The Importance of School Quality Ratings for School Choices: Evidence from a Nationwide System

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For school choice to engender beneficial competition between schools, parents must value education standards. This paper provides clean identification of parents' response to information about primary school quality in England, finding that school choices respond significantly. In turn, this provides direct causal evidence that parents value schools' education standards, and therefore that schools face pressure from the demand-side to improve. The increase in parents' school choices from the revelation of positive information is non-trivial for schools. For example, translated into funding, the increase in first choices would be equivalent to around £7,500 per school cohort, on average. The effect size varies across school and market characteristics, with stronger effects for schools initially at the bottom of their local hierarchy. In contrast to previous literature, we find that households across the income distribution respond similarly to information, and so schools serving all populations have incentives to improve education standards. A limited supply of school places constrains this demand-side pressure, however: although parents' choices are responsive, the eventual assignment of pupils to schools is less so. Limited school capacity therefore dampens primary schools' incentives.

Keywords

School choice, accountability, information, parents' preferences

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1 Introduction

Parents' decisions of where to send their child to school may have lifelong implications for their child's educational attainment, health and well-being, and can therefore be a difficult and fraught process.[1] For some parents, this choice is bound up with residential decisions, as school admission is determined by location. Increasingly, worldwide, some have a choice of schools, given their location. But how do parents choose the right school for their child? Education is an experience good, and school quality is uncertain ex ante. The provision and nature of information provided to parents prior to their school choice is therefore a crucial determinant of pupils' school assignment and educational outcomes. *If* parents can observe school quality and *if* parents value school quality, then a system of competition between schools and school choice (allowing parents' preferences for schools to be one input into their allocation to school) can drive up standards of education.

This paper answers the question of whether, and to what extent, parents value education standards in schools. We identify the causal effect of school quality information on school choices with the timing of school inspections throughout the school year. We exploit a natural experiment where some households receive updated information before the national deadline for school choices, while some households receive information only after the deadline. This natural experiment therefore isolates the short-run effect of school quality information on school choices, as the short window we study (within the school year) precludes endogenous changes in location.[2] We show that the treatment and control schools are balanced, and therefore that the timing of inspection is as good as random within the school year.[3] Additionally, we use an event study design to estimate the longer-run response to school quality information. This design compares schools with a change in inspection rating to those where the rating is constant. We show that school quality ratings contain new information for parents, as these 'treatment' and 'control' schools have largely common trends before the inspection.

We find that school choices respond to school quality information. Comparing schools with the same change in rating, but revealed before and after the school choice deadline, the total

[1] We refer to 'parent' choices throughout the paper as a shorthand for carer and/or household choices. We study choice of primary school, where parents/carers are likely to have the deciding say, rather than the child. See Ajayi, Friedman, and Lucas, 2017 for an experiment testing the targeting of school choice information on guardians' involvement in the process for senior high school choices in Ghana.

[2] Fiva and Kirkebøen, 2011 find that house prices (rather than school choices) respond to newly released information on school quality in the short-run in Oslo, Norway, where school admissions are tied to location. This is unlikely to hold in England, however, where property transactions typically take many months. In future work, we will test this hypothesis.

[3] This approach is similar in essence to that used by Greaves et al., 2021 to study parents' investments in response to school quality information. Greaves et al., 2021 study responses on the intensive rather than extensive margin, and use the additional variation induced by the timing of the parent survey in their setting.

number of choices made to the school increases immediately by 5% in response to a unit increase in their rating. The total number of first choices made to the school increases similarly, by 4%. For individual parents, the probability that they choose their nearest school as first choice increases by 2%. The medium-term response to a unit increase in Ofsted rating is larger, growing in the first and second years following the inspection. At the school-level, total and first choices increase by around 10% in the first year and 12% in the second year relative to the year before the inspection. At the pupil-level, the probability that the nearest school is chosen as first choice increases by 6% in the first year and 7% in the second year. Our findings are robust to multiple empirical specification checks.

Our first contribution is to isolate the causal effect of school quality information on school choices (net of residential choices), indicating parents' preferences. This is important, as it reveals parents' preferences across the income distribution. In contrast, evidence from the residential choice channel reveals preferences for school quality only for parents with sufficient income.[4] This is related to the body of literature that estimates parents' preferences for schools from parents' submitted school choices (Hastings, Kane, and Staiger, 2009; Borghans, Golsteyn, and Zölitz, 2015; Burgess et al., 2015; Denice and Gross, 2016; Glazerman and Dotter, 2017; Akyol and Krishna, 2017; Beuermann et al., 2018; Fack, Grenet, and He, 2019; Harris and Larsen, 2019; Ruijs and Oosterbeek, 2019; Abdulkadiroğlu et al., 2020; Bertoni, Gibbons, and Silva, 2020; Walker and Weldon, 2020). This literature faces the identification challenge that estimated preference parameters are biased if parents make residential choices to be close to their preferred schools. Most obviously, if parents move closest to their preferred school to gain admission, then the estimated dis-utility of distance will be biased upwards. In fact, the non-linear estimation of these models means that all parameters will be biased by such endogeneity. We validate the findings of this literature by showing that parents respond to school quality without the identification challenge of endogenous residential location.

Our national inspection and school choice environment, combined with national administrative data, enable us to study heterogeneity across areas and household types. We find that households across the income distribution (proxied by eligibility for free school meals) respond similarly to school quality information. This is an important finding in relation to previous literature (that infers preferences from school choices, under the strong assumption that location is assumed to be exogenous) that typically finds that less advantaged households place lower weight on academic standards when choosing schools (Hastings, Kane, and Staiger, 2009, Burgess et al., 2015, Borghans, Golsteyn, and Zölitz, 2015, Harris and Larsen, 2019, Walker and Weldon, 2020). Our results therefore suggest that schools in all areas have incentives to improve their education standards to attract parents/pupils. Households with English as an additional language and non-White British households respond less to the immediate release of information, however, suggesting that there may be some information frictions in the nationwide setting for these groups.

Our second contribution is to study the effect of an established school inspection regime on

[4] A long literature studies the causal effect of school quality on local property prices, starting with Black, 1999 and summarised in Black and Machin, 2011. Greaves and Turon, 2021 show that the effect of local school quality on property prices is correlated with parents' preferences, but does not reveal the strength of parents' preferences, as the presence of non-parents in the market dilutes the observed property price response.

school choices in the context of an entrenched school choice system.[5,6] This is in contrast to previous research that mainly studies the effect of new, experimentally provided, school quality information (Hastings and Weinstein, 2008; Ajayi, Friedman, and Lucas, 2017; Andrabi, Das, and Khwaja, 2017; Corcoran et al., 2018; Neilson, Allende, and Gallego, 2019; Ainsworth et al., 2020). These experiments typically provide parents with personalised test score information for local schools, in combination with information about the admission system (Hastings and Weinstein, 2008; Ajayi, Friedman, and Lucas, 2017; Corcoran et al., 2018) or the importance of school choice in general (Neilson, Allende, and Gallego, 2019). Andrabi, Das, and Khwaja, 2017 provide the only existing market-level analysis of the experimental provision of school test scores in Pakistan. Ainsworth et al., 2020 extend the literature to consider the provision of information about school value-added (pupil progress) rather than absolute attainment that conflates schools' value-added and student composition.

Our results have higher external validity than previous studies with an experimental design and/or single city/area setting. For example, Ainsworth et al., 2020 note that "the effects of information might be larger and of a general equilibrium nature if information can be delivered in greater doses and in a more sustained fashion than we did". Corcoran et al., 2018 caution that "It is natural to be concerned about the general equilibrium implications of an informational intervention like ours operating at scale". In general, the provision of non-targeted information, such as school-league tables published by government and/or media, may have different effects than the targeted information provided to specific households under experimental conditions. Neilson, Allende, and Gallego, 2019 build a structural model of demand and supply responses to information provision to extrapolate the findings from their RCT to the population. This is not necessary in our case, as the information provision is already 'at scale', and so incorporates responses on the demand and supply-side.

Our period of study is also relevant to the school choice environment for many parents worldwide today, which is increasingly online. The context for school choice has changed dramatically from the first experiments that were provided in paper format, or natural experiments derived from newspaper rankings. This is pertinent, as the content and format of information provision are important determinants of choice outcomes (Glazerman et al., 2020; Valant and Newark, 2020).

There are a handful of relevant studies in non-experimental contexts (Koning and Van der Wiel, 2012; Mizala and Urquiola, 2013; Nunes, Reis, and Seabra, 2015; Hussain, 2020).[7] Koning and Van der Wiel, 2012 and Nunes, Reis, and Seabra, 2015 study the publication of school rankings provided in newspapers (based on pupils' test scores), in the Netherlands and Portugal

[5] The system of school choice is well-established in England. Since 1988, parents have had the opportunity to express a preference for the school they would like their child to attend rather than be assigned by their location, although location is often a decisive factor in admission to over-subscribed schools. Through funding linked to pupil enrolment, schools have an incentive to compete for students.

[6] A separate strand of literature studies the effect of accountability systems on pupil attainment. See, for example, Hanushek and Raymond, 2005, Chiang, 2009, Rockoff and Turner, 2010, Burgess, Wilson, and Worth, 2013, Rouse et al., 2013, Camargo et al., 2018 and Dee, 2020. Burgess, Wilson, and Worth, 2013 find that abolishing school league tables in Wales decreased school effectiveness relative to the counterfactual in neighbouring England. This implies that the nationwide provision of information affects school quality, presumably through demand-side pressure.

[7] Hastings and Weinstein, 2008 also study the effect of the No Child Left Behind mandatory information provision on school enrolment, in addition to their experimental provision. These results are relevant only for the parents of pupils attending the minority of schools identified as 'failing', however.

respectively, Nunes, Reis, and Seabra using a difference-in-differences design. Koning and Van der Wiel, 2012 find that school choices respond to information about the quality of schools published in a national newspaper. The effects are only large for the college-preparatory track, however, which suggests that not all schools face demand-side pressure from this form of accountability in the Netherlands. Nunes, Reis, and Seabra, 2015 find that school enrolment declines in response to a poor accountability rating, although underlying school choices are not observable. For Chile, Mizala and Urquiola, 2013 use a regression discontinuity design to estimate the effect of being labelled as an effective school (akin to value-added), rather than absolute test score data. In comparison to Mizala and Urquiola, 2013, we study the effect of school quality information for all schools across the distribution, rather than only the 25% identified as high performing. We extend the analysis in Hussain, 2020 by using a natural experiment in addition to an event study design, and using national administrative school choice data rather than for one London borough.

The direction of our effects are consistent with previous experimental and non-experimental work, with the exception of Mizala and Urquiola, 2013. This is notable, as Mizala and Urquiola, 2013 are the only paper to study a value-added type classification, finding no effect on choice outcomes. Indeed, the dominant finding in the wider literature is that parents respond less to value-added measures than absolute test scores. For example, Imberman and Lovenheim, 2016 show that local property prices respond to test score but not value-added measures of school quality. Our results are therefore an important contribution to this literature, showing that households do respond to non-test score based school quality information.[8]

Our third contribution is to study the equilibrium effects of information on demand for schools, and therefore competition between schools, in a setting where supply is constrained. This is the typical school choice setting (unlike in Chile or Sweden, say, where a for-profit sector has been allowed to emerge). The broader context in England of increasing pupil numbers and relatively few new or expanding schools means that demand-side pressure induced by information provision is limited by capacity constraints. We find that although parents' choices are responsive to new information about school quality, schools' actual intakes are less affected, on average. This is because less popular schools 'fill up' with pupils that were not allocated to one of their preferred schools. This may be in contrast to other settings, for example Chile and Sweden, where school choice systems incentivise an increased supply of school places. This finding potentially reconciles conflicting evidence from other settings about whether the provision of information about school quality affects school choices and/or enrolments, and cautions the use of enrolment as a proxy for parental demand.

Finally, we are the first to study the effects of information on segregation and market shares at the local education market-level. We define local education markets across England, with an average size of 4 primary schools per market. We find that an increase in the variation in Ofsted ratings at the market-level increases the variation in market shares, but has no significant effect on the level of segregation. This implies that changes in school quality information affects the distribution of school choices across schools, but, in contrast to previous research, not in a way that increases segregation.

The following section discusses the English education context in more detail, in particular the

[8] Abdulkadiroğlu et al., 2020 find that New York parents' preferences are correlated with peer quality and effectiveness. As more effective schools enrol higher ability students, however, the correlation between preferences and effectiveness, conditional on peer quality, is zero. In contrast, other recent work that study a broad range of school characteristics find that parents do value school-effectiveness (Beuermann et al., 2018; Harris and Larsen, 2019).

system of school choice and accountability measures. Section 3 describes the data and empirical strategy we employ. Section 4 presents the results at the school and pupil-level, in the short and medium-term. Section 5 concludes.

2 Context

Parents in England have the right to express a preference for the state school that they would like their child to attend (if any). The first year of compulsory education is the school year in which the child turns five, in an infant or primary school.[9] For this stage of education, the deadline for parents to express these preferences is the 15th January for entry in September of the same calendar year. The Local Authority (LA) collects parents' preferences through a centralised system that includes all government funded schools, regardless of type.[10] Through this co-ordinated admissions system parents can provide up to 3-6 choices of school, depending on the LA, in preference (rank) order.

The LA is responsible for allocating pupils to schools, considering all parents' choices alongside published school priorities and admission numbers. If no school was over-subscribed, then each household would be allocated to their most preferred school. When a school is over-subscribed, each school's published admissions criteria are applied to rank students. Higher priority is typically given to students with a statement of special educational need or looked-after by the LA, students with a sibling already at the school, and closer home-school distance. Faith schools are likely to also consider the faith of the child, with information provided by parents on a supplementary information form.

The allocation of pupils to schools is done using an algorithm known as Equal Preferences, which is equivalent to the student optimal stable allocation (see Pathak and Sönmez, 2013).[11] This algorithm is weakly truth revealing, meaning that parents can do no better than reporting their truly preferred schools. As described in Burgess, Greaves, and Vignoles, 2019:

> The algorithm works by first taking a list of pupils for each school, ranked in order of priority, and provisionally assigning pupils to the school they ranked most highly where they are ranked within the school's capacity. Next, these pupils are removed from the ranked lists of schools that are less preferred than their provisional allocation. Where this creates space at a school, pupils that prefer this school to their provisional allocation are reassigned, again according to the original ranking of pupils. This process is repeated until all pupils are assigned to their most pre-

[9] Primary schools teach pupils up to age eleven, before pupils transition to secondary schools. National (externally marked) assessment takes place at the end of primary school, known as Key Stage 2 (KS2) assessment. Average school-level results are publicly available. Infant schools teach pupils up to age seven, and are therefore excluded from test-based school performance measures.

[10] For example, autonomous state-funded schools such as faith schools and academies are part of the co-ordinated admissions system. Private schools are outside the co-ordinated admissions system. Parents can apply to both private and state schools simultaneously.

[11] The school choice system in England was amended in 2007 to encourage parents to choose their truly most preferred schools rather than to make safety-first or strategic choices, although there remains an incentive to list strategic school choices due to the restricted number of possible choices. For example, listing one 'safe' school may be advantageous to avoid allocation to a school with spare capacity.

ferred school subject to the schools' admission arrangements. This may not be fully understood by parents, however, who may believe that they are more likely to be allocated their most preferred school by only making one choice, or that they will be penalised for entry to their second-choice school by making an 'ambitious' first choice.

In the resulting allocation, each child is allocated to their highest ranked school where they are admitted according to the criteria of each school. If a pupil is not allocated to any preferred school, they are assigned to a school with spare capacity (which is less popular by definition). Schools care about the number of pupils allocated to their school, as three-quarters of their funding is determined by the number of pupils on roll (National Audit Office, 2021). School funding is progressive, with schools receiving more funding for pupils with higher need, on average.

In England, parents have access to a vast array of information about schools in their local area (and more widely). Each LA produces a guide for parents which includes the location, admissions criteria and 'mission statement' for each school in their domain. The Department for Education produces school performance tables which summarise key academic indicators, pupil demographics and even school funding and spending for all schools. Additional information is provided through Ofsted (the Office for Standards in Education) inspections, which feature prominently on school performance tables, school websites, and even property search engines.

Schools are typically inspected by Ofsted once every few years, although historically this has been less frequent for schools judged to be 'Outstanding'. A school's rating is based on hard performance data (test scores) and a wealth of qualitative evidence gathered by inspectors during their visit. For further information on the process of school inspections, see Hussain, 2015. Schools have little opportunity to game the system, as inspections are announced only one or two days in advance. The headline Ofsted rating is: 4 (Outstanding), 3 (Good), 2 (Requires Improvement) and 1 (Inadequate/failing). These ratings are immediately disseminated to all parents currently at the school via a letter, and a full inspection report is made publicly available online within 3 to 4 weeks. Previous research shows that Ofsted inspections provide new information to parents, as house prices respond to updated information (Hussain, 2020) and parents' time investments in their child respond to new information if at the school (Greaves et al., 2021).

3 Data

Our main sources of data are administrative data on parents' school choices, linked to the National Pupil Database, and Ofsted inspection ratings. These are described in turn, followed by some descriptive statistics for our final sample.

3.1 Parents' school choices

We use administrative data on parents' school choices for the whole cohort of pupils entering primary (or infant) school in four cohorts: September 2014, September 2015, September 2016 and September 2017. These data contain, for each cohort and each pupil: the unique identifier of each nominated school (e.g., first, second, and third choices in some areas and up to six choices in others) and the identity of the school that the child was offered, that may differ from the

school that the pupil eventually attends.

The main limitation of these data is that pupil characteristics that determine priority ordering at schools in their local area are unknown. In particular, we do not know whether the child has an older sibling at the school or whether the child is looked after by the LA, both of which have high priority in over-subscribed schools, generally overriding proximity. We are therefore unable to estimate the effect of new school quality information on parents' choices separately for oldest and younger siblings, although we hypothesise that school choices will be most responsive for older siblings (as found in Hussain, 2020). This is because the cost of children attending multiple primary schools may outweigh the benefit of attending a higher quality school, or because parents with children already at the school may have inside information about school quality.

From these data, we create the following dependent variables. At the pupil-level, a binary indicator for whether the pupil chooses their closest school as their first choice. In sensitivity analysis, we introduce and vary a radius to define 'close' to a school. At the school-level, we calculate the total number of *first* choices the school receives, the total number of choices the school receives, and the total number of offers made as a result of the LA co-ordinated admissions system. This last variable allows us to determine whether changes in parents' choices translate into demand-side pressure for schools.

3.2 National Pupil Database

The school choice data are linked to the National Pupil Database (NPD), which is a census of all pupils in the English state-funded education system. The NPD enhances the school choice database by including pupil demographics, which we use for heterogeneity analyses, and precise home location (postcode). Relevant pupil characteristics are eligibility for Free School Meals (FSM), as a marker of poverty, and aggregate ethnic groups.[12] We also explore heterogeneity by whether a pupil has English as an Additional Language (EAL), as home language may affect information acquisition. Precise home location is used to determine each household's closest open primary school, where a school is classified as 'open' for each household if it receives any school choices or offers any seats to pupils in the relevant year of application.

We define local (sub-LA) education markets using the NPD, to explore heterogeneity across market-level characteristics and schools' position in the local hierarchy. Following the motivation and intuition in Taylor, 2009, we define school A and school B to be in the same local education market if at least 10% of pupils flow from school A's catchment area to school B, or vice versa.[13] School C (and any associated schools) will join this market if at least 10% of pupils flow from school C's catchment area to school A or B, or vice versa. This process leads to a classification of local education markets with a mean of four schools per market.[14] Schools within each

[12] Ethnic group is derived from the National Pupil Database, based on minor ethnic group classification. The categories used are 'White British', 'White Other' ('White - Irish', 'Traveller Of Irish Heritage', 'Gypsy/Romany' and 'Any Other White Background'), 'Asian' ('Bangladeshi', 'Indian', 'Pakistani' and 'Any Other Asian Background'), 'Black' ('African', 'Caribbean', 'Any Other Black Background'), 'Chinese', 'Mixed' ('Mixed White and Asian', 'Mixed White and Black African', 'Mixed White and Black Caribbean' and 'Any Other Mixed Background') and 'Any Other Ethnic Group'.

[13] 'Catchment area' is defined as the area around the school in which it is pupils' closest school.

