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Abstract

School quality is an important determinant of students' long-term outcomes. Assignment to schools is therefore an important lever to address inequality in society. This thesis studies two aspects of the design of school choice that influence this assignment. First, I study parents' preferences for school quality. I find that parents in England often make active school choices and bypass the nearest school if it has low quality. Using a triple differences design, I estimate the causal effect of school quality information on parental school choice. I find that positive information increases the likelihood parents choose a school, regardless of their socio-economic status. In addition, positive information increases the number of applications schools receive regardless of their place in the local hierarchy. Consequently, school choice increases incentives to improve standards for all schools. I find that incentives are dampened in dense markets, however. Second, I study the interaction between school and residential choices. Residential choices may be influenced by local school quality if schools rank applicants by geographical proximity. Using a difference-in-differences design, I study the causal effect of geographical admissions on residential choices. In line with existing literature, I find that geographical admissions lead to relocations. My results suggest that it is a very local effect, however, driven by a minority of high social class parents trying to access a minority of high performing schools. Studying one such case in depth, I develop a structural model of household decisions in a dynamic setting. This model reveals spillovers from geographical admissions to households without children. In contrast to existing literature, it shows that price premia around 'good' schools are driven by relocation decisions of households with and without children. Lastly, the model allows me to study the effect of an alternative policy on school and neighbourhood integration.

Dedication and Acknowlegdements

I dedicate this thesis to my children, Edward, Harry and Ida, who have taught me much about school choice and education alongside this research. Thank you for all the joy you bring to our lives, and for teaching me to be compassionate. To my children and loving partner, Stuart, I look forward to more time together and our next adventure.

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Author's declaration

I declare that the work in this dissertation was carried out in accordance with the requirements of the University's Regulations and Code of Practice for Research Degree Programmes and that it has not been submitted for any other academic award. Except where indicated by specific reference in the text, the work is the candidate's own work. Work done in collaboration with, or with the assistance of, others, is indicated as such. Any views expressed in the dissertation are those of the author.

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1 Introduction

Preface: *This introduction is written informally for a general audience. The following chapters present the existing literature in more detail, and provide the full methodology, identifying assumptions and results.*

Many parents place great importance on the quality of their child's school. This is evident in the existence and popularity of private schools, and, within the state-sector, in various strategies employed to access the 'right' school. These strategies include moving home to be in a preferred catchment area (alternatively known as 'school zone' or 'la carte scolaire'), extensive preparation for passing a selective test, or changing or accentuating one's faith to access a religious school. Whatever policy is put in place, (some) parents will respond to the resulting incentives. This means that the design of the education system needs to be carefully constructed, accounting for the second-order (or 'general equilibrium') effects.

Economists care about the design of the education system given the role of human capital in long-term economic growth, and individuals' education outcomes, employment, health, and well-being. The design also matters for inequality of education outcomes, for example between richer and poorer households.[1] In turn, this has important implications for inequality in individuals' later outcomes and societal outcomes, such as cohesion. For example, previous research has shown that pro-social behaviour and generosity increases by integrating pupils from different backgrounds.

My thesis studies the design of school choice in England, including the general equilibrium effect of how households make residential choices in response to their school choice environment. This is important for policy-makers to make informed decisions considering the welfare effects of alternative policy options, and contributes to multiple strands of academic research. Although the precise results of my research are generalisable only to England, many of the general findings are applicable to other contexts.

'School choice' is broadly defined as any system in which parents' preferences are one input into school assignment. In England, this takes the form of an ordered list of school 'preferences' submitted by each household (with a child of the relevant age) to their Local Authority. This Local Authority then assigns pupils to schools, using an algorithm that induces truthful ordering of preferences, and taking into account school capacities and school admissions priorities. School capacities and priorities must be published in advance, and meet the requirements of the School Admissions Code. This code bans the use of overt or covert selection by schools, for example by interviewing parents.

School choice breaks the deterministic link between neighbourhood and school, by giving parents some alternative school options. School priorities affect how 'real' this choice is for parents, however, which is an understudied area of research. For example, if a popular school

[1] Throughout the thesis, 'parent' and 'household' will be used interchangeably.

has a catchment area, or prioritises pupils by distance to the school, then pupils living further away have no option to attend this school, even under a system of school choice. This relates to the distributional consequences of school choice design. Policy-makers may be concerned about equity in school admissions (and therefore pupils' outcomes) in addition to the efficiency of the system.

I focus on the interaction between housing and school markets to provide some evidence about the effect of education design choices on neighbourhood formation and spillovers to households without children, in addition to equity in school access across pupil groups. Key inputs into my overall conclusions are: whether parents value school quality; whether preferences are heterogeneous across household types; whether, and to what extent, households make endogenous residential choices in response to their school choice environment. These inputs are the focus of chapters 3 to 7. Based on these, my concluding chapter summarises the contribution my thesis makes, acknowledging its limitations and suggesting areas requiring further research.

First, chapter 2 summarises relevant existing literature on school choice and school accountability, based on a published literature review that is co-authored with Simon Burgess. This review focuses on the economics literature, leaving the important contributions from other disciplines, for example geography, sociology and education, to be included in each relevant chapter.

Here, I provide a brief, non-technical, summary of the two existing research areas that are most relevant for the rest of this thesis. First, that property price premiums exist around schools with higher test-scores (and in some contexts higher school-effectiveness) where there are geographical admissions criteria. This implies that parents value school quality, and that prices rise around popular schools so that demand equals supply in the area that improves or guarantees the chances of admission. This literature is well-established and uses a robust methodology (boundary discontinuity design). The school attributes that parents value most, for example test scores, effectiveness, or pupil composition, are difficult to disentangle, however. Also, the mechanisms through which prices rise, and how the price elasticity relates to parents' demand for school quality, remain a 'black box'.

The second strand of literature focuses more explicitly on parents' preferences for school attributes using discrete choice models and parents' submitted school choices. Using the revealed preference approach (studying what parents actually choose, rather than what they say they value), this literature consistently finds that parents value school quality (with mounting evidence that parents also value school-effectiveness), proximity and the peer group. Most papers find evidence for heterogeneity in preferences between more and less advantaged households, with less advantaged households typically placing less weight on school quality relative to distance. This literature is, to date, estimated assuming that households' location is fixed. That is, it is assumed that households do not choose their location taking into account access to preferred schools. Rather, households choose their preferred schools, given their location. Allowing for households' location to respond endogenously to their school preferences implies that the estimates from these models will be biased. For example, most obviously, if households move close to their preferred school, then the estimated weight they put on proximity would be too high.

The following chapters contribute to these research strands by: investigating households' preferences for school attributes, including the response to new information about school quality; the determinants of school segregation; and, focusing on the interaction between school and housing markets, allowing for endogenous residential mobility. The results from these chapters illuminate the 'black box' of property price premiums around good schools, and quantify the likely bias in existing estimates of parents' preferences for school attributes.

Chapter 3 uses national administrative data on secondary school choice in England to present some key descriptive statistics. This is based on a published journal article, co-authored with Simon Burgess and Anna Vignoles. Through this initial interrogation of the data, we find that school choice is actively used by many households in England. Studying heterogeneity in choices, we show that engagement with school choice does not vary significantly by households' social class. This is in contrast to some existing qualitative literature that has relied on smaller and less representative samples of parents and pupils. This chapter presents descriptive evidence that the current geographical school admissions criteria (used in most parts of England) penalise poorer families.

Moving to causal evidence, chapter 4 tests how parents respond to new information about school quality, revealed by independent inspection (Ofsted) ratings. This chapter is co-authored with Iftikhar Hussain. Our identification strategy relies on the timing of the primary school choice deadline (in mid-January) and plausibly exogenous variation in the timing of primary school inspections around this deadline. Comparing parents' school choices to otherwise identical schools but with different information revealed, we identify the *pure information effect* of information on parental decisions. We find that parents' school choices respond significantly. For example, revealing a one-unit increase in inspection rating before the school choice deadline increases the number of households that choose the school as 'first choice' by 1.5 (4%). This increase in demand from the revelation of positive information is non-trivial for schools. For example, translated into per-pupil funding, this increase would be equivalent to one-third of a newly qualified teacher's salary.

These findings suggest that schools have an incentive to improve their education standards (as captured by the school inspection framework) in order to attract pupils, and therefore funding. We study heterogeneity across household, school and local market characteristics to inform whether these incentives apply to all schools, and whether the provision of information exacerbates segregation. First, the effects are similar for schools across the local market hierarchy. Second, effects are similar for households across the income distribution. As expected, given these two key results, we do not find any effects of information provision on segregation at the market-level. It is important to note that these estimates are net of any endogenous residential mobility, as the short time window (weeks or a few months) between the school inspection and school choice deadline would prohibit moving. This is a difference to discrete choice models that estimate parents' preferences for school characteristics that must *assume* a fixed location. This might explain why those models find heterogeneous preferences whereas we find homogeneous preferences.

Chapter 5 studies segregation explicitly, questioning whether the overall equilibrium level of segregation in England's secondary schools is due to parents' school choices, or constraints in accessing their preferred schools. The starting point for this chapter, co-authored with Mat Weldon, is the high level of segregation in England's schools, that is hypothesised to stem partly from the system of school choice. We examine whether segregation is *by choice* using universal administrative data. Using counterfactual simulation, we find that households' school choices lead to segregation in most areas, which suggests a more limited role for constraints. Whether school choices can be interpreted as preferences is crucial to the interpretation of this chapter, however. This is because, despite the truth-revealing assigning mechanism used in England, the short list-length means that popular schools with zero probability of admission may be omitted from some households' lists. School choices could therefore incorporate constraints in access to popular schools, rather than preferences. This does not appear to be the case, however, as choices are more segregating when school choice is more 'free', suggesting that segregation is in part due to parents' preferences. We find that implementing a policy of 'neighbourhood' schools would, in contrast, reduce segregation in most areas.

This chapter does not attempt to incorporate second-order effects, however. For example, under a neighbourhood schools policy, some households would move home to access their preferred school. At the mean, we find that the reduction in segregation from a neighbourhood schools policy would be reversed if a small proportion of households made residential moves in response. The final two chapters focus on this residential channel to provide evidence about the likely overall effects of alternative school choice policies.

