

DISCUSSION PAPER SERIES

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Place at a Preferred Secondary School in
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ISSN: 2365-9793

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ABSTRACT

Heterogeneous Effects of Missing out on a Place at a Preferred Secondary School in England*

Schools vary in quality, and high-performing schools tend to be oversubscribed: there are more applicants than places available. In this paper, we use nationally representative cohort data linked to administrative education records to study the consequences of failing to gain admission to one's first-choice secondary school in England. Our empirical strategy leverages features of the institutional setting and the literature on school choice to make a case for a selection-on-observables identifying assumption. Failing to gain a place at a preferred school had null to small impacts on short-run academic attainment, but was associated with large reductions in mental health and increased fertility in early in adulthood. These effects are especially pronounced in areas which deployed a manipulable assignment mechanism to allocate school places, where we detected larger detrimental effects on high-stakes examination outcomes. A potential channel is increased early engagement in risky behaviours. Our results show that schools are important in shaping more than test scores, and that the workings of the school admission system play a fundamental role in ensuring access to good schools.

JEL Classification: I21, I24, J24, H44, D47

Keywords: education, school choice, human capital, market design, risky behaviours

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* Many thanks to the ESRC for funding this project via ES/R003629/1; OFSTED for providing (experimental) inspection outcomes data; David Church at the Centre for Longitudinal Studies UCL for his work preparing confidentialised distance-to-school variables for the project; John Coldron and colleagues for kindly providing data on variation in school admission policies; the UK Data Service for access to the data via their UKDS Secure Lab. The authors are grateful for helpful comments from participants at the 3rd IZA Economics of Education workshop, the VUW Applied Econometrics Workshop 2018 and SOLE 2019. Declarations of interest: none.

1 Introduction

Demand for places at popular schools exceeds supply, and not all children are able to attend their preferred school. Attending a preferred school may have potentially important consequences for long term outcomes, if parents choose schools based on test score improvements or other school attributes important for child outcomes. In addition to academic outcomes, parents may rationally value a wider range of outputs that schools might provide, for example non-cognitive skill development or a safe environment that supports pupil well-being—all of which can have important consequences for success in later life (Heckman et al., 2006; Mendolia et al., 2018).

The evidence to date on whether parents choose schools based on causal improvements in test scores, rather than peer mix, pedagogy or other school attributes, is mixed (Gibbons and Silva, 2011; Burgess et al., 2014; Abdulkadiroglu et al., 2017b). The evidence on effects of attending a preferred school is also mixed, although the majority of studies document little or no effect of attending a preferred school on short-run test scores (Cullen et al., 2006; Dobbie and Fryer, 2014; Deming et al., 2014; Abdulkadiroglu et al., 2014, 2017a; Beuermann and Jackson, 2020). These studies are often based on data from specific cities (for example, Boston or New York), and specific settings, such as academically “elite” schools. This paper provides new evidence by combining administrative data and detailed cohort data, which follows pupils who entered secondary school in 2001, from age 14 to age 25 years, to study the short- and long-run outcomes associated with missing out on a place at a first-choice school. Our setting is England, which, with a largely centralised school funding system and admissions policies, represents an informative laboratory for studying school choice.

The majority of school places in England are allocated using a centralised system, which endeavours to honour parental preferences subject to school capacity constraints. Our study focuses on secondary schools, which children attend from age 11 to at least 16 years. Secondary school test score averages are well-publicised and a number of empirical regularities have been established. We know that parents value secondary schools which perform well on accepted academic metrics (Gibbons and Silva, 2011; Burgess et al., 2019), and there is regional

and demographic patterning in the share of families gaining entrance to their preferred school (Weldon, 2018). On average, parents value schools perceived as academically “good” schools, and are willing to travel to attend them (Burgess, 2016; Weldon, 2018; Burgess et al., 2019). About 84% of children are offered a place at their first-choice secondary school. Yet, to date, there is little evidence on differences in outcomes for those attending (or not) their first-choice secondary school in the UK.

This paper contributes to the literature in at two important ways. First, we consider longer-run outcomes, in contrast to many papers which focus on the effects of missing out on short-run test scores only. For instance, Ovidi (2019) studies the causal effects of missing out on preferred primary schools in London, and does not detect achievement losses from missing out on a first-choice school at ages 6 to 7 years—aside from small detrimental effects on writing skills among affluent children. However, parents may choose schools based on other dimensions, the effects of which may not be revealed until later in life (Beuermann and Jackson, 2020). Hence in addition to short-run academic achievements, we consider longer run and broader outcomes, including mental health, fertility, and income in early adulthood. Second, two mechanisms for allocating school places (given parental preferences, school priorities and capacity constraints) were employed at the time that the children in our cohort were making their school selections: “first-preference” (also called immediate acceptance, which is now illegal) and “equal preference” (also called deferred-acceptance, which remains in force today). Theoretically, first-preference is inefficient, because it incentivises parents, when they rank schools, to trade off their true preferences against acceptance probabilities (Ergin and Sönmez, 2006). Moreover it is, in practice, likely to be inequitable, because not all families have the knowledge to engage with this system to ensure a satisfactory school placement (Pathak and Sönmez, 2008). Geographical variation in the allocation system allows us to estimate effects separately for pupils exposed to these two mechanisms. While there is a body of theoretical work studying these mechanisms that suggest that their allocations might differ, there is little empirical evidence that tests whether there are differential consequences for child outcomes.

To apply for a place at a state secondary school in England, parents submit a ranking of

schools to the local admissions authority. Parents can list between 3 and 6 schools depending on their local area. High-performing secondary schools tend to be oversubscribed: there are more applicants than places available. In the case of oversubscribed schools, places are rationed based on a set of published criteria, including whether the applicant had special educational needs, has older siblings at the school, and the family's distance from the school. Faith will be considered for religious schools. If a pupil cannot be allocated to any of their nominated schools (often because the parents do not list enough schools), they are assigned to a school with spare capacity (which is often of lower quality). We study the consequences of not attending a first-choice school, henceforth termed "preferred" school.

We employ data from the Longitudinal Study of Young People in England (LSYPE, also known as *Next Steps*), a nationally representative birth cohort study which tracks the lives of a cohort of young people in England through secondary secondary until age 25 years (Calderwood, 2018). This data is confidentially linked to the National Pupil Database (NPD), a database of administrative school records. From this data we can characterise the attributes of schools, and track the achievement of the LSYPE pupils in high-stakes examinations. An ideal study design would involve using administrative data on the parental rankings of schools, linked to complete data on admissions probabilities, in order to model the admissions process and identify the outcomes of pupils on the margins of acceptance to their preferred school. We do not have this ideal complete administrative parental preference data. Rather, we have reports of whether a pupil is attending their preferred school, or not, and assumptions must be imposed for our results to have a causal interpretation. We rely on a selection-on-observables strategy, where our choice of covariates is informed by the admissions criteria and the literature on parental preferences. We have detailed information on the variables which determine school admission, proxies for parental preferences and engagement with the school choice system, socio-economic status, prior ability of the child, and variables characterising the density and quality of the local choice set of schools. One important advantage of our linked survey data is that we have information on important admissions-relevant variables—religion and presence of older siblings—which are absent from the available administrative data. We pair our analyses with tests of sensitivity to unobserved selection (Oster, 2019).

We use Ordinary Least Squares, and then a two-stage combination of geographical matching and regression adjustment, to estimate the average effects of failing to gain a place at a preferred school. For the matching procedure, we first create a matched sample of pupils, matching each pupil who missed out on a place at their preferred school, to a set of control pupils who attend schools in close proximity to the treated pupil. Using this geographically matched sample, we use regression-adjustment to estimate the average treatment effect of missing out on a preferred school, where the contributions of the control observations are weighted in inverse proportion to their distance from the treated unit's school.

Our main findings show, at best, only weak evidence for detrimental effects of missing out a preferred school on high-stakes examination scores. We find no significant effects on gaining 5 or more 'good' passes in examinations taken at age 16 years (GCSEs), and a statistically significant, but small, reduction of 5% in the total grade points achieved at this level. We find some evidence of reduced rates of staying on in school after 16 years, the age of compulsory schooling—about a 12% reduction from the mean. In terms of longer run outcomes, we fail to detect any effects on university attendance. The results show small detrimental effects on income at age 25 years, but sensitivity analyses indicate that this figure is not robust to unobserved selection. We do find robust evidence of negative effects on mental health in early adulthood, of about one-quarter of a standard deviation, and a 5-6 percentage point increase in the probability of having a child by age 25 years.