[14] The 10th and 25th percentiles are two schools per market, the median is three, 75th is five and 95th is seven. This process could be refined by using a community detection algorithm (Fortunato and Hric, 2016), to ensure that the definition of markets maximises the intra-cluster connectivity and

local education market are ranked according to baseline characteristics: popularity (a higher proportion of pupils flowing to the school from other schools' catchment areas), the percentage of pupils with FSM, and test score performance. In each case, above median is defined as 'better' (more popular, lower FSM and higher test scores).

3.3 Ofsted inspection outcomes

Ofsted inspection outcomes (Outstanding, Good, Requires Improvement or Inadequate) are taken from publicly available management information for recent years. Our final dataset provides every school inspection outcome and publication date from 2005 to 2019. We create a panel of school inspection outcomes in order to classify schools that improve, stay the same, or decline in their Ofsted rating over time. This linkage comprehensively takes account of schools that change school identifiers over time, for example if they convert to an academy.

The Ofsted inspection framework changed from September 2015. The main changes were the background of inspectors (who were now more likely to be practising teachers and school leaders) and the introduction of a short rather than full inspection. Under the old system, all schools would receive a full inspection. Under the new system, a full inspection would follow a short inspection only when the inspectors find evidence that the performance of the school is significantly worse or better than the previous Ofsted rating. Appendix Table A1 shows how we classify short inspection outcomes into the four traditionally used Ofsted ratings. We hypothesise that short inspections provide information to parents in the same way as inspections under the old system, as the short inspections either confirm or update existing information about school quality and provide a commentary on a school's key strengths and weaknesses. We test this hypothesis in Table 12 where we restrict the sample to inspections under the old or new inspection regime. Results are economically and statistically significantly the same.

Ofsted typically visits each school every 3 to 4 years, although the frequency of inspection depends on the previous inspection outcome. Schools judged as 'Inadequate' are inspected more frequently, commonly in the following school year, while schools judged as 'Outstanding' are inspected only if school performance data suggests concern. Table 1 unpicks this endogenous inspection timing further, showing the mean, standard deviation, and percentiles for the length between inspections. It is common for previously 'Outstanding' schools to have a long time between inspections, with a mean and median of 5 years. In contrast, the mean and median for 'Inadequate' and 'Requires Improvement' schools is around 2. Our final sample excludes schools with a previous inspection in the preceding school year, and therefore disproportionately drops previously 'Inadequate' schools.

3.4 Final sample selection

Appendix Table A2 shows the number of schools included in our final analysis sample, starting from an initial sample of 16,564 primary schools that are ever observed in the school choice data. 16,257 of these schools are also present in the pupil-level data, meaning that they are the closest school for at least one pupil. Only a handful of schools in the school choice data are lost due to an invalid school identifier, but we lose 4% of the original sample once we condition on linking

minimises the inter-cluster connectivity. This will be subject of future research.

to the database of Ofsted inspections. We drop a few schools that merge with other schools or split to become numerous schools over our period, as it is difficult to assign previous and current Ofsted inspections. We drop a small of schools that are "special schools", "independent", or "other" schools. We then choose to define a common sample of schools: those where choice data is observed in at least two years, have a valid current and previous Ofsted inspection, are not inspected twice in the same year, and where all outcome variables are observed. We also choose to select the sample to exclude schools that were last inspected in the previous school year, which are predominantly those rated as 'Inadequate'. This is because the new revelation of information follows very soon after the previous information for this group, and is likely to be anticipated by parents. The final sample consists of 10,301 schools. This is reduced depending on the methodology employed (explained fully in section 4).

3.5 Descriptive statistics

This section presents some descriptive statistics of our final dataset that provide context for our evidence. The relation to our identifying assumptions is given in sections 4.1.1 and 4.2.1.

3.5.1 School choices

Figures 1 and 2 describe the dependent variables. At the pupil-level, Figure 1 shows the percentage of households that choose their closest school as their first choice. This decreases with distance to the school. That is, households are more likely to choose their closest school if they live very close to it. The percentage is low even for immediate neighbours, however. For example, for households within 50m of their closest school, only around 60% of households choose it as their first choice. This is true even households with an 'Outstanding' school on their doorstep, where the percentage is only around 70%. That more than a quarter of households choose to travel further to avoid a nearby 'Outstanding' school suggests that many parents make an active school choice that might depend on a wide range of school characteristics. Despite this, Figure 1 shows that parents are more likely to choose nearby schools with higher Ofsted ratings, which shows that ratings are, or are correlated with, desirable school attributes for parents. There is little difference between schools with ratings of 'Requires Improvement' and 'Inadequate', where around half of parents living very close to the school choose it as first choice.

At the school-level, Figure 2 shows the average number of choices to schools with different Ofsted ratings. Consistent with the pupil-level dependent variable in Figure 1, schools currently rated as 'Outstanding' receive more first choices and more total choices than those with lower Ofsted ratings. Also, schools with the lowest two ratings are typically more similar to each other than other ratings. These patterns suggest that schools currently rated as 'Requires Improvement' or 'Inadequate' are the ones that some parents would avoid, although the figures show that a sizeable number of parents do choose them.

Panel (c) of Figure 2 provides the first descriptive evidence that there is constrained capacity for primary schools in England. This is because although 'Outstanding' schools do make more offers, the gradient across Ofsted rating is less marked than the choices made in panels (a) and (b). If there is little spare capacity, unpopular schools will make offers to parents that were unsuccessful at any of their preferred school choices.

Table 2 shows the summary statistics for the pupil-level school choice data in our final sample.

Across school years, between 42% and 46% of households choose their closest school as first choice. This rises for 'Outstanding' schools (between 51% and 55% across years) and is lowest for 'Inadequate' schools (between 31% and 36% across years).[15]

Around 90% of pupils receive an offer from their first choice school, while only slightly fewer (around 87%) eventually attend this school in the Spring following admission in September. Burgess, Greaves, and Vignoles, 2019 are among those to discuss the interpretation of this high percentage of pupils admitted to their first choice school. Upon consideration, it is likely that the high percentage reflects some realisation by parents about which schools are feasible to access when making choices, rather than a flexible, responsive school system.

Around 14% of pupils are eligible for free school meals (FSM) in our sample of primary school applicants. Slightly more (around 19%) are recorded as having English as an Additional Language (EAL). Pupils that are White British are around 70% of pupils, with pupils that are 'White Other', 'Asian', 'Mixed' and 'Black' making up between less than 10% each.

3.5.2 Ofsted ratings

Schools' Ofsted ratings vary across inspections. Table 3 shows that there is movement between previous and current Ofsted rating in each case, for our final sample of schools. For example, 7% of schools previously rated as 'Inadequate' remain so, 25% move to 'Requires Improvement', 61% to 'Good' and even 7% to 'Outstanding'. In the other direction, there is also movement from 'Outstanding' to 'Inadequate', by around 4% of schools. The most likely current inspection rating is 'Good', for each previous inspection rating. This is most true for schools previously 'Good', where 81% remain so, and schools previously 'Requires Improvement', where 80% become so.

Panel (a) of Figure 3 shows that the stock of schools with each Ofsted rating has remained quite stable across our period of interest, although the percentage of schools rated 'Requires Improvement' has declined slightly over time. The stock of schools masks changes over time in the schools inspected in each school year, however. Panel (b) shows that from the 2015/2016 school year, the percentage of schools rated 'Good' declined while the percentage rated 'Requires Improvement' increased. Fewer schools are also awarded 'Outstanding' in the later years. This is likely to be a result of the change in inspection regime described in section 3.3, but raises the potential concern that the schools inspected over time varies. This potential concern is addressed in sections 4.1.1 and 4.2.1.

Table 4 shows school-level summary statistics for Ofsted ratings. The increase in the percentage of schools rated as 'Inadequate' and 'Requires Improvement' over time, and slight decreases in the percentage of schools rated as 'Outstanding' and 'Good' mirrors Figure 3. The percentage of schools inspected early (the treatment schools) varies over time, from 28% in 2015/2016 to 46% in 2014/2015. This could be the result of the change in inspection regime in the 2015/2016 school year. The percentage of schools that receive a decline, the same, or improvement in Ofsted rating relative to their previous rating is roughly constant across school years, aside from the final year in our sample (2016/2017) where a lower percentage of schools improve.

The following section outlines our empirical approaches and the identifying assumptions necessary to estimate the causal effect of information provision rather than underlying characteristics

[15] These patterns were also shown in Figure 1, which shows the variation in choosing the closest school by Ofsted rating and distance to the home.

of the schools. This identifying assumptions are then interrogated in sections 4.1.1 and 4.2.1.

4 Empirical Strategy and identifying assumptions

This section details our empirical approach to estimate the effect of school quality information (Ofsted inspection ratings) on school choices. The crucial identifying assumption is that school choices respond to Ofsted inspection ratings rather than other factors that are correlated with Ofsted inspection outcomes. For example, inspectors look in part at a school's test scores to determine its rating. Parents' school choices could therefore respond to a simultaneous improvement in school test scores, rather than the Ofsted inspection outcome.

We overcome this concern using two complementary approaches that are explained in the following two subsections. First, we validate our difference-in-differences specification using an event study framework to compare the evolution of school choices in response to changing versus constant school inspection outcomes. This approach relies on common trends up to the year of inspection, that we show to (largely) hold, that in turn relies on school inspection outcomes being unpredictable to parents. This is reasonable in a context of noisy school test score measures and a holistic school inspection based on non-test score components.

Second, we exploit a natural experiment in the timing of Ofsted inspections relative to the school choice admissions deadline. Under this natural experiment, some parents receive information from Ofsted before the school choice deadline, while others receive information after the deadline. We can therefore compare school choices to like-for-like schools, where the only difference is whether Ofsted inspection outcomes were revealed to parents in advance of submitting their school choices.

4.1 Difference-in-differences design

We first present our difference-in-differences research design where we compare outcomes for schools receiving an up or down-rating with schools experiencing no change in their inspection rating. The key benefit of this approach is that we can investigate longer-term effects of inspection ratings to understand the dynamic effects of ratings over a number of years. This provides evidence about whether the effect of ratings is concentrated in the immediate months after the information is released, or whether the effect increases over time, perhaps because of social learning effects.

The identifying assumption in this case is that trends in school choices at up or down-rated schools would have been the same as 'no-change' schools without disclosure of the inspection rating. This is reasonable to assume if short-term changes in school quality proxied by test score movements are less observable, pertinent, or reliable to parents. Perhaps because of the noisy nature of year-to-year test score movements (Chay, McEwan, and Urquiola, 2005; Kane and Staiger, 2002), independent inspection ratings may provide 'news' to which parents respond. For example, Kane and Staiger, 2002 find that the housing market does not respond to short-term changes in test scores.

The estimated model at the pupil-level is as follows:

$$Y_{ist} = \alpha + \beta_1 Rating_{st} + \lambda_t + \mu_s + \epsilon_{ist} \tag{1}$$

where Y_{ist} is a binary variable equal to one if the nearest school s was chosen by family i in year t as their first choice school, $Rating_{st}$ is the inspection rating for school s in year t, and λ_t and μ_s are year and school fixed-effects, respectively. We choose this dependent variable and binary choice setting rather than modelling choice of school in a discrete choice setting to avoid the necessity of defining the school choice set from which parents choose. Choice sets are difficult to observe with the available data, and may respond endogenously to the provision of information.

The parameter of interest is β_1, the effect of a unit change in the school's rating on the probability that local families will select this school as their first choice school. Despite being in levels, β_1 represents the effect of unit change in Ofsted rating, as schools with unchanged Ofsted ratings are inseparable from the school fixed-effects included in the model.

We restrict Y_{ist} to non-inspection years, as the effect in inspection years is muted by schools that are inspected after the school choice deadline, as no information is revelaed to parents for these schools. In robustness checks, we add a vector of family characteristics (Z_i) and a set of time-varying school covariates (X_{st}). Further robustness checks restrict the sample of schools to those with a previous Ofsted inspection rating of 'Good' or 'Requires Improvement' that are not bound by ceiling or floor effects in the current rating. The school-level equivalent regression has dependent variable Y_{st}, which are defined in section 3.1: the total number of choices and first choices made to, and offers made by, each school s in year t.

To test asymmetric responses to improvements and declines in ratings, we estimate the following alternative model:

$$Y_{ist} = \alpha + \delta_1 post_{st} + \gamma_1 Improve_s * post_{st} + \sigma_1 Decline_s * post_{st} + \lambda_t + \mu_s + \epsilon_{ist} \tag{2}$$

where Y_{ist}, λ_t and μ_s are as defined in equation 1, $post_{st}$ is a binary variable equal to one if year t is after school s was inspected. $Improve_s$ is a time-invariant binary variable equal to one if school s receives a higher Ofsted rating than their previous inspection rating. Alternatively, $Decline_s$ is a binary variable equal to one if school s receives a *lower* Ofsted rating than their previous inspection rating. The reference category is therefore schools with no change in their rating across inspection cycles. γ_1 represents the effect of an improvement in Ofsted rating relative to no change, while σ_1 is the effect of a decline. As for equation 1, we restrict Y_{ist} to non-inspection years when the effect is muted by schools that are inspected after the school choice deadline.

Finally, we estimate an event study design to test the validity of our common trends assumptions and show the evolution of school choices over time. Here, the year of inspection is included to show the dynamic effect over time. This model is as follows:

$$Y_{ist} = \alpha + \sum_{\tau=-3}^{2} \delta_\tau D_{st}^\tau + \sum_{\tau=-3}^{2} \gamma_\tau Improve_s * D_{st}^\tau + \sum_{\tau=-3}^{2} \sigma_\tau Decline_s * D_{st}^\tau + \lambda_t + \mu_s + \epsilon_{ist} \tag{3}$$

where $\tau = -1$ is the reference category and is excluded from the summations. D_{st}^τ are event time dummies, equal to one if t is τ years before or after school s was inspected. For example, if school s was inspected in the school year 2015/2016, then D_{st}^1 would equal one in 2016/2017 and zero in all other years. The common trends assumption is met if $\gamma_\tau = 0$ and $\sigma_\tau = 0$ for $\tau \in [-3, -2]$.

4.1.1 Identifying assumptions for the difference-in-differences design

The identifying assumption for this design is that parents respond to the Ofsted rating rather than factors that are correlated with the Ofsted rating, such as test scores. Table 5 shows the correlation between Ofsted rating and other observable school characteristics that are published annually by the Department for Education. There is a positive correlation with lagged school test scores, as expected. The R^2 is 0.23, however, meaning that most of the variation in the Ofsted rating is unexplained by published school characteristics or previous rating. If parents respond to Ofsted rather than these time-varying school characteristics, there should be common trends prior to the new inspection outcome in parents' choices between schools with a change versus no change in rating.

Figure 5 shows the trends in school-level total choices for schools with an improvement and decline in rating compared to schools with no change in rating over time. Year 0 is the year of inspection, year 1 is the year following the inspection, and so on. Each line shows the average school-level difference in the dependent variable relative to year -1 (the year before inspection). Panel (a) provides descriptive evidence that there are parallel trends in total choices at the school-level. This is also convincing for the schools with a previous rating of 'Good' shown in panel (b). The total number of choices diverges slightly in the year of inspection then by more in the first and second years after inspection. Figure 6 shows the equivalent parallel trends for the first choices, while Figure 7 shows the equivalent for the probability of choosing the closest school as first choice.

Our event study specification (Table 16) shows that school choices are differentially responsive between schools that change versus stay the same largely only in the years following an inspection. Appendix Table A3 shows that this is even more convincing for schools that were previously rated 'Good'.

4.2 Triple-difference design

The difference-in-differences design described in the previous subsection estimates the medium-term effect of a change in inspection rating relative to no change. In this case, school choices in the medium-term can be affected through two channels. First, total school choices may respond for households in a fixed location. Second, total school choices may respond for households that move closer to the desirable school following information provision. In the difference-in-differences design, the parameter of interest is the effect of a change in school quality rating on school choices and offers.

Our triple-difference design instead isolates the short-term effect of *information* about school quality. Rather than comparing across schools that receive different inspection ratings, this design compares schools that receive the same inspection ratings, but at different times within the school year. The short-term response we study with this design captures only school choices conditional on a fixed location. We exploit the timing of parents' school choice applications alongside the within-year variation in release of inspection ratings in order to assess families' causal response. We do this by comparing applications to schools inspected before or after the application deadline.

The sample of schools is all schools inspected in the school years 2013/2014 to 2017/2018. Schools that are inspected (and the outcome published) in the early part of each school year

(September to 15th January) are classified as the set of treatment schools. Those that inspected/rated in the later part of the year (16th January to August) are the control set of schools.[16] As illustrated in Figure 4, the key idea is that families with schools inspected early in their choice set have access to updated information, whilst those families with schools inspected later in the year do not yet know the outcome of the inspection at the time of application. The characteristics of treatment and control schools should be balanced. This is demonstrated in section 4.2.1.

We implement a triple-difference research design, where the first difference is between schools that change vs retain the same Ofsted rating, and the second difference is before and after the information is revealed, using school fixed effects. The third difference is between schools where this information is revealed or not to parents. For our pupil-level analysis, the estimated model is as follows:

$$Y_{ist} = \alpha + \beta_1 Rating_{st} + \eta_1 Rating_{st} * Early_s + \lambda_t + \mu_s + \epsilon_{ist} \tag{4}$$

where Y_{ist} is a dummy indicating whether the nearest school s was chosen by family i in year t as their first choice school, $Early_s$ is a binary variable equal to one if school s is inspected in the early part of the year and $Rating_{st}$ is the inspection rating for school s in year t. λ_t are year fixed-effects and μ_s are school fixed-effects as in equations 1 to 3. In robustness checks, we additionally add a vector of family characteristics (Z_i) and a set of time-varying school covariates (X_{st}). For this model, we restrict t to pre-inspection and inspection years only (the short-term response), as in the post-inspection years both treatment and control group have the same information available to parents. As in the previous section, the school-level equivalent regression has dependent variable Y_{st} rather than Y_{ist}.

The parameter of interest is η_1, the effect of *observing* a unit change in the school's rating on the probability that local families will select this school as their first choice school. Despite being in levels, η_1 represents the effect of unit *change* in observed Ofsted rating, as schools with unchanged Ofsted ratings are inseparable from the school fixed-effects included in the model. Identification in this model can be illustrated as follows. First consider the control group of schools that are inspected late in the school year: the difference in the change in the outcome for schools receiving a unit improvement in their rating versus schools experiencing no change is given by β_1. For the treatment group (inspected early in the school year) the difference in the change in the outcome for schools receiving a unit improvement in their rating versus schools experiencing no change is given by $\beta_1 + \eta_1$. The difference between these two differences, η_1, identifies the effect of parents' *receiving information* about the change in the rating.

We also run the equivalent specifications to the difference-in-differences case to test for asymmetric responses (equation 5) and parallel trends (equation 6).

$$\begin{aligned} Y_{ist} =& \alpha + \delta_1 post_{st} + \gamma_1 Improve_s * post_{st} + \gamma_2 Improve_s * post_{st} * Early_s \\ &+ \sigma_1 Decline_s * post_{st} + \sigma_1 Decline_s * post_{st} * Early_s + \lambda_t + \mu_s + \epsilon_{ist} \end{aligned} \tag{5}$$

[16] Only a minority of school inspection reports are published in August.

$$
\begin{aligned}
Y_{ist} =& \alpha + \sum_{\tau=-3}^{0} \delta_\tau D_{st}^\tau + \sum_{\tau=-3}^{0} \gamma_\tau Improve_s * D_{st}^\tau + \sum_{\tau=-3}^{0} \gamma_\tau Improve_s * D_{st}^\tau * Early_s \\
&+ \sum_{\tau=-3}^{0} \sigma_\tau Decline_s * D_{st}^\tau + \sum_{\tau=-3}^{0} \sigma_\tau Decline_s * D_{st}^\tau * Early_s \\
&+ \lambda_t + \mu_s + \epsilon_{ist} \qquad (6)
\end{aligned}
$$

4.2.1 Identifying assumptions for the triple-difference design

The identifying assumption is less stringent than the difference-in-differences case, that there is no selection of schools by month of inspection. This is required to isolate the effect of provision of information (for early inspected schools) from the characteristics of those early inspected schools.

