or below (columns 7-9) in the application form. Reported is the coefficient on school offer dummy in separate regressions for each cell, where the dependent variable is indicated in row headers. Running variable is controlled for by a linear polynomial estimated separately on either side of admission cut-off. Regressions are estimated by weighted least squares employing triangular kernel weights around the admission threshold. Optimal bandwidth is selected for each outcome following Calonico at al. (2014). Columns (1), (4) and (7) include number of preferences fixed effects. Columns (2), (5) and (8) add controls for all covariates other than the one used as dependent variable. Columns (3), (6) and (9) add school of application fixed effects. Reported at the bottom is p-value testing all covariate discontinuities at once. See Section 3 for details. Standard errors are clustered by school where enroled and reported in parenthesis. ***p<0.01. ** p<0.05. * p<0.1.

## Table 3: Average achievement effects

| | School ranked 1st | | | School ranked 2nd | | | Schools ranked 3rd or below | | |
|---|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) |
| | | | | **Panel A.** Year 2 achievement above expectations | | | | | |
| Mathematics | 0.0411 | 0.0416 | 0.0497 | 0.1378* | 0.1312* | 0.1626* | 0.1742** | 0.1838** | 0.2279** |
| | (0.0349) | (0.0350) | (0.0340) | (0.0782) | (0.0794) | (0.0903) | (0.0864) | (0.0883) | (0.113) |
| Reading | -0.0045 | -0.0161 | -0.0081 | 0.1254* | 0.1342* | 0.1632** | 0.1259 | 0.1376* | 0.1249 |
| | (0.0340) | (0.0339) | (0.0333) | (0.0737) | (0.0738) | (0.0780) | (0.0830) | (0.0835) | (0.105) |
| Writing | 0.0458 | 0.0437 | 0.0603* | 0.1159 | 0.1233* | 0.1928** | 0.1396* | 0.1372 | 0.1083 |
| | (0.0341) | (0.0339) | (0.0331) | (0.0747) | (0.0746) | (0.0840) | (0.0846) | (0.0864) | (0.111) |
| N | 29,755 | 29,496 | 29,496 | 3,618 | 3,566 | 3,566 | 2,742 | 2,684 | 2,684 |
| | | | | **Panel B.** Year 0 - Year 2 achievement gains at or above expectations | | | | | |
| Mathematics | 0.0184 | 0.0227 | 0.0191 | 0.1717** | 0.1478* | 0.1543 | 0.1653** | 0.1654** | 0.1792* |
| | (0.0323) | (0.0324) | (0.0311) | (0.0788) | (0.0785) | (0.0981) | (0.0807) | (0.0823) | (0.105) |
| Reading | -0.0348 | -0.0394 | -0.0454 | 0.1288** | 0.1346** | 0.1220* | 0.1092 | 0.1150 | 0.0506 |
| | (0.0305) | (0.0310) | (0.0300) | (0.0618) | (0.0621) | (0.0653) | (0.0728) | (0.0739) | (0.0886) |
| Writing | 0.0203 | 0.0229 | 0.0276 | 0.1221* | 0.1248* | 0.1399** | 0.1164 | 0.1144 | 0.0498 |
| | (0.0310) | (0.0312) | (0.0303) | (0.0643) | (0.0641) | (0.0703) | (0.0733) | (0.0748) | (0.0932) |
| N | 29,017 | 28,851 | 28,851 | 2,796 | 2,774 | 2,774 | 2,690 | 2,662 | 2,662 |
| N. of preferences | X | X | X | X | X | X | X | X | X |
| Individual controls | | X | X | | X | X | | X | X |
| School of application FEs | | | X | | | X | | | X |

**Note.** The table reports regression estimates of Year 2 achievement discontinuity around admission threshold for schools ranked first (columns 1-3), second (columns 4-6) and third or below (columns 7-9) in the application form. Reported is the coefficient on school offer dummy in separate regressions for each cell, where the dependent variable is indicated in row headers. All outcomes are standardised to have zero mean and unit variance among London primary school applicants. Panel A reports estimates for absolute achievement effects. Achievement gains are estimated controlling for Year 0 achievement and results are reported in Panel B. Running variable is controlled for by a linear polynomial estimated separately on either side of admission cut-off. Regressions are estimated by weighted least squares employing triangular kernel weights around the admission threshold. Optimal bandwidth is selected for each outcome following Calonico at al. (2014). Columns (1), (4) and (7) include number of preferences fixed effects. Columns (2), (5) and (8) add controls for ethnicity, gender, language, special education neeeds and free school meal eligibility. Columns (3), (6) and (9) add school of application fixed effects. See Section 4 for details. Standard errors are clustered by school where enroled and reported in parenthesis. ***p<0.01. ** p<0.05. * p<0.1.