Our subgroup analyses show that the negative consequences of missing out are more pronounced in local areas which used a more manipulable mechanism ('first-preference first', FPF) for allocating school places. This mechanism is now illegal because it rewarded strategic behaviour by parents in ranking schools, giving rise to a concern that this system advantages families who could successfully engage with this complexity. Conversely, under an 'equal-preference' (or 'deferred acceptance', DA) system, parents can (in theory¹) do no better than reveal their true preferences over schools, and playing strategically yields no advantage. In FPF areas, we do find detrimental effects on short-run academic attainment, including test perfor-

¹Constrained list lengths induce some degree of strategic behaviour in practice.

mance, staying on in school after age 16 years, university attendance, in addition to longer run outcomes of mental ill-health and income. The findings are similar to Abdulkadiroğlu et al. (2017), who find test score and graduation rate improvements from switching to a less manipulable school choice mechanism in Boston. The differences in distance-to-school, between those who get in and miss out on a place at a preferred school, are larger in areas where the FPF mechanism was used, possibly indicating poorer quality school matches. Pupils who miss out in FPF areas also show increased patterns of early engagement in risky behaviours.

We also find (weaker) evidence that consequences of missing out are larger for pupils of White ethnic origin, compared with Black, Asian and Minority Ethnic (BAME) pupils, specifically for poor mental health in adulthood, and risky behaviours in early adolescence. While White pupils are less likely to miss out on a preferred school, they more commonly express a preference for only one, strategically chosen, school in their preference ranking, rather than providing a complete list of 3 to 6 preferences (depending on the area). Hence if they do miss out on this first-choice school, they are more likely to be allocated to a less popular school which has spare capacity remaining—most likely of lower quality.

Overall, our findings reveal only weak evidence for negative effects of missing out on a preferred school on short-run test scores, with null to small effect sizes—except in those areas which used a manipulable mechanism to assign school places, where we find more pronounced detrimental effects. These findings suggest that the nature of the school choice process and assignment mechanism, and how parents understand and engage with it, are important for ensuring equal access to satisfactory schools. The results also demonstrate that which school a child attends shapes more than test scores: we find that missing out on a preferred school is associated with poorer life outcomes in a broader sense, including mental health and early engagement in risky behaviours.

2 Related literature

There are a number of studies that combine market design theory with modern methods for policy evaluation to assess how missing out on a preferred school impacts outcomes (Pop-Eleches and Urquiola, 2013; Cullen et al., 2006; Dobbie and Fryer, 2014, 2015; Abdulkadiroglu et al., 2017a; Ovidi, 2019). Several of these papers focus on peer effects as a possible channel (Abdulkadiroglu et al., 2014; Hoekstra et al., 2018). Nonetheless, the majority of these studies do not find any causal effect of attending a preferred secondary school in academic outcomes. This puzzle could have a number of possible explanations. First, many of these studies use a compelling empirical design—comparing the outcomes of pupils who “just” got into a school compared with those who just missed out, either side of a threshold based on a continuous test score. On this margin, there are at least two possible offsetting effects. First, pupils who just get in will be at the bottom of the ability rank in a top tier school, whereas those who miss out will be at the top of the ability rank. Rank has been documented to have a direct effect on outcomes (Elsner and Isphording, 2017). This issue is less relevant for our results, as for most schools in England selection on ability is explicitly prohibited—meaning we escape the confounding effect of ability rank faced by other studies, and our estimates are more generalisable across the ability distribution. Second, parents and families may increase (reduce) their effort invested in their child if she gets in (misses out), again exerting an offsetting effect (Pop-Eleches and Urquiola, 2013).

More generally, parents may not choose schools based on school effectiveness; either because they are not aware of the effectiveness of different schools, or they mistake the quality of the intake mix for school effectiveness (Abdulkadiroglu et al., 2017b), or they value something else. Parents may value other school attributes, such as “elite” status of the school, diversity in the school, or expected skill improvements on other margins, such as non-cognitive skills. Indeed, choosing a school based on considerations other than test scores can be a rational choice, which might show up in a wider set of outcomes, such as well-being and mental health, and longer-term outcomes. For instance, Beuermann and Jackson (2020) provide compelling evidence that while attending preferred school in Barbados does not improve short-run test scores,

it does reduce early pregnancy and enhance employment rates among women.

There are a number of papers looking at the effects of attending a “good school” in the UK. Studies looking at the effects of gaining a place at the academically selective “grammar” schools typically find positive effects on secondary school educational tests scores and years in school. The effects on other outcomes are more mixed: Clark and Del Bono (2016) find large positive effects on income and wages, and reduced fertility among women, but no similar effects among men, in a sample from one district in England. Pastore and Jones (2019) study the effects of attending academically selective grammar schools in a nationally representative sample, and do not detect any effects on adult labour market outcomes or measures of health and well-being. These papers look at attendance at grammar schools, which comprise only about 3% of pupils (see Appendix A for a description of school types).

There is little evidence in the English literature looking at the effects of attending a preferred secondary school. One exception is Ovidi (2019), who estimates the causal effects of missing out on a place at a preferred state primary school in London, where the school admissions system is centrally coordinated. Comparing the outcomes of marginally accepted students, Ovidi (2019) finds no achievement losses from missing out on a first-choice school, but does find negative effects from missing out on a second-choice school. These losses are driven by more affluent families, for whom there are effects of missing out on second, third and lower ranked choices, as well as small detrimental effects of missing out on a first-choice on writing skills only. However, no effects of missing out are detected for disadvantaged pupils, whether a school is first-choice or lower in the preference list. The implications put forward are that, while both low and high socio-economic status (SES) families appear to value academic achievement, affluent families are more able to sort into neighbourhoods with at least two good schools, in order to boost achievement and reduce the chance of being assigned an undesirable school. This reflects a theme emerging in the English literature that it is constraints (via the proximity tie-breaker), rather than preferences for quality, which drive differences in the quality of schools attended by low versus high SES families.

3 Setting

“School choice” is defined as policies which allow parental preferences to influence which school their child will attend (Cantillon, 2017). The central problem in designing a school choice system is to allocate pupils to schools, while accounting for parental preferences, school capacities and wider policy objectives (e.g. diversity in schools). One motivation for school choice focuses on the importance of respecting parental preferences, and allowing them to choose from a diversity of school types. A second motivation, not necessarily implied by the first, is to induce competition between schools with the aim of improving school standards. Both of these motivations have been referenced by different UK governments in relation to school choice policy. In England, features of parental “choice” and a “quasi-market” in compulsory schooling started to emerge from 1988, formalising the role of parental preferences and tying funding to enrollment numbers.

While many changes in the operation of the school choice system have occurred over recent decades (West et al., 2011), the share of children being offered a place at their first choice school has remained static at about 84%. Not everyone can attend their preferred school, so why does missing out matter? Taking the stock of “good schools” as fixed, this statistic has both equity and allocative efficiency concerns at its core. First, if particular segments of society are more likely to miss out on a place at a preferred school, this raises an equity concern about equal access to good schools. In England, there are large differences in attending a good school by ethnic group, which are unlikely to be explained by differing preferences (Weldon, 2018). A second issue is potential school-pupil match effects. If schools are heterogeneous and their productivity varies depending on intake, then matching the right pupils to the right schools improves school performance and child outcomes. Therefore, whether pupils attend their preferred school can have consequences for the performance of the sector overall.

Generally speaking, the process of allocating school places in England works as follows: parents submit a ranking of their preferred schools to the local government. They can list between 3 and 6 places depending on their local area. For oversubscribed schools, the allocation

of places is prioritised based on a set of criteria (“school priorities”). In 2000/01, these criteria were similar to today’s setting, including: whether the child has exceptional “medical or social needs”; whether the child has an older sibling at the school; and the distance from the school as the tie-breaker ². For religious schools, faith is often considered. Once the families’ preference rankings have been submitted, a procedure is required to allocate school places, given school capacities and priorities. There are a number of approaches for doing this. The two used in 2000 were: the *first-preference* (FPF), algorithm (also termed the “immediate-acceptance”, or Boston, mechanism); and the *equal-preference* algorithm, also termed “deferred-acceptance” (DA).

The FPF approach proceeds in rounds. First, each school starts by assigning places to pupils *who put that school as their first choice*, based on the ranking determined by the school’s admission criteria—until either there are no places left, or all students who put that school as first choice are assigned. The next round conducts the same process for pupils who put that school as their second-choice, and so on. This approach maximises the share of families getting their first-choice school. The drawback of this approach is that families who are not offered a place at their first preference school can also be rejected by their second preference school in favour of a child of parents who placed that school first, despite living further away. If families put their true preferred school as their first choice, but where they have a very low chance of acceptance, they may not only fail to secure a place at their preferred school, but also fail to be placed at any school that they deem acceptable. This means that families have an incentive to trade-off their true preferences against their probability of acceptance, in order to increase the chance of gaining a place at a satisfactory, but not most preferred, school. 41% of schools used a ‘first-preference’ approach to offer places for the September 2001 intake (West et al., 2004).