First, to replicate the evidence presented for the difference-in-differences case, Figures 8, 9 and 10 show the trends in dependent variables for schools inspected early (treatment) or late (control) in the school year. The separate panels show the trends for schools with an improvement, the same, or a decline in rating compared to their previous rating. As for Figures in the previous subsection, year 0 is the year of inspection, year -1 is the year prior to the inspection, and so on. These figures show that there are parallel trends in the dependent variables before the year of inspection, particularly for schools with a previous rating of 'Good'.

To further this descriptive evidence, we explore the characteristics of schools inspected across the school year (that form treatment and control groups). Figure 11 shows how the inspection ratings change within (rather than across school years, as shown in Figure 3). In each school year aside from 2015/2016, the percentage of schools rated 'Good' is relatively stable across months. That is, there are no obvious seasonal patterns across months. In the first half of 2015/2016, however, the proportion of schools rated 'Good' is markedly lower than in the later half of 2015/2016. This is the case even for schools previously rated 'Good', shown in panel (b). This is likely to be due to the change in inspection regime from September 2015 — either the change in inspectors or rating criteria — but raises the potential concern that the schools in the treatment group for this school year (inspected early) are systematically different to those inspected later in the year. Our results are robust to using years before the inspection change only (Table 12).

We formally examine the assumption that month of inspection is as good as random using two strategies. First, Table 6 explicitly tests the balance of school characteristics across treatment and control groups across the whole final sample. Schools characteristics are generally balanced across treatment and control schools. There is some suggestion that treatment schools have higher test scores and lagged test scores, however, but the difference is economically small. For the main sample of schools, the difference is not significant for the current test scores and less than 0.1 standard deviation for lagged test scores, for example.

Table 7 then provides the balance across treatment and control groups within each school year. As for Figure 11, the balance is generally good, except for in the 2015/2016 school year. There are a handful of significant differences across treatment and control groups in other years, but these are small in magnitude. We note that our results are robust to the inclusion of time-varying school covariates in addition to fixed effects, but the inclusion of covariates is problematic given

our empirical design (Sant'Anna and Zhao, 2020).[17]

Second, we regress ratings on the month of inspection, where the month before the school choice deadline (December) is the reference category. Table 8 shows that the month of inspection is typically uncorrelated with Ofsted rating, conditional on previous Ofsted rating. The exception is July, where Ofsted ratings are around 0.09 Ofsted levels higher than in December. The interpretation would be moving from a mean of 3 ('Good') in December to a mean of 3.09 in July, for example. Our main results are robust to excluding schools inspected in July or August from the sample of control schools (Table 12). Again, excluding schools inspected in the 2015/2016 school year improves the balance (column 5). Column 6 of Table 8 shows that the results are robust to using an ordered probit model rather than ordinary least squares regression. Finally, columns 7 and 8 have an alternative binary dependent variable equal to one if the school receives a 'Good' and 'Requires Improvement' inspection, respectively. The probability of being awarded 'Good' is equal across the school year, but the probability of being awarded 'Requires Improvement' is slightly lower in May, July and September.

Our event study specification (Table 11) shows that school choices are differentially responsive between treatment and control schools only in the year of inspection. We also run placebo specifications (Table 12), where the $Rating_{st}$ is artificially shifted to $t-1$. We convincingly find that school choices do not respond to Ofsted information in the year before the inspection, giving further credibility to the assumption that the change in rating is largely unpredictable to parents.

5 Results

This section presents results at the pupil and school-level, beginning with the short-term (estimated in a triple differences framework) before showing how school choices evolve in the medium-term (estimated in a difference-in-differences framework). Subsection 5.3 considers the effect on actual enrolment in contrast to school choices, while subsection 5.4 shows the overall effect of school quality information on pupil segregation.

5.1 Short-term response (triple-differences design)

This section shows the within-school year response to school quality information, namely how school quality information revealed between September and mid-January affects school choices made by 15th January. As explained in section 4.2, the control group of schools are those inspected after the school choice deadline in the same school year. These schools receive the same Ofsted ratings and/or change in Ofsted ratings, but the information is not revealed to parents before school choices are submitted.

[17] Including time-varying school covariates requires the additional assumption that treatment effects are homogenous across covariates, and there are parallel trends in covariates across treatment and control groups.

5.1.1 Main effects

Results from the triple-differences model specified in equation 1 are presented in Table 9. The first row ('Ofsted') is the effect of a unit change in Ofsted rating, as schools with no change in Ofsted rating during the period are absorbed by school fixed-effects. The second row is our variable of interest, the effect of a unit change in Ofsted rating being revealed to parents before the school choice deadline. At the pupil-level, a unit rise in the Ofsted rating that is revealed to parents leads to a 1 percentage point rise in the probability that the closest school is listed as the first choice school. This is a 2.3% increase from a baseline probability of 0.43. The coefficients for the school-level dependent variables are also positive. A unit rise in the rating that is revealed to parents leads to 4.2 more total school choices on average, which is equivalent to around 5% of the baseline mean. The number of first choices also increases by 1.5 on average, which is equivalent to around 4% of the baseline mean. All estimates are statistically significant at the one percent level. To provide some interpretation, each pupil in England attracts an additional £5,000 to their school, on average (National Audit Office, 2021). The increase in first choices of around 1.5 equates to around £7,500 additional funding per cohort, or one third of a newly qualified teacher's salary.

These results suggest that parents' choices are immediately responsive to new information about school quality, conditional on their current location. Schools therefore face demand-side pressure to improve, even when ruling out households' endogenous movements in response to school quality information. These results are consistent with the experimental and non-experimental literature, although Mizala and Urquiola, 2013 are a notable exception where value-added type information provision has no effect on school choice outcomes in Chile. Our results are important to show that parents can respond to school quality measures that are not defined by school test scores. We discuss the interpretation of the Ofsted inspection regime in the conclusion (section 6).

Note that the response to a unit change in Ofsted rating (not revealed before the school choice deadline) also has a positive and significant coefficient, although smaller in magnitude than the unit response to information revealed before the school choice deadline. One interpretation is that parents are able to predict movements in Ofsted ratings from correlated school quality indicators, such as test scores. That the coefficients for choice of the nearest school are similar is suggestive evidence that parents might also have "inside information", for example from neighbourhood networks, about their closest school. The strongest effect is from information revealed before the deadline, however, which implies that the revelation of Ofsted rating nevertheless holds important information for parents.

The comparison of pupil-level and school-level results suggests that school choices change from outside the immediate area around the school. Parents therefore consider applying to schools from a broader geographical area, and respond to school quality information from across this wider set of schools. This interpretation is consistent with descriptive work for England (Burgess, Greaves, and Vignoles, 2019) and the low percentage of households that choose their closest school as first choice (around 43%, as shown in Table 9). Ainsworth et al., 2020 also find that providing school information led to households re-ordering preference ranks outside their top two choices. The pattern of our results, from a nationwide inspection regime, is consistent with this. The percentage change in total choices is greater than the percentage change in first choices, suggesting that it may be the lower ranked choices that are more susceptible to change after receiving information.

5.1.2 Asymmetric response

Table 10 reports results from the specification in equation 5 which separates out the effect of a rise versus a fall in the inspection rating at the pupil and school-level. The results show a broadly symmetric effect: an improvement in a school's rating leads to a 1.3 percentage point rise in the probability that the local school is listed as the first choice school, whilst a decline in the rating leads to a decline in this probability of 1.3 percentage points. At the school-level, the effects of an improvement and decline are slightly more suggestive of an asymmetric response, with a stronger effect for an improvement in the rating. An improvement leads to 2.3 additional first choices, on average, while a decline does significantly decrease the number of first choices. This pattern of asymmetric responses is the same for total choices, although in this case the coefficients for improvement and decline are not significantly different. To attract more (in particular first) choices, it may therefore be optimal for schools to mitigate against falls in Ofsted rating, rather than taking actions to improve their Ofsted rating.

5.1.3 Event study design

The event study design given in equation 6 allows us to further test the identifying assumption that parents respond to school quality information rather than contemporaneous or pre-existing changes in other observable school characteristics.

Table 11 shows that the pre-trends at the school-level are the same between schools that improve/decline when this information is revealed to parents, versus the same change that is not revealed to parents. For example, the probability that the closest school is chosen as first choice in the year of inspection increase for schools that improve their rating before the school choice deadline (by 1.5 percentage points, on average) while there is no significant difference in the 2 or 3 years prior to inspection. The lack of pre-trends is also the case for the schools that decline, and for improving and declining schools' first choices. The exception is that for schools' total choices, schools have significantly fewer total choices two years before the inspection when the school goes onto improve in the year of treatment. This is of the opposite sign of the main effect, however, and not evident for schools that decline in the year of treatment.

Overall, the evidence presented in Table 11 suggests that the schools inspected before and after the school choice deadline have similar pre-trends in school choice behaviours, with the exception that at the school-level, improving schools that are inspected early receive significantly fewer total choices two years prior to the inspection year.

5.1.4 Robustness checks

Table 12 presents a set of comprehensive robustness checks for the main specification shown in Table 9. Panels (a) and (b) are for the school-level dependent variables: total choices and first choices. Panel (c) is at the pupil-level: the probability that the nearest school is chosen as first choice.

After the main effect, presented in the first column, the next two columns of Table 12 show the first specification check that the results are robust to the inclusion of pupil-level (where relevant) and school-level time-varying covariates (in addition to the school fixed-effects already

included in the specification). The conditional results are very similar to the main effects, which suggests that Ofsted provides new information to parents, or that Ofsted information that is more relevant or pertinent to parents than short-term movements in test scores or other school characteristics. Similarly, column 8 shows that the results are also robust to including a lagged dependent variable as an alternative school-level covariate. As discussed in section 4.2.1, it is only appropriate to include time-varying covariates if the treatment effects are homogenous across covariates, and there are parallel trends in covariates across treatment and control groups.

Column 4 shows the results of a doughnut design which includes the month of the school choice deadline, so that treated schools are only those inspected between September and December, and control schools are only those inspected between February and July. Again, the effects are slightly higher (but not significantly) for the school-level dependent variables.

Column 5 instead drops inspections that are published in the summer months (July and August) as 8 suggested that ratings might be slightly higher in the summer relative to December. The coefficients of interest are economically and significantly unchanged.

Columns 6 and 7 show that the effect of Ofsted ratings are also similar in the years before and after the Ofsted inspection framework changed. This might suggest that the overall Ofsted ratings have the same meaning to parents, despite a change in the underlying framework that inspects schools. The pupil-level results suggest that parents living closer to the school respond more strongly to information under the full inspection framework, however. These specifications have the additional advantage of removing differential timing of treatment that can be problematic for the interpretation of the difference-in-differences design as an average effect of treatment on the treated (Goodman-Bacon, 2021).

Columns 9 and 10 restrict the sample of schools to those that were initially rated in the middle of the scale ('Good' and 'Requires Improvement') so there is room for an improvement and decline in their current inspection rating. This selection of schools also has the property that the movement in rating is more predictable at the aggregate level, however. 80% of schools that were previously rated as 'Requires Improvement' are rated 'Good' in the current inspection, while 81% of previously 'Good' schools remain so (see the transition matrix in Table 3).[18] For these sets of schools, where the rating is both unbound by floor and ceiling effects and slightly more predictable at the aggregate level, the effects for the school-level dependent variables are slightly stronger than in the main specification (although not significantly so) and at the pupil-level are almost identical (higher for previously 'Good' schools).

In column 11, we exclude religious schools that may select students on the basis of households' religious practice rather than distance from the school. Across all dependent variables, the effects are very similar to the main specification when applying this sample selection, although slightly higher (but not significantly) for the school-level dependent variables.

Finally, column 12 shows the results of a placebo specification, where the timing of the Ofsted inspection is artificially moved forward by one year. School choices do not respond to the rating in the placebo year, which supports the identifying assumption that Ofsted ratings are 'news' to parents and there is no differential selection into treatment. The event study design presented in section 5.1.3 was also largely consistent with this interpretation.

[18] This compares to the modal change of 65% for previously 'Outstanding' schools (that move to 'Good') and 61% for previously 'Inadequate' schools (that also move to 'Good').

5.1.5 Heterogeneity

We next investigate heterogeneity in response for the pupil-level analysis by cutting the sample by different dimensions of household characteristics. The results reported in Table 13 reveal that households eligible for Free School Meals (FSM) respond similarly to households not eligible for FSM, although the effect is not statistically significant for FSM pupils. This is consistent with evidence from Burgess, Greaves, and Vignoles, 2019 who find that the patterns of school choices are similar for households with and without eligibility for FSM, but is in stark contrast to early qualitative literature which suggested that households could be divided into 'privileged' and 'disengaged' along social class lines (see, for example, Ball, Bowe, and Gewirtz, 1996). Previous quantitative work studying parents' preferences for school characteristics from their school choices also routinely finds that lower socio-economic households place lower weight on school quality (Hastings, Kane, and Staiger, 2009; Borghans, Golsteyn, and Zölitz, 2015; Burgess et al., 2015; Denice and Gross, 2016; Glazerman and Dotter, 2017; Akyol and Krishna, 2017; Beuermann et al., 2018; Fack, Grenet, and He, 2019; Harris and Larsen, 2019; Ruijs and Oosterbeek, 2019; Abdulkadiroğlu et al., 2020). Studying the medium-term effect on school choices, Hussain, 2020 also finds that households eligible for FSM respond less to Ofsted inspections in one London borough. We therefore provide important evidence that both advantaged and less-advantaged households respond similarly to Ofsted information across England, but this conclusion is tentative given the lack of statistical significance for FSM households.

Table 13 also shows there are significant differences between families where English is the first or Additional Language (EAL) in the home. We find that households who have English as a second language do not respond immediately to changes in Ofsted ratings. This is in contrast to Burgess, Greaves, and Vignoles, 2019 who find that the patterns of school choice for households with EAL tend to be consistent with more active and potentially more ambitious choices, and Corcoran et al., 2018 who find that the "benefits of simplified information may be greater for families with limited English proficiency". The difference in our triple-difference setting is that we study the immediate response to recently revealed school-quality information, where language barriers or social networks may delay the spread of information.

The response to recently revealed information is also stronger for White British than non-White British households. Note that in both cases where there is heterogeneity, non-White British households and households with EAL do choose schools with higher Ofsted ratings, but not those where this information is revealed before the school choice deadline. There is evidence of preferences for school quality, but not an immediate response to a change in inspection rating for these groups.

At the pupil-level, we also experiment with alternative distance from school to home radii (second panel in Table 13). For example, the first column shows the effect for households that live within 200m of their closest school. The final column is the effect for households that live within 800m and 1km to their closest school. The magnitudes of the point estimate generally increase slightly as pupils live further from their closest school. Note that households are less likely to choose their closest school as first choice the further away they live, however, from 52% to 37% across distance bands. This means that the implied percentage effect of information revelation increases more than the percentage point effect, from 2.2% to 5.1% across distance bands, excluding those within 200m of the school where there is no effect.

Only households living within 400m of the school respond significantly to changes in Ofsted rating, not those revealed before the school choice deadline. As discussed in relation to the

main results, this is suggestive evidence that households very close to a school might have some "inside information" about underlying school quality, perhaps from neighbourhood networks, while households further away are more reliant on published indicators.

Turning to heterogeneity across school-level characteristics, we study the effect across schools in different quintiles of test-score (KS2 performance) at the end of primary school (which necessarily excludes infant schools from the sample) and the percentage of pupils with FSM. The response of total choices and first choices is shown in Tables 14 and 15, respectively. The effect size is almost universally positive across quintiles, although not always statistically significant. We first describe the results for KS2 classification followed by FSM classification.

For the probability of choosing the nearest school as first choice, the effect is only statistically significant for schools in the third and highest KS2 quintile, although the effect size is similar to the main estimate for all but the lowest quintile. For the pupil-level dependent variable, the Ofsted rating appears to be a validation of information gleaned from already high test-scores. This is not evident for the school-level dependent variables, where total choices and first choices are equally responsive across the distribution (not typically significantly in the case of first choices). The difference between the total choices and pupil choice of their closest school implies that schools can attract choices from further afield by improving their Ofsted rating, but schools with high test-scores are more likely to attract those living close to their school.

For all dependent variables, the results differ between infant and primary schools. For infant schools, where no test score information is available to parents, there is only an effect of the revelation of the Ofsted rating, and no effect of the level change in the Ofsted rating. This is suggestive evidence that information provision is more important to parents where other school-level information is limited. Independent school inspections may therefore become more important in the era of cancelled or less reliable school-level test score information as a result of Covid-19.

The effect across FSM quintiles is more consistent across the pupil-level and school-level dependent variables. In each case, the most disadvantaged schools are not significantly benefited from an improvement in Ofsted information that is revealed to parents. The interpretation of this finding is complex and requires further investigation. One explanation is that schools with very poor pupil composition are affected more by reputation or parents' preferences for the peer group than by changes in school-quality information. For example, previous research has found that, on average, parents prefer schools with a "good" (more affluent) peer group (Schneider and Buckley, 2002, Hastings, Kane, and Staiger, 2009, Abdulkadiroğlu, Agarwal, and Pathak, 2017, Glazerman and Dotter, 2017). Schools with a very high percentage of pupils with FSM may be automatically discounted by some parents, regardless of the Ofsted rating, while those with a very low percentage may be automatically selected. In essence, peer composition may outweigh information from independent school inspections.

Our rich data also allow us to explore heterogeneity across local market position, which provides a different picture. For each derived local education market, we assign schools to either above or below median popularity, percentage of pupils with FSM, and KS2 score.[19] For both total choices and first choices, there is a larger effect of information from Ofsted for schools at the bottom of the local hierarchy. In relation to the discussion above, note that this is true for schools with below median FSM. Within a local market, schools with relatively more pupils with FSM receive *more* school choices when a higher Ofsted rating is revealed. This may mask a

[19]Popularity is defined by the flows of pupils from school catchment areas to other schools, with more popular schools receiving the highest flows 'in'.

null effect for schools with very high FSM within a market, but suggests that it is important to consider the local market composition rather than only absolute levels.

Returning to the issue of whether schools face demand-side pressure to improve, our results suggest that schools face different incentives to improve quality (as classified by Ofsted ratings) but the picture is different according to national and local definitions of school characteristics. According to the national distribution, we find that schools in the middle of the distribution face the strongest incentives to improve. This is in contrast to earlier experimental work that found that there was disparate pressure to improve in more affluent areas (Hastings and Weinstein, 2008). Within local markets, however, schools with more a disadvantaged population gain the most from the revelation of school quality information. Andrabi, Das, and Khwaja, 2017 find greater effects for initially lower performing schools, which is consistent with our results at the local education market-level. Comparing to other 'real-world' natural experiments, Koning and Van der Wiel, 2012 find stronger effects of information for the track that prepares students for university, which may be due to differences in preferences across tracks or an understanding that data for other tracks is noisier.

The different patterns in our national and local market-level analysis suggest that considering the local context is important to draw conclusions about the demand-side effects of policies such as the provision of information. Overall, evidence from the local education market-level shows that all schools face demand-side pressure to improve education standards, but particularly those serving more disadvantaged pupils.

5.2 Medium-term response (difference-in-differences design)

The previous section focused on the immediate response of school choices to school quality information, within a few months. We now turn to study the medium-term response of school-quality information, including the evolution of school choices across years. As discussed in section 4, the identifying assumptions are stronger in this case. We compare schools with a change (either improvement or decline) in rating with those with no change, and therefore rely on common trends in school choice behaviour for schools with different trajectories in Ofsted ratings.[20]

Table 16 presents the results for the event study design (equation 3), effect in levels (equation 1) and effect in changes (equation 2).

Unlike the triple-differences case (section 5.1.3), the event study specification reveals some divergence in pre-trends for schools with an improvement or decline in their Ofsted rating, relative to those with no change. These pre-trends much smaller in magnitude than the main effects, however, and of the opposite sign for schools that decline. That is, schools with a decline in Ofsted rating receive more school choices relative to schools that stay the same in the pre-inspection period, but only marginally. This finding bolsters support for the identifying assumption that school choices for the treatment group would be similar to the control group in the absence of the information provision, but is not decisive. Appendix Table A3 provides even more convincing evidence for common trends among schools with a previous 'Good' inspection. Despite the good

[20] In the previous section, we compare schools with the same change in rating but revealed before and after the school choice deadline. In this case, we rely on "early" and "late" schools having the same common trends in choice behaviour.

but imperfect common trends, we explore the results from the difference-in-differences design as it allows us to present the evolution of school choices in response to school quality information.