Table 4: Heterogeneous achievement effects by individual characteristics

| | School ranked 1st | | School ranked 2nd | | Schools ranked 3rd or below | |
|---|---|---|---|---|---|---|
| | Main effect | Interaction | Main effect | Interaction | Main effect | Interaction |
| | (1) | (2) | (3) | (4) | (5) | (6) |
| **Panel A.** Female student | | | | | | |
| Mathematics | 0.0737 | -0.0500 | 0.2603* | -0.1862 | 0.5089*** | -0.5404** |
| | (0.0480) | (0.0684) | (0.1328) | (0.1843) | (0.1416) | (0.2098) |
| Reading | 0.0332 | -0.0880 | 0.0689 | 0.1849 | 0.2120 | -0.1793 |
| | (0.0447) | (0.0671) | (0.1083) | (0.1546) | (0.1453) | (0.2081) |
| Writing | 0.0601 | -0.0014 | 0.1821 | 0.0208 | 0.1481 | -0.0862 |
| | (0.0431) | (0.0659) | (0.1122) | (0.1666) | (0.1506) | (0.2300) |
| **Panel B.** High deprivation in area of residence | | | | | | |
| Mathematics | 0.0748 | -0.0467 | 0.3394** | -0.3826** | 0.2436 | -0.0561 |
| | (0.0526) | (0.0676) | (0.1344) | (0.1869) | (0.1584) | (0.2161) |
| Reading | 0.0246 | -0.0613 | 0.2962** | -0.2813* | 0.0911 | 0.0492 |
| | (0.0507) | (0.0665) | (0.1173) | (0.1578) | (0.1444) | (0.2017) |
| Writing | 0.1285** | -0.1297* | 0.3839*** | -0.4124** | 0.1632 | -0.1091 |
| | (0.0504) | (0.0678) | (0.1189) | (0.1711) | (0.1440) | (0.2144) |
| **Panel C.** Low achievement at baseline | | | | | | |
| Mathematics | 0.0333 | 0.0066 | 0.3361** | -0.3520* | 0.3642** | -0.2824 |
| | (0.0546) | (0.0638) | (0.1438) | (0.1846) | (0.1753) | (0.2163) |
| Reading | -0.0131 | -0.0283 | 0.3302*** | -0.3542** | 0.0702 | 0.0972 |
| | (0.0497) | (0.0604) | (0.1158) | (0.1506) | (0.1348) | (0.1640) |
| Writing | 0.1036* | -0.1158* | 0.3607*** | -0.3370** | 0.0873 | 0.0119 |
| | (0.0554) | (0.0650) | (0.1333) | (0.1616) | (0.1579) | (0.1945) |
| N. of preferences | X | | X | | X | |
| Individual controls | X | | X | | X | |
| School of application FEs | X | | X | | X | |

**Note.** The table reports regression estimates of heterogeneous school offer effects on Year 2 achievement. The corresponding average effects are those reported in columns (3), (6) and (9) in Panel A of Table 3. Reported here are estimates from separate regressions for schools ranked first (columns 1 and 2), second (columns 3 and 4) and third or below (columns 5 and 6) in the applcation form. Columns (1), (3) and (5) report estimated coefficients on school offer dummy. Columns (2), (4) and (6) report estimated coefficients on school offer dummy interacted with a dummy variable indicating female students (Panel A), pupils in areas with deprivation higher than median (Panel B) or pupils with Year 0 achievement lower than median (Panel C). Dependent variables are indicated in row headers. Running variable is controlled for by a linear polynomial estimated separately on either side of admission cut-off. Regressions are estimated by weighted least squares employing triangular kernel weights around the admission threshold. Optimal bandwidth is selected for each outcome following Calonico at al. (2014). See Section 4 for details. Standard errors are clustered by school where enroled and reported in parenthesis. ***p<0.01. ** p<0.05. * p<0.1.

## Table 5: Heterogeneous achievement effects by school characteristics

| | School ranked 1st | | School ranked 2nd | | Schools ranked 3rd or below | |
|---|---|---|---|---|---|---|
| | Main effect | Interaction | Main effect | Interaction | Main effect | Interaction |
| | (1) | (2) | (3) | (4) | (5) | (6) |
| **Panel A.** School where applied has final test scores above-median | | | | | | |
| Mathematics | -0.0283 | 0.0947 | 0.1045 | 0.0247 | 0.2810* | -0.1958 |
| | (0.0472) | (0.0736) | (0.1303) | (0.2031) | (0.1438) | (0.2779) |
| Reading | -0.0508 | 0.0469 | 0.0892 | 0.0093 | 0.1385 | -0.1632 |
| | (0.0465) | (0.0730) | (0.1092) | (0.1705) | (0.1441) | (0.2485) |
| Writing | 0.0073 | 0.0490 | 0.1105 | 0.0712 | 0.1542 | -0.2304 |
| | (0.0452) | (0.0716) | (0.1173) | (0.1881) | (0.1431) | (0.2646) |
| **Panel B.** School where applied is outstanding (Ofsted) | | | | | | |
| Mathematics | 0.0059 | 0.0635 | 0.1586 | 0.0063 | 0.1481 | 0.3380 |
| | (0.0450) | (0.0701) | (0.1230) | (0.2191) | (0.1394) | (0.2575) |
| Reading | -0.0377 | 0.0432 | 0.1150 | 0.1646 | 0.0272 | 0.3368 |
| | (0.0463) | (0.0699) | (0.1013) | (0.1867) | (0.1291) | (0.2466) |
| Writing | 0.0451 | -0.0089 | 0.1525 | 0.0526 | 0.0167 | 0.1754 |
| | (0.0445) | (0.0689) | (0.1062) | (0.1993) | (0.1334) | (0.2754) |
| **Panel C.** Applied to academy school | | | | | | |
| Mathematics | 0.0517 | -0.0123 | 0.1476 | 0.0975 | 0.2209* | 0.2347 |
| | (0.0366) | (0.0948) | (0.0969) | (0.2865) | (0.1186) | (0.3415) |
| Reading | -0.0228 | 0.1065 | 0.1032 | 0.4709* | 0.1274 | 0.0310 |
| | (0.0362) | (0.0949) | (0.0822) | (0.2745) | (0.1107) | (0.3642) |
| Writing | 0.0496 | 0.0796 | 0.1281 | 0.6135** | 0.0885 | 0.2274 |
| | (0.0354) | (0.1011) | (0.0900) | (0.2407) | (0.1169) | (0.3347) |
| N. of preferences | X | | X | | X | |
| Individual controls | X | | X | | X | |
| School of application FEs | X | | X | | X | |