This practice of using preference order as a priority was prohibited in the School Admissions Code 2007: [schools must not] “*give priority to children according to the order of schools named as preferences by their parents, including ‘first preference first’ arrangements;*” (School

²In 2000, it was not required to give “looked-after” children first priority. From 2006, it was a statutory requirement that such children in care should be given top priority in the event of a school being oversubscribed, and in 2008, almost all schools (99%) had an admissions criterion relating to children in care compared with 2% in 2001 (West et al. (2011)).

Admissions Code 2007, 2.16 (b)); “*the ‘first preference first’ criterion made the system unnecessarily complex to parents*” and “*forces many parents to play an ‘admissions game’ with their children’s future.*” (Foreward, p.7.). In the nationally representative survey conducted in Flatley et al. (2001), about 25% of parents indicated that they took into account the nature of the oversubscription criteria when considering which schools to apply to. Parents who were familiar with the oversubscription criteria in their preferences tended to be more highly educated, owner occupier and of white ethnic origin.

In contrast, the DA approach does not use the preference ranking as a way for schools to rank applicants. In this method, parents submit their preference ranking over schools. The families’ application is considered at each school that they listed as a preference without reference to where the school was ranked by parents. Places are offered by ordering applicants by the oversubscription criteria, such that priority goes to families who meet these criteria to the greatest extent. This may lead to a parent whose child does not meet the criteria of his first preference school being offered a place at a nearby school for which she has nominated as a second preference. Since proximity is almost always the tie-breaker, one effect is to confer advantage to parents who live near to several popular schools.

The LSYPE cohort were applying to secondary schools in 2000, to start in September 2001. Admissions were similar in principle to today, but rather less regulated and subject to less stringent reporting standards. The legal guidance on admissions at this time was 2001 Schools Admissions Code (DfE, 2001), enacted on 01 April 1999, and in force for admissions for intakes in September 2000 onward. The Admissions Code is a document providing guidance on best practice in admissions. The 2001 and subsequent editions of the School Admission Code contain descriptions of the primary legislation from which they stem, extensive guidance, and reference to statutory responsibilities which must be met, but are not themselves a legal document. Schools and admission authorities were required to “have regard to” the indications in the Code. The 2001 Code encourage increased uptake of common admission processes: “*LEAs should consider, with other admission authorities, having coordinated admission arrangements - including standard application forms and common timetables - for all schools*” (DfE (2001),

para. 3.9). West et al. (2004) collected survey evidence to explore the use of various admission criteria at this time, and identified older siblings at the school (96%), distance from home to the school (86%), medical/social need (73%), catchment area (61%) and 'first preference' (41%) as the most common admission criteria practices in the presence of oversubscription. We do not have data on the exact admission arrangements used in each school or admission authority at this time, therefore it is not possible to precisely model the admissions process or to rely on this type of modeling for causal inference.

There are a range of sources of uncertainty about whether one will be admitted to a particular school, such that while parents can increase their chances of admission for their child, for instance by moving closer to a school, the probability of admission remains uncertain and difficult to predict in advance. Catchment area policies, while becoming less popular up to 2001, were used in both general admissions (14.9%) and featured somewhere as an oversubscription criteria in (63.1%) of LEAs (Williams et al., 2001). Catchment area policies create uncertainty because the *de facto* catchment areas can change over time in unpredictable ways. For example, in the survey by Williams et al. (2001), one LEA deployed a flexible catchment area system based on the numbers of children applying to particular secondary schools from particular primary schools over a period of time. Additionally, in some schools sibling and catchment criteria interact; for example, prioritising the children in a catchment area with older siblings in the school, followed by other children living in a catchment area, then siblings living outside a catchment area and, finally other children outside a catchment area. Given random fluctuations in the sibling distribution, this creates some random variations in the effects of the catchment area on the probability of admission.

The changing interaction of sibling, distance and the other criteria makes it difficult for parents to predict their chances of gaining a place at a particular school. Distance to school is itself an unpredictable criterion, because families do not know in advance whether or not they live close enough to a school in any given year, because the distance cut-off changes every year due to population fluctuations, changes in siblings at the schools, house-building programmes, changes in LEA boundaries, and school closures. Although some individual schools and Local

Authorities publish information over several years about the ‘cut-off point’ for distance, typically it is not clear how near ‘near enough’ will be the next year (Williams et al., 2001). This is least predictable factor under the FPF system because it depends, among other things, upon how many parents apply to a school in a given year, which fluctuates year-on-year and cannot be known in advance (Williams et al., 2001).

4 Data

Longitudinal Study of Young People in England

We employ data from the Longitudinal Study of Young People in England (LSYPE, or *Next Steps*). This is a nationally representative birth cohort study which tracks the lives of a cohort of around 15,000 young people in England who were born in school year 1989/90 (Department for Education, 2011; Calderwood, 2018; University College London et al., 2017, 2018). The study begins when the children are in Year 9, the third year of secondary school, and follows them until they are aged 25 years (annually until age 19/20, at which point there is a break until the age 25 wave). The LSYPE contains detailed information on school choice, including whether the child attends their preferred school, as well as family background, experiences in school, and crucially labour market and university outcomes.

The sampling frame comprised all pupils attending maintained schools, independent schools and pupil referral units in England on February 2004. Pupils from an ethnic minority or deprived background were oversampled to provide sufficient sample sizes for subgroup analyses. The first 7 waves of the study were funded by the Department for Education (DfE), commissioned to explore the factors shaping educational attainment and transitions out of compulsory schooling. The final wave, at age 25 years, was funded by the Economics and Social Research Council, and management was transferred to the Center for Longitudinal Studies, now with a broader remit to explore wider aspects of the transition to the labour market.

The LSYPE records have been confidentially linked to the National Pupil Database (NPD),

a database of administrative records on schools and pupils. The NPD contains individual test scores, individual characteristics, and school-level attributes (e.g., socio-economic and ethnic mix). From this data we can track the achievement of the LSYPE pupils in statutory examinations: primary school tests taken at age 10/11 (known as Key Stage 2 (KS2)), tests taken at age 13/14 in secondary school (Key Stage 3); the high stakes national examinations taken at age 15/16 in at least five subjects, including English and Mathematics, known as General Certificate of Secondary Education (GCSE). Finally, subject to satisfactory performance at GCSE, further national examinations known as A-levels can be taken at age 17/18 .

The LSYPE achieved cross-sectional responses rates ranging from 51% (in wave 8), to 92% (in Waves 3 and 4). For wave 1 to 7, the sample issued (response rate denominator) at each wave comprised respondents from the immediately preceding wave who agreed to be re-contacted (rather than all participants identified by the sampling frame in wave 1). The exception is the Wave 4 sample, which also included an ethnic minority boost of six hundred Black African and Black Caribbean young people, this sample was selected from schools who did not cooperate in the initial Wave 1 sampling frame. This boost had a response rate of 59%, adding an additional $n=352$ participants. Therefore, despite reasonable cross-sectional response rates, by Wave 8 the sample size was reduced to $n=7,707$ from an initial $n=15,774$ productive responses at Wave 1. The fact that many of the participants in wave 8 had not participated in each wave, due to the sequential sampling method, means that out of the 7,707 Wave 8 participants, many do not have a complete history from participation in all waves. Out of the sample of 16,122 participants at any Wave, 33.7% ($n=5,426$) of all respondents partook in all 8 Waves, 16.7% ($n=2,694$) had interrupted response—largely due to dropping out somewhere between Wave 1 and 7, and then being contacted for Wave 8; 49.6% ($n=8,002$) had a monotone pattern of response, that is, they partook in some number of consecutive waves then permanently dropped out. This type of non-response and attrition could pose a problem for our study if, conditional on observed covariates, survey drop-out is related to the true relationship between missing out and later outcomes, which is our parameter of interest. While it is not possible to completely rule out this possibility, because it relates to unobserved selection as well as data, we do investigate the issue in our analyses by performing analyses with the survey weights (main analyses) and

without (in robustness checks) the survey weights.

To classify Local Education Authorities (LEAs) by use of deferred-acceptance or first-preference mechanism, we use data collected by Coldron et al. (2008) and Pathak and Sönmez (2013). Coldron et al. (2008) surveyed the admissions policies of English Local Authorities for pupils entering the 2006 academic year, indicating whether an area (or in some cases specific schools) used a ‘first-preference’ approach (30% of schools used FPF in 2006, compared with 41% in 2001). To identify the few areas which had switched from FPF to DA before 2006, and hence while coded as DA in 2006, would have used FPF in 2001, we use the data in Pathak and Sönmez (2013), who compile a list of the year in which areas switched from FPF to DA. An important caveat is that, in 2000, when our cohort were applying to schools, there was variation both between and within LEAs in use of first-preference as a mechanism, and it is not possible to completely characterise an area by type of admissions policy. While for the majority of schools (i.e., government-controlled maintained schools), the Local Authority coordinated and implemented admissions, a minority of schools, especially church schools, were their own admissions authority and may not necessarily have coordinated to have the same mechanism as the rest of that Local Authority. Therefore, this area-based classification is inevitably fuzzy, but still gives an indication of areas where at least some places, if not all, were allocated by FPF.