Chapter 6, co-authored with Hélène Turon, uses a structural approach to model households' residential and school choices across four life-stages. Households are forward-looking; this allows us to include dynamic considerations, such as moving costs and the likelihood their child is admitted to the 'good' school (if they expect to become a parent). Specifically, we compare sorting into schools and neighbourhoods under two priority rules if a school is over-subscribed: geography and random assignment. Our model illustrates the importance of school priorities for equilibrium outcomes and welfare, as school priorities affect sorting into neighbourhoods as well as schools, by household type (age and family size) as well as household income. The overall implications for household welfare are sizeable, with significant spillovers of the school choice environment to households without dependent children. Unlike previous empirical research, our model illustrates the mechanisms through which the design of school choice affects local property prices and school composition. This allows us to explore the nuances and interpret existing empirical reduced form estimates, for example of the relationship between school quality and local property prices. We match our model to a city in England (Bristol) and find that allocating places by lottery increases the probability of attending the 'above average' school from outside the school zone/catchment area. This therefore increases integration by household income, decreases local property prices in this area, and increases mixing across household types. The limitations of this model are that we model choices across two-adjacent neighbourhoods and schools only.

Chapter 7 explicitly tests whether geographical residential priorities in school admissions affect households' residential choices across England, using a reduced form approach. I test both *whether* households make additional moves in response to incentives induced by a geographical school admissions system, and *where* they choose to move to. This chapter uses two sources of variation across England, in a difference-in-differences design. The first comparison is between areas that use a primarily geographical admissions system to ration places at popular schools, compared with areas that primarily use non-geographical (test based) admissions criteria. The second comparison is between households that ever become parents and households that never become parents, measured through nationally representative longitudinal data. The intuition is that households that never become parents gain no flow utility from the quality of the local school, but do value neighbourhood characteristics that may be correlated with it, such as neighbourhood composition and public amenities. The results indicate that sorting for school quality in response to geographical admissions is not widespread in England. Only higher social classes migrate towards areas with higher school quality, and to a limited extent.

My overall conclusions are that, first, parents actively use the school choice system in England. Households value school quality, making 'ambitious' school choices and responding to positive information provided by independent school inspections. I find that households' responses are homogenous across poorer and richer households, in contrast to previous literature using a different methodology. These results have positive implications for the effect of school choice on schools' incentives to improve their standards of education: schools will compete to raise standards only if parents value it. These incentives are limited by the limited supply of school places in England, however, as in some areas even unpopular schools become full, and enrolment and funding therefore become unresponsive.

Second, despite relatively homogeneous engagement with school choice, and response to information about school quality, there is segregation in England's schools. This is in part driven by the choice of admissions criteria, either by location or test-score, that prioritise access for more affluent households. Parents' preferences might also play a role, however, as segregation remains high even if all households are allocated to their first choice school, simulating the effect of removing certain constraints from admission.

Third, school segregation would be lowered with the introduction of a lottery, rather than catchment area, to determine admission to oversubscribed schools. Neighbourhood segregation would be affected to a more limited extent, as in our context there is a high correlation between school and neighbourhood amenities. That is, richer households still sort according to neighbourhood quality, even without priority in admission to the 'good' school. Finally, residential mobility in response to school admissions priorities is not a widespread phenomenon. Only households with high social class make strategic moves in response to geographical admissions criteria, and to a limited extent.

Together, these final two conclusions have useful implications for policy and future research. For policy, re-designing the school choice environment is unlikely to dramatically change neighbourhoods, especially in the short-run, although some movement by higher social class households would be expected. The lever of school choice design therefore has a more direct effect on school composition than on neighbourhood composition, although it can serve to integrate both. For future research, it is important to model endogenous residential location in response to policy reforms, but it is unlikely to be a central determinant in the resulting efficiency of the system.

2 Literature Review

Preface: *This literature is re-produces a published article, co-authored with Simon Burgess (Burgess and Greaves, 2021). I took the lead on all sections aside from 'school accountability' for the original publications, searching for relevant articles and summarising each research theme. The structure for this review was based on the relevant sections from Simon Burgess' previous (wider-ranging) review (Burgess, 2016).*

2.1 A framework for studying school choice and accountability

Education is an important determinant of later outcomes, for example employment, health and well-being. Public provision is therefore common, due to positive externalities and economies of scale. The quality of public education is consequently of central importance to governments spending public funds. School choice and accountability are two mechanisms commonly used to improve school-effectiveness, where an 'effective' school is one that generates causal improvements in student outcomes (Rothstein, 2006).

A defining feature of a school system is the way in which pupils are assigned to particular schools. While, typically, there are enough public school places overall to meet demand, schools are not equally attractive to parents or equally effective, and so how pupils are assigned to schools matters for education outcomes. School choice is one such assignment mechanism. Defined broadly, school choice is any system in which parents' preferences over schools are an input to their child's allocation to school.[2] 'School choice' is interpreted and implemented differently across countries, states and districts. Friedman, 1955 initially conceived of the 'denationalization' of education to broaden the availability of choice for parents. By providing a subsidy to parents to attend any private school (now coined a voucher) they could 'express their views about schools directly', rather than through residential location or political channels. Friedman was initially sceptical that government could provide greater freedom to choose between public schools. Since 1955, however, the scope and interpretation of school choice has expanded dramatically. Examples include parents submitting a rank ordered list of school choices to a central authority; households receiving a voucher to attend a private school; and, households being able to opt-out of a 'default' or 'neighbourhood' school. The right for parents to express a preference for their child's school was established in England in 1988, while in other countries, co-ordination of school applications is in infancy, or yet to emerge.[3]

How does school choice induce improvements in school effectiveness? Overall, schools must be incentivised to compete to attract pupils/parents through raising their quality. From the demand-side, the conditions for this to hold are that: parents care about school effectiveness; they are able to identify the most effective schools; and, they are able to choose the most effective

[2] Throughout the review, 'parent' and 'household' will be used interchangeably.

[3] Ireland's school admissions process is still largely decentralised, for example (matching-in-practice.eu).

schools. School accountability feeds into the second of these conditions, providing information for parents to select their school(s) of preference. Why would parents not be able to choose their most preferred or most effective schools? A large body of research studies the importance of the design of the assignment mechanism which allocates pupils to schools according to their choices and schools' capacity constraints and admission priorities. Different assignment mechanisms have different properties, such as whether they induce parents' to make truthful school choices. School admission priorities, such as proximity, can also limit the likelihood of gaining admission for some households, and therefore the likelihood that they make that school choice.

On the supply-side, there must be spare capacity in the system so that changes in school choices leads to changes in school allocations (and therefore funding for schools). In the absence of funding incentives, there must be reputation, career or altruistic concerns for school leaders which drive school improvements. Schools must also have autonomy in school management/organisation to deliver improvements. An initial rationale for school choice was that schools would specialise and diversify to meet diverse parent preferences (Friedman, 1955), although this has not been the focus of empirical research. Finally, it must be rational for schools to attract pupils through improvements in quality rather than alternative strategies such as 'cream-skimming' - selecting pupils to improve school accountability measures - or marketing strategies. More generally, Hatfield, Kojima, and Narita, 2016 articulate that it must be in a school's interest to attract more pupils, specifically that the 'quality' of its students does not worsen if the number of school choices increases.

School choice can also affect the composition of schools. Active diversity programs within a system of school choice can lead to more integrated public schools, but the majority of research has concentrated on the potential for school choice to lead to more segregated schools. The primary mechanism is through parents' heterogeneous preferences. For example, in a coarse, stylised example, segregation increases if all higher educated parents choose a highly performing school while all lower education parents choose a less academic school.

School accountability feeds into school improvement not only through providing information to parents (as discussed in relation to school choice above) but also through providing information to schools themselves and school authorities. Alongside information, school accountability provides incentives. In a principal-agent setting, and in the absence of other strong incentives, the public provision of performance information can exert pressure on school leaders of low performing schools. Sanctions or rewards can be explicit, such as the replacement of school leaders, or implicit, such as raising applications to the school.

This review summarises theoretical and empirical research organised around these themes. We set this material out as follows. First, the 'rules of the game' given by the school place allocation mechanism. Second, the components of a school choice process: the preferences and choices of parents (observed through residential demand and direct school choices) and how these are affected by information provision. Third, the incentives for schools and teachers provided by the accountability system, and the implications for pupil attainment. After providing evidence on each of these research areas, we examine the overall effect of school choice on outcomes of interest, principally market-level attainment and sorting. Before concluding, we assess the evidence that schools have a causal effect on pupil attainment and wider indicators of well-being.

2.2 Allocation mechanisms

Where the school choice system invites parents to express a preference for their child's school, it is common that a central authority allocates pupils to schools. Typically, parents submit a rank ordered list (ROL) of schools, which the central authority uses, in combination with schools' priorities, to assign pupils to schools. Central co-ordination itself is important, increasing the number of applications and students' welfare from the school match (Abdulkadiroğlu, Pathak, and Roth, 2005, Abdulkadiroğlu, Agarwal, and Pathak, 2017). This section will briefly review the choice of assignment mechanism, or algorithm, used to assign pupils to schools which is relevant for the rest of this review. See Abdulkadiroğlu and Sönmez, 2013, Pathak, 2017 and Cantillon, 2017 for more detailed reviews of theoretical and practical considerations in market design.

Economists have been active in theory and practice to shape the allocation mechanisms that assign students to schools (Abdulkadiroğlu et al., 2005, Abdulkadiroğlu, Pathak, and Roth, 2005, Abdulkadiroğlu, Pathak, and Roth, 2009, Pathak, 2017). The choice of allocation mechanism (or algorithm) is important, as it affects whether parents choose their most preferred schools and whether the eventual outcome is Pareto efficient (Chen and Sönmez, 2006). In their seminal paper, Abdulkadiroğlu and Sönmez, 2003 formalise three criteria to evaluate school choice algorithms. First, ex post efficiency: a measure of how the assignment respects preferences. Second, absence of justified envy: no student should be admitted to a school when another student (who prefers the school) has priority and is not accepted. Third, strategy-proofness: there is no better strategy for parents than to list their most preferred schools. This final property maps to the framework discussed in 'A framework for studying school choice and accountability' because to provide incentives for school improvement parents should nominate their most preferred school(s).