Columns 1 to 3 of Table 16 shows that the response in the year of inspection is muted, but grows in the two years post inspection (roughly constant between years 1 and 2). For example, total choices increase by 1.7 for in the year of inspection for schools that improve (around 2% from the baseline) but by 9 and 11 (around 10% and 13%) in years 1 and 2, respectively. At the pupil-level, the probability of choosing the closest school as first choice increases by 7% two years after the inspection, but only 1% in the year of the inspection. This could be due to social learning effects — the information spreads more widely in subsequent years — but is also likely to be because not all inspections will have taken place before the school choice deadline. The results of our design that exploit this within-year variation, presented in section 5.1.1, suggest that this second explanation is relevant — the timing of inspection matters for school choices. Further study of how school-quality information spreads across networks is an interesting avenue for future research.

Columns 4 to 6 of Table 16 show the combined effect across post-inspection years, which is consistent with the event study specification. Columns 7 to 9 show the results by improvement or decline across post-inspection years. As in the triple-differences case, there is no clear evidence that the response is asymmetric, although the effect size is larger for schools with a decline in rating rather than increase in rating.

5.3 Demand versus supply: Does choice engender competition?

Table 17 reports the results for the outcome 'number of places offered' by the school in the short- and medium-term (triple differences and difference-in-differences specifications). This outcome measures whether final school place offers made to students increase or decrease in response to ratings. This is an important indicator, as the number of offers made is the result of school choices and school capacity. Places at popular schools are rationed according to their published admissions priorities. Increases in demand (school choices) may not therefore equate to increases in offers at already popular schools.

The results reveal that there is only a relatively low (but statistically significant) relationship between ratings and offers from the school, despite the increase in total choices and first choices shown in sections 5.1.1 and 5.2. A unit rise in the rating revealed to parents before the school choice deadline leads to a rise of 0.8 places offered, on average. This is equivalent to 2% of the baseline mean of 38 offers per school. For comparison with the main results, total choices increase by 5%, first choices by 4%, and the probability that the nearest school is selected as first choice by 2%. For the difference-in-differences design, a unit increase in the Ofsted rating, relative to schools that stay the same, increases the number of offered places by 1.5, on average. This is equivalent to around 4% of the baseline mean of 39 offers per school. Again, the response of offers is low compared to the response of choices: for this specification total choices and first choices increased by around 10%, and the probability of choosing the closest school as first choice increased by around 5.5%.

These results indicate that there is little spare capacity in the primary school sector in England: popular schools are already at capacity before the ratings are revealed, and/or less popular schools 'fill-up' with pupils that didn't choose the school. Although parents' choices respond strongly to new information about school quality, these results suggest that demand-side pressure is

limited by the lack of excess capacity in England. This factor is an important consideration when comparing results across contexts with different incentives to increase the supply of school places. The findings also suggest caution for interpreting school enrolment as parents' demand, particularly in contexts where school capacity is largely fixed.

To explore this further, the final columns of Table 17 show the response by a measure of market capacity. Local education markets are grouped into quintiles by the ratio of pupils to school places. 'Q1' has the fewest pupils per school places, while 'Q5' has the most pupils per school places. In fact, in Q5 there are more pupils than school places, so changes in choices within this market will not affect overall enrolment. Table 17 shows the intuitive result that school offers respond more to the revelation of information where there is excess capacity in the local market: 4.5% in 'Q2', 3.3% in 'Q3' and 2.2% in 'Q4'. There is no significant change in offers as a result of the revelation school choice information in 'Q5', as schools with fewer choices are filled by those without a place elsewhere. There is also, surprisingly, no effect in 'Q1', where schools have the most spare capacity. These might be schools in rural areas where choices are most responsive to distance rather than information.

Our 'real-world' findings echo those in structural work by Neilson, Allende, and Gallego, 2019. Simulating the effect of their experimental information provision to the general equilibrium effects, the authors find that when schools' capacity constraints are taken into account "the average effect of the policy is still positive, but only half as large, as increased demand for higher quality schools in disadvantaged neighborhoods crowds itself out".

5.4 Market-level effects: segregation

Results presented in sections 5.1.1 and 5.3 illustrate that school choices are responsive to information from Ofsted, but the eventual allocation of pupils to schools is constrained by the lack of spare capacity. The overall effect of information on the distribution of pupils across schools therefore depends on parents' school choices *and* schools' capacity constraints and admissions criteria. A key question is whether more advantaged households gain disproportionately from this system, and so whether the provision of information might be regressive.

To explore this, we study the relationship between local education market-level characteristics: the variation in Ofsted ratings in the local market, the variation in market shares of total and first choices, and segregation across pupil types (FSM/non-FSM, EAL/non-EAL and White/non-White). We use market-level fixed-effects to account for time invariant characteristics of the market. Identification therefore comes from changes in the variation in Ofsted ratings in the local market over time. The results in this section have a causal interpretation only if the variation in Ofsted ratings within the market is unrelated to variation in other relevant attributes (such as test scores) that affect school choices and allocation.

Segregation is defined as the dissimilarity index (D, Duncan and Duncan, 1955) that has an intuitive interpretation. From Greaves and Weldon, 2021:

> If D takes its maximum value of 1, this implies that no two members of different sub-groups share the same geographical unit or school. At its minimum value of 0, D implies that the empirical distribution of each sub-group is identical to that of the other. The index has an intuitive interpretation as the proportion of either of the groups who would have to move between geographical units (for example schools)

to equalise the spatial distributions of the two groups.

Panel (a) of Table 18 shows that the variation in market shares across schools within a local market is positively correlated with the variation in Ofsted ratings within the market. That is, market shares of school choices are more disperse when the Ofsted ratings are more disperse. This is consistent with evidence presented in previous sections: school choices shift to schools with higher Ofsted ratings, that may in turn concentrate the market shares of these schools.

Panel (b) of Table 18 shows that this has no correlation with market-level measures of segregation, however, for any pupil group. As the variation in Ofsted ratings increases within a market, the variation in market shares increases, but does not affect the level of segregation. This has a positive policy interpretation that parents benefit from the provision of information without any potentially negative effects on segregation.

6 Conclusion

Ainsworth et al., 2020 concisely summarise the importance of determining whether the provision of information (in this case about value-added) changes households' school choices:

> Distinguishing between preferences and information has important policy implications. If information is the obstacle, then making it available would improve households' choices and spur providers to compete on value added. By contrast, if preferences are the constraint, then policy options to boost value added may be more limited. For instance, school choice may cause schools to invest in other, possibly less desirable, quality dimensions.

The traditional narrative in existing literature is that households from lower socio-economic households have different preferences for school characteristics, most notably lower preferences for academic quality. This would imply that the provision of information to these households would have a limited effect on their school choices.

Early experiments were designed to explicitly test this hypothesis. For example, Hastings and Weinstein, 2008 tested information provision to lower income families in Charlotte-Mecklenburg Public School District. The experimental and non-experimental information led to an increase in the proportion of parents choosing alternative (non-guaranteed) schools, suggesting that there were information frictions for these types of households. Recent experiments are also targeted at higher poverty households (Corcoran et al., 2018).

Using national data and a quasi-experimental setting, we find that information provided by a national school inspection regime — Ofsted — provides new and valuable information to parents. School choices at the pupil and school-level increase in response to a unit increase in Ofsted rating, for example from 'Requires Improvement' to 'Good'. This is in the English context, where annual test score and pupil-progress accountability measures are widely disseminated in an established school choice framework. School inspections therefore provide additional information to parents above more traditional accountability measures.

This response is evident in the year of the inspection and grows over time. That school choices respond immediately to independent inspections suggests that the demand-side pressure to improve standards from the school choice channel, as opposed to only from residential demand.

Both channels matter for schools, as any increase in pupil numbers increases school funding (around £5,000 per pupil, on average, relative to a starting salary for teachers of around £26,000). From the pupil/parent perspective, the school choice channel is of more policy relevance as it is available to all households, regardless of income. In contrast, only high-income households have the option to move closer to their preferred school in response to information. Our results therefore suggest that school choice allows all parents the option of choosing a school with higher quality, regardless of whether they can afford to move to a desirable school catchment area.

Our results validate previous empirical work that has inferred parents' preferences for school quality from observed school choices and discrete choice models, under the empirical challenge that location is assumed to be exogenous. In contrast to the previous literature, we find that households across the income distribution respond similarly to information about school quality. This has the important policy interpretation that schools across all areas face pressure from the demand-side to improve performance.

There are multiple possible interpretations of Ofsted inspection ratings. At face value, the independent inspection provides parents with a general measure of 'school-quality' which could be interpreted as 'school-effectiveness'. This is because test score outcomes for children and learners are only one of four components that inspectors use to award the rating. Other components are the 'effectiveness of leadership and management', 'quality of teaching, learning and assessment', and 'personal development, behaviour and welfare'. As such, the Ofsted rating provides a more holistic measure of school-quality than is possible through test scores alone. Future work could also consider whether Ofsted's broad remit affects schools' incentives to improve pupils' wider outcomes, such as well-being and emotional intelligence. Andrabi, Das, and Khwaja, 2017 caution against the common practice in the literature to only testing the effects of interventions against 'cognitive' measures.

Other interpretations aside from 'school-effectiveness' are feasible, however. Ofsted ratings could reduce uncertainty in parents' evaluations that are based on school test scores, although we note that there is no effect for a school receiving no change in their rating, which also reduces uncertainty. Ofsted ratings could also act as a co-ordination mechanism for parents seeking a 'good' peer group for their child. In other words, a high Ofsted rating increases the likelihood that an 'acceptable' peer group attends the school.

Regardless of the precise interpretation of information provided by Ofsted, the results suggest that schools have an incentive to improve (or maintain) their rating in order to attract school choices (and per pupil funding). This is positive for the education sector as a whole if Ofsted inspection criteria are consistent with improving pupil progress and welfare. These positive incentives are limited by the constrained supply of school places in England, however. Actual enrolment at a school is less influenced by Ofsted rating than underlying school places because there is little excess capacity in the system.

We conclude that providing independent school quality information is valued by parents and acts as an incentive (even if dampened) for school improvement. Other education systems could consider the introduction of independent inspections rather than, or in addition to, accountability through test scores. This is particularly valid if parents do not respond to a value-added threshold measure (Mizala and Urquiola, 2013) but do respond to inspection ratings. School inspections would be especially valuable in contexts where it is difficult to create a ranking of schools that is not volatile or entirely dependent on the socio-economic status of the pupils (Mizala, Romaguera, and Urquiola, 2007). Independent inspection information is also likely to be more relevant in the

era of Covid-19, as tests used for school accountability have been cancelled in many countries.

While our results are encouraging, information provision may not be the only barrier to households' school choices. Hastings and Weinstein, 2008 caution that "proximity to high-scoring school alternatives" is an even more important determinant of the quality of the chosen school than information. The finding that "even with transparent information, school choice can only be as effective as the options offered to parents" is crucial. Information provision will not equalise school outcomes across socio-economic groups if there are structural barriers to households accessing 'good' schools. Further work is therefore required to identify and address barriers to school choice across household types.

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7 Figures

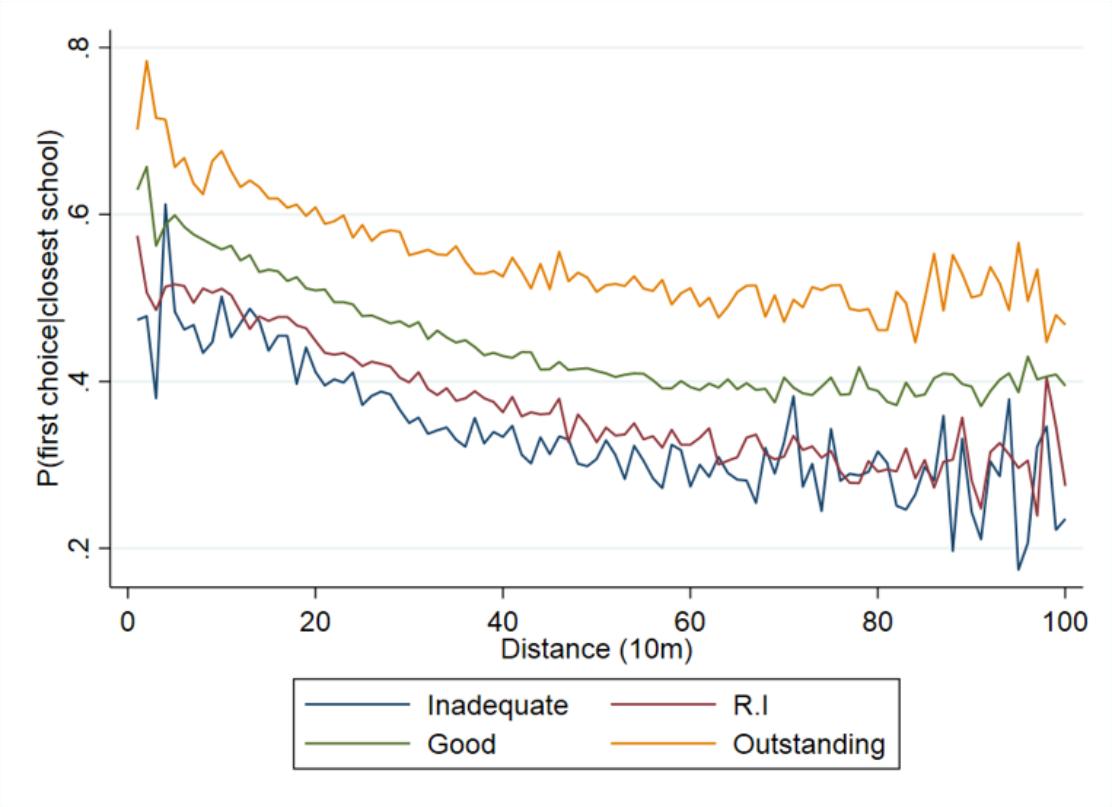
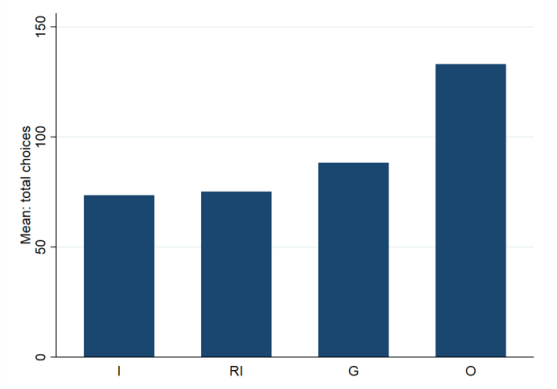
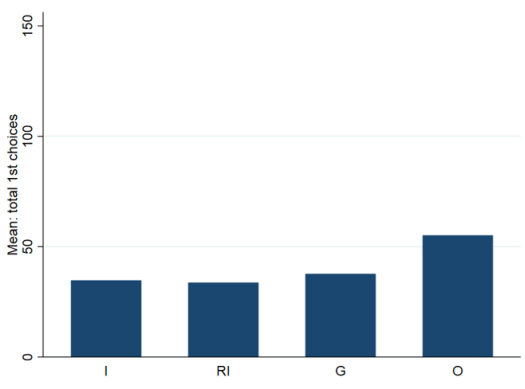


Figure 1: Percentage of households choosing their closest school as first choice, by Ofsted rating and distance to the school

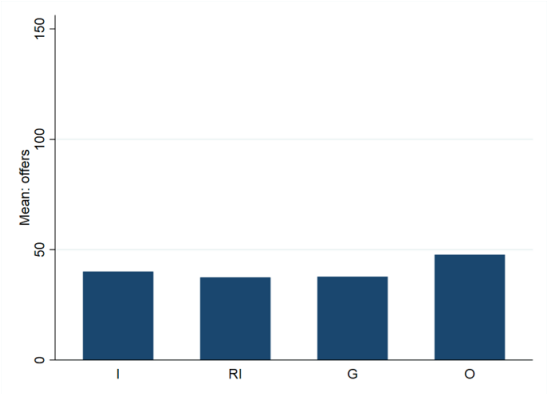
Source: Ofsted management information and National data on school choices/preferences linked to the National Pupil Database, provided by the Department for Education.



(a) Total choices



(b) Total first choices

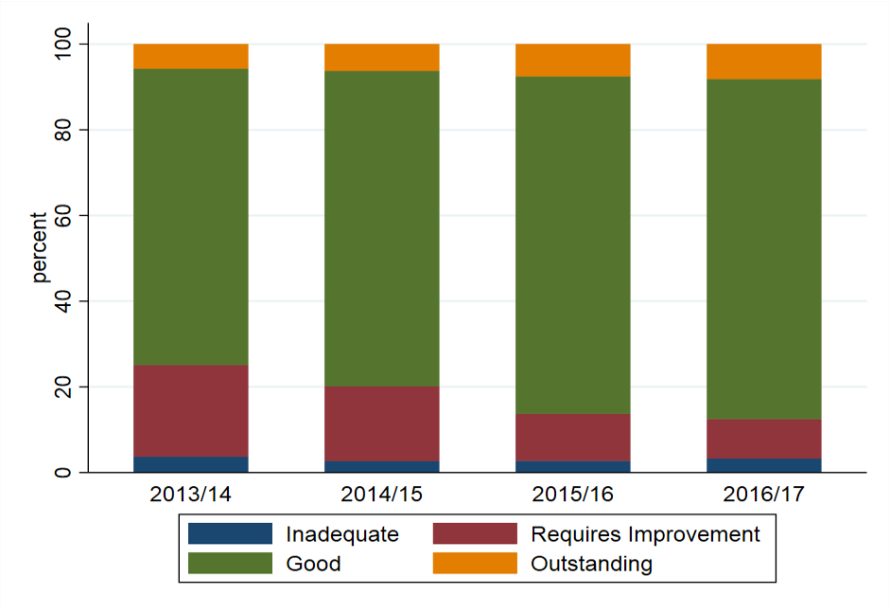


(c) Total offers

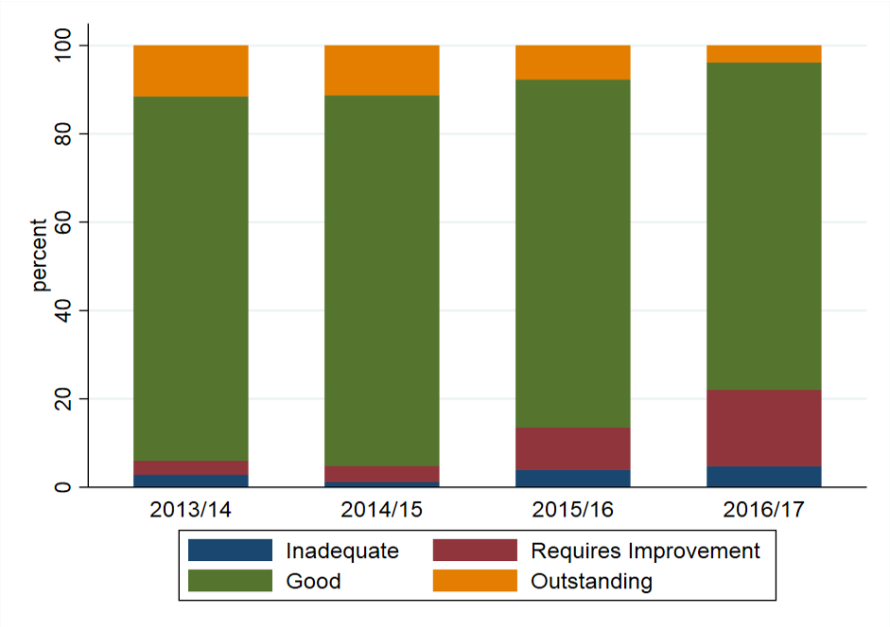
Figure 2: Mean school-level dependent variables

Source: Ofsted management information and National data on school choices/preferences linked to the National Pupil Database, provided by the Department for Education.

Note: "Total choices" is the total number of school choices that a school receives, from first to third or sixth (depending on the number permitted by the Local Authority). "Total first choices" is the total number of school choices where the school is first choice. "Total offers" is the total number of school places the school offers, resulting from the choices made by parents, the schools' capacity constraints and over-subscription criteria, and the assignment mechanism run by the Local Authority.