Treatment variable

After being asked a number of questions about school choice and what information they used to form their school preferences, each parent is asked whether their child’s current school was their first-choice school. From this, we construct the variable *Missed out* which is equal to one if the current school is not the first-choice school, and zero otherwise. The proportion of families attending their first-choice school is 85%, very similar to administrative figures of 84%.

Outcomes

We examine a number of outcomes, both “short-run” academic outcomes, as well as longer run outcomes. In Appendix A we provide further detail on the nature of these qualifications and

the English school system. We first consider variables summarising attainment at secondary school: a binary variable indicating whether the pupil gained at least 5 GCSEs (or GNVQ equivalents) at grade C or above at age 16 years (“5+ A*-C”), the total points attained in Key Stage 4 at age 16 years (“KS4 points”) and a binary variable indicating whether the pupil stayed on after age 16 years to study for a Key Stage 5 qualification (“Stayed on”). We then consider whether pupils go on to attend university by age 20 (“Attends Uni”). In terms of longer run outcomes, we look at mental ill-health measured using the GHQ 12 point scale (“Mental ill-health”), where a higher score indicates poorer mental health. We also look at fertility: Does the young person have a child by age 25 years (“Fertility”)? Finally, we consider the log of income measured at age 25 years (“ln(income)”).

In terms of mechanisms, we explore a set of risky behaviours, measured at age 14 to 15 years, which could represent mechanisms connecting school effects and later outcomes. We look at a summary variable, which ranges from 0 to 12, counting how many “risky events” the child has engaged in over the last 12 months (playing truant; ever smoked; frequent smoking; ever alcohol; alcohol in last 12 months; frequent alcohol; ever tried cannabis; ever graffiti-ed; ever vandalised; ever shoplifted; ever been in a fight or public disturbance).

5 Empirical strategy

Which school a child attends is shaped by parental preferences and admission constraints. Our aim is to estimate the average outcomes associated with missing out on a place at a first-choice school. There is some conditionally random variation in whether families miss out a preferred school, because the distance cut-offs for admission to schools are unpredictable to parents in advance, and change year-on-year. The children of families who look observationally very similar could get in or miss out on a school due to living slightly different distances from a school, in a way which is unrelated to child outcomes. However, because we do not have complete data on parental preferences and admissions probabilities, we cannot employ a regression discontinuity, or similar design, to explicitly compare pupils on the margin of acceptance to a preferred school.

Rather we must impose more stringent assumptions for a causal interpretation, in particular a selection-on-observed variables assumption—that missing out on a preferred school is conditionally independent of the child’s potential outcomes. We condition on variables determining school admission probability, proxies for parental preferences, detailed information on socioeconomic status, prior ability of the child and the nature of the feasible choice set of schools, and we explore the results of robustness checks to probe the sensitivity of our estimates to this assumption.

To generate baseline estimates, we estimate the parameters of a parametric linear model using Ordinary Least Squares (OLS) with a specification as detailed in Equation 1. Y_{ijk} denotes the outcome under consideration, for pupil i , in school j in Local Authority k . c_o denotes a constant. D_{ijk} is the treatment indicator: equal to 1 for pupils are not attending their first-choice school, and 0 for pupils who are attending their first-choice school. X_{ijk} is a vector of covariates, LA_k are Local Authority fixed effects and ϵ_{ijk} is an idiosyncratic error term.

$$Y_{ijk} = c_o + \beta D_{ijk} + X'_{ijk}\gamma + LA_k + \epsilon_{ijk} \quad (1)$$

Details of covariates

We pair this model with estimates of statistics indicating the sensitivity of estimates to unobserved variables as developed in Oster (2019). We report the estimate of δ —how large the selection on unobserved variables (as a proportional to selection on observed variables) would need to be to drive the estimated treatment effect to zero³.

Second, we incorporate detailed geographical information in a two-stage procedure⁴ Nearby pupils who gained a place at their first choice school are informative about the counterfactual

³The assumptions underlying the calculation of δ can be varied. In particular, the researcher can vary the assumed value of $R-max$, the R-squared from a hypothetical regression of the outcome on the treatment and both observed and unobserved controls. The default option is to set this to 1, which may not always be appropriate. A rule of thumb proposed in Oster (2019) is to set $R-max$ equal to 1.3 times the R-squared from a regression of the outcome on the treatment and observed control variables (denoted \tilde{R}). The suggested cut-off to consider is that if the estimate of δ , calculated with $R-max = 1.3\tilde{R}$, exceeds 1, then this could be considered an ‘acceptable’ level of selection. This was a level consistent with what was observed in a sample of papers using RCTs in Oster (2019). Therefore, we report δ based on a $R-max$ of $1.3\tilde{R}$.

⁴Implemented using the Stata user-written software *kmatch* (Jann, 2017).

outcome of those who missed out on their preferred school, controlling for the socio-economic and other variables. We match observations based on geographical distance, and use regression adjustment on this matched sample, to estimate the average effect of missing out on a preferred school among those who missed out, the Average Treatment effect on the Treated (ATT).

Specifically, we match each pupil who missed out on a place at their preferred school (treated) to a set of pupils who gained a place at their preferred school (controls)⁵. Potential control pupils are identified based on the straight-line distance between the population-weighted centroid of the treated pupil’s home Lower Super Output Area (LSOA), and the post-code centroid of the school attended by a control pupil. The idea is that these control pupils attending a preferred school within a close proximity to the pupil who missed out are informative about the counterfactual outcome of the treated pupil—had they got into a potential preferred school.⁶ The bandwidth within which control pupils are considered candidate donors is selected “optimally” as 1.5 times the 90% quantile of the (non-zero) distances in pair matching with replacement, the default option in Jann (2017) (see Huber et al. (2015)).

Using this geographically matched sample, the second-stage uses regression-adjustment to estimate the average treatment effect on the treated (ATT) of missing out on a preferred school. The contributions of the control pupils are weighted in inverse proportion to their straight-line distance from their school to the treated pupil’s home, using an biweight kernel. The purpose of this two-stage approach (henceforth termed ‘distance-matching’) is to ensure the sample is balanced on geography, such that less reliance is put on capturing geography correctly in the functional form of the regression. Residential location is a choice which may also act as a sufficient statistic for unobserved variables—unmeasured wealth or preferences for example.

There are a range of factors shaping parental preferences over schools, and the probability

⁵Using those who miss out as the treatment group, rather than the other way around, ensures a larger pool control observations, as those who miss out are the minority group.

⁶Unfortunately, we do not have the co-ordinates of each school attended by LSYPE pupils in our data as this information has been anonymised during the course of this project, which places some restrictions on the type of analyses we can conduct. However, the Centre for Longitudinal Studies has kindly derived the relevant straight-line distances from cohort member home Lower Super Output Area population-weighted centroid to the other schools in the area attended by LSYPE cohort members in a secure environment for this project. A LSOA contains between 400 and 1200 households.

of admission to a school. The variables we include in X'_{ijk} can be loosely grouped into three (overlapping) categories: admissions-relevant variables; variables proxying for preferences over schools; and socio-economic variables.

To capture factors which shape admissions probabilities, we adjust for the following variables. First, the presence of resident older siblings in the household (interacted with region), and an indicator for whether the child has a certificate of Special Educational Needs (a small group of high need pupils who may qualify for admission under the “medical and social needs” category).⁷ The density and quality of schools available to families varies by area, which correlates with application strategy, the probability of getting into a preferred school and subsequent outcomes. Using data on the population of schools (from Edubase), we construct a variable characterising the local market for schools; a distance-weighted share of Good or Excellent rated schools by OFSTED (the Office for Standards in Education is the regulatory monitor of school quality based on school inspections), within a 20 kilometre radius of the pupil’s home LSOA centroid.

Socio-economic and demographic variables are included to capture indirectly aspects of both admission probability and preferences, as well as being associated with child outcomes. For example, family income shapes a family’s ability to move closer to their preferred school. In this category we control for (i) the child’s prior academic test score—their average points score from their KS2 tests completed at the end of primary school; average KS2 points score from child’s primary school; child’s month-of-birth; family income, housing tenure, parental education and occupational social class (NS-SEC).