Strategy-proofness is also important as empirical work from around the world has shown that parents make strategic errors if truth-telling is not a weakly dominant strategy under immediate acceptance ('Boston') mechanisms (for example, Lai, Sadoulet, and de Janvry, 2009 in Beijing, Lucas and Mbiti, 2012 in Kenya, Ajayi, 2013 in Ghana) and a significant proportion of parents act strategically (Pathak and Sönmez, 2013 in Chicago, He, 2017 in Beijing, Agarwal and Somaini, 2018 in Cambridge, US, Calsamiglia, Fu, and Güell, 2020 in Barcelona, Gortázar, Mayor, Montalbán, et al., 2020 in Madrid, and Kapor, Neilson, and Zimmerman, 2020 in Connecticut, US) and have a vested interest in the algorithm's continuation (Pathak and Sönmez, 2008). This creates an unequal playing field, as more advantaged households are more likely to understand the strategic incentives. Additional advantages of strategy-proofness are that parents' true preferences are respected, and that therefore it is easier for central authorities to advise parents and monitor supply and demand for schools (Cantillon, 2017). Revealing true preferences is a weakly dominant strategy in two common mechanisms, Student Proposing Deferred Acceptance (SPDA, Gale and Shapley, 1962, also called Student Optimal Stable Matching) and Top Trading Cycles (TTC).

Refinements show that truth telling is not optimal in some circumstances even with an SPDA mechanism, for example when parents can make only limited nominations (Haeringer and Klijn, 2009 and Calsamiglia, Haeringer, and Klijn, 2010). Fack, Grenet, and He, 2019 overcome this limit to truth-telling by formalising the concept of 'stability': households' school choices will reflect true preferences from the set of schools that have a positive probability of admission, 'skipping the impossible'. There is also evidence that individuals make mistakes in their ROL even under a truth telling allocation mechanism, which is summarised by Hassidim et al., 2017. For example, for higher education track/funding choice in Israel, Hassidim, Romm, and Shorrer, 2016 find that around 20% of ROLs obviously misrepresent true preferences, by omitting options that are attached to funding over identical options without funding, or reversing the ranking of these options. Other examples cited within Hassidim et al., 2017 are consistent with 'skipping the impossible', although there may be other strategy at play too.

There are trade-offs between manipulable and non-manipulable allocation mechanisms used in practice, however, in that manipulable mechanisms allow parents to express the *strength* of their preferences (Abdulkadiroğlu, Che, and Yasuda, 2015).[4] Kapor, Neilson, and Zimmerman, 2020 state that the optimal assignment mechanism depends on whether 'applicants' ability to express cardinal preferences through strategic play in the Boston mechanism outweighs the welfare costs of strategic mistakes due to misunderstandings about the mechanism or lack of information about demand conditions'. In this context, Kapor, Neilson, and Zimmerman, 2020 find that imperfect information leads to higher welfare under deferred acceptance than immediate acceptance algorithms.

Hatfield, Kojima, and Narita, 2016 are the first to consider the implications of the allocation mechanism for schools' incentives to improve performance. The key insight is that schools must not be penalised for becoming more popular by attracting a 'worse' set of students. Hatfield, Kojima, and Narita, 2016 conclude that no allocation mechanism commonly used (SPDA, Boston or TTC) respect this condition. SPDA approximately respects this condition in larger markets, however, while 'Boston and the TTC mechanisms provide incentives for schools to make themselves less attractive to "less desirable" students'.

[4] Abdulkadiroğlu, Che, and Yasuda, 2015 propose the Choice-Augmented Deferred Acceptance (CADA) algorithm to allow the strength of parents' preferences to be taken into account (rather than a random assignment) when multiple pupils have the same priority at a school, while retaining the truth-revealing property of SPDA. This has not, to our knowledge, been used in practice.

Overall, theoretical and empirical work point to the importance of centralising school admissions to reduce inefficiency, and choosing an assignment algorithm that is transparent and easy to explain in addition to desirable formal properties. In his review of implementations of school choice in practice, Pathak, 2017 argues that ‘what really matters for school choice market design are basic insights about straight forward incentives, transparency, avoiding inefficiency through coordination of offers and well-functioning aftermarkets, and influencing inputs to the design, including applicant decision-making and the quality of schools’.

The following section (‘Parents’ preferences for schools’; ‘Evidence from school choices’) reviews research that infers the characteristics of schools that parents value from their school choices. The allocation mechanisms described in this section imply that parents’ school choices do not always reflect their true preferences for schools, which researchers must address in this empirical work.

2.3 Parents’ preferences for schools

Parents’ preferences for schools, particularly whether they value school-effectiveness, is a crucial determinant of whether school choice has the potential to improve educational standards and/or increase segregation. Preferences for other school attributes are also important, however. For example, Barseghyan, Clark, and Coate, 2019 show that schools’ incentives to improve productivity are weakened or eliminated if households have strong preferences for the peer group in the school. This section summarises the literature on what we can infer about parents’ preferences for school attributes through their ‘revealed preference’. First, from residential demand - increased demand around the ‘best’ schools. Second, from school choices. Finally, this section summarises evidence on how the provision of information affects school choices, which is an important policy consideration.

2.3.1 Evidence from residential demand

There is a large empirical literature, beginning with Black, 1999, on the willingness to pay for local school quality using boundary discontinuities. Black finds that house prices respond to local school quality. Typically, households are willing to pay a premium of around 3-4% for access to a one standard deviation increase in school average test scores. (See Gibbons and Machin, 2008, Black and Machin, 2011 and Nguyen-Hoang and Yinger, 2011 for detailed summaries.) Bayer, Ferreira, and McMillan, 2007 disentangle household preferences for school and neighbourhood attributes, while taking into account endogenous sorting across neighbourhoods. They show that previous estimates of willingness to pay for higher school quality are upward biased if households also have preferences for more affluent neighbours. Bibler and Billings, 2020 find evidence of a higher premium for households with a strong preference for school quality and time constraints.

Exploiting reforms, Machin and Salvanes, 2016 use a 1997 reform which abolished neighbourhood schooling in Norway to estimate the change in willingness-to-pay for houses close to above average schools, finding a fall in the house price premium by 50 percent. Ries and Somerville, 2010 use a re-zoning reform in Vancouver in 2001 and find that house prices respond to changes in school quality only in the top quartile of the distribution.

A group of papers study the effect of school accountability classifications of schools on local prices. Figlio and Lucas, 2004 identify the effect of 'school grades' in Florida, over the effect of publicly available test score data. Using property and neighbourhood fixed-effects, Figlio and Lucas, 2004 find that new 'school grades' were capitalised into house prices, but the effect was damped once the variability of grading over time was observed by households. Fiva and Kirkebøen, 2011 find consistent evidence using the same identification strategy in Olso, Norway, where a measure of 'intrinsic school quality' was released in 2005. Hussain, 2020 finds consistent evidence in response to established independent 'Ofsted' inspections in England. Like Ries and Somerville, 2010, Hussain, 2020 also finds that the demand side response increases with school quality: prices around lower quality schools (as proxied by test performance and social composition) are not affected by the revelation of inspection information.

There is evidence that school quality is capitalised into house prices even where school quality information is not disclosed. Harjunen, Kortelainen, and Saarimaa, 2018 study Helsinki, Finland, where no standardised test or inspection data is available to parents, but, using the boundary discontinuity research design common in the literature, the house price premium is similar in magnitude to contexts where school quality is observable. In this context, the socio-economic composition of the school is found to drive demand, rather than school effectiveness itself.

Evidence from across this literature suggests that households value the peer composition in addition to (or as a proxy for) school quality. Imberman and Lovenheim, 2016 explicitly explore the characteristics of schools that parents' value through residential demand. Following the introduction and publicity of school value-added measures by the "Los Angeles Times" newspaper, using a difference-in-differences framework, Imberman and Lovenheim, 2016 find no evidence that value added measures are capitalised into local house prices. This evidence suggests that parents do not respond to 'school effectiveness', but rather than measures of school quality that conflate peer group composition and absolute measures of test scores. In contrast, through a carefully executed boundary-discontinuity approach, Gibbons, Machin, and Silva, 2013 find that house prices respond by a similar magnitude to published measures value-added and academic attainment (as a proxy for peer group) for primary schools in England. Gibbons, Machin, and Silva, 2013 conclude that 'The most plausible explanation that is consistent with our results is that parents value both academic effectiveness and composition aspects of school quality because they are interested in their own child's academic progress, as well as the social status of their child's peers.' The different conclusions may be due to the context, for example, established performance indicators in England vs new performance indicators in Los Angeles. The following sub-section ('Evidence from school choices') will also consider whether parents value school-effectiveness over and above peer composition, and also finds a mixed picture.

2.3.2 Evidence from school choices

There are a number of empirical challenges to estimate preferences for school attributes. Early research use conditional logits (and variants) and rank order lists (ROLs) of parents' submitted school choices to estimate parents' preferences for school attributes. The first challenge is whether ROLs reflect strategic choices rather than true preferences. Second, it is generally impossible to know the pool of schools that parents consider when making their choices. Finally, as admissions to popular schools are rationed, it is difficult to disentangle preferences from constraints.

ROLs may reflect true preferences for school attributes; the 'revealed preference argument' is that the researcher can infer preferences by observing the types of schools that parents choose (Agarwal and Somaini, 2020). Papers in this strand of the literature must justify why ROLs are treated as truthful, through the context studied or method adopted. This section first summarises the literature that assumes truthful reports on ROLs, before discussing subsequent research that allows ROLs to be strategic choices for all or some households.

Consistent findings across this strand of the literature are that parents value school 'quality', typically defined as test scores (Hastings, Kane, and Staiger, 2009, Burgess et al., 2015, Borghans, Golsteyn, and Zölitz, 2015, Abdulkadiroğlu, Agarwal, and Pathak, 2017, Ruijs and Oosterbeek, 2019, Glazerman and Dotter, 2017, Beuermann et al., 2018, Oh and Sohn, 2019, Harris and Larsen, 2019, Ajayi and Sidibe, 2020, Walker and Weldon, 2020, Abdulkadiroğlu et al., 2020, Bertoni, Gibbons, and Silva, 2020) and value proximity (Hastings, Kane, and Staiger, 2009, Burgess et al., 2015, Borghans, Golsteyn, and Zölitz, 2015, Glazerman and Dotter, 2017, Beuermann et al., 2018, Harris and Larsen, 2019, Walker and Weldon, 2020, Abdulkadiroğlu et al., 2020, Bertoni, Gibbons, and Silva, 2020). The school composition is also an important factor, in ability composition (Beuermann et al., 2018, Abdulkadiroğlu et al., 2020), social composition (Burgess et al., 2015, Abdulkadiroğlu, Agarwal, and Pathak, 2017, Glazerman and Dotter, 2017) and racial composition, with preferences for 'own group' (Hastings, Kane, and Staiger, 2009, Abdulkadiroğlu, Agarwal, and Pathak, 2017, Glazerman and Dotter, 2017). There is also evidence from this strand of literature that parents are aware of supply-side constraints. There are strong preferences for the home-school which gives priority for admission, all else equal (Hastings, Kane, and Staiger, 2009), indicators for previous over-subscription (Ruijs and Oosterbeek, 2019) and probability of admission (Beuermann et al., 2018, Walker and Weldon, 2020).