(a) Stock



(b) Ofsted rating in each year

Figure 3: Ofsted rating over time

Source: Ofsted management information and National data on school choices/preferences linked to the National Pupil Database, provided by the Department for Education.

Note: Panel (a) shows the stock of Ofsted ratings in each year, for all schools in England. Panel (b) shows the Ofsted ratings awarded in each year, for the final sample of schools in our sample.

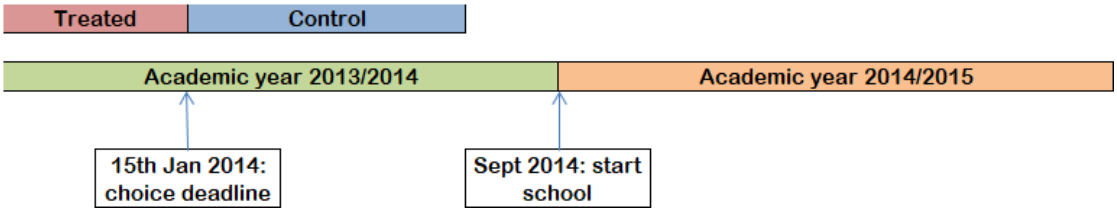
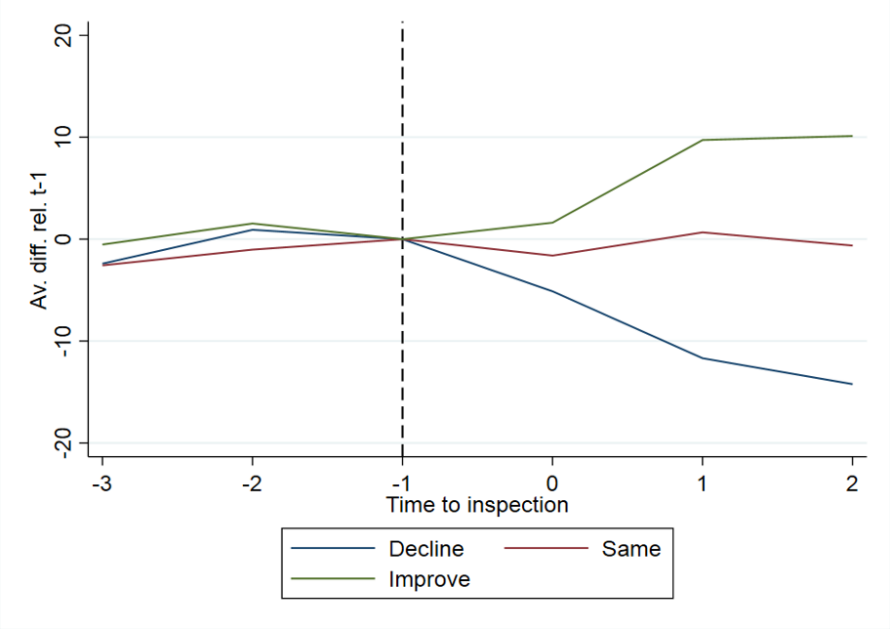
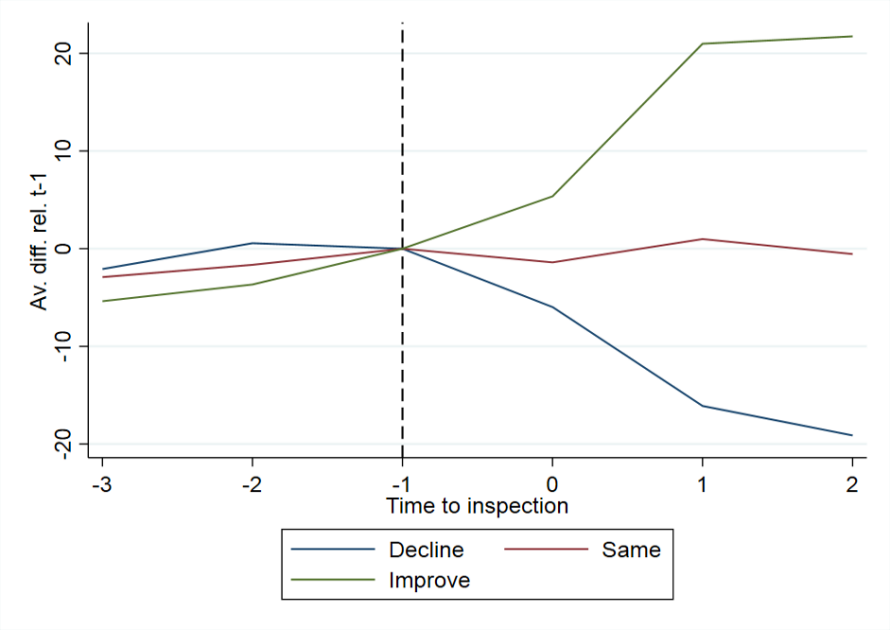


Figure 4: Research design for one cohort of pupils

Note: Schools are inspected across the school year (September to July). Treated schools are those that are inspected early, so that their Ofsted inspection rating is released before the school choice deadline on 15th January. Control schools are those that are inspected late, so that the information from Ofsted is revealed only after the school choice deadline.



(a) All schools

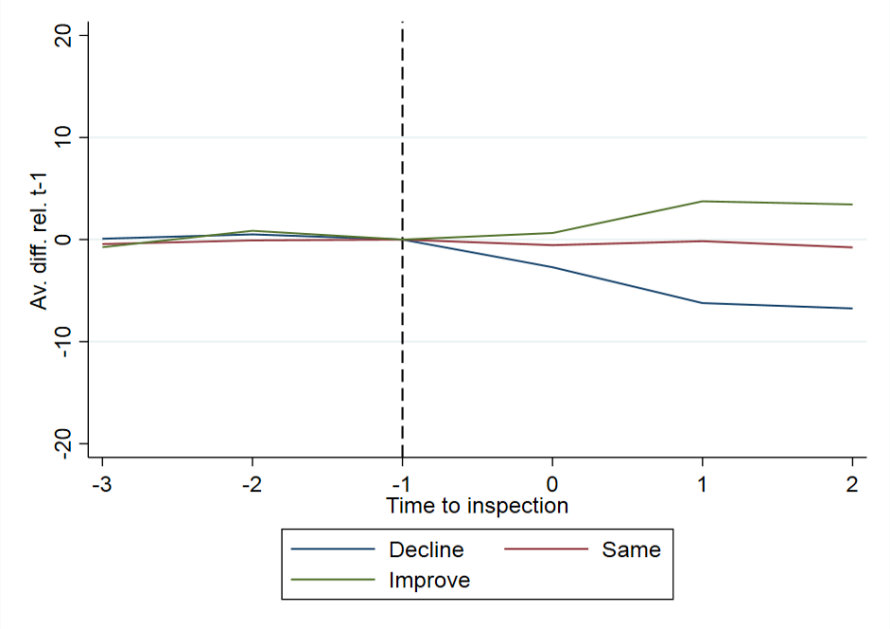


(b) Previously rated 'Good'

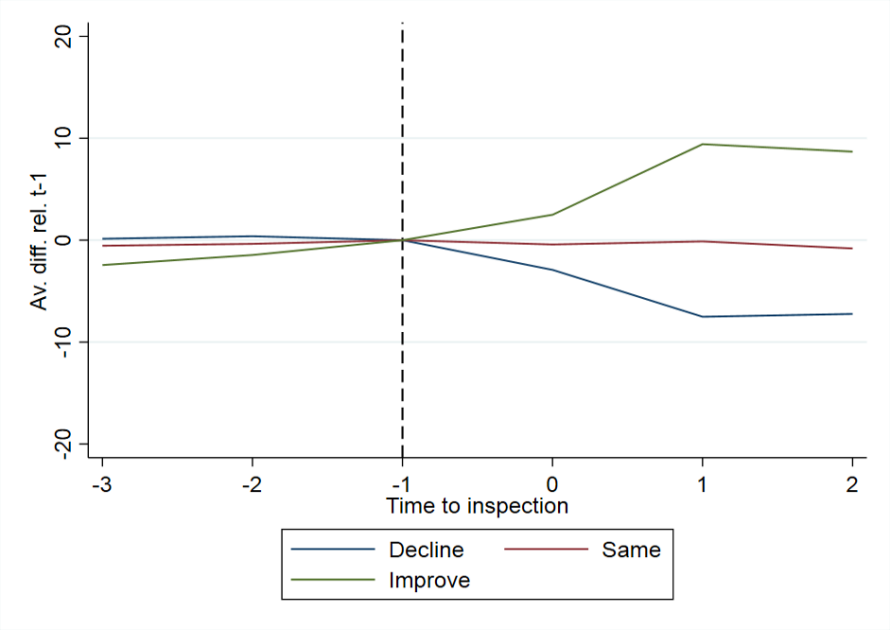
Figure 5: Common trends in total choices per school

Source: Ofsted management information and National data on school choices/preferences linked to the National Pupil Database, provided by the Department for Education.

Note: Year 0 is the year of inspection, year 1 is the year following the inspection, and so on. Each line shows the average school-level difference in the dependent variable relative to year 0.



(a) All schools

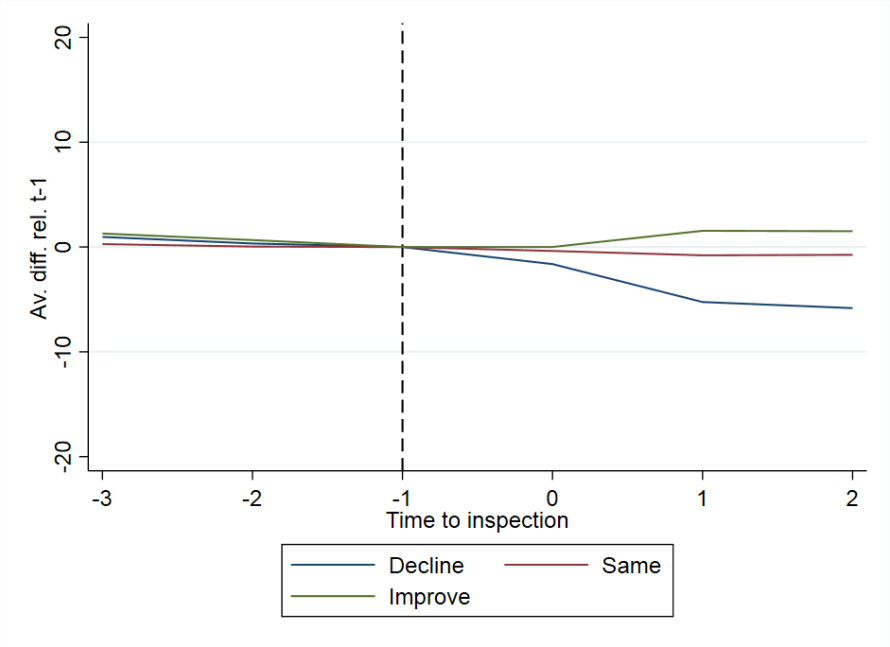


(b) Previously rated 'Good'

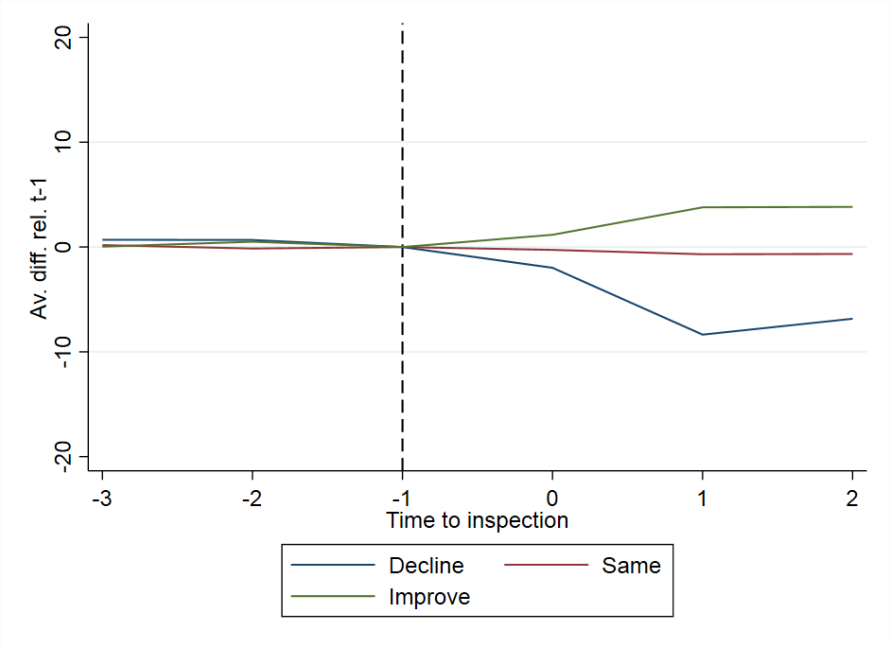
Figure 6: Common trends in total first choices per school

Source: Ofsted management information and National data on school choices/preferences linked to the National Pupil Database, provided by the Department for Education.

Note: Year 0 is the year of inspection, year 1 is the year following the inspection, and so on. Each line shows the average school-level difference in the dependent variable relative to year 0.



(a) All schools

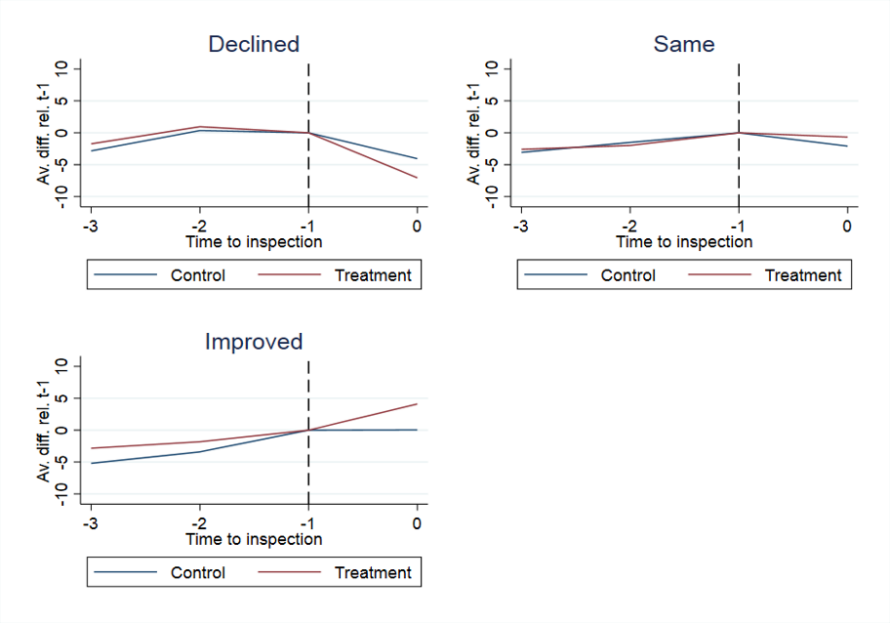


(b) Previously rated 'Good'

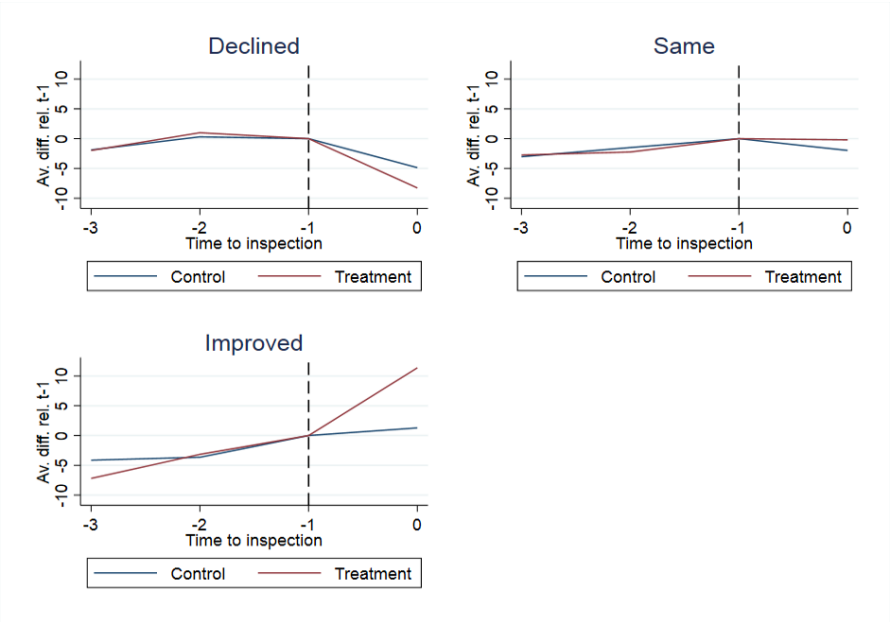
Figure 7: Common trends in being chosen as the first choice if nearest

Source: Ofsted management information and National data on school choices/preferences linked to the National Pupil Database, provided by the Department for Education.

Note: Year 0 is the year of inspection, year 1 is the year following the inspection, and so on. Each line shows the average school-level difference in the dependent variable relative to year 0.



(a) All schools

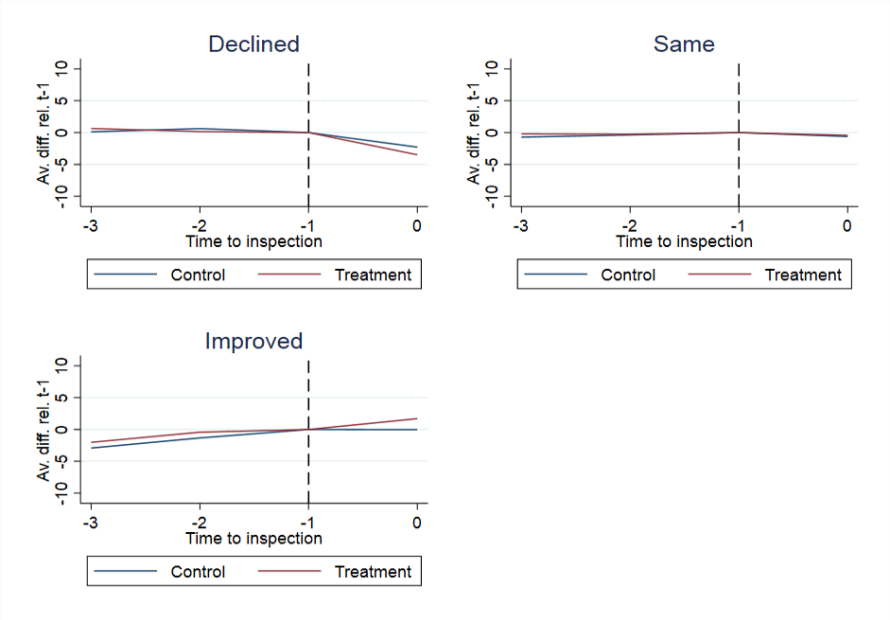


(b) Previously rated 'Good'

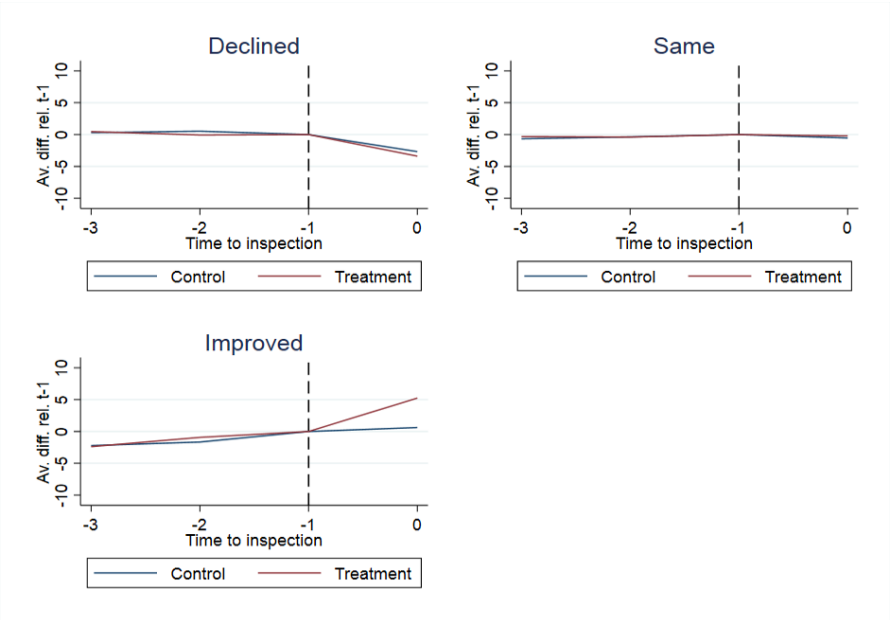
Figure 8: Common trends in total choices per school

Source: Ofsted management information and National data on school choices/preferences linked to the National Pupil Database, provided by the Department for Education.