We adjust for variables which proxy for the families’ preferences (the type of schools they prefer). In this category we have: religious denomination of parents; ethnicity of the child; and the information used to choose a school. Religion and ethnicity capture parental preference for attending religious schools, and a school with children of the same ethnicity. The LSYPE

⁷At the time when our cohort were applying for school, it wasn’t required by law to admit pupils with a SEN statement as it is now. It was also not required to admit pupils in state care, as this was introduced as explicit advice (but not a statutory requirement) in the Education Act 2002 and associated School Admissions Code 2003. However these children could be given priority by schools based on the commonly used “medical and/or social needs” priority.

asks about school preferences. One question asks what information the parents used in their school application decision, listing options such as “looked at league tables on the internet”, used “Local Authority brochure”, and so on. We use Principal Components Analysis to condense these responses into one summary variable (the first principal component), intended to capture the extent to which parents seek out information about schools.

Another important issue is the interaction between the state-school non-selective system with ‘outside options’: grammar schools and independent (“private”) schools. Families’ choices will depend on the availability of these alternative options, generating variation in the association between missing out and outcome by area. In the LSYPE, around 4% of pupils attend private schools (the weighted estimate is 7%), and 3% of the sample attend grammar schools (the weighted figure is about 3%). The main specifications control for whether the Local Authority has any Grammar schools. Regarding the private school sector, we control for the distance from home to the closest private school using external data on the full population of schools.

To summarise, the covariates included in the main specifications are: gender, month-of-birth, child ethnicity, whether the child has special educational needs, family income, parental occupational social class, parental education, parental religion, Principal Components Analysis summary of information used to choose schools, presence of local grammar school, distance to closest private school, share of good to excellent rated schools within 20km distance of the pupil’s home, and Local Authority fixed effects.

Channels

Attending a preferred school is a bundle of treatments: the preferred school may have smaller class sizes, better teachers, better peers, or some idiosyncratic pupil-level factors which mean a school is a good fit for a particular child. Missing out on first choice school could impact outcomes through two channels. First, the indirect effect of being exposed to different school attributes at a non-preferred school compared with a preferred school. Second, a direct “match effect”: abstracting from the particular school characteristics, some pupils may do worse than average at a given school, solely due to the fact that it is not their preferred school. Parents

choose schools based on headline measures of “school quality”, but also on factors specific to their child: parents are likely to have information about their child’s suitability for a given school, which is unobserved to the researcher.

To disentangle these two channels, Equation 2 adds current school fixed effects, α_j , such that β is now informed only by within-school variation (this specification omits Local Authority fixed effects indexed by k). Looking at the reduction in the size of the coefficient β , in Equations 1 and 2, indicates what share of the total effect is mediated via school attributes, as opposed to the remaining variation, which could be construed as a “match effect”.

$$Y_{ij} = c_o + \beta D_{ij} + X'_{ij}\gamma + \alpha_j + \epsilon_{ij} \quad (2)$$

6 Findings

Descriptive statistics

Table 1 shows mean differences in key outcomes by whether a pupil got in (control group) or missed out (treated group) on their preferred school. Two samples are shown, one is the “full sample” of all cohort members who responded at Wave 1 ($n=15,744$). The second is the estimation sample which comprises those who remain to Wave 8 ($n=7,707$) and have non-missing data on all the background covariates listed in Table 1 ($n=5,806$). Overall, the distributions of key covariates are fairly similar in the full and final sample. Attrition could be a concern if, conditional on observed covariates and survey-weights, survey drop-out is related to the relationship between missing out and later outcomes—our parameter of interest. Our analyses are weighted throughout using the survey weights, although we have also reproduced the results without the weights, which does not alter the main conclusions. In a regression of whether a cohort member remains in the final sample on initial characteristics⁸, the treatment variable is not a significant predictor of attrition. While it is not possible to completely rule out this possibility of non-ignorable attrition, because it relates to unobserved selection and data, these

⁸Results not shown.

investigations provide some reassurance that our findings are not driven by differential survey drop-out.

The key characteristics of the pupils which are over-represented among those gaining a place at their preferred school include having resident older siblings, being of White ethnic origin, being an owner occupier, and being of a Christian faith. There is little difference by prior attainment (Key Stage 2), and only small differences by parental education. This is consistent with most studies based on more recent administrative preferences data, which tend to find the strongest patterning in admissions by ethnicity, rather than by socio-economic status (Weldon, 2018; Burgess et al., 2019). In terms of the attributes of schools themselves, Table 2 shows the attributes of preferred and non-preferred schools. Preferred schools tend to have a higher share of White pupils, fewer FSM pupils, higher levels of achievement in key headline GCSE outcomes, higher OFSTED inspection outcomes, and higher value-added.

Table 3 shows the raw differences in pupil outcomes by whether they attend their preferred school. There are relatively small, insignificant, differences in key academic outcomes—for instance, pupils who miss out have fewer points at GCSE, but a similar probability of having 5 or more ‘good’ GCSE passes. In terms of longer run outcomes (mental ill-health, income, and fertility) there are large, and statistically significant, differences.

Estimation results

Turning to the regression findings, Table 4 reports two sets of estimates characterising the association between missing out on a preferred school on short run academic outcomes, and then longer run outcomes measured at age 25 years. The upper panel reports estimates from OLS, and the lower panel reports estimates from the distance-matching procedure. Because we do not have exogenous variation in attending a preferred school, we use the method that has been developed in Oster (2019) to explore the sensitivity of estimates to unobserved selection. This method generates estimates of the parameter δ , which measures the degree of selection on unobserved variables proportional to selection on observed variables. δ is reported alongside the OLS estimates. Following Oster (2019), we assess whether δ exceeds 1, with a higher

value indicating increased robustness to selection-on-unobservables (we set R_{max} at $1.3 * \tilde{R}$ as recommended). The lower panel report the Average Treatment Effect on the Treated (ATT) based on the distance-matching, along with the estimate of Y_o , the estimates counterfactual outcome mean, and the bandwidth used in the matching, measured in kilometres. Note that the distance-matched estimates are based on smaller, and variable, sample sizes. This is because, for each treated pupil used in the estimation, we match only to those controls within a restricted geographical bandwidth (in kilometres) which is chosen via a data-driven optimal bandwidth selection procedure. For instance, a bandwidth of 4km would mean that for the treated unit under consideration.

Considering the academic outcomes first, the results suggest only a small average effect of missing out on KS4 achievement. There is no significant effect on gaining 5 or more ‘good’ GCSE passes across either estimation method. While the effect on KS4 points is statistically significant across both specifications, the effect size is small, at a reduction in KS4 points from the counterfactual mean (denoted Y_o) of 5.8% in the distance-matched specification, and a 4.8% in the OLS specification. There is evidence, at least in the distance-matched results, that the probability of staying on in academic or vocational education post-16 years is reduced, with an effect size of a reduction of 5 percentage points, or 11.6%. The corresponding figure is of a similar size, but not statistically significant at the 95% level, in the OLS results. The estimates of δ all exceed 1, aside from the estimates for attending university and income at age 25 years, lending less confidence to these estimates.

Turning to the analogous figures for the longer run outcomes (mental ill-health, income, and fertility), pupils who miss out on attending a preferred school have significantly poorer mental health at age 25 years (by about one quarter of a standard deviation in the distance-matched results), and reduced income by 2% (on a base of about 25,000 GBP per year) although this latter estimate has a small δ suggesting that this is likely to be sensitive to selection on unobservables. In addition to the low value of δ , the estimate of the effect on income is not significant across both the regression-adjustment and distance-matched sample, which together suggests this figure is not robust—to either the estimation or identification assumptions. The

probability of having a child by age 25 years (“Fertility”) is increased by 5 percentage points in the OLS specification, and 6 percentage points in the distance-matched specification, which are both statistically significant.

Heterogeneous effects

Previous research has identified ethnic origin as a factor that contributes to a families’ probability of attending a preferred school (Weldon, 2018). The reasons for this remain unclear, but one possible explanation is differences by ethnicity in engagement with the school choice process. White families are much more likely to list only one school, which is often their local school, and they are more likely to be admitted through the distance or older sibling criteria. Ethnic minority families are more likely to list many schools, and select, on average, more ambitious (further away and better quality) schools as their first choice.

Table 5 splits the sample by ethnic origin of the child, and reports a p -value from a test of no difference between groups—again presenting OLS and distance-matched results. The distance-matched results are less precise than OLS, as they are based on smaller sample sizes, especially within subgroup. The only outcome for which the difference in effect size between subgroups is statistically significant is mental ill-health, where the effects appear substantially larger among White pupils across both OLS and distance-matching. The effects on staying on in school after age 16 years, the minimum age of compulsory schooling at this time, are also consistently larger across OLS and the distance-matching results among White pupils, however the difference between subgroups is not statistically significant. Given the imprecision of the estimates it difficult to be confident in differences by ethnicity, but it does appear that, especially in the OLS figures, some effects are more pronounced among White pupils, and there is statistically significant difference for mental health. This may be due to White parents not listing enough schools, such that when they do miss out they may be allocated a school which they would not have chosen at all, and perhaps be separated from their primary school friend group. While if BAME parents list more schools, if they miss out then the child has a higher chance of ending up in a fallback local school that they at least feel comfortable in.