Exploring heterogeneity in preferences across parent types, there is consistent evidence that more advantaged households (defined by income or education) have stronger preferences for school 'quality' (Hastings, Kane, and Staiger, 2009, Burgess et al., 2015, Borghans, Golsteyn, and Zölitz, 2015, Harris and Larsen, 2019, Walker and Weldon, 2020) and according to prior test score or academic track (Hastings, Kane, and Staiger, 2009, Abdulkadiroğlu, Agarwal, and Pathak, 2017, Beuermann et al., 2018, Ruijs and Oosterbeek, 2019, Ajayi and Sidibe, 2020).

Hastings, Kane, and Staiger, 2009 and Walker and Weldon, 2020 also find stronger preferences for school 'quality' for non-white parents. Glazerman and Dotter, 2017 are the only example of variation in preferences for proximity according to race, however.

Harris and Larsen, 2019 study school choices in New Orleans after Hurricane Katrina, described as 'arguably the most competitive school market ever developed in the United States'. Harris and Larsen, 2019 study preferences for a wide range of school characteristics (extra-curricular activities, wrap-around care (free and paid) and school size) which reduces the possibility of omitted variable bias. Strong preferences for typically unobserved school attributes such as these 'can have a substantive influence on the estimated demand and preference parameters'. This study, in contrast to previous literature, finds that parents value school-effectiveness.

The earliest and still widely cited paper in this strand of the literature is Hastings, Kane, and Staiger, 2009, who use school choice data from Charlotte-Mecklenburg, North Carolina, to estimate a mixed-logit demand model for schools. The allocation mechanism (giving priority to first choices within priority groups) may have induced strategic listing, which is a limitation the authors acknowledge and present evidence against. Hastings, Kane, and Staiger, 2009 find that demand at high-performing schools is more responsive to increases in mean test scores than demand at low performing schools. Their model also implies a 'mobile' more affluent group of families exerting pressure on school performance, and a less mobile less affluent group essentially going to the local school.

Studying primary school choices in England, Burgess et al., 2015 define a set of schools for each family in the data that they could almost surely access. By comparing households of higher and lower socio-economic background with the same choice sets, Burgess et al., 2015 demonstrate that two-thirds of the observed variation in choices for academic quality are driven by constraints in admission, rather than preferences. Walker and Weldon, 2020 explicitly model the admission probability to secondary schools in England as a subjective probability, given multiple sources of uncertainty. That is, 'preferences' for the probability of admission are estimated in a random utility model alongside preferences for school quality and distance. This approach is also taken by Beuermann et al., 2018. Also for England, Bertoni, Gibbons, and Silva, 2020 find positive preferences for autonomous converter 'Academy' schools. Demand is particularly high for academies with high standards that were previously over-subscribed, suggesting that academy status is a signal for quality.

Abdulkadiroğlu et al., 2020 and Beuermann et al., 2018 study whether parents value school attributes such as causal effects on academic and non-academic outcomes, which are estimated from their data. In New York, under a deferred acceptance algorithm, Abdulkadiroğlu et al., 2020 find that preferences are correlated with peer quality and effectiveness. But, as more effective schools enrol higher ability students, the correlation between preferences and effectiveness, conditional on peer quality, is zero.

Beuermann et al., 2018 adopt a similar approach but find different results in Barbados, under a deferred acceptance algorithm. Beuermann et al., 2018 study whether parents choose schools that have positive causal effects, studying whether educational outputs are multi-dimensional. First, they estimate individual public schools' causal impacts on a range of outcomes in Trinidad and Tobago: 'high-stakes test scores, low-stakes test scores, dropout, teen motherhood, teen arrests, and labor market participation'. Second, these estimated impacts are linked to parents' school ROLS to explore whether parents choose schools with positive causal effects on these multiple outcomes. In contrast to Abdulkadiroğlu et al., 2020, Beuermann et al., 2018 find that 'parents of high-achievers *can and do* disentangle schools that causally improve test scores from schools with strong average performance'. In addition, parents in this context 'have strong preferences for schools that reduce crime and increase labor market participation – impacts that are only weakly correlated with school impacts on tests'. In this context, and including a wide range of school attributes typically unobserved by the researcher, parents value school-effectiveness, across a number of dimensions.

Several papers model preferences as strategic responses to the context and assignment mechanism (Fack, Grenet, and He, 2019, Agarwal and Somaini, 2018, Calsamiglia, Fu, and Güell, 2020). Fack, Grenet, and He, 2019 study school choice in Paris, where the students are assigned to schools after submitting their ROLs (length 8) using the school-proposing deferred acceptance algorithm. Rather than assume truth-telling, Fack, Grenet, and He, 2019 formalise the concept of 'stability' or 'non-justified envy': each student is matched to her/his most preferred school within their specific constraints, 'skipping the impossible' by not ranking schools with zero probability of admission. That is, Fack, Grenet, and He, 2019 'apply the revealed preferences argument conditional on the set of schools that each student is eligible for' (Agarwal and Somaini, 2020). Calsamiglia, Fu, and Güell, 2020, studying Barcelona, recognise that the allocation mechanism will affect only strategic households: non-strategic households will always list their most preferred schools. Calsamiglia, Fu, and Güell, 2020 estimate both household preferences and the distribution of strategic types in a parametric model 'in which strategic agents solve for the optimal report in an immediate acceptance mechanism using backward induction from lower- to higher-ranked choices'. Rather than assuming households choose the school that maximises utility, Agarwal and Somaini, 2018 assume that households maximise the expected utility from a lottery over assignments to different schools, where 'the lottery implied by a rank-order list consists of the probabilities of getting assigned to each of the schools on that list'. In turn, these probabilities depend on the student's priority type, listed schools and randomly generated tie-breaker, and competition from other students (their priority type and listed schools). In relation to the previous body of research, Agarwal and Somaini, 2018 note that: 'Our empirical results indicate that treating preferences as truthful is likely to result in biased estimates in markets where students face stiff competition for their preferred schools'.

The results from these papers are generally in line with the first strand of papers that relied on truthful reports or modelled aspects of the strategic nature of choices, such as the probability of admission or over-subscription in the previous year. Parents care about school 'quality', again proxied by test scores (Akyol and Krishna, 2017, Agarwal and Somaini, 2018, Fack, Grenet, and He, 2019, Calsamiglia, Fu, and Güell, 2020). Again, this is increasing with parents' characteristics that are correlated with advantage: increasing with prior test scores in Paris (Fack, Grenet, and He, 2019), with parents' education in Barcelona (Calsamiglia, Fu, and Güell, 2020), and 'competitiveness' is valued more by paid-lunch students in Cambridge, US (Agarwal and Somaini, 2018). In Barcelona, higher education parents are also less price sensitive (Calsamiglia, Fu, and Güell, 2020). Parents value proximity, modelled as convex with discontinuous jumps at 500m and 1km in Barcelona (Calsamiglia, Fu, and Güell, 2020) and the peer group (Fack, Grenet, and He, 2019). Additional findings are the importance of siblings (Agarwal and Somaini, 2018, Calsamiglia, Fu, and Güell, 2020); language immersion programmes, which are valued more by Spanish and Portuguese speakers (Agarwal and Somaini, 2018); and, larger schools/more resources (Calsamiglia, Fu, and Güell, 2020). In addition, these papers unanimously find that a subset of parents respond to the strategic incentives in their school choice environment (Akyol and Krishna, 2017, Fack, Grenet, and He, 2019, Agarwal and Somaini, 2018, Calsamiglia, Fu, and Güell, 2020).

Overall, this strand of the literature is consistent that distance to school and some measure of academic quality are important factors for parents. Whether this finding implies that school choice provides incentives for schools to improve educational standards depends on whether parents value raw levels of academic attainment or value-added/school-effectiveness. Two papers which include a broad range of school characteristics (Harris and Larsen, 2019 and Beuermann et al., 2018) find that parents do value school-effectiveness, suggesting some promise. Parents also value the school composition, however, which may shift schools' incentives to compete on this dimension rather than educational standards, if permitted (Abdulkadiroğlu et al., 2020). Heterogeneity in parents' preferences is evident, which may reflect parents' preferences or constraints in access or information. The following section ('The effect of school quality information provision on school choices') reviews evidence for whether information provision affects school choices.

2.3.3 The effect of school quality information provision on school choices

The previous section ('Evidence from school choices') described estimated preferences for school attributes, with a common finding that more advantaged households typically value school 'quality' more. Hastings and Weinstein, 2008 note that:

> If lower-income families face higher costs of gathering and interpreting statistics on academic achievement, they may choose schools based on easier-to-determine characteristics such as proximity, instead of school test scores.

A small but growing number of papers explore the effect of information provision on school choices, through experiments at the household, school or market-level. This section will summarise the evidence that information provision can alleviate imperfect information and influence parents' school choices, informing whether heterogeneity in school choices across households is due to differences in preferences or access to information. Neilson, Allende, and Gallego, 2019 state that 'Taken together, the empirical evidence to date indicates that information interventions do have the potential to change behaviour but that policy details can matter quite a lot'. Relevant policy details include the complexity of the choice environment and properties of the allocation mechanism (truth-revealing or otherwise), which were described in the 'Allocation mechanisms' section.

Ajayi, Friedman, and Lucas, 2020 present results from a student-level randomised controlled trial in Ghana, which provided comprehensive and accessible information about (the complex) secondary school choice to the treatment groups. The information provided was internalised, and changed students' application decisions. The intervention did not improve students' overall outcomes in the transition to secondary school, however, which the authors conclude is likely to be due to constraints, such as expense and/or distance to preferred schools. In an earlier paper also in Ghana, Ajayi, Friedman, and Lucas, 2017 find that information can increase the involvement of guardians in the senior high school decision process, through a school-level randomised controlled trial.

In the first study in this literature, using a mix of field and natural experiments (induced by 'No Child Left Behind' accountability measures), Hastings and Weinstein, 2008 find that the provision of additional information on school characteristics shifts school choices towards non-guaranteed and higher-attaining schools, particularly where a high-attaining school is in close proximity.