Note: Year 0 is the year of inspection, year -1 is the year prior to the inspection, and so on. Each line shows the average school-level difference in the dependent variable relative to year 0. 'Control' refers to schools inspected after the school choice deadline. 'Treatment' refers to schools inspected before the school choice deadline.



(a) All schools

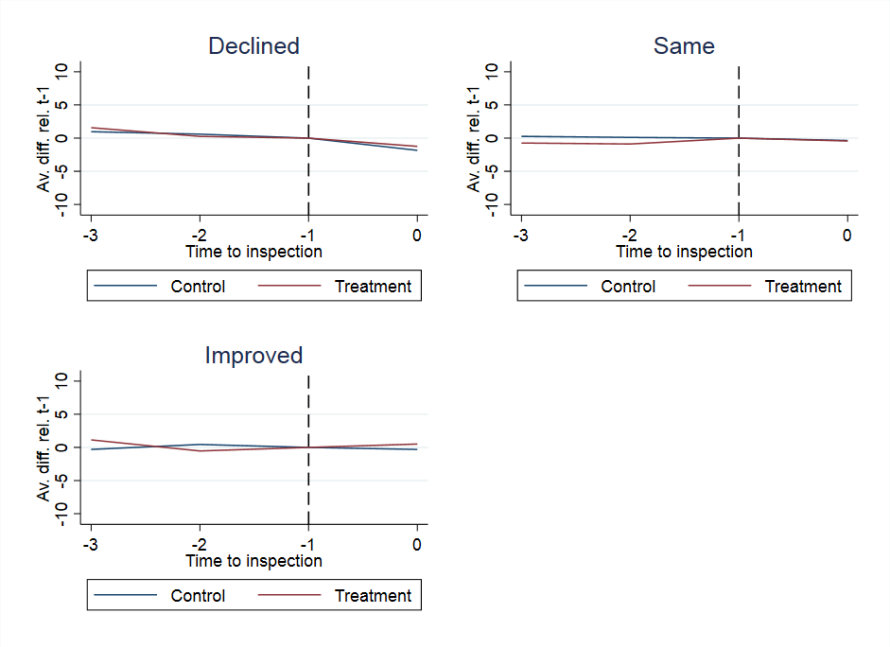


(b) Previously rated 'Good'

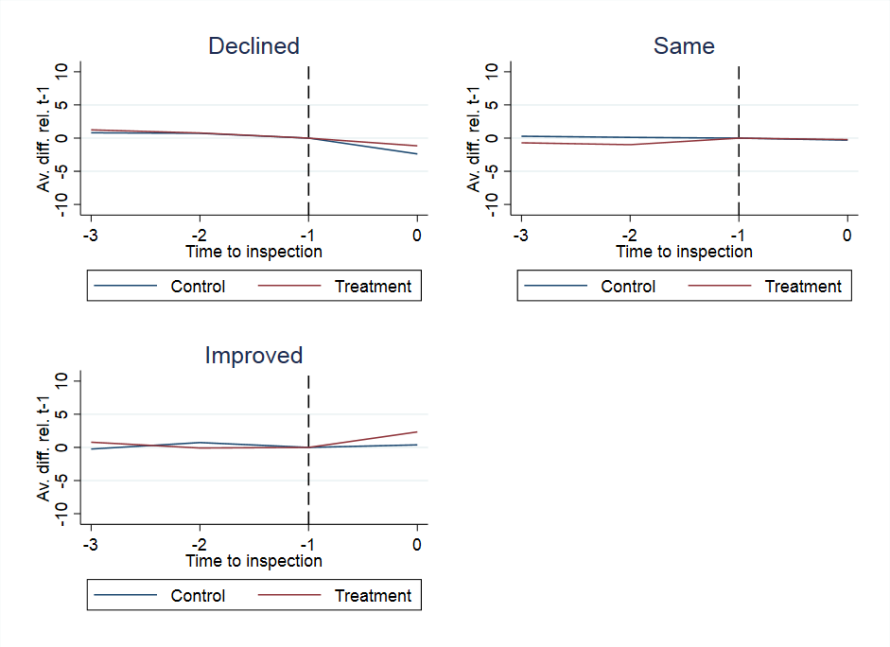
Figure 9: Common trends in total first choices per school

Source: Ofsted management information and National data on school choices/preferences linked to the National Pupil Database, provided by the Department for Education.

Note: Year 0 is the year of inspection, year -1 is the year prior to the inspection, and so on. Each line shows the average school-level difference in the dependent variable relative to year 0. 'Control' refers to schools inspected after the school choice deadline. 'Treatment' refers to schools inspected before the school choice deadline.



(a) All schools

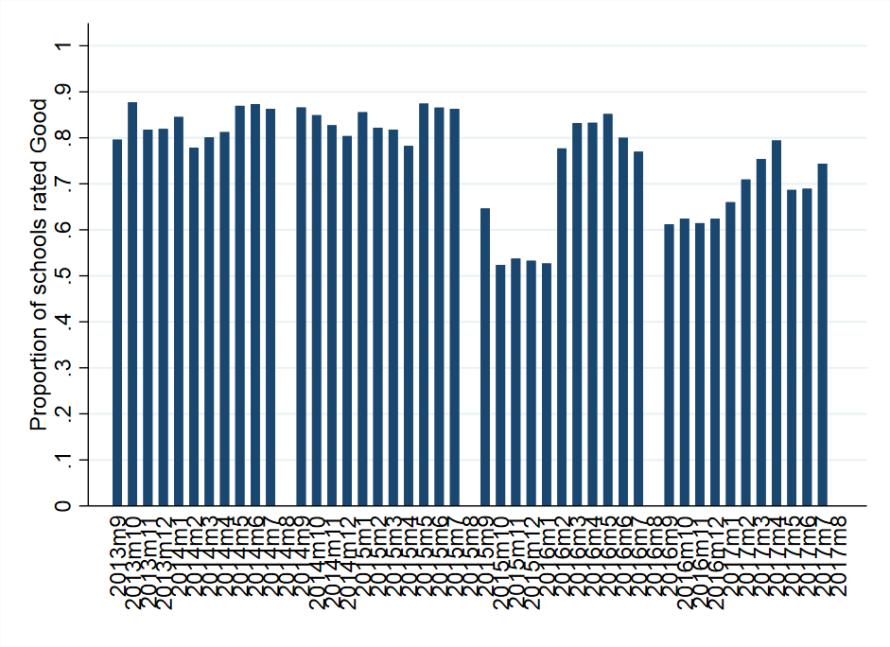


(b) Previously rated 'Good'

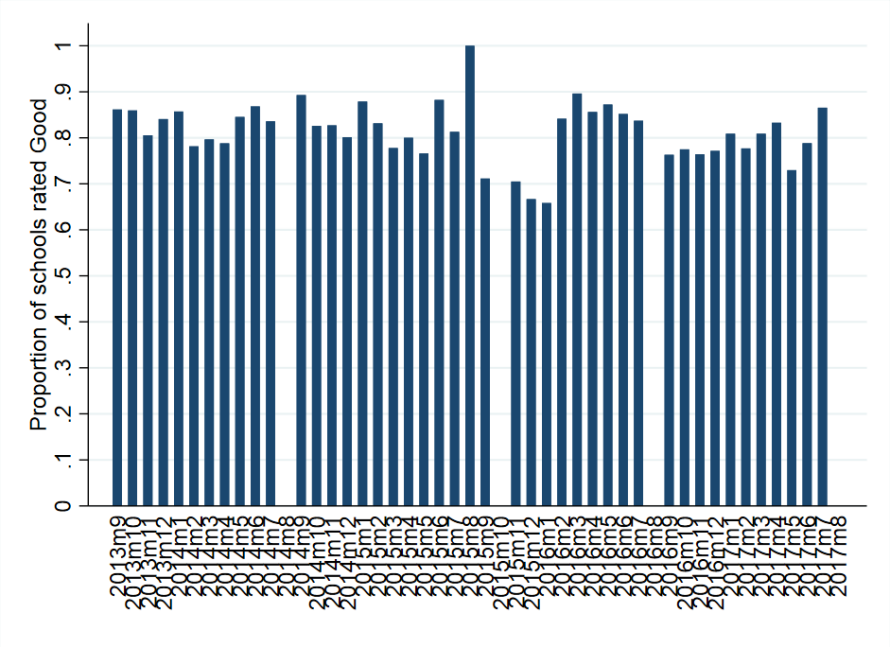
Figure 10: Common trends in total first choices per school

Source: Ofsted management information and National data on school choices/preferences linked to the National Pupil Database, provided by the Department for Education.

Note: Year 0 is the year of inspection, year -1 is the year prior to the inspection, and so on. Each line shows the average school-level difference in the dependent variable relative to year 0. 'Control' refers to schools inspected after the school choice deadline. 'Treatment' refers to schools inspected before the school choice deadline.



(a) All schools

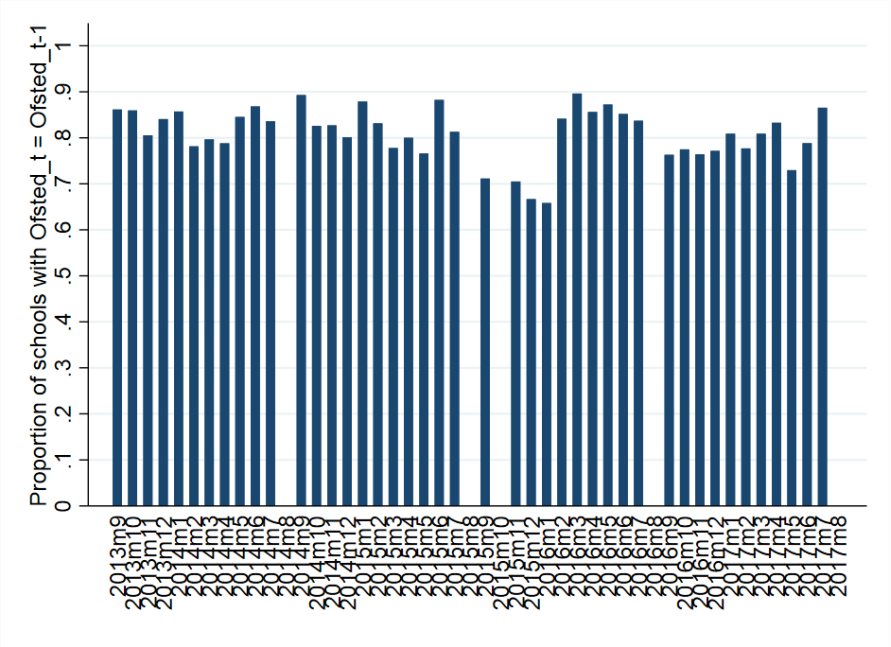


(b) Previously rated 'Good'

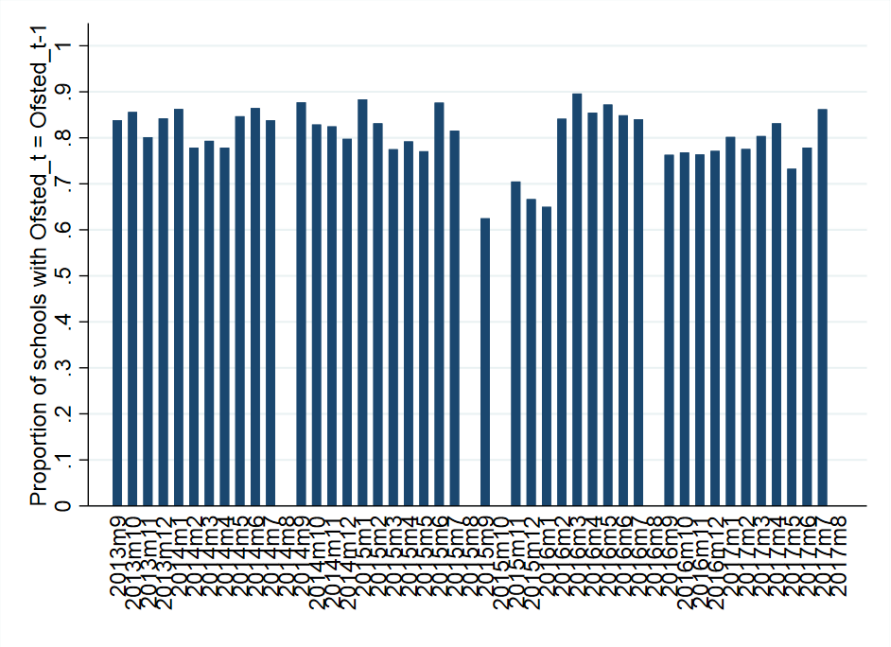
Figure 11: Ofsted rating over time within school years

Source: Ofsted management information and National data on school choices/preferences linked to the National Pupil Database, provided by the Department for Education.

Note: Figures show the proportion of schools receiving the Ofsted rating "Good" in each month in our sample period. Panel (a) is all schools in our final sample. Panel (b) is all schools in our final sample that were previously rated as "Good". October 2015 is suppressed due to small sample sizes for the sample of previously 'Good' schools.



(a) All schools



(b) Previously rated 'Good'

Figure 12: Change in Ofsted rating over time within school years

Source: Ofsted management information and National data on school choices/preferences linked to the National Pupil Database, provided by the Department for Education.

Note: Figures show the proportion of schools receiving the same Ofsted rating as their previous Ofsted rating in each month in our sample period. Panel (a) is all schools in our final sample. Panel (b) is all schools in our final sample that were previously rated as "Good". October 2015 is suppressed due to small sample sizes for the sample of previously 'Good' schools.

8 Tables

Table 1: Timing of Ofsted inspections

		Summary statistics				
Previous Ofsted rating	N	Mean	S.D.	25th	50th	75th
Inadequate	507	2.13	1.15	1	2	3
Requires Improvement	4,037	1.95	0.68	2	2	2
Good	6,438	4.4	0.88	4	5	5
Outstanding	580	5.37	1.85	4	5	7

Source: Ofsted management information.
Note: The sample is all schools inspected between school years 2014/2015 and 2017/2018.

Table 2: Pupil-level summary statistics

Variable	2014	2015	2016	2017
Prob choose closest school	0.46 (0.50)	0.44 (0.50)	0.43 (0.49)	0.42 (0.49)
Prob choose closest school: closest outstanding	0.55 (0.50)	0.51 (0.50)	0.50 (0.50)	0.51 (0.50)
Prob choose closest school: closest good	0.47 (0.50)	0.47 (0.50)	0.49 (0.50)	0.44 (0.50)
Prob choose closest school: closest requires improvement	0.39 (0.49)	0.40 (0.49)	0.37 (0.48)	0.36 (0.48)
Prob choose closest school: closest inadequate	0.33 (0.47)	0.36 (0.48)	0.31 (0.46)	0.33 (0.47)
Offer from first choice school	0.90 (0.30)	0.89 (0.31)	0.89 (0.31)	0.91 (0.29)
Attend first choice school	0.87 (0.34)	0.86 (0.34)	0.87 (0.34)	0.88 (0.33)
FSM	0.15 (0.35)	0.14 (0.35)	0.13 (0.34)	0.14 (0.34)
EAL	0.18 (0.39)	0.19 (0.39)	0.19 (0.39)	0.19 (0.39)
Ethnic group: White British	0.71 (0.46)	0.69 (0.46)	0.69 (0.46)	0.68 (0.47)
Ethnic group: White Other	0.07 (0.25)	0.07 (0.26)	0.07 (0.26)	0.07 (0.26)
Ethnic group: Asian	0.10 (0.30)	0.11 (0.31)	0.11 (0.31)	0.10 (0.31)
Ethnic group: Black	0.05 (0.21)	0.05 (0.22)	0.05 (0.21)	0.05 (0.22)
Ethnic group: Chinese	0.01 (0.07)	0.00 (0.07)	0.00 (0.07)	0.00 (0.07)
Ethnic group: Mixed	0.06 (0.23)	0.06 (0.24)	0.06 (0.24)	0.07 (0.25)
Ethnic group: Other	0.02 (0.12)	0.02 (0.13)	0.02 (0.13)	0.02 (0.13)

Source: Ofsted management information and National data on school choices/preferences linked to the National Pupil Database, provided by the Department for Education.
Note: Standard deviation in brackets. Sample is all schools in our final sample that are treated or control in the relevant inspection year. "FSM" refers to free school meals, a marker of pupil economic disadvantage. "EAL" refers to English as an additional language.

Table 3: Transition matrix

	Overall effectiveness			
Previous Ofsted rating	Inadequate	Requires improvement	Good	Outstanding
Inadequate	0.07	0.25	0.61	0.07
Requires improvement	0.05	0.12	0.80	0.03
Good	0.03	0.07	0.81	0.09
Outstanding	0.04	0.06	0.65	0.26

Source: Ofsted management information.
Note: The sample is the final analysis sample that excludes schools inspected in adjacent school years. This sample restriction excludes predominantly 'Inadequate' schools.

Table 4: School-level summary statistics

Variable	2014	2015	2016	2017
Panel (a): Ofsted rating				
Outstanding	11.5	11.3	7.7	3.8
Good	82.5	83.9	78.9	74.2
Requires Improvement	3.2	3.7	9.5	17.2
Inadequate	2.8	1.2	4.0	4.8
Panel (b): Treatment				
Control	55.6	54.3	71.6	67.6
Treatment	44.4	45.7	28.4	32.4
Panel (c): Changes				
Decline	11.7	8.2	6.5	15.8
Same	52.6	49.1	47.4	63.6
Improve	35.7	42.8	46.1	20.7

Source: Ofsted management information.

Note: The sample is the final analysis sample that excludes schools inspected in adjacent school years. This sample restriction excludes predominantly 'Inadequate' schools.

Table 5: Correlation between Ofsted rating and observable school characteristics

	(1)	(2)
	Ofsted rating	Ofsted rating
Inadequate	-0.307***	-0.076**
	(0.035)	(0.037)
Requires Improvement	-0.155***	-0.023
	(0.014)	(0.016)
Outstanding	0.044	0.036
	(0.034)	(0.036)
L1. KS2 (std.)		0.201***
		(0.009)
L2. KS2 (std.)		0.091***
		(0.009)
L3. KS2 (std.)		0.025***
		(0.009)
L4. KS2 (std.)		0.004
		(0.008)
L1. % FSM (std.)		-0.043
		(0.043)
L2. % FSM (std.)		0.024
		(0.058)
L3. % FSM (std.)		0.043
		(0.040)
L1. % EAL (std.)		0.049
		(0.052)
L2. % EAL (std.)		-0.361***
		(0.074)
L3. % EAL (std.)		0.360***
		(0.052)
L1. N pupils (std.)		-0.002
		(0.014)
L2. N pupils (std.)		-0.047***
		(0.018)
L3. N pupils (std.)		0.038***
		(0.014)
Religious		-0.052***
		(0.014)
Infant		-0.076
		(0.120)
N	7,832	4,999
r2	0.02	0.23

Table 6: Balance across treatment and control schools: Observable characteristics in the year of, and prior to, inspection

Panel (a): All schools in final sample			
		All years	
Variable (lag 0)	Treatment	Control	Difference
KS2 (std.)	-0.02 (0.95)	-0.05 (0.96)	0.03
% EAL (std.)	0.05 (1.01)	-0.02 (0.99)	0.06***
% FSM (std.)	0.07 (1.00)	0.06 (1.00)	0.01
Infant school	0.12 (0.33)	0.12 (0.32)	0.01
Religious	0.41 (0.49)	0.43 (0.49)	-0.02
Variable (lag 1)			
KS2 (std.)	0.04 (0.91)	-0.05 (0.94)	0.09***
% EAL (std.)	0.05 (1.01)	-0.02 (1.00)	0.06**
% FSM (std.)	0.07 (1.01)	0.06 (1.00)	0.01
Infant school	0.12 (0.33)	0.12 (0.32)	0.01
Religious	0.41 (0.49)	0.43 (0.49)	-0.02
Panel (b): Schools in final sample previously 'Good'			
		All years	
Variable (lag 0)	Treatment	Control	Difference
KS2 (std.)	0.12 (0.91)	0.04 (0.94)	0.08**
% EAL (std.)	-0.03 (0.96)	-0.06 (0.98)	0.02
% FSM (std.)	-0.10 (0.96)	-0.07 (0.96)	-0.03
Infant school	0.16 (0.37)	0.14 (0.35)	0.02*
Religious	0.42 (0.49)	0.44 (0.50)	-0.02
Variable (lag 1)			
KS2 (std.)	0.21 (0.88)	0.05 (0.94)	0.16***
% EAL (std.)	-0.03 (0.96)	-0.05 (0.99)	0.02
% FSM (std.)	-0.10 (0.97)	-0.06 (0.96)	-0.04
Infant school	0.16 (0.37)	0.14 (0.35)	0.02*
Religious	0.42 (0.49)	0.44 (0.50)	-0.02

Source: Ofsted management information and National data on school choices/preferences linked to the National Pupil Database, provided by the Department for Education.