Table 6 now splits the sample by whether the Local Authority was, at that time, using a deferred-acceptance (DA) or first-preference (FPF) mechanism to allocate places. There is a theoretical suggestion that missing out under FPF may be worse than under DA since in the latter missing out on one's first preference may increase the chance of missing out on your second too. Here we can see stronger evidence for group differences, with statistically significantly larger effect sizes in the FPF areas, including now for short-run academic outcomes. For example, considering the outcomes which have statistically significant differences across groups, the OLS effects of missing out in the FPF subsample show that missing out is associated with a 10 percentage point reduction in the probability of gaining 5 or more 'good' GCSE passes, compared with a figure of 0.00 for the DA group; a 13 percentage point reduction in the probability of staying on in education, compared with a 1 percentage point reduction in the DA group; and a 5% reduction in income at 25 years, compared with a figure of 0.00% in the DA group.

These differences could, however, be confounded by other characteristics that differ between areas. Table 7 reports the means of various attributes (collected variously at the school-level and pupil-level) by treatment status and FPF vs DA area-type. FPF areas tend to be very similar to DA areas in terms of school value-added, headline measures of attainment (measured for earlier cohorts, preceding our cohort, to avoid simultaneity) as well as similar KS2 attainment of the pupils in our cohort. Schools in FPF areas have a lower proportion of FSM pupils in schools, a higher proportion of White pupils and higher OFSTED inspection outcomes on average. Our estimates of the consequences of missing out are worse in FPF areas, and this finding is unlikely to be explained by these differential patterns. In terms of mechanisms, the difference in the distance-to-school between those who get in and miss out is larger in FPF areas. Those who miss out in an FPF area travel 0.18km further than those who get in, compared with a difference of 0.05km in DA area. This suggests that pupils who miss out are being allocated a school further from their home, perhaps further than they would prefer to travel and further from local friends who they might feel more comfortable with. This may be an important factor behind our results, especially if it indicates a poorer match on other, unobserved, dimensions.

Channels

Table 8 adds school fixed effects to the previous specifications. Our intention here is to assess the extent to which the findings are explained by between or within-school variation. That is, missing out on a preferred school could impact outcomes via an indirect effect of attending a poorer quality school (a between-school effect). Alternatively, part of the missing out effects could arise solely from ‘attending a school that they do not want to be attending’, abstracting from between-school quality differences. When we include school FEs we isolate the latter effect, so our FE estimates only provide within-school estimates. The school that the child gets into could reasonably be conceived to be a ‘bad control’, which is one reason why it is not included in the main specification.

Th estimates including fixed effects in Table 8 follow a similar pattern of magnitude and significance to those in the initial specification. This suggests that our results are not entirely explained by between-school differences, and suggest that the within-school effect is relevant. Two outcomes of interest, where the difference between the two specifications are largest, are KS4 points and mental ill-health. For KS4 points, the estimate from the specification including fixed effects is smaller by 4 points (-13.66 when including fixed effects, compared with -17.60 when excluding fixed effects). However, given the mean of KS4 points is 354 points, this represents a negligible difference. Secondly, for mental ill-health, the effect size when included fixed effects is substantively larger (an increase from 0.50 to 0.66, compared with a mean value of 2.31). One interpretation of this is to suggest the effect of ‘being at a school you don’t prefer’ is especially detrimental to mental health, perhaps more so than between-school differences in school quality and other attributes. These findings may suggest that parents and children know something unobserved to the researcher about their idiosyncratic suitability, or “match quality”, for a given school. Thus attending a non-preferred school can generate detrimental outcomes, abstracting from considerations of between-school variation in school attributes and typical quality metrics.

Finally, we explore the role of early engagement in risky behaviours, as an intermediate mechanism, measured when the cohort members are aged 13 to 15 years. We look at effects on an additive variable summarising the number of ‘risky behaviours’ the child has engaged in

over the last 12 months (playing truant; ever smoked; frequent smoking; ever alcohol; alcohol in last 12 months; frequent alcohol; ever tried cannabis; ever graffiti-ed; ever vandalised; ever shoplifted; ever been in a fight or public disturbance). Table 9 shows the effects of missing out on this intermediate outcome. While there is little evidence of effects of missing out in the full sample, effects on risky behaviours are again more pronounced in FPF areas and among White pupils. The effect remains significant after the additional of school fixed effects, again which may be consistent with the idea of a poor school match.

7 Conclusions

This paper examines the association between missing out on a place at a first-choice (preferred) secondary school in England on short- and long-run outcomes. Employing nationally representative cohort data, confidentially linked to administrative records on education outcomes, we compare a range of outcomes between pupils who get into their preferred school and those who miss out. The data follows pupils from the academic year 2003/04, in their third year of secondary school, until they are aged 25 years.

We contribute to a growing literature tracing the consequences of attending schools which are preferred by parents, and we provide new evidence on this topic for secondary schools in England. Our empirical strategy leverages features of the institutional setting and the literature on school choice to make a case for a selection-on-observed variables assumption, which we pair with sensitivity tests to selection-on-unobservables. We use regression-adjustment and geographical matching to estimate average treatment effects of missing out on a place at a preferred school. Our findings show at best small negative effects of missing out on a preferred school on short-run test scores taken at age 15/16 years. We do find some evidence for higher rates of leaving school at age 16 years, the age of mandated compulsory schooling during our sample. In terms of longer run outcomes, we find a detrimental effect on mental health, and evidence of increased fertility at age 25 years. The results show small negative effects on income at age 25 years, but this finding was not robust to unobserved selection.

During our time period of interest, two mechanisms for allocating school places were used, given preferences, school priorities and capacities. ‘First-preference-first’ rewarded strategic behaviour by parents in ranking school, giving rise to a concern that this system advantages families who could successfully play this admissions “game”. Conversely, under a ‘deferred acceptance’ system, parents can (at least, in theory) do no better than reveal their true preferences over schools, and playing strategically yields no advantage. Geographical variation in the allocation system allows us to estimate treatment effects separately for pupils exposed to these two systems.

The detrimental effects on mental health were more pronounced in areas which used the more manipulable mechanism (FPF) to assign school places. In these areas we also find negative effects on short-run test scores, staying on in education or training after the age of compulsory schooling, in addition to reduced income in adulthood. In terms of potential mechanisms driving these effects, we find evidence for longer distances to school, and early engagement in risky behaviours, as channels. These findings suggest that legislation outlawing FPF, enacted in 2007, may have led to improved outcomes across multiple dimensions for young people in England.

Overall, our findings have a number of suggestive policy implications. The results show that which school a child attends is important for more than just test scores: missing out on a preferred school is associated with poorer life outcomes in a broader sense, including mental health and early engagement in risky behaviours. These broader measures could fruitfully be considered alongside traditional measures of school quality. Second, the nature of the assignment mechanism, and how parents understand and engage with it, is important for ensuring equal access to schools. The school choice process, and oversubscription criteria which act as the gatekeeper to popular schools, are areas ripe for innovation to ensure fair access to schools.

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Table 1: Summary statistics for pupils

	Got in n=15,774 <i>Full sample</i>	Missed out	Got in n=5,806 <i>Final sample</i>	Missed out
Female	0.49	0.50	0.49	0.54
Any resident older siblings?	0.86	0.81	0.83	0.79
<i>Child's ethnic group</i>				
White	0.88	0.76	0.88	0.73
Indian, Pakistani, Bangladeshi	0.05	0.08	0.05	0.10
Black African, Black Caribbean	0.03	0.08	0.02	0.09
Other, Mixed	0.04	0.08	0.04	0.08
<i>MP highest qual</i>				
Higher ed.	0.24	0.20	0.21	0.20
A-levels, A-C GCSE	0.14	0.13	0.14	0.12
Lower GCSE	0.41	0.39	0.42	0.44
Other qualification	0.02	0.02	0.03	0.04
No qualification	0.20	0.26	0.20	0.21
<i>Housing tenure</i>				
Owner occupier	0.71	0.60	0.70	0.63
Renting from council/LA	0.23	0.31	0.25	0.34
Private renter/Other	0.07	0.09	0.05	0.04
<i>MP religion</i>				
None	0.23	0.25	0.24	0.24
Christian	0.68	0.61	0.67	0.60
Buddhist, Hindu, Jewish	0.04	0.06	0.03	0.07
Muslim	0.05	0.08	0.05	0.09
KS2 average points	27.04	26.56	26.94	26.72
Distance to school (km)	3.17	3.33	2.93	2.94

Notes: This table reports survey-weighted proportions (or means, for continuous variables) of each covariate, by treatment status, for the full sample of respondents to Wave 1, and then final sample of those who respond to Wave 8 and have non-missing data on all relevant covariates.