**Note.** The table reports regression estimates of heterogeneous school offer effects on Year 2 achievement. The corresponding average effects are those reported in columns (3), (6) and (9) in Panel A of Table 3. Reported here are estimates from separate regressions for schools ranked first (columns 1 and 2), second (columns 3 and 4) and third or below (columns 5 and 6) in the applcation form. Columns (1), (3) and (5) report estimated coefficients on school offer dummy. Columns (2), (4) and (6) report estimated coefficients on school offer dummy interacted with a dummy variable indicating school of application is performing above median (Panel A), was deemed outstanding by latest Ofsted report available (Panel B) or is an academy (Panel C). School performance in Panel A is measured as share of students achieving above expectations at Year 6 in mathematics averaged over previous ten years. Dependent variables are indicated in row headers. Running variable is controlled for by a linear polynomial estimated separately on either side of admission cut-off. Regressions are estimated by weighted least squares employing triangular kernel weights around the admission threshold. Optimal bandwidth is selected for each outcome following Calonico at al. (2014). See Section 4 for details. Standard errors are clustered by school where enroled and reported in parenthesis. ***p<0.01. ** p<0.05. * p<0.1.

Table 6: Effects on school characteristics

| | School ranked 1st | School ranked 2nd | School ranked 3rd or below |
|---|---|---|---|
| | (1) | (2) | (3) |
| % above standards at Year 6 (mathematics) | 0.3144*** | 0.3200*** | 0.2133* |
| | (0.0518) | (0.0877) | (0.1203) |
| % above standards at Year 6 (reading) | 0.3912*** | 0.4217*** | 0.2126 |
| | (0.0437) | (0.0731) | (0.1870) |
| % making expected Year 2 - Year 6 progress (mathematics) | 0.1285** | 0.0740 | 0.2440 |
| | (0.0556) | (0.0879) | (0.1756) |
| % making expected Year 2 - Year 6 progress (reading) | 0.2094*** | 0.2350*** | 0.1693 |
| | (0.0504) | (0.0726) | (0.1714) |
| Outstanding school (Ofsted) | 0.3871*** | 0.3153*** | 0.0595 |
| | (0.0479) | (0.0789) | (0.0920) |
| Share of schoolmates on FSM | -0.2659*** | -0.2359*** | -0.2910*** |
| | (0.0312) | (0.0478) | (0.0764) |
| Schoolmates deprivation index | -0.1433*** | -0.1390*** | -0.1631*** |
| | (0.0171) | (0.0308) | (0.0620) |
| Distance to school | -0.5685*** | -0.2031** | -0.3288** |
| | (0.0495) | (0.0872) | (0.1384) |
| N. of preferences | X | X | X |
| Individual controls | X | X | X |
| School of application FEs | X | X | X |

**Note.** The table reports regression estimates of discontinuity in schoool characteristics around admission threshold for schools ranked first (column 1), second (column 2) and third or below (column 3) in the application form. Reported is the coefficient on school offer dummy in separate regressions for each cell, where the dependent variable is indicated in row headers. All outcomes are standardised to have zero mean and unit variance among London primary school applicants. A student is above expectations at final year standardised tests if attained level 5 or above. Expected progress is two levels gain from Year 2 to Year 6. Running variable is controlled for by a linear polynomial estimated separately on either side of admission cut-off. Regressions are estimated by weighted least squares employing triangular kernel weights around the admission threshold. Optimal bandwidth is selected for each outcome following Calonico at al. (2014). All columns include controls for number of prefences fixed effects, individual characteristics and school of application fixed effects, as in columns (3), (6) and (9) of Table 3. See Section 5 for details. Standard errors are clustered by school where enroled and reported in parenthesis. ***p<0.01. ** p<0.05. * p<0.1.

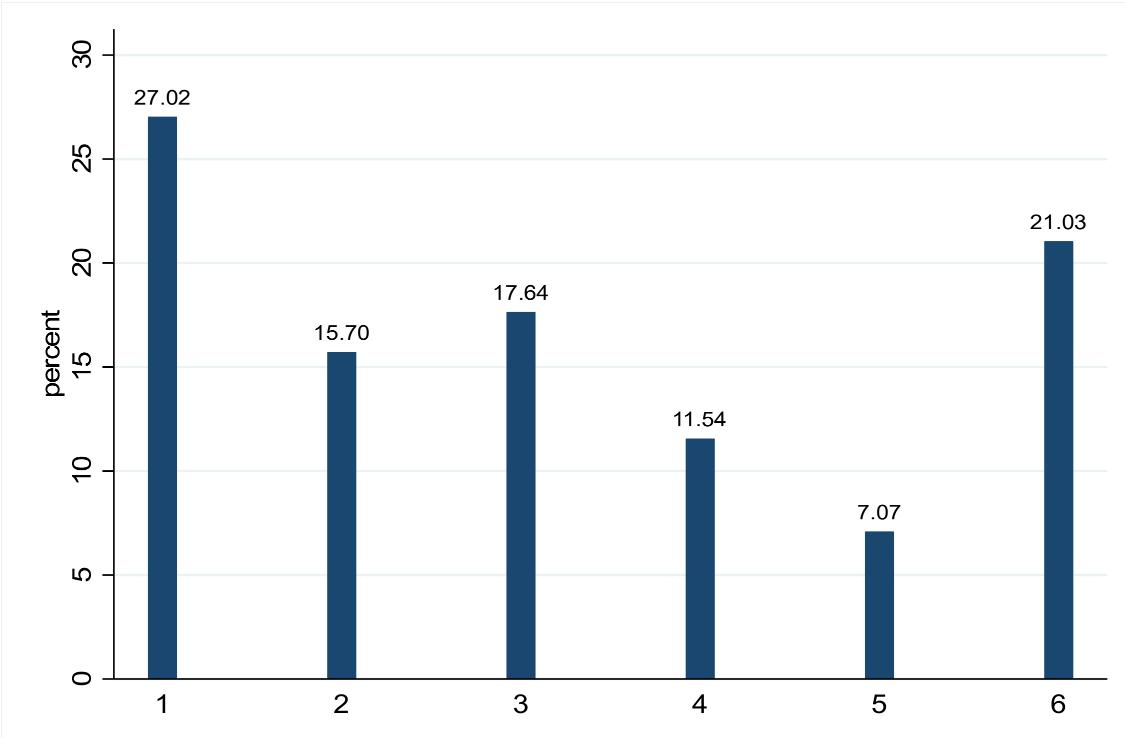
Table 7: Effects on school characteristics by deprivation in pupil's local area