Note: The table shows group means, with standard deviations in parentheses. * p<0.1, ** p<0.05, *** p<0.01. The sample is the final analysis sample that excludes schools inspected in adjacent school years. This sample restriction excludes predominantly 'Inadequate' schools. The table tests the balance of school-level covariates across treatment and control groups in each school year, and jointly, in the year of inspection and the year prior to inspection. 'KS2 (std.)' is the test score performance of primary schools, standardised within school year to have a mean of 0 and standard deviation of 1. '% EAL (std.)' is the percentage of pupils with English as an Additional Language. '% FSM (std.) is the percentage of pupils with Free School Meals. Both school-level variables are standardised within school year to have a mean of 0 and standard deviation of 1. 'Infant school' is a binary indicator equal to 1 if the school is ever recorded as being an infant school (educating pupils until age 7) and 0 otherwise. 'Religious' is a binary indicator equal to 1 if the school is ever recorded as having a religious denomination.

Table 7: Balance across treatment and control schools: Observable characteristics in the year of, and prior to, inspection

Panel (a): All schools in final sample									
	Year 2015/2016			Year 2016/2017			Year 2017/2018		
Variable (lag 0)	Treatment	Control	Diff.	Treatment	Control	Diff.	Treatment	Control	Diff.
KS2 (std.)	0.09 (0.91)	-0.01 (0.92)	0.11***	-0.04 (0.93)	-0.03 (0.96)	-0.01	-0.13 (0.99)	-0.09 (0.97)	-0.04
% EAL (std.)	0.08 (1.04)	-0.03 (0.97)	0.10**	0.03 (1.02)	-0.00 (0.99)	0.04	0.02 (0.98)	-0.02 (1.00)	0.04
% FSM (std.)	-0.01 (0.97)	0.09 (1.01)	-0.09**	0.01 (0.93)	0.02 (0.99)	-0.01	0.17 (1.05)	0.07 (1.00)	0.10***
Infant school	0.15 (0.35)	0.09 (0.28)	0.06***	0.12 (0.33)	0.16 (0.36)	-0.03*	0.11 (0.31)	0.11 (0.32)	-0.01
Religious	0.41 (0.49)	0.40 (0.49)	0.01	0.43 (0.50)	0.44 (0.50)	-0.01	0.40 (0.49)	0.43 (0.50)	-0.04**
Variable (lag 1)									
KS2 (std.)	0.21 (0.78)	-0.10 (0.93)	0.31***	-0.03 (1.02)	0.03 (0.90)	-0.06	-0.09 (0.95)	-0.07 (0.97)	-0.02
% EAL (std.)	0.09 (1.03)	-0.03 (0.98)	0.12***	0.04 (1.02)	-0.00 (1.00)	0.04	0.02 (0.98)	-0.02 (1.01)	0.03
% FSM (std.)	-0.01 (0.98)	0.08 (1.01)	-0.09**	0.00 (0.93)	0.02 (0.99)	-0.02	0.17 (1.05)	0.07 (0.99)	0.10***
Infant school	0.15 (0.35)	0.09 (0.28)	0.06***	0.12 (0.33)	0.16 (0.36)	-0.03*	0.11 (0.31)	0.11 (0.32)	-0.01
Religious	0.41 (0.49)	0.40 (0.49)	0.01	0.43 (0.50)	0.44 (0.50)	-0.01	0.40 (0.49)	0.43 (0.50)	-0.04**
Panel (b): Schools in final sample previously 'Good'									
	Year 2015/2016			Year 2016/2017			Year 2017/2018		
Variable (lag 0)	Treatment	Control	Diff.	Treatment	Control	Diff.	Treatment	Control	Diff.
KS2 (std.)	0.16 (0.85)	0.09 (0.87)	0.08	0.09 (0.89)	0.22 (0.93)	-0.12	0.08 (0.97)	-0.03 (0.96)	0.11**
% EAL (std.)	0.00 (0.98)	-0.02 (1.01)	0.02	0.02 (1.04)	-0.08 (0.98)	0.1	-0.10 (0.90)	-0.06 (0.98)	-0.04
% FSM (std.)	-0.07 (0.96)	0.03 (1.04)	-0.10*	-0.20 (0.88)	-0.26 (0.86)	0.06	-0.08 (0.99)	-0.03 (0.97)	-0.05
Infant school	0.18 (0.39)	0.10 (0.31)	0.08***	0.17 (0.38)	0.24 (0.43)	-0.07**	0.13 (0.33)	0.11 (0.32)	0.01
Religious	0.44 (0.50)	0.41 (0.49)	0.03	0.40 (0.49)	0.46 (0.50)	-0.06*	0.41 (0.49)	0.44 (0.50)	-0.04
Variable (lag 1)									
KS2 (std.)	0.31 (0.78)	-0.10 (0.95)	0.40***	0.20 (0.96)	0.31 (0.80)	-0.11*	0.12 (0.94)	0.01 (0.96)	0.10**
% EAL (std.)	0.02 (0.98)	-0.02 (1.02)	0.03	0.03 (1.03)	-0.07 (0.98)	0.1	-0.11 (0.89)	-0.06 (0.98)	-0.05
% FSM (std.)	-0.08 (0.97)	0.05 (1.04)	-0.13**	-0.20 (0.88)	-0.26 (0.86)	0.06	-0.08 (1.00)	-0.03 (0.96)	-0.06
Infant school	0.18 (0.39)	0.10 (0.31)	0.08***	0.17 (0.38)	0.24 (0.43)	-0.07**	0.13 (0.33)	0.11 (0.32)	0.01
Religious	0.44 (0.50)	0.41 (0.49)	0.03	0.40 (0.49)	0.46 (0.50)	-0.06*	0.41 (0.49)	0.44 (0.50)	-0.04

Source: Ofsted management information and National data on school choices/preferences linked to the National Pupil Database, provided by the Department for Education.

Note: The table shows group means, with standard deviations in parentheses. * p<0.1, ** p<0.05, *** p<0.01. The sample is the final analysis sample that excludes schools inspected in adjacent school years. This sample restriction excludes predominantly 'Inadequate' schools. The table tests the balance of school-level covariates across treatment and control groups in each school year, and jointly, in the year of inspection and the year prior to inspection. 'KS2 (std.)' is the test score performance of primary schools, standardised within school year to have a mean of 0 and standard deviation of 1. '% EAL (std.)' is the percentage of pupils with English as an Additional Language. '% FSM (std.) is the percentage of pupils with Free School Meals. Both school-level variables are standardised within school year to have a mean of 0 and standard deviation of 1. 'Infant school' is a binary indicator equal to 1 if the school is ever recorded as being an infant school (educating pupils until age 7) and 0 otherwise. 'Religious' is a binary indicator equal to 1 if the school is ever recorded as having a religious denomination.

Table 8: Ofsted rating by month of inspection

	(1) Month	(2) +Prev. rating	(3) +Time to prev. rating	(4) +School X	(5) !2016	(6) O. probit	(7) P(good)	(8) P(RI)
January	-0.071**	-0.065**	-0.057*	-0.040	-0.055*	-0.141	0.013	-0.024
	(0.031)	(0.031)	(0.030)	(0.026)	(0.030)	(0.087)	(0.025)	(0.019)
February	0.067**	0.055*	0.067**	0.056**	-0.001	-0.014	-0.013	0.011
	(0.030)	(0.030)	(0.030)	(0.026)	(0.030)	(0.088)	(0.025)	(0.019)
March	0.047*	0.029	0.045	0.036	-0.008	-0.038	-0.000	0.003
	(0.028)	(0.028)	(0.028)	(0.024)	(0.027)	(0.081)	(0.023)	(0.018)
April	0.114***	0.090***	0.114***	0.089***	0.051*	0.154*	0.015	-0.029
	(0.030)	(0.030)	(0.030)	(0.026)	(0.030)	(0.090)	(0.026)	(0.020)
May	0.056*	0.068**	0.099***	0.081***	0.041	0.146*	-0.018	-0.034*
	(0.030)	(0.030)	(0.030)	(0.026)	(0.029)	(0.087)	(0.025)	(0.019)
June	0.012	0.035	0.082***	0.045*	0.029	0.090	0.015	-0.029
	(0.030)	(0.030)	(0.030)	(0.026)	(0.030)	(0.089)	(0.026)	(0.020)
July	0.045	0.054*	0.103***	0.091***	0.094***	0.287***	0.041	-0.055***
	(0.030)	(0.030)	(0.030)	(0.026)	(0.031)	(0.091)	(0.026)	(0.020)
September	0.064**	0.107***	0.095***	0.105***	0.060*	0.207**	0.026	-0.050**
	(0.032)	(0.031)	(0.033)	(0.028)	(0.032)	(0.095)	(0.027)	(0.021)
October	0.012	0.010	0.002	0.003	-0.008	-0.024	0.027	-0.012
	(0.035)	(0.034)	(0.034)	(0.030)	(0.031)	(0.094)	(0.027)	(0.020)
November	-0.048	-0.036	-0.037	-0.031	-0.038	-0.092	0.004	-0.003
	(0.030)	(0.029)	(0.029)	(0.025)	(0.028)	(0.084)	(0.024)	(0.018)
N schools	7,815	7,815	7,815	7,815	5,934	5,934	5,934	5,934
R^2	0.06	0.09	0.10	0.32	0.31	0.23	0.12	0.14
F test: month (p-value)	0.00	0.00	0.00	0.00	0.00	0.00	0.45	0.00
F test: Nov-March (p-value)	0.00	0.00	0.00	0.00	0.21	0.42	0.63	0.43

Source: Ofsted management information and National data on school choices/preferences linked to the National Pupil Database, provided by the Department for Education.

Note: Standard errors in parentheses. * p<0.1, ** p<0.05, *** p<0.01. The sample is the final analysis sample that excludes schools inspected in adjacent school years. This sample restriction excludes predominantly 'Inadequate' schools. Column 1 controls for the year of inspection only. Column 2 additionally controls for the previous Ofsted rating. Column 3 additional controls for the time since previous inspection (in school years). Column 4 additionally controls for time varying school covariates (Standardised test scores (KSs), % English with an additional language, % Free School Meals (ever), binary variable for academy status, binary variable for ever recorded as an infant school, and a binary variable for ever recorded as a religious school. Column 5 excludes schools that were inspected in 2015/2016. Column 6 uses an ordered probit rather than ordinary least squares regression. Column 7 uses a binary dependent variable equal to one if the Ofsted rating was 'Good' and zero otherwise. Column 8 is a binary dependent variable equal to one if the Ofsted rating was 'Requires Improvement' and zero otherwise.

Table 9: Short-term (triple-differences) response to the revelation of Ofsted ratings

	Total choices	First choices	Choose closest
Ofsted	0.482***	0.633***	0.005***
	(0.289)	(0.157)	(0.002)
Ofsted * Early	4.227***	1.546***	0.010***
	(0.455)	(0.248)	(0.003)
Year F.E.	Y	Y	Y
School F.E.	Y	Y	Y
School covariates	N	N	N
Pupil covariates	N/A	N/A	N
N	24,627	24,599	850,406
N schools	7,828	7,828	7,823
Mean	84.76	36.65	0.43
R^2	0.03	0.02	0.15

Source: Ofsted management information and National data on school choices/preferences linked to the National Pupil Database, provided by the Department for Education.

Note: Standard errors in parentheses. * p<0.1, ** p<0.05, *** p<0.01. The sample is the final analysis sample that excludes schools inspected in adjacent school years. This sample restriction excludes predominantly 'Inadequate' schools.

Table 10: Short-term (triple-differences) response to the revelation of Ofsted ratings: Asymmetric response

	Total choices	First choices	Choose closest
Decline * post	-1.772***	-1.879***	-0.014***
	(0.716)	(0.390)	(0.004)
Improve * post	-1.999***	-0.593**	-0.005
	(0.523)	(0.285)	(0.003)
Decline * post * Early	-4.248***	-0.824	-0.013*
	(1.229)	(0.669)	(0.007)
Improve * post * Early	5.755***	2.349***	0.013**
	(0.843)	(0.459)	(0.005)
Year F.E.	Y	Y	Y
School F.E.	Y	Y	Y
School covariates	N	N	N
Pupil covariates	N/A	N/A	N
N	24,627	24,599	850,406
N schools	7,828	7,828	7,823
Mean	84.76	36.65	0.43
R^2	0.03	0.01	0.15

Source: Ofsted management information and National data on school choices/preferences linked to the National Pupil Database, provided by the Department for Education.
Note: Standard errors in parentheses. * p<0.1, ** p<0.05, *** p<0.01. The sample is the final analysis sample that excludes schools inspected in adjacent school years. This sample restriction excludes predominantly 'Inadequate' schools.

Table 11: Short-term (triple-differences) response to the revelation of Ofsted ratings: Event study design

	Total choices	First choices	Choose closest
Decline * -3 * Early	-0.394	-0.925	0.014
	(1.775)	(0.968)	(0.011)
Decline * -2 * Early	0.319	-0.873	-0.008
	(1.604)	(0.874)	(0.010)
Decline * 0 * Early	-4.195***	-1.273*	-0.014*
	(1.418)	(0.773)	(0.008)
Improve * -3 * Early	-0.686	0.292	0.008
	(1.436)	(0.783)	(0.009)
Improve * -2 * Early	-2.666**	-0.667	0.003
	(1.134)	(0.619)	(0.007)
Improve * 0 * Early	4.789***	2.121***	0.015***
	(0.946)	(0.516)	(0.006)
N	24,627	24,599	850,406
N schools	7,828	7,828	7,823
Mean	84.77	36.65	0.43
R^2	0.03	0.02	0.15

Source: Ofsted management information and National data on school choices/preferences linked to the National Pupil Database, provided by the Department for Education.

Note: Standard errors in parentheses. * p<0.1, ** p<0.05, *** p<0.01. The sample is the final analysis sample that excludes schools inspected in adjacent school years. This sample restriction excludes predominantly 'Inadequate' schools. "Decline" ("Improve") denotes schools that receive a worse (better) Ofsted rating than in their previous inspection. "0" represents the year of Ofsted inspection, where schools could be inspected early (before the school choice deadline) or late (after the school choice deadline). "-2" is two years before the inspection year, and "-3" is three years before the inspection year. The reference group is schools that receive no change in their inspection in the year before the inspection year.

Table 12: Short-term (triple-differences) response to the revelation of Ofsted ratings: Robustness

Total choices	Main	Pupil X	School X	Doughnut	!Summer	2014/2015	2016/2017	Lag	Prior Good	Prior RI	!Religious	Placebo
Ofsted	0.482*		0.825**	0.221	0.803**	0.338	0.349	0.671*	1.903***	1.624***	0.466	-0.770**
	(0.289)		(0.357)	(0.300)	(0.314)	(0.599)	(0.333)	(0.354)	(0.523)	(0.460)	(0.421)	(0.386)
Ofsted*Early	4.227***		3.500***	4.694***	3.915***	4.245***	4.234***	3.529***	5.644***	4.953***	5.116***	0.959
	(0.455)		(0.565)	(0.485)	(0.472)	(0.870)	(0.527)	(0.563)	(0.863)	(0.680)	(0.672)	(0.598)
N	24,627		16,798	22,459	22,278	4,790	19,837	16,798	15,801	7,312	13,924	13,258
N schools	7,828		7,828	7,173	7,065	2,395	5,433	7,828	4,843	2,448	4,431	7,828
Mean	84.77		84.57	84.60	84.38	90.84	83.30	84.57	86.98	77.06	98.77	85.30
R^2	0.03		0.05	0.03	0.03	0.05	0.03	0.05	0.04	0.06	0.03	0.00
First choices	Main	Pupil X	School X	Doughnut	!Summer	2014/2015	2016/2017	Lag	Prior Good	Prior RI	!Religious	Placebo
Ofsted	0.633***		0.803***	0.513***	0.706***	0.341	0.689***	0.756***	1.600***	1.053***	0.700***	-0.065
	(0.157)		(0.201)	(0.164)	(0.170)	(0.342)	(0.180)	(0.195)	(0.291)	(0.243)	(0.225)	(0.221)
Ofsted*Early	1.546***		1.043***	1.742***	1.475***	1.965***	1.418***	0.997***	1.804***	1.890***	1.842***	-0.055
	(0.248)		(0.318)	(0.265)	(0.256)	(0.497)	(0.285)	(0.310)	(0.479)	(0.360)	(0.360)	(0.342)
N	24,599		16,780	22,438	22,253	4,790	19,809	16,763	15,781	7,304	13,913	13,247
N schools	7,828		7,828	7,173	7,065	2,395	5,433	7,826	4,843	2,448	4,431	7,828
Mean	36.65		36.45	36.61	36.42	39.26	36.02	36.48	37.02	34.55	42.02	36.85
R^2	0.02		0.03	0.02	0.02	0.02	0.02	0.07	0.02	0.05	0.02	0.00
Choose closest	Main	Pupil X	School X	Doughnut	!Summer	2014/2015	2016/2017	Lag	Prior Good	Prior RI	!Religious	Placebo
Ofsted	0.005***	0.005***	0.004**	0.007***	0.004**	-0.000	0.006***		0.011***	0.010***	0.008***	-0.002
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.004)	(0.002)		(0.003)	(0.003)	(0.002)	(0.002)
Ofsted*Early	0.010***	0.010***	0.010***	0.008***	0.013***	0.021***	0.007**		0.016***	0.010***	0.008**	0.001
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.006)	(0.003)		(0.005)	(0.004)	(0.003)	(0.004)
N	850,406	850,406	850,406	763,822	774,268	163,486	686,920		512,525	276,281	560,344	455,787
N schools	7,823	7,823	7,823	7,063	7,168	2,390	5,433		4,841	2,446	4,429	7,773
Mean	0.43	0.43	0.43	0.43	0.43	0.44	0.43		0.46	0.38	0.45	0.43
R^2	0.15	0.16	0.16	0.15	0.15	0.17	0.15		0.15	0.13	0.13	0.17

Source: Ofsted management information and National data on school choices/preferences linked to the National Pupil Database, provided by the Department for Education.

Note: Standard errors in parentheses. * p<0.1, ** p<0.05, *** p<0.01. The sample is the final analysis sample that excludes schools inspected in adjacent school years. This sample restriction excludes predominantly 'Inadequate' schools. 'Pupil X' additionally controls for pupil-level covariates: free school meals (FSM) eligibility; English as an additional language (EAL); major ethnic group. 'School X' additionally controls for school-level covariates: first, second and third lags of prior-attainment at KS2; first lag of number of pupils; first lag of the percentage of pupils with EAL; first lag of the percentage of pupils with FSM (ever); first lag of academy school status. 'Doughnut' uses a doughnut design, excluding the month of the school choice deadline (January). '!Summer' excludes schools where the inspection report was released in July and August. '2014/2015' uses years before the change to the school inspection regime, while '2016/2017' uses only years after the change. 'Lag' includes a lagged dependent variable. 'Prior Good' and 'Prior RI' include only the schools previously 'Good' or 'Requires Improvement', respectively, in the sample. '!Religious' excludes schools with a religious denomination. 'Placebo' artificially shifts the Ofsted inspection one year before the actual inspection.