Neilson, Allende, and Gallego, 2019 show that an experimental intervention providing personalised information on elementary schools in Chile changes families' choices towards more ambitious schools. They show that this affects outcomes too – student scores are 0.2SD higher five years later. Neilson, Allende, and Gallego, 2019 also set out a structural model to assess the systemic response to national rollout of the intervention; they note that capacity constraints are important, but show that typically supply side responses are positive for school effectiveness. In contrast, non-experimental evidence from Chile, where schools are identified as school-effectiveness 'winners' from within a homogeneous group of schools (approximating a value-added measure), Mizala and Urquiola, 2013 find that this signal does not significantly affect schools' market outcomes.

Corcoran et al., 2018a run an intervention in New York City. Focused on more disadvantaged neighbourhoods, the treatment is provision of a simple, short list of near-by schools with above-average performance. The findings were positive, leading to more students in higher performing schools; this came about more by the students avoiding low graduation rate schools than applying to high-rate schools.

In the only market-level experiment in Pakistan, Andrabi, Das, and Khwaja, 2017 show that the introduction of report cards improved parents' knowledge of local schools' quality. This leads to improved enrolment and learning - average test scores increasing by 0.11 standard deviations. Private schools responded by reducing fees, and lower quality private schools were more likely to exit the market.

School choice is increasingly researched and conducted online. Lovenheim and Walsh, 2018 find that online search behaviour is influenced by the school choice environment, suggesting that parents seek out information under certain incentives. Theoretical work explores endogenous information provision under alternative school choice environments (Bade, 2015, Harless and Manjunath, 2018). Glazerman et al., 2017 reviews the 'choice architecture' of 'school shopping websites' in 14 US school districts, finding substantial variation, for example in how schools are sorted, whether a map of schools is given, what school attributes are included, and how data are presented. Glazerman et al., 2020 explore whether such variation in design features affects understanding, satisfaction and ease of use for low-income households, in a low-stakes experiment with hypothetical schools. Glazerman et al., 2020 conclude that design decisions can 'affect how people interpret that information and evaluate schools relative to one another'. This is in contrast to earlier (off-line) information provision, where there is no marginal gain from simplified and ordered ranking of schools (Hastings and Weinstein, 2008). Through an online experiment with a nationally representative US sample, Valant and Newark, 2020 find that 'perceptions of school quality are heavily influenced by parent comments even when these comments appear alongside official ratings'. This research area appears to be moving quickly, therefore, to follow technological developments in how school choice information is provided.

Overall, evidence suggests that the provision of relevant information to parents induces changes in school choices. Most of this research is from experimental settings, which may have limited external validity. The context is also changing rapidly with more information available online. The following section ('School accountability') will consider the wider effects of school accountability, typically studied at scale.

2.4 School accountability

We define school accountability as the public provision of school performance information, on a regular basis, in the same format, and using independent metrics. Typically, the school performance data will include measures of pupil achievement, which ideally derive from a common, comparable, externally marked exit exam. This has two intended functions: incentives and information. In a principal-agent setting, and in the absence of other strong incentives, the public provision of performance information can exert pressure on school leaders of low performing schools. Sanctions or rewards can be explicit, such as the replacement of school leaders, or implicit, such as falling applications to the school. Second, accountability provides information for school improvement, perhaps for school authorities to act on, and certainly for parents to consider in their school choice.

The major focus for researchers has been the implications of school accountability for pupil achievement. Of course, this achievement comes via teachers, so there has also been substantial investigation of teachers' reactions to accountability. Finally, as with every performance management system, there is inevitably scope for gaming (and cheating) the metrics and this has also been the subject of study. Whilst almost all the research has focused on high/secondary schools, Bassok, Dee, and Latham, 2019 note that in the 2010s, most US states have ratings of child-care settings. They show similar effects: lower-rated child-care programmes see enrolment fall in response to the introduction of accountability, and respond with relative improvements.

2.4.1 What effect does the accountability system have on pupil performance?

Researchers face two main difficulties in trying to establish the causal impact of accountability systems on pupil performance. First, finding an adequate control group for the counterfactual is difficult as typically a performance management reform covers a whole area. Second, it is difficult to evaluate individual components of accountability systems, as, typically, a multifaceted reform is introduced all at once (Figlio and Ladd, 2008).

These problems are well illustrated by the case of the first national school accountability framework in the US, the No Child Left Behind (NCLB) Act, which came into force nationwide in 2002. This mandated annual testing of primary school pupils in maths and reading, and required the state to measure and publicise the proficiency scores of the pupils for each school. Measures were published both for socio-demographic subgroups and averaging over all pupils in the school, and Dee, 2020 notes that this strong focus on subgroups 'was one of its most universally admired features'.

Studies of NCLB comprise the majority of the published research on school accountability, though all such papers have identification challenges. Another important issue, particularly with small sub-groups, is the importance of measurement error and the scope for simple random variation to influence school accountability ratings (Kane and Staiger, 2002). The early evidence is usefully summarised in Figlio and Loeb, 2011, and they note that all papers have to manage "the difficulty of isolating the effect of NCLB from other concurrent changes". Dee and Jacob, 2009 use the federal introduction of NCLB and compare states that had implemented a system of school accountability before NCLB. They found that NCLB had no impact on reading scores and a 0.15 pupil-level standard deviation impact on maths scores. Hanushek and Raymond, 2005 use state-level accountability, pre-NCLB, and adopt a state-level fixed effects model and find a positive effect of around 0.2 of a (student-level) standard deviation on test scores. Other studies exploit discontinuities in school accountability ratings and adopt a regression discontinuity approach. They show that schools receiving low ratings subsequently showed positive conditional impacts on pupil achievement gains, with strong and substantial effects (Figlio and Rouse, 2006, Rouse et al., 2013, Chiang, 2009, Rockoff and Turner, 2010).

Figlio and Loeb, 2011 conclude that '...taken as a whole, the body of research on implemented programs suggests that school accountability improves average student performance in affected schools'. A retrospective of the lessons from the past 30 years of school accountability in the US concludes similarly that NCLB was effective in changing performance (Dee, 2020). Dee, 2020 is less optimistic about its successor, Every Student Succeeds Act (ESSA), with its greater flexibility and diversity diluting the single-minded focus on student performance.

Other studies in other countries have cleaner identification strategies. In England and Wales, the school accountability programme was established in 1992. Burgess, Wilson, and Worth, 2013 exploit a policy experiment that reduced school accountability in Wales but not in England. Using a difference-in-differences analysis, Burgess, Wilson, and Worth, 2013 find significant and robust evidence that this reform markedly reduced school effectiveness in Wales. The impact is sizeable, 0.23 of a (school-level) standard deviation, equivalent to 0.09 of a pupil-level standard deviation. In this study, the significant heterogeneity shows a much stronger effect on attainment of low-achieving pupils.

Two other studies have evaluated the introduction of school accountability. Nunes, Reis, and Seabra, 2015 show that the publication of school rankings significantly affects parents' school choices and eventual enrolment in Portugal. For the Netherlands, Koning and Van der Wiel, 2012 show that the lowest ranked schools raised performance substantially (up to 12% of a standard deviation of school average test scores) after the publication of school quality scores.

There is consensus in this literature that accountability measures raise student performance, and typically more for low-performing pupils. There are fewer studies showing how this is achieved. The following section will focus on teachers' career choices as one mechanism. In the US, teachers are also found to change their teaching practices, for example spending more of the school day on instruction (Rouse et al., 2013) and work harder but also narrow the curriculum (Reback, Rockoff, and Schwartz, 2014).

In addition, Craig, Imberman, and Perdue, 2015 show that the ratings are reinforced by school district administrators, who reward high-performing schools with more funds. Standardised testing is also important: Bergbauer, Hanushek, and Woessmann, 2018 use data from PISA across 6 waves and 59 countries, and find a positive impact of standardised testing on student outcomes, the effect being larger in low-performing countries.

There have been few studies of the *long-run consequences* of accountability. One valuable exception is a study of Texas public schools, under accountability from the 1990s (Deming et al., 2016). Results show different effects of accountability according to school quality: there are long-run positive effects from accountability systems focused on schools at risk of failing a low threshold, but accountability relative to a high threshold target showed no student gains, and may in fact have caused harm.

2.4.2 Effect on teacher career choices

The introduction of test-based accountability would generally imply changes in teachers' working conditions. For example, life as a teacher in schools newly publicly recognised as high-performing might be more rewarding, whilst in schools seen as low-performing, teacher performance may be much more closely inspected. This might potentially affect decisions on where to work, on teacher decisions to quit or be replaced, on whether to join the profession at all, and on teacher sorting between schools.

Clotfelter et al., 2004 show that the introduction of accountability in North Carolina reduced teacher retention rates, particularly in low-performing schools. This did not lead to lower qualifications of the teachers coming in to replace the leavers, however, so the overall impact on students is unclear. Elacqua, Hincapié, and Martínez, 2019 show a similar effect: after the introduction of a new accountability programme in Chile, between-school mobility among less effective teachers in low-performing schools increased; however, there was no increase in mobility out of the profession as a whole.

Gjefsen and Gunnes, 2020 study the introduction of school accountability in Oslo in 2003. While they too find substantially increased teacher mobility in response to the provision of school quality information, by contrast they found that most teachers who changed jobs actually left the profession entirely. Again, this is likely to be transiently detrimental for students, as these teachers were typically replaced by high-ability teachers. In Florida, Feng, Figlio, and Sass, 2018 explore teacher mobility in more detail across the school-performance distribution. They show that teacher turnover is not generally affected by their school's performance grade, but this is not true for schools designated as "failing". Counter-productively, in such schools, it is the most effective teachers who disproportionately leave; even in this case, though, student scores improved.

Within schools, Boyd et al., 2008 show that high-stakes testing and accountability also altered the allocation of teachers to grades in New York. Teachers also face greater work pressure from accountability. Reback, Rockoff, and Schwartz, 2014 show that accountability pressure from NCLB lowers teachers' perceptions of job security and causes untenured teachers in high-stakes grades to work longer hours than their peers.

Overall, the evidence suggests that school accountability increases scrutiny to some degree on teachers' work, but has not made major differences to career decisions. While teacher turnover does appear to increase somewhat with accountability, particularly at less effective schools, this is not inherently damaging to students. Studies differ in estimates of which teachers leave, but in most cases it is not clear that there is a negative impact on student outcomes.

2.4.3 Unintended consequences

Schools may undertake strategies to game the system under accountability, in addition to or instead of efforts to improve pupils' learning. These behavioural distortions can take many forms, from concentration of teacher time and effort, to outright cheating in the exams.