| | School ranked 1st | | School ranked 2nd | | School ranked 3rd or below | |
|---|---|---|---|---|---|---|
| | Main effect | Interaction | Main effect | Interaction | Main effect | Interaction |
| | (1) | (2) | (3) | (4) | (5) | (6) |
| % above standards at Year 6 (mathematics) | 0.3536*** | -0.0773 | 0.3772*** | -0.1273 | 0.3490** | -0.2782 |
| | (0.0518) | (0.0878) | (0.1085) | (0.1423) | (0.1615) | (0.2222) |
| % above standards at Year 6 (reading) | 0.4112*** | -0.0382 | 0.4897*** | -0.1489 | 0.5922** | -0.7354** |
| | (0.0526) | (0.0744) | (0.1008) | (0.1241) | (0.2801) | (0.3553) |
| % making expected Year 2 - Year 6 progress (mathematics) | 0.0845 | 0.0833 | 0.0796 | -0.0107 | 0.4595** | -0.4379 |
| | (0.0718) | (0.0939) | (0.1180) | (0.1585) | (0.2205) | (0.2924) |
| % making expected Year 2 - Year 6 progress (reading) | 0.2087*** | -0.0010 | 0.2788*** | -0.0967 | 0.6234*** | -0.8664*** |
| | (0.0603) | (0.0864) | (0.0856) | (0.1331) | (0.2070) | (0.3202) |
| Outstanding school (Ofsted) | 0.3777*** | 0.0193 | 0.3196*** | -0.0086 | 0.0582 | -0.0302 |
| | (0.0623) | (0.0905) | (0.1148) | (0.1451) | (0.1316) | (0.1739) |
| Share of schoolmates on FSM | -0.2567*** | -0.0185 | -0.2487*** | 0.0271 | -0.1902** | -0.1929 |
| | (0.0371) | (0.0585) | (0.0678) | (0.0903) | (0.0954) | (0.1399) |
| Distance to school | -0.6059*** | 0.0720 | -0.3222*** | 0.2500 | -0.3020 | -0.0375 |
| | (0.0733) | (0.0942) | (0.1200) | (0.1696) | (0.2113) | (0.2756) |
| N. of preferences | X | | X | | X | |
| Individual controls | X | | X | | X | |
| School of application FEs | X | | X | | X | |

**Note.** The table reports estimates of heterogeneous discontinuity in school characteristics by deprivation in pupil's local area. Corresponding average effects are reported in Table 5. Regressions consider applicants around admission threshold for schools ranked first (columns 1-2), second (columns 3-4) and third or below (columns 5-6) in the application form. Columns (1), (3) and (5) report estimated coefficients on school offer dummy. Columns (2), (4) and (6) report estimated coefficients on school offer dummy interacted with a dummy variable indicating pupils in areas with deprivation higher than median. All outcomes are standardised to have zero mean and unit variance among London primary school applicants. A student is above expectations at final year standardised tests if attained level 5 or above. Expected progress is two levels gain from Year 2 to Year 6. Running variable is controlled for by a linear polynomial estimated separately on either side of admission cut-off. Regressions are estimated by weighted least squares employing triangular kernel weights around the admission threshold. Optimal bandwidth is selected for each outcome following Calonico at al. (2014). All columns include controls for number of prefences fixed effects, individual characteristics and school of application fixed effects. See Section 5 for details. Standard errors are clustered by school where enroled and reported in parenthesis. ***p<0.01. ** p<0.05. * p<0.1.