Table 2: Summary statistics for schools

	Preferred	Non-preferred	Difference	<i>p</i> -value
KS2-KS4 value added measure (2004)	986.77	977.43	9.34	0.00
% 5+ A*-C (2001)	49.80	38.82	10.97	0.00
% of pupils FSM (2004)	14.27	21.47	-7.20	0.00
% English first language (2004)	91.52	84.50	7.02	0.00
% White pupils	84.29	75.19	9.10	0.00
OFSTED Good or above	0.71	0.58	0.13	0.00

Notes: This table reports the survey-weighted means of various school-level attributes of the schools attended by pupils attending their first-choice school (Preferred) and schools of pupils reporting not to be at their first-choice school. The difference in means and *p*-values from a test of equality of means are also reported. *p*-values are computed from robust standard errors clustered by school (the Primary Sampling Unit)

Table 3: Summary statistics for pupils (outcomes)

	Got in	Missed out	Difference	<i>p</i> -value
Risky behaviours	1.43	1.67	-0.23	0.02
5+ A*-C	0.56	0.53	0.03	0.31
Key Stage 4 points	356.32	339.76	16.56	0.09
Stayed on	0.43	0.39	0.04	0.12
UCAS pts	212.94	203.79	9.15	0.25
At Uni (Age 20)	0.36	0.37	-0.01	0.83
Ln(income)	9.64	9.58	0.06	0.00
Mental ill-health (GHQ12)	2.34	2.92	-0.58	0.00
Fertility	0.26	0.32	-0.06	0.02

Notes: This table reports the survey-weighted means of outcomes reported by pupils attending their first-choice school (Preferred) schools of pupils reporting not to be at their first-choice school. The difference in means and *p*-values from a test of equality of means are also reported. *p*-values are computed from robust standard errors clustered by school (the Primary Sampling Unit)

Table 4: OLS and distance-matched effects of missing out on a first-choice school

	5 A*-C	KS4 points	Stayed on	Attended uni	Mental ill-health	ln(income)	Fertility
<i>OLS</i>							
β	-0.03	-17.60***	-0.04*	-0.03	0.50***	-0.01*	0.05**
(s.e.)	(0.02)	(6.23)	(0.02)	(0.02)	(0.19)	(0.01)	(0.02)
δ	18.58	126.4	44.33	-2.663	10.67	0.935	14.55
N	5,213	5,213	5,799	4,338	5,567	5,799	5,799
<i>Distance-matching</i>							
ATT	-0.03	-22.06***	-0.05**	-0.02	0.60***	-0.02**	0.06**
(s.e.)	(0.02)	(7.02)	(0.02)	(0.03)	(0.22)	(0.01)	(0.03)
Y_0	0.55	361.81	0.43	0.36	2.31	9.60	0.28
Bandwidth	6.9	6.9	6.8	6.9	6.7	6.8	6.8
N	3422	3422	4228	2370	3834	4228	4228

Notes: β : OLS coefficient on treatment variable; ATT: average treatment effect on the treated; Y_0 : adjusted mean for control group; N : observations; Bandwidth (km) is the radius in kilometres from which control units are selected; δ : size of proportional selection on unobservables to drive ATT to zero; estimates in brackets are robust standard errors clustered by school. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. All analyses are weighted using the survey weights.

Table 5: Estimation results by ethnicity subgroup

	5 A*-C	KS4 points	Stayed on	Attended uni	Mental ill-health	ln(income)	Fertility
<i>OLS</i>							
BAME							
β	0.01	-7.29	0.00	0.02	-0.15	-0.01	0.02
(s.e.)	(0.03)	(11.65)	(0.04)	(0.04)	(0.24)	(0.01)	(0.03)
δ	1.12	-3.36	0.51	23.17	-4.54	-3.14	2.30
<i>N</i>	1,445	1,445	1,663	1,199	1,547	1,663	1,663
White							
β	-0.03	-15.71**	-0.04*	-0.02	0.71***	-0.01	0.06**
(s.e.)	(0.02)	(7.14)	(0.02)	(0.02)	(0.23)	(0.01)	(0.03)
δ	2.737	5.193	4.275	3.808	23.26	3.714	6.198
<i>N</i>	3,768	3,768	4,136	3,139	4,020	4,136	4,136
<i>p</i> -value	0.28	0.52	0.19	0.24	0.02	0.99	0.39
<i>Distance-matching</i>							
BAME							
ATT	-0.09*	-35.52**	-0.01	-0.08	-0.32	-0.03*	0.05
(s.e.)	(0.05)	(14.17)	(0.05)	(0.06)	(0.33)	(0.01)	(0.04)
Bandwidth (km)	8.34	8.34	7.85	8.34	7.78	7.85	7.85
<i>N</i>	982	982	1,278	715	1,079	1,278	1,278
White							
ATT	-0.02	-19.70**	-0.07**	0.01	0.99***	-0.02	0.06
(s.e.)	(0.03)	(8.38)	(0.03)	(0.03)	(0.27)	(0.01)	(0.04)
Bandwidth (km)	3.58	3.58	3.63	3.58	3.58	3.63	3.63
<i>N</i>	1,671	1,671	1,980	1,126	1,870	1,980	1,980
<i>p</i> -value	0.27	0.34	0.33	0.24	0.00	0.58	0.85

Notes: β : OLS coefficient on treatment variable; ATT: average treatment effect on the treated; *N*: observations; δ : size of proportional selection on unobservables to drive ATT to zero; estimates in brackets are robust standard errors clustered by school. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. All analyses are weighted using the survey weights. Bandwidth (km) is the radius in kilometres from which control units are selected. *p*-val is the *p*-value from a test of equality of coefficients between groups. BAME; child's reported ethnicity is Black, Asian and Minority Ethnicity. White; child's reported ethnicity is White British.

Table 6: Estimation results by area-type subgroup

	5 A*-C	KS4 points	Stayed on	Attended uni	Mental ill-health	ln(income)	Fertility
<i>OLS</i>							
DA							
β	-0.00	-15.21**	-0.01	-0.01	0.35*	-0.00	0.06**
(s.e.)	(0.02)	(7.05)	(0.02)	(0.03)	(0.20)	(0.01)	(0.03)
δ	0.50	36.98	-105.60	-0.94	8.39	0.22	67.96
N	3,851	3,851	4,310	3,202	4,136	4,310	4,310
FPF							
β	-0.10**	-20.87*	-0.13***	-0.06	1.02**	-0.05***	0.05
(s.e.)	(0.04)	(11.34)	(0.04)	(0.04)	(0.43)	(0.02)	(0.05)
δ	89.19	23.45	21.82	4.956	31.32	4.550	4.513
N	1,362	1,362	1,489	1,136	1,431	1,489	1,489
p-value	0.03	0.51	0.00	0.23	0.18	0.02	0.81
<i>Distance-matching</i>							
DA							
ATT	-0.02	-17.89**	-0.02	-0.02	0.49**	-0.01	0.06*
(s.e.)	(0.03)	(8.32)	(0.03)	(0.03)	(0.25)	(0.01)	(0.03)
Bandwidth (km)	6.89	6.89	8.77	8.77	6.72	8.77	8.77
N	2,629	2,629	3,300	1,824	3,006	3,300	3,300
FPF							
ATT	-0.11**	-25.02**	-0.15***	-0.06	1.12*	-0.06***	0.06
(s.e.)	(0.06)	(12.57)	(0.05)	(0.9)	(0.60)	(0.02)	(0.07)
Bandwidth (km)	6.55	6.550	6.53	6.55	6.53	6.53	6.53
N	676	676	821	473	698	821	821
p-value	0.12	0.64	0.01	0.68	0.32	0.02	0.97

Notes: β : OLS coefficient on treatment variable; ATT: average treatment effect on the treated; N: observations; δ : size of proportional selection on unobservables to drive ATT to zero; estimates in brackets are robust standard errors clustered by school.*** p<0.01, ** p<0.05, * p<0.1. All analyses are weighted using the survey weights. Bandwidth (km) is the radius in kilometres from which control units are selected. p-val: p-val from a test of no significant difference in the ATT between subgroups.