It has been generally established that schools will tend to focus their resources on whatever is tested: the subjects that are tested, the topics within subjects that are tested, the topics in which scores can be increased most easily, the school grades that are tested, and on the pupils who may be pivotal in reaching a threshold. Figlio and Loeb, 2011 summarise all this evidence, and Rouse et al., 2013 also review evidence on a range of responses by schools. Whether this focus on the things tested is a bad thing depends on the tests: this focus may in fact be what society wants and intends, and if the test is well-designed it may be wholly appropriate. Conversely, if the high-stakes tests are not well-designed, then the lack of broader knowledge and skills can be deleterious.

One way of gauging the degree of 'teaching to the test' is to compare performance on high-stakes tests with that on low-stakes tests covering the same material. Jacob, 2005 compared test score gains in maths in high stakes test to those on a comparable, but lowstakes, test; he showed that the gains for eighth graders were confirmed in the low-stakes tests, but that those for fourth grade pupils were not. Similarly, Figlio and Rouse, 2006 find a smaller impact of accountability on low-stakes tests than on high-stakes tests.

Beyond focusing school resources on a subset of subjects, topics and pupils, researchers have documented other practices which, while not illegal, are certainly not as intended. Figlio and Getzler, 2006 show that students expected to be low-performing were disproportionately assigned into "special education" categories that were exempt from accountability measures. Figlio and Winicki, 2005 show that schools change their lunch menus at the time of the tests, 'substantially increasing calories in their menus on testing days'; Bokhari and Schneider, 2011 show that pupils in schools under stronger accountability threat 'are more likely to be diagnosed with Attention Deficit/Hyperactivity Disorder (ADHD) and consequently prescribed psychostimulant drugs'; and Anderson, Butcher, and Schanzenbach, 2017 show that pupils in such schools have a higher chance of being obese through less exercise in school.

Finally, there is straightforward cheating on the test by teachers or school administrators. Jacob and Levitt, 2003 show that the frequency of cheating appears to respond strongly to relatively minor changes in incentives, such as those implied by school accountability measures. Bertoni, Brunello, and Rocco, 2013 also implicitly detect cheating by noting that test scores in Italy are lower when external monitoring of tests takes place.

The existence of these inappropriate behaviours does not mean that accountability measures should be abandoned; the costs need to be weighed against the benefits, and appropriate policy designs to attenuate such behaviours.

2.5 School choice and market-level attainment

The previous section 'School accountability' presents evidence to suggest that accountability - public information about schools' performance - raises standards, although potentially with unintended consequences. The review now turns to the overall effect of school choice as a market mechanism on attainment, before considering the overall effect on segregation in the following section.

Whether school choice has the potential to raise market-level attainment is the subject of three, separate but related, strands of research. The first relates indices of competition to schools' performance in systems of co-ordinated admissions, where schools compete for students without the introduction of voucher schemes or Charter schools. The second assesses the impact of competition from independent schools through the introduction or expansion of school vouchers. The third instead estimates the impact of competition from alternative 'outside options': Catholic schools and Charter schools. In each strand of research, the identification problem is to isolate the impact of competition from factors correlated with competition, such as neighbourhood composition, which might also affect productivity. In a meta-analysis, Jabbar et al., 2019 focus on how the context of school choice - the 'rules of the game' - determines the competitive effects. Overall, the effects of school competition on student achievement were positive, but, the authors conclude that 'the effects are too small to have a major impact on educational quality and inequality on their own'.

2.5.1 Co-ordinated admissions

As described in 'Allocation mechanisms', co-ordinated admissions are school choice systems where parents have a choice of schools within a school district or region. These areas may be more or less competitive, and may have higher or lower attainment as a result. The key issue is to identify a causal effect between competition and pupil attainment; many studies report associations between them (reviewed in Levin and Belfield, 2003, see for example Borland and Howsen, 1992) but plausibly exogenous differences in competition across areas are rare.

Hoxby, 2000 uses natural landscape features to instrument for historical school district boundaries and the Herfindahl-Hirschman Index (a spatial measure of competition). She shows that metropolitan areas with exogenously more school districts – and therefore higher competition – have higher attainment. The findings have been strongly questioned by Rothstein, 2007, however, arguing that they are not robust to simple changes in data coding or sample selection (including students in private schools); taking these into account he finds no impact of competition. These suggestions, in turn, are strongly rejected by Hoxby, 2007.

Dijkgraaf, Gradus, and Jong, 2013 study the impact of competition in the Netherlands, which has a long history of school choice characterised by the authors as 'a full voucher program with 100% funding'. To construct an instrument for the concentration of schools in the local area, Dijkgraaf, Gradus, and Jong, 2013 use the number of school sites (rather than schools), argued to be a plausible instrument due to historical institutional factors that largely prohibit new schools. Increases in competition are associated with a small *decrease* in attainment in this context. De Haan, Leuven, and Oosterbeek, 2016 study school consolidation in the Netherlands as a result of reform, which reduced competition by creating fewer, larger, schools. Exploiting variation across municipalities (driven by non-linearities from the reform), De Haan, Leuven, and Oosterbeek, 2016 find no negative effect of the reduction in competition, and an overall positive effect resulting from economies of scale.

An alternative approach to studying the impact of competition is to exploit administrative boundaries. For the UK, Gibbons, Machin, and Silva, 2010 use the distance of a primary school from its nearest local authority boundary to instrument the amount of competition it faces; they find no overall effect of choice or competition on school performance.

Finally, some research studies the *implementation* of school choice. Lavy, 2010 studies the switch from inter-district bussing to a school choice system in Tel-Aviv. As this is not experimental variation, Lavy uses alternative identification strategies (difference-in-differences) and comparison groups (untreated tangent neighbourhoods and other cities) to show that the choice system increases school completion and raises cognitive achievement.

2.5.2 Voucher schemes

An educational voucher entitles a child to attend a different school than their 'default'. Details vary across schemes, but in essence it is seen as an 'escape' from a low quality or poorly matched local school. (See Epple, Romano, and Urquiola, 2017 for a summary of key features of voucher schemes worldwide.) The biggest voucher programmes are in Chile, Sweden and the Netherlands, but they are present in a wide range of other countries: Belize, Canada, Colombia, Denmark, India, Japan, New Zealand and Poland (Bettinger, 2011, Epple, Romano, and Urquiola, 2017); and of course in the US, where there is normally specific entitlement (based on income, for example) rather than general entitlement.

This section focuses on the empirical evidence for whether competition induced by voucher schemes raises attainment.[5] The identification problem is that voucher school enrolment (and therefore competitive pressure) is likely to be endogenous to the quality of local public schools. Residential location may also be endogenous to the quality of local schools for some households (discussed further in sub-section 'Evidence from residential demand').

There appear to be no definitive answers yet. In their substantial review, Epple, Romano, and Urquiola, 2017 argue that the bulk of the findings suggest no significant effect, yet 'multiple positive findings support continued exploration'. Similarly, Urquiola, 2016 surveys the literature on the impact of competitive pressure from private schools following voucher provision on the

[5] The section 'Do schools matter' summarises the evidence that attending a voucher school improves attainment.

performance of public (state) schools, concluding that the evidence is mixed. Egalite, 2013 reviews the evidence up to November 2012, and concludes that the findings from studies with the most robust design are uniformly positive: competition from private school vouchers increases student attainment. These peer-reviewed papers (Figlio and Rouse, 2006, Rouse et al., 2013, West and Peterson, 2006) study Florida, however, where vouchers are combined with public accountability, so are unlikely to isolate the impact of competition.[6]

Turning to research outside the US, comparing areas with varying expansion of free schools, Böhlmark and Lindahl, 2015 find small positive results from competition and choice in Sweden, ten years on from the reform. The benefits are realised for longer-term outcomes such as university attendance, and are due to competition effects rather than higher productivity at the independent 'free schools'. These findings are consistent with earlier research from Sweden (Sandström and Bergström, 2005). Using a difference-in-differences approach for Chile, Hsieh and Urquiola, 2006 find that areas with higher growth of private schools have the same test score growth as areas with lower growth of private schools. Böhlmark and Lindahl, 2015 suggest that the difference between findings for Chile and Sweden is due to the degree of selection by schools, subsequent sorting of students by socio-economic background, and therefore household choices driven more by consideration of peer-group rather than school productivity. In contrast, using instrumental variables for the growth of private schools across Chile, Gallego, 2013 finds that 'a one-standard-deviation increase in the ratio of voucher school to public schools in a market increases test scores by about 0.10 standard deviations'.

Muralidharan and Sundararaman, 2015 provide the first experimental evidence on the provision of vouchers to private schools. In India (Andhra Pradesh), a 'two-stage lottery-based allocation of vouchers' created student-level and market-level experiments. Muralidharan and Sundararaman, 2015 find no evidence of competition induced spillovers to public school students. Those in public schools in villages with competition from private school vouchers have the same attainment as those in public schools in control villages.

In earlier work, Hoxby, 2003b finds that public schools respond to competition from the choice programme by raising the achievement levels, although subsequent work challenges these findings (Ladd, 2002, Ladd, 2003, Bettinger, 2005, Bifulco and Ladd, 2006).

Overall, there seems to no unanimous evidence (outside Florida where the reforms also included accountability measures) that competition induced by voucher schemes raises performance of public schools. The only experimental evidence, from India, finds no effect, which is perhaps the over-riding finding - students in public schools are not typically harmed or helped through competition induced by voucher schemes.

[6] See also following work (Figlio, Hart, and Karbownik, 2020).

2.5.3 Other 'outside options': competition from Catholic and Charter schools

Competition for public/state schools can come from alternative schools, such as Catholic schools, and, in the US, Charter schools. The evidence for each source of competition is discussed in this section.

Card, Dooley, and Payne, 2010 evaluate whether competition between publicly funded secular and Catholic primary schools in Canada lead to more productive schools. Areas with more children from a Catholic background have greater demand side pressure for secular schools, which should raise standards. The identifying assumption is that the proportion of Catholic families across areas is otherwise uncorrelated with school outcomes. The authors find that, conditional on the percentage of Protestant families in an area, there are small positive effects from competition.

In contrast, evidence for England finds little evidence that competition from Catholic schools raises area-wide pupil attainment. Allen and Vignoles, 2016 use national pupil-level data for secondary schools, instrumenting the number of Catholic schools with the historical Catholic population. This research design is similar to West and Woessmann, 2010, who find that historically induced competition from Catholic private schools increases attainment, across a sample of OECD countries that participated in PISA 2003.