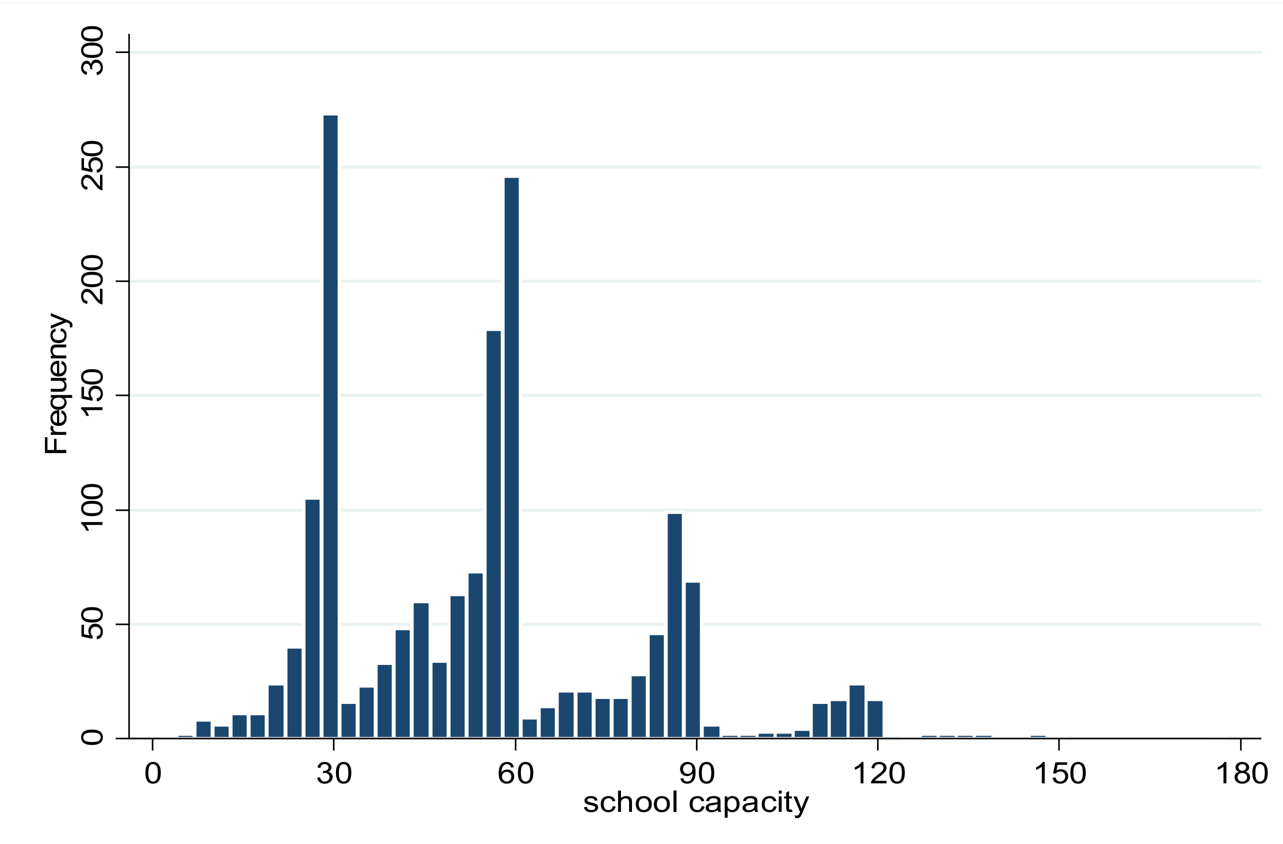
# Appendix

Figure A.1: Number of schools listed



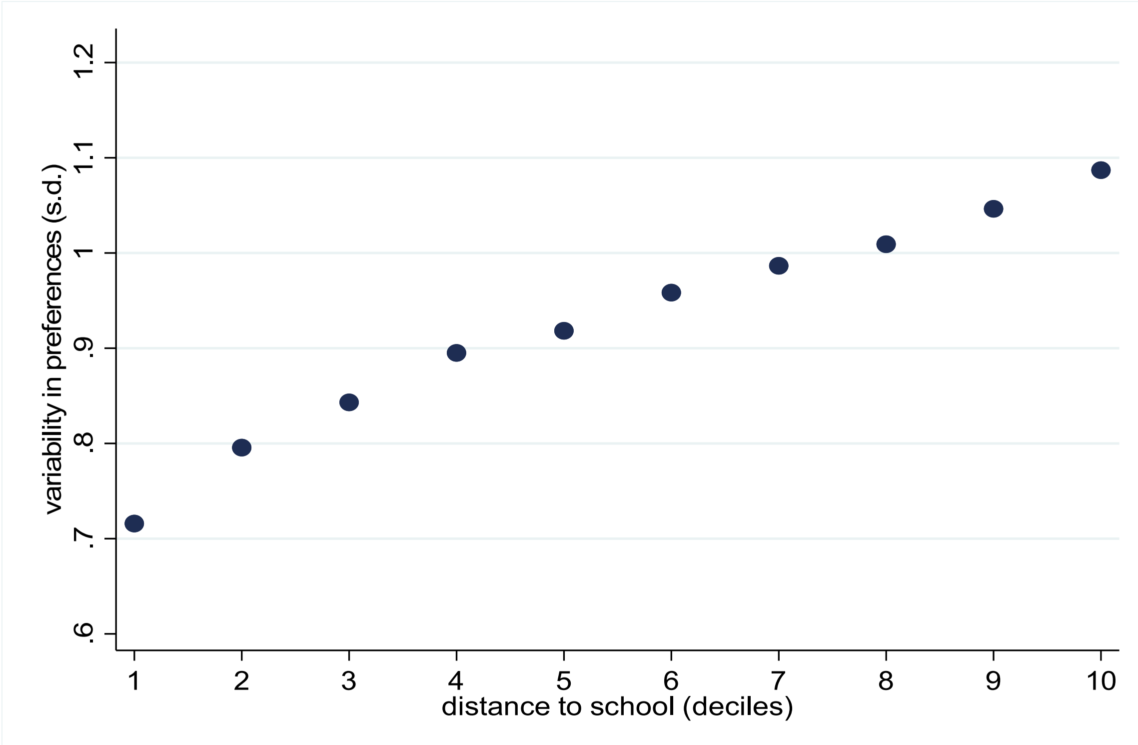

**Note.** The figure depicts the share of parents naming one to six schools, the maximum number allowed, in the application form. Shown in bar labels are percentage points. Considered are applicants to at least one London primary school entering the reception year in 2014. See Section 2 for details.