Table 13: Short-term (triple-differences) response to the revelation of Ofsted ratings: Heterogeneity in the probability of choosing the closest school as first choice

Pupil char.	FSM=0	FSM=1	EAL=0	EAL=1	White=0	White=1	
Ofsted	0.006***	0.008*	0.004**	0.013***	0.007**	0.005**	
	(0.002)	(0.004)	(0.002)	(0.004)	(0.003)	(0.002)	
Ofsted*Early	0.010***	0.011	0.012***	0.001	0.006	0.011***	
	(0.003)	(0.007)	(0.003)	(0.006)	(0.005)	(0.003)	
N	730,328	120,078	689,736	160,670	262,475	587,931	
N schools	7,823	6,897	7,822	6,065	7,101	7,789	
Mean	0.43	0.43	0.43	0.41	0.39	0.45	
R^2	0.17	0.19	0.17	0.16	0.14	0.18	
Distance	0-200m	200-400m	400-600m	600-800m	800-1000m		
Ofsted	0.014***	0.006**	0.003	0.001	-0.011		
	(0.004)	(0.003)	(0.003)	(0.004)	(0.008)		
Ofsted*Early	-0.001	0.010**	0.011**	0.014**	0.019*		
	(0.007)	(0.004)	(0.005)	(0.007)	(0.012)		
N	135,066	296,956	218,524	104,876	41,271		
N schools	7,316	7,417	7,071	5,833	3,962		
Mean	0.52	0.45	0.40	0.37	0.37		
R^2	0.22	0.19	0.21	0.27	0.34		
FSM quintile	Lowest	2nd	3rd	4th	Highest		
Ofsted	0.005	-0.005	-0.004	-0.005	0.012***		
	(0.007)	(0.006)	(0.005)	(0.004)	(0.003)		
Ofsted*Early	0.012	0.021**	0.019**	0.026***	-0.007		
	(0.011)	(0.009)	(0.008)	(0.006)	(0.005)		
N	91,357	128,358	165,696	213,432	221,716		
N schools	1,957	2,437	2,417	2,251	1,863		
Mean	0.56	0.53	0.46	0.39	0.33		
R^2	0.22	0.19	0.15	0.10	0.09		
KS2 quintile	Lowest	2nd	3rd	4th	Highest	Infant	Primary
Ofsted	0.007*	0.009	-0.003	0.011	-0.009	-0.001	0.006***
	(0.004)	(0.006)	(0.007)	(0.010)	(0.011)	(0.005)	(0.002)
Ofsted*Early	-0.000	0.015	0.024**	0.006	0.045***	0.024***	0.008***
	(0.007)	(0.010)	(0.012)	(0.015)	(0.014)	(0.008)	(0.003)
N	177,625	162,127	155,643	127,502	78,635	125,474	724,932
N schools	2,916	3,239	3,204	2,900	2,060	921	6,902
Mean	0.37	0.40	0.43	0.46	0.45	0.52	0.41
R^2	0.13	0.15	0.17	0.19	0.21	0.12	0.15

Source: Ofsted management information and National data on school choices/preferences linked to the National Pupil Database, provided by the Department for Education.

Note: Standard errors in parentheses. * p<0.1, ** p<0.05, *** p<0.01. The sample is the final analysis sample that excludes schools inspected in adjacent school years. This sample restriction excludes predominantly 'Inadequate' schools. "FSM" refers to free school meals, a marker of pupil economic disadvantage. "EAL" refers to English as an additional language. "White" refers to White British pupils. Distance bands are distance between the closest school and home, in metres. KS2 quintile splits primary schools in five bands according to test scores taken at the end of primary school (KS2). Infant schools are excluded from these bands as they teach pupils to KS1 only.

Table 14: Short-term (triple-differences) response to the revelation of Ofsted ratings: Heterogeneity in total choices

FSM quintile	Lowest	2nd	3rd	4th	Highest		
Ofsted	0.163	-0.486	0.786	-0.387	1.167**		
	(0.919)	(0.970)	(0.871)	(0.803)	(0.567)		
Ofsted*Early	3.360**	6.206***	1.560	7.213***	1.415		
	(1.429)	(1.564)	(1.447)	(1.261)	(0.942)		
N	4,164	4,654	4,862	5,004	5,043		
N schools	1,969	2,492	2,523	2,380	1,923		
Mean	76.30	85.71	90.51	95.49	78.42		
R^2	0.02	0.03	0.04	0.04	0.03		
KS2 quintile	Lowest	2nd	3rd	4th	Highest	Infant	Primary
Ofsted	0.993	1.365	-0.596	-0.177	1.698	-0.654	0.610**
	(0.678)	(1.101)	(1.328)	(1.438)	(1.291)	(1.028)	(0.300)
Ofsted*Early	2.777**	1.083	6.101***	7.893***	5.136***	3.721**	4.262***
	(1.164)	(1.758)	(2.087)	(2.142)	(1.890)	(1.668)	(0.471)
N	4,480	4,602	4,357	4,098	3,229	2,881	21,746
N schools	2,756	3,313	3,226	2,970	2,175	921	6,907
Mean	67.97	84.41	91.78	92.32	83.41	107.02	81.82
R^2	0.05	0.04	0.04	0.05	0.05	0.03	0.03
Market position	Popular	Not popular	Low FSM	High FSM	High KS2	Low KS2	
Ofsted	0.595*	0.394	0.862**	-0.113	1.475***	-0.815	
	(0.359)	(0.536)	(0.379)	(0.52)	(0.386)	(0.564)	
Ofsted*Early	3.165***	5.695***	3.808***	5.009***	3.302***	5.832***	
	(0.567)	(0.842)	(0.603)	(0.812)	(0.615)	(0.866)	
N	13,049	9,325	13,084	8,978	12,016	7,225	
N schools	4,132	2,993	4,144	2,883	3,786	2,350	
Mean	71.98	106.46	81.63	95.41	82.32	90.18	
R^2	0.02	0.05	0.03	0.04	0.04	0.04	

Source: Ofsted management information and National data on school choices/preferences linked to the National Pupil Database, provided by the Department for Education.

Note: Standard errors in parentheses. * p<0.1, ** p<0.05, *** p<0.01. The sample is the final analysis sample that excludes schools inspected in adjacent school years. This sample restriction excludes predominantly 'Inadequate' schools. "FSM" refers to free school meals, a marker of pupil economic disadvantage. KS2 quintile splits primary schools in five bands according to test scores taken at the end of primary school (KS2). Infant schools are excluded from these bands as they teach pupils to KS1 only. Market position is according to the local education market. The local education market is derived by assigning school A and school B to the same market if at least 10% of pupils flow between school A's catchment area and school B, or vice versa. Columns show the split of schools into above or below median within the local education market. 'Popular' schools have the largest flows of pupils to the school from other schools' catchment areas.

Table 15: Short-term (triple-differences) response to the revelation of Ofsted ratings: Heterogeneity in first choices

FSM quintile	Lowest	2nd	3rd	4th	Highest		
Ofsted	0.606	0.456	0.253	0.671	0.927***		
	(0.524)	(0.509)	(0.476)	(0.422)	(0.317)		
Ofsted*Early	1.554*	2.536***	0.866	2.478***	0.500		
	(0.816)	(0.821)	(0.791)	(0.663)	(0.527)		
N	4,156	4,651	4,856	4,997	5,040		
N schools	1,967	2,491	2,521	2,376	1,923		
Mean	31.12	36.21	39.03	40.96	36.43		
R^2	0.02	0.02	0.02	0.02	0.02		
KS2 quintile	Lowest	2nd	3rd	4th	Highest	Infant	Primary
Ofsted	0.832**	1.181**	0.513	0.014	1.732**	0.162	0.672***
	(0.393)	(0.579)	(0.707)	(0.791)	(0.702)	(0.592)	(0.162)
Ofsted*Early	1.665**	0.029	1.339	2.729**	1.278	1.605*	1.534***
	(0.675)	(0.925)	(1.112)	(1.178)	(1.027)	(0.960)	(0.254)
N	4,474	4,601	4,357	4,096	3,228	2,880	21,719
N schools	2,751	3,312	3,226	2,968	2,174	921	6,907
Mean	32.03	37.02	38.60	37.73	32.81	49.72	34.92
R^2	0.04	0.02	0.01	0.02	0.02	0.02	0.02
Market position	Popular	Not popular	Low FSM	High FSM	High KS2	Low KS2	
Ofsted	0.587***	0.701**	0.771***	0.41	0.917***	0.434	
	(0.194)	(0.289)	(0.204)	(0.283)	(0.206)	(0.31)	
Ofsted*Early	1.046***	2.183***	1.032***	2.307***	1.209***	1.804***	
	(0.307)	(0.454)	(0.323)	(0.441)	(0.328)	(0.476)	
N	13,024	9,325	13,069	8,971	12,008	7,222	
N schools	4,132	2,993	4,144	2,883	3,786	2,350	
Mean	30.06	46.45	35.10	40.45	34.76	38.16	
R^2	0.01	0.02	0.01	0.03	0.02	0.02	

Source: Ofsted management information and National data on school choices/preferences linked to the National Pupil Database, provided by the Department for Education.

Note: Standard errors in parentheses. * p<0.1, ** p<0.05, *** p<0.01. The sample is the final analysis sample that excludes schools inspected in adjacent school years. This sample restriction excludes predominantly 'Inadequate' schools. "FSM" refers to free school meals, a marker of pupil economic disadvantage. KS2 quintile splits primary schools in five bands according to test scores taken at the end of primary school (KS2). Infant schools are excluded from these bands as they teach pupils to KS1 only. Market position is according to the local education market. The local education market is derived by assigning school A and school B to the same market if at least 10% of pupils flow between school A's catchment area and school B, or vice versa. Columns show the split of schools into above or below median within the local education market. 'Popular' schools have the largest flows of pupils to the school from other schools' catchment areas.

Table 16: Medium-term (difference-in-differences) response to the revelation of Ofsted ratings

	Event study			Levels			Changes		
	Total choices	First choices	Choose closest	Total choices	First choices	Choose closest	Total choices	First choices	Choose closest
Ofsted				9.349***	4.034***	0.025***			
				(0.392)	(0.201)	(0.002)			
Decline * post							-13.799***	-6.191***	-0.035***
							(1.121)	(0.574)	(0.006)
Improve * post							8.793***	3.747***	0.025***
							(0.590)	(0.302)	(0.003)
Decline * -3	0.112	0.275	0.008						
	(0.859)	(0.459)	(0.005)						
Decline * -2	1.725**	0.519	0.011**						
	(0.789)	(0.421)	(0.004)						
Decline * 0	-2.764***	-1.951***	-0.014***						
	(0.717)	(0.383)	(0.004)						
Decline * 1	-12.405***	-6.073***	-0.031***						
	(1.055)	(0.563)	(0.006)						
Decline * 2	-13.502***	-6.014***	-0.031***						
	(1.317)	(0.704)	(0.007)						
Improve * -3	4.568***	1.166***	0.011***						
	(0.734)	(0.392)	(0.004)						
Improve * -2	2.816***	1.044***	0.010***						
	(0.567)	(0.303)	(0.003)						
Improve * 0	1.693***	0.761***	0.005*						
	(0.485)	(0.259)	(0.003)						
Improve * 1	8.787***	3.666***	0.025***						
	(0.577)	(0.308)	(0.003)						
Improve * 2	10.994***	4.268***	0.031***						
	(0.737)	(0.393)	(0.004)						
N	31,320	31,280	1,108,000	12,848	12,833	464,374	12,848	12,833	464,374
N schools	7,837	7,837	7,837	4,287	4,287	4,286	4,287	4,287	4,286
Mean	86.23	37.20	0.43	89.52	38.58	0.44	89.52	38.58	0.44
R^2	0.06	0.03	0.15	0.09	0.06	0.15	0.08	0.05	0.15

Source: Ofsted management information and National data on school choices/preferences linked to the National Pupil Database, provided by the Department for Education.

Note: Standard errors in parentheses. * p<0.1, ** p<0.05, *** p<0.01. The sample is the final analysis sample that excludes schools inspected in adjacent school years. This sample restriction excludes predominantly 'Inadequate' schools. "FSM" refers to free school meals, a marker of pupil economic disadvantage. KS2 quintile splits primary schools in five bands according to test scores taken at the end of primary school (KS2). Infant schools are excluded from these bands as they teach pupils to KS1 only. The event study specification includes event time dummies. All specifications include school and year fixed-effects.

Table 17: Short-term (triple-differences) and medium-term (difference-in-differences) response of the number of school placed offered to the revelation of Ofsted ratings

					Quintiles of pupils to school places				
Offers	DIDID	DIDID	DID	DID	Q1 (fewest)	Q2	Q3	Q4	Q5 (most)
Ofsted	0.174	0.280*	1.521***	1.491***	-0.192	-0.201	-0.088	0.141	0.149
	(0.126)	(0.162)	(0.156)	(0.162)	(0.262)	(0.332)	(0.277)	(0.372)	(0.367)
Ofsted*early	0.787***	0.825***			0.450	1.538***	1.312***	1.086*	0.720
	(0.199)	(0.257)			(0.387)	(0.526)	(0.439)	(0.582)	(0.603)
Year F.E.	Y	Y	Y	Y	Y	Y	Y	Y	Y
School F.E.	Y	Y	Y	Y	Y	Y	Y	Y	Y
School covariates	N	Y	N	Y	N	N	N	N	N
Pupil covariates	N/A	N/A	N/A	N/A	N/A	N/A	N/A	N/A	N/A
N	24,604	16,783	12,836	12,706	4,064	4,557	7,385	4,940	3,658
N schools	7,828	7,828	4,287	4,287	1,621	2,429	3,687	2,830	1,798
Mean	37.99	37.89	39.41	39.37	18.11	33.70	39.87	47.48	48.81
R^2	0.02	0.03	0.02	0.03	0.01	0.02	0.02	0.03	0.02

Source: Ofsted management information and National data on school choices/preferences linked to the National Pupil Database, provided by the Department for Education.

Note: Standard errors in parentheses. * p<0.1, ** p<0.05, *** p<0.01. The sample is the final analysis sample that excludes schools inspected in adjacent school years. This sample restriction excludes predominantly 'Inadequate' schools. The quintiles of pupils to school places are defined at the local education market-level . First, the total number of pupils in the catchment area of each school in the local market is counted. This is then calculated relative to the total number of school places available across all schools in the local market, rounding each school's cohort size up to a multiple of 30 to reflect maximum class size rules. Q1 has the fewest pupils per school place, while Q5 has the most pupils per school place.

Table 18: Relationship between market-level variation in Ofsted ratings and variation in market-level market shares and market-level segregation

Panel (a) Market shares			
	Variation in share of total choices	Variation in share of first choices	Variation in share offers
Variation in Ofsted within the market*year (standardised)	0.024*** (0.004)	0.025*** (0.007)	0.008 (0.006)
N	13,884	13,862	13,863
N markets	3,481	3,479	3,479
Mean	-0.14	-0.09	-0.10
R^2	0.00	0.00	0.00
Panel (b) Segregation			
	D FSM	D EAL	D White
Lag: Variation in Ofsted within the market*year (standardised)	-0.001 (0.003)	-0.005 (0.004)	-0.002 (0.003)
N	10,406	10,378	10,380
N markets	3,480	3,480	3,480
Mean	0.30	0.31	0.27
R^2	0.00	0.00	0.00

Source: Ofsted management information and National data on school choices/preferences linked to the National Pupil Database, provided by the Department for Education.
Note: Standard errors in parentheses. * p<0.1, ** p<0.05, *** p<0.01. The sample is the final analysis sample that excludes schools inspected in adjacent school years. This sample restriction excludes predominantly 'Inadequate' schools. All variables are created at the local education market-level. D denotes the index of dissimilairy (Duncan and Duncan, 1955).

Table A1: Short inspection to full inspection Ofsted rating

Short inspection outcome	**Ofsted inspection outcome**	**N**
School remains Outstanding	Outstanding	
School remains Outstanding (Concerns) - S5 Next	Depends on next inspection	
Yes - improving (outstanding)	Outstanding	
School remains Good	Good	SUPP
School remains Good (Concerns) - S5 Next	Depends on next inspection	
School remains Good (Improving) - S5 Next	Depends on next inspection	
Yes - improving (good)	Good	2,462
Not taking effective action to tackle the areas requiring improvement	Requires Improvement	SUPP
Taking effective action to tackle the areas requiring improvement	Requires Improvement	SUPP
School is not taking effective action towards the removal of serious weaknesses designation	Inadequate	SUPP
Taking effective action towards the removal of serious weaknesses designation	Inadequate	
School is not taking effective action towards the removal of special measures	Inadequate	SUPP
Taking effective action towards the removal of special measures	Inadequate	

Source: Ofsted management information.
Note: Authors' classification based on reasoning. Where a second inspection follows, the result of the final Ofsted inspection is recorded, although this does not apply to any schools in our final sample. SUPP denotes a sample size less than 10.

Table A2: Final sample selection

Selection criteria	**N**	
N schools in school-level choice data	16,564	
& school is in pupil-level choice data	16,257	
& School ID is recognised	15,699	
& link to Ofsted data	15,684	
& not split school	15,681	
& not merging school	15,651	
& not special independent/other	15,642	
& Ofsted inspection in period	11,573	
& relevant Ofsted data (!9/ungraded)	11,410	
& have prior rating	11,389	
& not inspected twice in the same year	11,351	
& prior rating more than one school year ago	10,313	
& all outcomes observed	10,301	
Sample by research design	**DIDID**	**DID**
& design specific year selection	**7,828**	**4,287**

Source: Ofsted management information; National data on school choices/preferences linked to the National Pupil Database, provided by the Department for Education. The triple differences design requires schools to have at least two observations in the year of inspection and before this. As such, schools inspected in 2014 are excluded from this design. The difference-in-differences design requires schools to have at least two observations in the year excluding the treatment year (which excludes 2017), and also restricts the sample to those inspected after 2014 (as no pre-inspection data is available).

Table A3: Medium-term (difference-in-differences) response to the revelation of Ofsted ratings: previous rating 'Good'

	Event study			Levels			Changes		
	Total choices	First choices	Choose closest	Total choices	First choices	Choose closest	Total choices	First choices	Choose closest
Ofsted				17.167***	7.562***	0.044***			
				(0.788)	(0.411)	(0.004)			
Decline * post							-17.434***	-7.035***	-0.056***
							(1.651)	(0.863)	(0.009)
Improve * post									
Decline * -3	0.862	0.809	0.010*						
	(0.942)	(0.515)	(0.006)						
Decline * -2	1.894**	0.612	0.012**						
	(0.880)	(0.481)	(0.005)						
Decline * 0	-2.784***	-1.988***	-0.014***						
	(0.845)	(0.462)	(0.005)						
Decline * 1	-15.074***	-6.747***	-0.049***						
	(1.505)	(0.824)	(0.009)						
Decline * 2	-18.306***	-6.263***	-0.022						
	(2.266)	(1.237)	(0.014)						
Improve * -3	0.554	-0.488	-0.001						
	(1.549)	(0.845)	(0.009)						
Improve * -2	-0.579	-0.662	0.002						
	(1.182)	(0.645)	(0.007)						
Improve * 0	5.096***	2.448***	0.020***						
	(0.963)	(0.525)	(0.006)						
Improve * 1	18.379***	8.791***	0.040***						
	(1.104)	(0.602)	(0.007)						
Improve * 2	21.016***	9.138***	0.050***						
	(1.346)	(0.734)	(0.008)						
N	19,382	19,357	645,158	6,799	6,792	237,892	6,799	6,792	237,892
N schools	4,849	4,849	4,849	2,268	2,268	2,267	2,268	2,268	2,267
Mean	88.60	37.72	0.46	94.94	40.58	0.47	94.94	40.58	0.47
R^2	0.07	0.04	0.15	0.13	0.08	0.15	0.13	0.09	0.15

Source: Ofsted management information and National data on school choices/preferences linked to the National Pupil Database, provided by the Department for Education.

Note: Standard errors in parentheses. * p<0.1, ** p<0.05, *** p<0.01. The sample includes all schools in the final sample that were rated 'Good' in the previous inspection. "FSM" refers to free school meals, a marker of pupil economic disadvantage. KS2 quintile splits primary schools in five bands according to test scores taken at the end of primary school (KS2). Infant schools are excluded from these bands as they teach pupils to KS1 only. The event study specification includes event time dummies. All specifications include school and year fixed-effects.