Competition from Charter schools is not exogenous. Identification strategies to overcome this problem are to exploit the timing of Charter school entry (Cordes, 2018) or instrumental variables (Bettinger, 2005, Imberman, 2011, Gao and Semykina, 2020), most often combined with student fixed effects. Cordes, 2018 finds small positive effects of Charter school competition on public school performance in New York City, which increase with proximity to the Charter school (highest in schools which are 'co-located' - in the same building). Cordes, 2018 notes that previous research has typically studied the impact of competition from Charter schools at the district or larger geographical area level. These studies find either small positive (Sass, 2006) neutral effects (Bifulco and Ladd, 2006, Bettinger, 2005, Zimmer and Buddin, 2009) or negative effects (Imberman, 2011, Ni, 2009). Gao and Semykina, 2020, adopts an alternative measure of competition from Charter schools based on travel time based distance, finding typically neutral competition effects.

Consistent with previous reviews (Gill, 2016 and Epple, Romano, and Zimmer, 2016) it is clear that there is no overwhelming support for strong competitive effects from Charter schools. Research is largely consistent (with the exceptions of Imberman, 2011 and Ni, 2009) that students in public schools are not harmed by the presence of local Charter schools, at least. Small positive or neutral competitive effects are typical, providing some evidence for 'healthy competition hypothesis' (Gill, 2016).

2.6 School choice and market-level sorting

The section 'School choice and market-level attainment' presented no overwhelming evidence that school choice and/or competition between schools induces a strong improvement in pupil attainment. This is surprising, as section 'Parents' preferences for schools' concluded that parents typically value school quality, at least as measured by test scores. This inconsistency will be considered in the final section 'Taking stock'. The framework for studying school choice and accountability also highlighted the potential for school choice to influence segregation across schools, which will be the subject of this section.

Theoretical and empirical research has contributed to the complex interaction between school choice and sorting of students across schools. Beginning with the theoretical literature, Hoxby, 2003b argues that there are no very general theoretical predictions about student sorting under school choice. For example, Nechyba, 2003 shows that a pure state school system leads to more spatial segregation than a private system.

Similarly, Epple and Romano, 2003 analyse three different student assignment regimes: neighbourhood schooling (a strict residence requirement for admission); school choice with no choice costs; and choice over many school districts and show that different public policy regimes have dramatic effects on the nature of sorting. Neighbourhood schooling leads to strong income stratification across neighbourhoods, whereas costless, frictionless choice equalises peer groups across schools. Calsamiglia, Martínez-Mora, and Miralles, 2015 assess the impact of school allocation rules on socio-economic sorting into schools and neighbourhoods. The main findings are that priority in over-subscribed schools to local applicants leads to segregation, whether a truth-revealing allocation mechanism is used.

Previous empirical research has used either event analysis or counterfactual simulation to study segregation under school choice. In the former strand of research, the consensus is that introducing school choice has not led to markedly higher segregation between social groups in England (Allen and Vignoles, 2007, Goldstein and Noden, 2003, Gorard and Fitz, 2000, Noden, 2000). Analysing student-level data from England, Burgess et al., 2007 find that the degree of student sorting by ability and socio-economic status varies considerably across the country, however.

Elsewhere, school choice is related to increases in segregation across schools. In Chile, vouchers led to increased sorting as the 'best' students left for private schools (Hsieh and Urquiola, 2006). In New Zealand, where schools were permitted to charge non-compulsory fees in addition to per-pupil funding from government and interview parents for their 'enrollment scheme', sorting between European and Minority students increased (Ladd and Fiske, 2001). In Stockholm, segregation by ability increased in response to reforms that replaced residence based admissions criteria with free school choice, where admission to oversubscribed schools is dependent on academic grades (Söderström and Uusitalo, 2010) and in Sweden more generally (Böhlmark, Holmlund, and Lindahl, 2016). For the US, segregation by race increases in North Carolina in response to Charter school expansion (Bifulco and Ladd, 2007). The only paper in this strand of literature from Asia finds consistent patterns. In Seoul, South Korea, schools became more segregated by student performance after a school choice policy was introduced, with segregation increasing from a low level (Oh and Sohn, 2019).

In the latter, counterfactual, strand of research, segregation is typically found to decrease under 'neighbourhood' allocation to schools (Allen, 2007, Taylor, 2009, Bifulco, Ladd, and Ross, 2009, Östh, Andersson, and Malmberg, 2013, Bernelius and Vaattovaara, 2016). Glazerman and Dotter, 2017 find that a neighbourhood schools policy would decrease segregation by race but increase segregation by income.

It is known that higher residential segregation is linked to higher segregation in schools (Taylor and Gorard, 2001, Burgess, Wilson, and Lupton, 2005, Lindbom, 2010, Böhlmark, Holmlund, and Lindahl, 2016). Lindbom, 2010, drawing on analysis from Lindbom and Almgren, 2007, concludes that residential segregation in Sweden is the 'main villain of the piece', being the predominant cause of school segregation. Aside from residential segregation, possible drivers include those on the supply-side, for example 'cream-skimming' of desirable pupils (West, Hind, and Pennell, 2004, West, Ingram, and Hind, 2006), and, on the demand-side, differences in preferences and inequalities in information between groups (see section 'Parents' preferences for schools'). The contribution of households' preferences to segregation is particularly under-explored, with only one working paper decomposing segregation between multiple sources (Oosterbeek, Sóvágó, and Klaauw, 2019).

Overall, the evidence from a number of countries suggests that the combined process of choice by parents and proximity rationing by schools leads to greater sorting. Evidence from Madrid shows that school choice policies must have careful design to achieve diversity objectives (Gortázar, Mayor, Montalbán, et al., 2020). How to design school choice systems to encourage diversity is an area of on-going research. Bjerre-Nielsen and Gandil, 2020 demonstrate that re-designing school districts in Denmark is insufficient, as households respond by exiting to another public school, or, to some extent attending a private school or moving home.

2.7 Do schools matter?

This review has largely been concerned with the allocation of pupils to schools, and whether school choice and accountability improve pupils' outcomes. Before taking stock in the final section, this penultimate section summarises the evidence that schools do in fact matter - that they have a causal impact on pupil attainment. In each case, the identification problem is that 'better' students might select into 'better' schools. Identification strategies to overcome this problem typically use regression discontinuity design, comparing those marginally accepted or rejected, or exploit lotteries for admissions that act as natural experiments in allocation to schools. Even in this case, however, non-random attrition may bias the results: those that are marginally rejected are more likely to 'opt-out' of the state system (Howell, 2004, Bibler and Billings, 2020).

To summarise this literature, Beuermann and Jackson, 2020 conduct a meta-analysis on all publicly-available studies that use 'quasi-random assignment to a preferred (non-Charter) public school (either through lottery or selective enrollment exam)'. Beuermann and Jackson, 2020 find that:

> The precision-weighted average test score impact across all studies is positive. However, it is small and cannot be distinguished from zero. Importantly, this is not the result of averaging large positive and large negative effects, but rather reflects the fact that most studies (13 out of 17) cannot reject null impacts for the overall population.

Epple, Romano, and Urquiola, 2017 summarise the findings on the effect of attending a voucher school thus: 'A perhaps surprisingly large proportion of the best-identified studies suggest that winning a voucher has an effect on achievement that is statistically indistinguishable from zero.' In the only experimental evidence, Muralidharan and Sundararaman, 2015 find no differences in test scores 2 and 4 years after allocation between lottery winners and losers in India, except for Hindi, which is not taught in public schools.

For marginal students attending an 'elite' exam school, the estimated effect of admission on educational attainment is neutral (Clark, 2010, Abdulkadiroğlu, Angrist, and Pathak, 2014, Dobbie and Fryer, 2014, Lucas and Mbiti, 2014, Dee and Lan, 2015, Zhang, 2016) or positive (Pop-Eleches and Urquiola, 2013, Deming et al., 2014, Clark and Del Bono, 2016, Ding and Lehrer, 2007). Longer-run outcomes are more consistently positive (Clark, 2010, Berkowitz and Hoekstra, 2011, Clark and Del Bono, 2016) except for Dobbie and Fryer, 2014. Dustan, Janvry, and Sadoulet, 2017 highlight the trade-offs that households face in admission to elite schools, in the Mexican context. Marginal admission increases test scores in maths, but also significantly increases the probability of high-school drop out, particularly those with longer commutes and weaker prior attainment.

A limitation of this strand of research is that the estimated effects are generalisable to marginal students only. Exceptions are where the country context permits multiple 'cut-offs' for admission. Lucas and Mbiti, 2014 are able to study heterogeneous effects of entry to the top schools in Kenya through the multiple thresholds within each school caused by district specific quotas. Despite large changes in peer group (scoring around half a standard deviation higher on the baseline test compared to the alternative school), school resources and teacher experience, there are no significant improvements in test scores for marginally admitted students. There are no significant differences in test score effects by baseline scores, quality of alternative options, gender, or socio-economic status.

Finally, the effects of attending a 'preferred school' through co-ordinated admissions are typically positive and reasonably sized (Jackson, 2010 Deming et al., 2014, Abdulkadiroğlu et al., 2020) or neutral (Cullen, Jacob, and Levitt, 2006).

The discrepancy between high demand for these schools and lack of test score gains, overall, may be due to causal improvements in longer-run outcomes that are unrelated to shorter-run test score gains. To test this hypothesis, Beuermann and Jackson, 2020 use survey data from Barbados, in addition to administrative educational data to estimate the causal effect (from regression discontinuity in test scores) of attending a preferred school on 'a broad set of social and economic outcomes measured in adulthood'. Consistent with the meta-analysis, they find no effect of preferred school attendance on test scores at the end of secondary school. Longer-term outcomes are improved, however: marginal pupils are more likely to earn a post-secondary credential at age 18, and between ages 25 and 40 there is considerable improvement in an index of adult well-being.

This body of research is expanding rapidly, moving to explore the heterogeneous effects of school-effectiveness (Jackson et al., 2020). Research at the frontier estimates returns to school-effectiveness, rather than measures of academic attainment or indicators for 'elite' school status.

2.8 Taking stock

Research on school choice and accountability is important to determine whether these two commonly used tools to improve standards of public education are effective. The evidence suggests that accountability - public information about schools' performance - raises standards. In part, this works through changing the pattern of teacher turnover, with less effective teachers more likely to leave. Although accountability can lead to perverse incentives, the balance of evidence suggests that accountability should remain unless these costs outweigh the benefits.