Figure A.2: School capacity



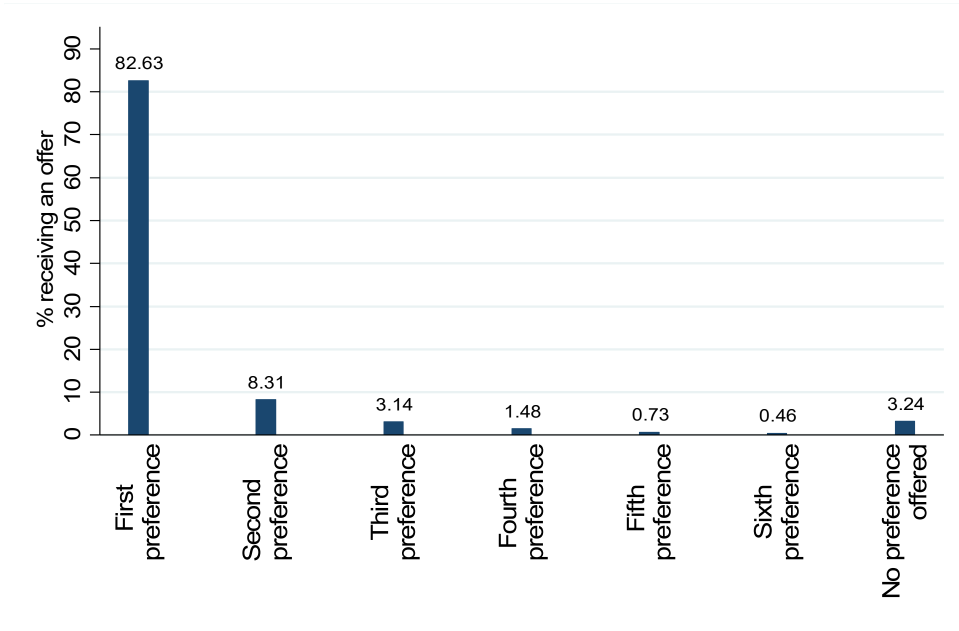

**Note.** The figure depicts the distribution of capacity of London primary schools, approximated by the number of offers issued. Bars represent frequency counts in three-units-wide bins, computed using one observation per school. See Section 2 for details.

Figure A.3: Variability of preferences by distance to school



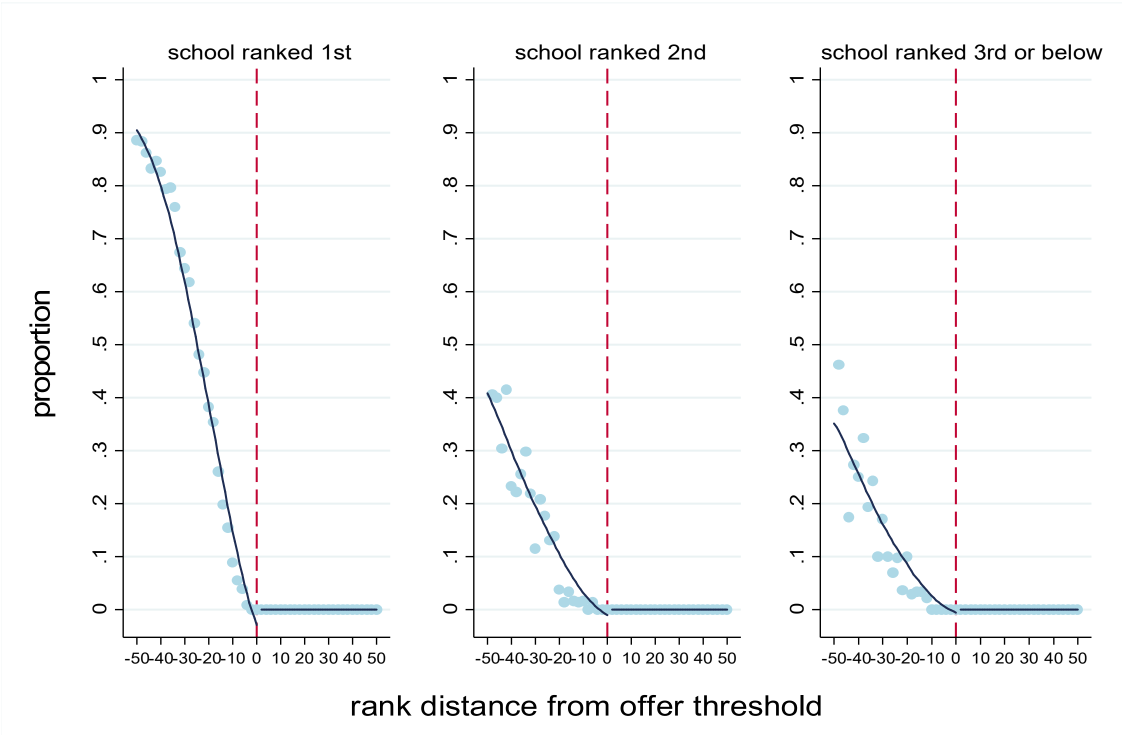

**Note.** The figure depicts variability in preference for the school among applicants conditional on distance. Preference ranks (from 1 to 6) are standardised to have zero mean and unit variance in the working sample. Markers represent their standard deviation by distance to school. Distance bins are deciles of within-school distribution of applicants. Outliers in the top 5% of the aggregate distance distribution are excluded. See Section 2 for details.

Figure A.4: Offered school




**Note.** The figure depicts the share of parents receiving an offer from listed or not listed schools by preference rank. Shown in bar labels are percentage points. Considered are applicants to at least one London primary school entering the reception year in 2014. See Section 2 for details.