Table 7: Means of selected variables by area-type and treatment status

	DA			FPF		
	Got in	Missed out	Diff.	Got in	Missed out	Diff.
KS2-KS4 value added in 2004	986.98	977.30	9.68	986.25	977.92	8.33
% 5+ A*-C (GCSE/GNVQ) in 2001	49.39	38.07	38.07	50.76	41.50	9.20
KS2 average points	26.95	26.69	0.26	26.92	26.80	0.12
% FSM in 2004	14.77	22.61	-7.84	13.05	17.41	-4.36
% White	82.01	72.42	9.59	89.87	85.06	4.81
OFSTED good and above	0.71	0.57	0.14	0.71	0.64	0.07
Distance to school (km)	2.97	2.92	0.05	2.81	2.99	-0.18

Notes: Means are weighted using the survey weights

Table 8: OLS effects of missing out with school fixed effects

	5 A*-C	KS4 points	Stayed on	Attended uni	Mental ill-health	ln(income)	Fertility
<i>OLS excluding school fixed effects</i>							
β_{NO-FE}	-0.03	-17.60***	-0.04*	-0.03	0.50***	-0.01*	0.05**
(s.e.)	(0.02)	(6.23)	(0.02)	(0.02)	(0.19)	(0.01)	(0.02)
δ	18.58	126.40	44.33	-2.663	10.67	0.935	14.55
N	5,213	5,213	5,799	4,338	5,567	5,799	5,799
<i>OLS including school fixed effects</i>							
β_{FE}	-0.02	-13.66**	-0.05**	0.00	0.66***	-0.02*	0.01
(s.e.)	(0.02)	(5.84)	(0.02)	(0.03)	(0.20)	(0.01)	(0.03)
δ	2.40	5.16	-226.9	1.63	13.38	0.78	0.71
N	5,213	5,213	5,799	4,338	5,567	5,799	5,799

Notes: β : OLS coefficient on treatment variable; N: unweighted cell count for all statistics in this table; Delta: size of proportional selection on unobservables to drive β to zero. Estimates in brackets are robust standard errors clustered by school. *** p<0.01, ** p<0.05, * p<0.1. All analyses are weighted using the survey weights.

Table 9: Effect of missing out on risky behaviours (additive derived variable) using OLS

	Full sample	DA	FPF	BAME	White	School FE
	<i>OLS</i>					
β	0.27***	0.16	0.67***	-0.03	0.40***	0.41***
(s.e.)	(0.10)	(0.11)	(0.23)	(0.12)	(0.13)	(0.15)
δ	-41.26	-12.10	21.84	2.781	15.65	5.321
N	5,723	4,248	1,475	1,626	4,097	4,097
	<i>Distance-matching</i>					
ATT	0.34***	0.27*	0.84***	0.46***	0.10	n.a.
(s.e.)	(0.12)	(0.14)	(0.28)	(0.16)	(0.18)	n.a.
Bandwidth (km)	6.83	6.32	8.77	7.85	3.66	n.a.
N	4,095	3,194	789	1,957	1,215	n.a.

Notes: β : OLS coefficient on treatment variable; N : unweighted cell count for all statistics in this table. δ size of proportional selection on unobservables to drive β to zero. Estimates in brackets are robust standard errors clustered by school. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. n.a.: not estimable. All analyses are weighted using the survey weights.

A English education system

Ages and stages:

The English school curriculum is divided into blocks of years called “Key Stages” (KS). At the end of each Key Stage, performance is examined via examination. After an initial Reception year at primary school, children enter into Key Stage 1 from age 5 (years 1–2 of primary school) and then transition into Key Stage 2 (years 3–6 of primary school) until age 11. Children are assessed based on national standards at the end of each Key Stage. At age 11, children transition to secondary school, where they must attend until age 16. Key Stage 3 runs from Year 7 to Year 9. During Key Stage 4 (years 10 and 11) children study a set of subjects in which they will sit the high-stakes General Certificate of Secondary Education examinations (GCSEs) at age 16. Pupils typically take between 5 and 10 subjects, and usually a passing grade in at least five of them—including Mathematics and English—is required to progress to many academic or other qualifications. A common school performance metric is the proportion of pupils achieving at least five GCSEs at grades A*-C (including English and Mathematics).

With satisfactory performance in GCSEs, pupils can study for A-levels, from age 16–18. A-Levels are the most common route to university entrance, and are taken either at the same secondary school, or some pupils move to another school or to a specialised “sixth form college” to take their A levels. Further Education (FE) colleges are an alternative to staying in an secondary school, and they provide vocational training or a mix of academic and vocational. Pupils typically enter Higher Education (University) from age 18.

School types:

The types of secondary schools in England in 2001 are described as follows. The first category are “state schools” which are controlled and funded by central government. All children in England between the ages of 5 and 16 are entitled to a free place at a state school. These schools follow the National Curriculum and are inspected by Ofsted (the government’s Office for Standards in Education, Children’s Services and Skills). About 93% of pupils attended such

schools when our data was collected (97% unweighted). The second category are “Independent Schools” (private schools) which about 7% of pupils attended (3% unweighted).

Within the category of state schools, there are community schools (67% of pupils in our data), voluntary-controlled (3%), voluntary-aided (11%), foundation schools (15%). *Community schools* are entirely run by the local council (Local Authority). *Foundation or Trust schools* are run by a local governing body. These schools were formerly called “Grant-maintained” schools. This was an initiative to allow more flexibility in provision of education where, by majority parental vote, schools could opt out of Local Authority control and be run by a governing body with more control over admissions and staffing. *Voluntary-aided schools* are typically religious or faith schools, which can admit pupils on religious affiliation grounds. *Voluntary-controlled schools* are almost all faith schools. They are a mix between community and voluntary-aided schools: similar to a community school, the local authority employs the staff and sets the entrance criteria, but the school land and buildings are owned by a charity, often a church, which also can appoint some members of the governing body.

Regardless of governance arrangement, all non-selective state schools have to comply with the school admissions code of practice which sets guidelines for fair admissions. About 3% of these state schools, largely community and foundation schools, are *Grammar schools*, a minority of academically selective state-funded schools. They select all or most of their pupils based on academic ability, assessed by the so-called “11 plus” exam. The 11-plus exam is distinct from the national Key Stage assessments that take place at the age of 11. The former are not nationally co-ordinated. Sometimes they are set and graded by individual grammar schools, and sometimes (usually in local authorities with several grammar schools) they are co-ordinated by the local authority.

How Grammar schools were accounted for in the admissions process in 2001 varied. The first possibility was that parents would submit completely separate preference lists to selective and non-selective schools. When the child’s 11-plus results became known, the appropriate list is consulted. The second approach was that parents would express their preferences for both selective and non-selective schools on the same form. When the 11-plus results are known,

the highest preference non-selective school becomes their effective first preference if they do not pass the 11-plus. The third approach was that parents express their preferences for both selective and non-selective schools on the same form. When the 11 plus results are known, the list is interpreted literally: if a selective school is first preference and a non-selective is second preference, the non-selective school is still counted as a “second preference”. This is a disadvantage when parents are unsure a priori of their children’s eligibility for the selective school (Flatley et al., 2001).

Our analysis is based on pupils attending secondary schools who started secondary school in September 2001. Our findings are robust to excluding pupils attending private and grammar schools (actual, rather than preferred, school type could be considered endogenous, providing an argument against selecting on school type in the main analyses).

Variable definitions:

- “5+ A*-C”: This is a binary variable indicating whether the pupil gained at least 5 GCSEs (or the vocational GNVQ equivalents) at grade C or above. GCSE is the examination taken at the end of Key Stage 4, when pupils are aged 15 or 16 years. This threshold was considered a “good” passing grade required progression to many routes of further study.
- “KS4 points”: The grade received for each GCSE subject is assigned points, and we use the variable “GCSE points” which is the sum of points over GCSE subjects (and other KS4 equivalents such as GNVQ).
- “Stayed on”: This is binary variable indicating whether a pupil stayed on after the age of compulsory schooling (16 years) to enter for any qualification at Key Stage 5. The options at Key Stage 5 include A-levels, but also vocational qualifications (such as BTECS, qualifications provided by the Business and Technology and Education Council). BTECs can contribute toward university entrance.
- “Attends Uni”: This is a binary variable indicating whether a cohort member reports attending university by age 19/20 years.

- “Mental ill-health”: This variable is the General Health Questionnaire 12 (GHQ12), a screening tool for identifying minor psychiatric disorders in the general population, and suitable for use in adolescents and upward. For each of 12 questions, participants are asked whether symptoms of mental ill-health are ‘not at all present’, present ‘no more than usual’, present ‘rather more than usual’, or present ‘much more than usual’. The responses are coded following a conventional approach, where the first two responses receive a score of zero, the latter two receive a score of one. These scores are summed to give a continuous variable ranging from 0 to 12 where a higher value indicate poorer mental health.
- “Fertility”: This is a binary variable indicating whether a cohort member has borne any children by age 25 years (the final wave of the study).
- “ln(income)”: gross weekly income measured at age 25 years.

We look at some key risky behaviours which could represent mechanisms connecting school effects and later outcomes: whether the child has played truant in the last year, tried cannabis in the last year; and a summary variable counting how many “risky events” the child has engaged in over the last 12 months (playing truant; ever smoked; frequent smoking; ever alcohol; alcohol in last 12 months; frequent alcohol; ever tried cannabis; ever graffiti-ed; ever vandalised; ever shoplifted; ever been in a fight or public disturbance).