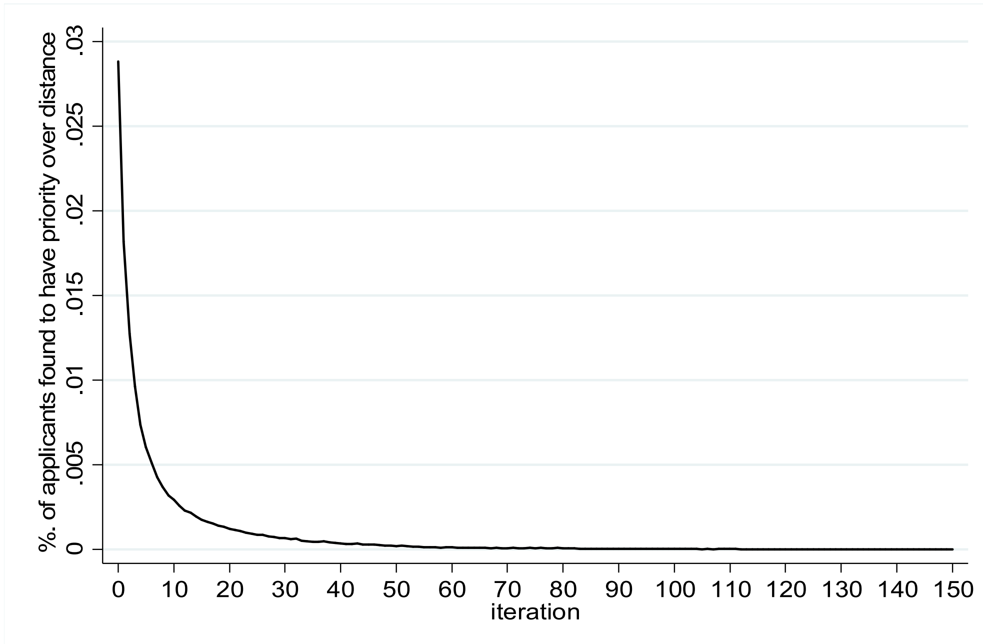
Figure A.5: Offer and enrollment rate before adjusting for priorities



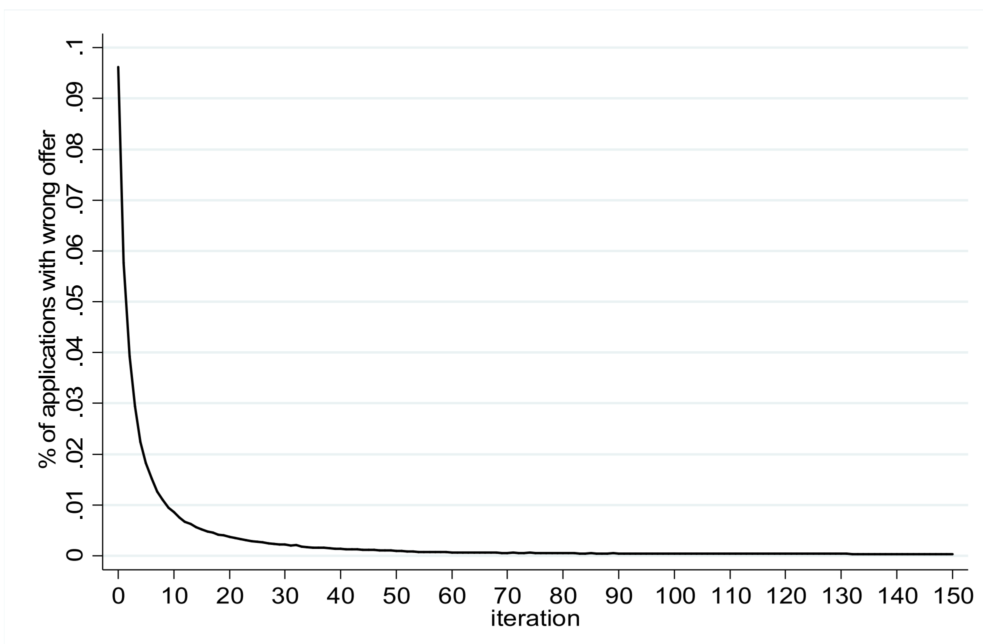

**Note.** The figure depicts offer rate around admission threshold for schools ranked first, second and third or below in the application form, when applicants are ranked solely by distance to school. Admission ranks are centred around zero so that negative values represent eligible students and applicant with rank zero is the last admitted. Markers represent average values in two-units-wide bins of admission rank and solid line is a local linear fit of underlying observations estimated separately on either side of the cut-off. Depicted are candidates who cannot enter any school they prefer more, identified by replicating school assignment based on school capacities, parental preferences and distance to school. Shown here is the figure at the first replication stage, where admission priorities are not yet detected. See Section 3 for details.

Figure A.6: Replication of school assignment

**Panel A.** Priority over distance



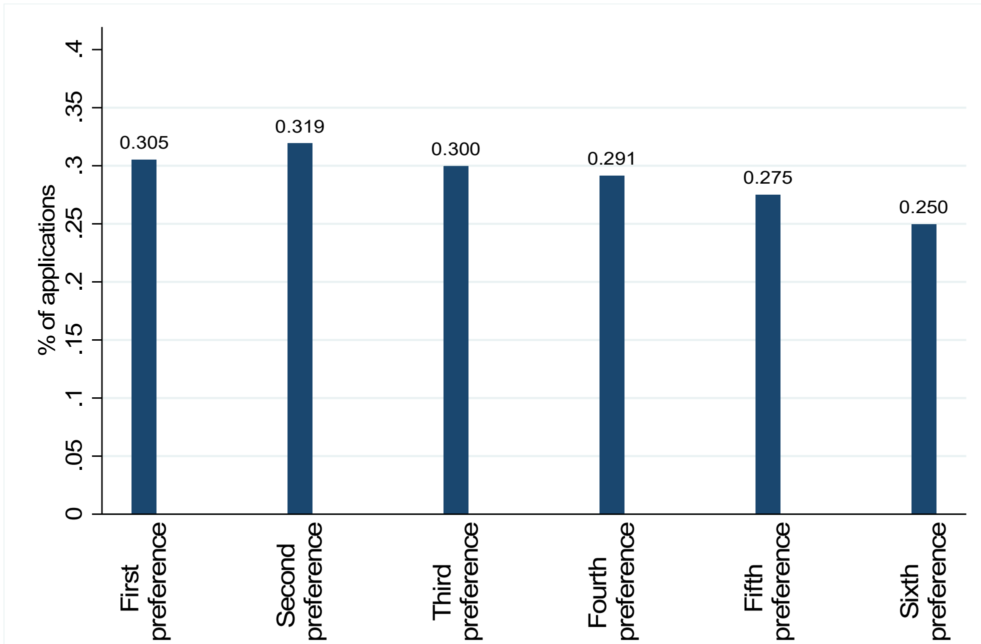

**Panel B.** School offer replication



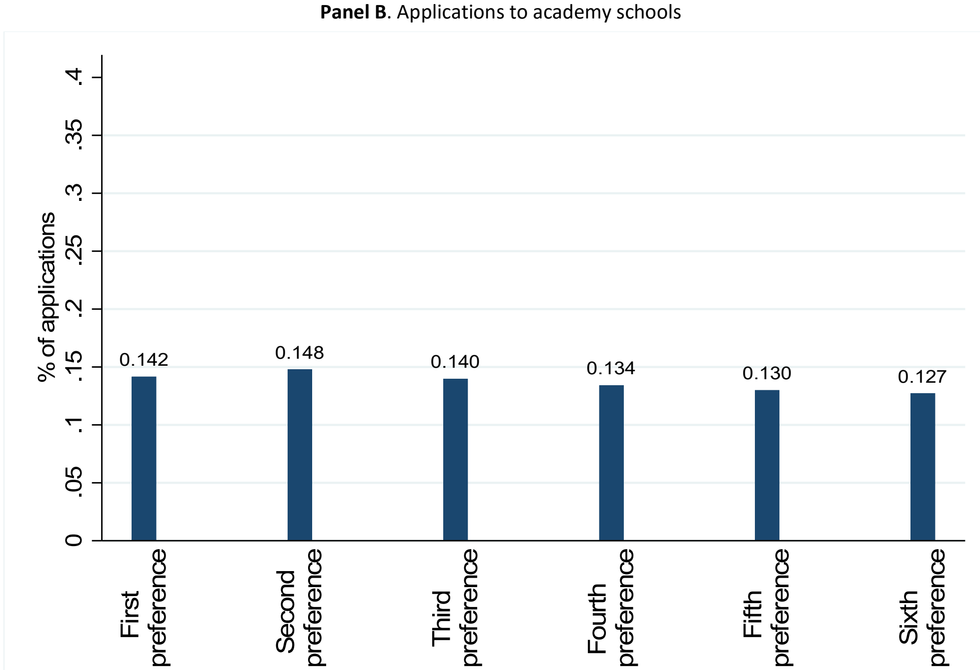

**Note.** The figure depicts fraction of applicants found to enjoy priority over distance (Panel A) and with wrong predicted offer (Panel B) by iteration of school assignment. Allocation mechanism is replicated based on school capacities, parental preferences and distance to school. Applicants are ranked solely by proximity in iteration 0 and those with offer beyond estimated threshold 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 for details.

Figure A.7: Application to academy schools

**Panel A**. Applications to schools deemed outstanding (Ofsted)




**Panel B**. Applications to academy schools




**Note.** The figure depicts share of applicants naming an school deemed outstanding by Ofsted inspectors (Panel A) or an academy school (Panel B) by preference rank. Vertical axis reports average of a dummy for application to an academy while horizontal axis depcits parental preference rank for the school in the application form. Bar labels show computation of bar heights. See Section 4 for details.

Table A.1: Descriptive statistics

| | Mean | Standard deviation |
|---|---|---|
| | (1) | (2) |
| **Panel A**. Pupil characteristics | | |
| FSM eligible | 0.161 | 0.367 |
| Native speaker | 0.420 | 0.493 |
| White | 0.419 | 0.498 |
| Asian | 0.191 | 0.384 |
| Black | 0.166 | 0.406 |
| Special education needs | 0.008 | 0.090 |
| Female | 0.490 | 0.500 |
| Exceeding expectations at Year 0: mathematics | 0.130 | 0.336 |
| Exceeding expectations at Year 2: mathematics | 0.260 | 0.439 |
| Exceeding expectations at Year 2: reading | 0.298 | 0.458 |
| Exceeding expectations at Year 2: writing | 0.199 | 0.399 |
| N. of schools listed | 3.195 | 1.870 |
| Take-up offered school | 0.876 | 0.329 |
| Left public education | 0.034 | 0.182 |
| Number of pupils | | 98,299 |
| **Panel B**. School characteristics | | |
| % exceeding expectations at Year 6: mathematics | 0.456 | 0.157 |
| % Exceeding expectations at Year 6: reading | 0.462 | 0.169 |
| % Expected in progress in Year2-Year 6: mathematics | 0.929 | 0.069 |
| % Expected in progress in Year2-Year 6: reading | 0.923 | 0.066 |
| Faith school | 0.187 | 0.390 |
| Academy school | 0.139 | 0.346 |
| Capacity | 54.645 | 25.707 |
| Oversubscription** | | |
| by 5 seats or more | 0.610 | 0.488 |
| by 10 seats or more | 0.522 | 0.500 |
| by 20 seats or more | 0.383 | 0.486 |
| Distance of last admitted pupil (km)* | 1.132 | 1.189 |
| Number of schools | | 1,708 |

**Note.** The table reports descriptive statistics about applicants to primary schools in the 33 London local authorithies in 2014. Means and standard deviations are computed using one observation per pupil in Panel A and one observation per school in Panel B. Statistics are conditional on non-missing observations. Columns (1) and (2) report respectively sample average and standard deviation. See Section 2 for details.

*Excluding outliers in the top 5% of distance to named school, conditional on not being prioritised in admission.

**After adjusting for pupils who can enter a preferred school (replication of the assignment algorithm is described in Section 3).