Despite intuitive theoretical arguments, a substantial body of empirical evidence finds that competition induced by school choice has had limited (though positive) effects on the performance of public schools. One condition for this occur would be if parents don't value standards of education. At face value, this is not the case - research on parents' revealed preferences and property prices around 'good' schools is unanimous that parents do value 'school quality'. Whether parents value *school-effectiveness*, however, rather than academic standards (that conflate school-effectiveness and the peer group) is more debated. MacLeod and Urquiola, 2019 provide a theoretical model for why parents may rationally care about test scores above value added, with education partly as an investment good, rather than only a consumption good. The essence is that school choices will be influenced by factors other than effectiveness if parents care about longer run outcomes, for example employability or prospects in the marriage market, and schools differ in their access to the most productive matches.

Overall, there is mounting evidence that parents do value school-effectiveness. Research that uses a rich set of school attributes and/or estimates school-effectiveness on longer-run non-academic outcomes tends to find that parents do value this dimension. The consensus that the competitive effect of school choice is small is therefore surprising, but may be due to parents' preferences for proximity and the peer group. Abdulkadiroğlu et al., 2020 note that estimated preferences for peer-quality may be because parents use this observable attribute of schools to proxy for unobservable measures of quality, concluding that 'Distinguishing between true tastes for peer quality and information frictions is another challenge for future work'.

A second conundrum is why parents have such strong preferences for 'quality', when there is inconsistent evidence that schools causally improve academic outcomes? Empirical research largely using boundary discontinuities finds limited support that attending a 'better' school improves attainment, either through a voucher to attend a private school, or an exam to attend an 'elite' school. There is slightly more evidence of benefits from assignment to a preferred school assigned through an algorithm. There is mounting evidence for an impact on longer-run and non-academic outcomes, however, particularly for 'elite' schools, which might suggest that parents have some information about the potential wider benefits to these schools. The insight from MacLeod and Urquiola, 2019 that education is partly as an investment good may also contribute.

There is evidence for heterogeneity in preferences between more and less advantaged households, with less advantaged households typically placing less weight on school quality. Further research should explore whether this reflects constraints in access or information, however, as the provision of information can reduce such inequalities (Hastings and Weinstein, 2008). Further research into the effect of information provision and the 'choice architecture' (Glazerman et al., 2020) in the digital age is required.

In response to competition, schools may choose alternative responses, covert or overt selection of students to improve observed 'quality', for example. Abdulkadiroğlu et al., 2020 provide suggestive evidence that schools in New York City have increased screening of students in the years following the introduction of centralised school choice. Outside the economics literature, there is also evidence that schools respond to competition by increased expenditure on marketing (Oplatka and Hemsley-Brown, 2012) as a lower risk alternative to pedagogical reform.

There are some unambiguous benefits of school choice. Centralising school choices is clearly welfare improving, but surprisingly not universally in place in developed and developing countries. The benefits of truth-revealing assignment mechanisms within a centralised system are evident, although with the trade-off that parents can't express the strength of their preferences in mechanisms currently used in practice. Further research is needed to determine the optimal design of school choice to meet policy objectives, for example reducing segregation and enhancing social mobility through more equal access to effective schools.

3 School choice in England: evidence from national administrative data

Preface: *This literature is based on a published article, co-authored with Simon Burgess and Anna Vignoles (Burgess, Greaves, and Vignoles, 2019). I conducted all data analysis and took the lead on drafting the article, in particular integrating our findings with the existing literature. Our motivation to write the article was to present nationally representative empirical evidence from (at the time) a new source of data on parents' school preferences, as part of a continuing research agenda on school choice and school admissions.*

3.1 Introduction

Schools matter and schools differ: educational attainment is key to a child's life chances, and schools vary in their ability to raise attainment. For this reason, the process that a country uses to assign its children to schools is important for their subsequent academic achievement and their life chances. Since 1988, England has used a system of school choice to do this. Parents nominate their preferred schools and, subject to school capacities, a set of published criteria are fed into an algorithm to determine the allocation of pupils to schools. This process has been standardised across areas and refined over time to prevent covert selection of pupils by schools (White et al., 2001, Allen, Coldron, and West, 2012).

This paper provides up to date quantitative evidence on the functioning of school choice in England, building on a large body of research on the process and impact of school choice that followed the introduction of market-based reforms in 1988. We use newly available national data on households' school choices to provide the most representative picture of the school choice market to date. The national approach that we take in this paper using quantitative data complements a range of qualitative approaches that have been used to explore this issue. For example, there is an important literature on the sociology of school choice which has relied on analysis of rich qualitative data from a small sample of parents (Ball, Bowe, and Gewirtz, 1996, Ball and Vincent, 1998, Reay and Ball, 1998, Bagley, Woods, and Glatter, 2001). There is also a literature which has taken a geographical approach, studying specific areas or group of areas in depth to account for the local context (Parsons, Chalkley, and Jones, 2000, Taylor, 2000, Taylor, 2009). Of course, these different approaches have relative strengths and weaknesses. Using national data has the benefit of representativeness but at the expense of detail. We argue that this is appropriate for the system wide research questions we pose. The contribution of this paper is therefore to provide a nationally representative and comprehensive picture of the current working of school choice in England, which will inform more detailed future research that can fully account for the local context and explore important emerging themes.

Our two main research questions are, first, to quantify the extent to which parents actively use the school choice system and the variation in active use across households and neighbourhoods. Previous studies have shown that parents in chosen areas and social circumstances make informed choices for their children. We are the first to show explore this for parents across the country, focussing on the variation in the extent of active choice across contexts. Second, we ask whether school choice is effective, motivated by the theoretical knowledge that school choice will improve standards in schools if and only if parents value this dimension of school quality. By effective we mean improving education standards for pupils in England, through access to more highly performing schools and improvements in education standards overall, and providing meaningful choice for parents, in that parents have a number of feasible schools to choose from, and that choices lead to admission to preferred schools.

The first research question is answered using several indicators of active choice, described in turn.

Do parents choose the closest school, regardless of academic standards? Choosing the closest school will dampen the incentives for schools to improve academic standards and is therefore a key indicator for the system. Existing evidence is mixed. Following the 1988 reform, Parsons, Chalkley, and Jones, 2000 found a progressive rise in transfer to out-of-catchment secondary schools in one Local Education Authority, from 33% to 39% between 1991 and 1996, suggesting increasing use of school choice. The rise was primarily due to choices by parents in 'struggling' or 'aspiring' neighbourhoods rather than those in more prosperous neighbourhoods, who presumably chose their school in advance through their residential choice. Allen, 2007, using an early census of national data, shows that around 50% of households attend their closest school, but her study lacked data on households' choices. From the sociology of education perspective, Ball, Bowe, and Gewirtz, 1996 discuss the importance of the local school for households defined as 'disconnected choosers', who are constrained by 'spatial horizons and the practicalities of travel'. In relation to travel time, Taylor, 2000 finds that 74% of households believe there is a maximum limit of travel time to school, with the acceptable limit varying across urban and rural respondents. It is also worth noting that although an important indicator for the working of school choice, choosing the closest school has a complicated interpretation. It may suggest a passive engagement with the system, but alternatively, may follow an active residential choice or a constrained choice if only the closest school is considered feasible by parents.

Do parents make the minimum or maximum number of choices available? The number of choices households make is indicative of how actively the system is used. Choosing zero or one school might imply that either the school choice system is not properly understood or that parents do not have strong preferences. It may also mean in some areas that there is only one choice that appears feasible to parents. There is little existing evidence on the number of choices that households make and the variation in this across areas. As an exception, from a sample of 215 parents in 8 schools in the mid-1990s, Taylor, 2000 finds that 41% of respondents consider only one school.

Is there variation by area and household type? Ball, Bowe, and Gewirtz, 1996 find that choice in education is systematically related to social class differences. For example, respondents classified as 'privileged/skilled choosers' were predominantly from higher social class households, while the 'disconnected choosers' were predominately from lower social class households. This may in part be due to the child's role in school choice, which in working class households has been found to be more influential (Reay and Ball, 1998), although Taylor, 2000 finds that the child is involved to some extent in 86% of households. There is also a small literature concerned with patterns of choice across ethnic groups. Weekes-Bernard, 2007 finds that, in common with the wider population, many Black and Minority Ethnic (BME) households were unable to exercise choice, in that their desired school was unattainable. From the sample of around 180 parents in three Local Authorities, Weekes-Bernard also finds a preference for Muslim schools for aspirant Muslim parents, which overrides a general preference for proximity, particularly among recent immigrants. Studying the impact of migration on school choices in Greater Manchester, based on semi-structured interviews with 11 migrant parents, Byrne and Tona, 2012 find that there are knowledge barriers to school choice, particularly for new migrants, and that 'hot' or 'grapevine' knowledge (Ball and Vincent, 1998) informed choices rather than published information. A finding common to Weekes-Bernard and Bryne and De Tona is that migrant parents search for the 'right' social and racial mix for their children, which may be distinct from the preferences of White British households. The academic environment of the school is particularly important to many immigrant families. This is typified by one respondent in the study who expressed agreement with a British South Asian journalist that 'the only thing we can get from this country is education, so we have to get that'. Studying specifically Polish migrants to England and Scotland, and based on 25 interviews, Trevena, McGhee, and Heath, 2016 discuss the complicated process of school choice for those without established cultural capital.

For our second research question, regarding the effectiveness of school choice, we consider whether parents value the academic standards of schools, which is a critical requirement for the market mechanism to improve the quality of education. We also discuss a commonly used indicator for the success of school choice, namely whether a household achieves its first choice of school.

Do parents value academic standards in school choice? For school choice to operate effectively, parents must value academic standards so that schools have an incentive to improve. If parents choose without reference to pupil progress, this will not provide a strong incentive for schools to try to improve in this dimension. The qualitative and quantitative approaches are broadly consistent in concluding that many parents value academic standards when choosing schools, but this may not be the deciding factor. The most commonly cited preferred school characteristics that parents value include academic quality (performance), distance between home and school and the social and ethnic composition of the school, although there are myriad other factors (Raveaud and Zanten, 2007, West and Hind, 2007, Gibbons and Silva, 2011, Burgess et al., 2015). For Scotland, Willms and Echols, 1995 find that parents who make an explicit choice away from their designated school did so for 'social and reputational factors' as well as the disciplinary climate, while the academic quality of the school was of lesser importance, particularly for parents from lower socio-economic backgrounds. Similarly, Burgess et al., 2011 use information from a nationally representative survey of parents in England and find that 'proximity' and a 'general good impression' of the school are most commonly cited reasons for school choice, followed by academic standards. While most research using parents' stated preferences has emphasised the importance of academic standards, Chakrabarti and Roy, 2010 note that this strand of literature tends to over-emphasise its importance in relation to research on parents' revealed preferences (their observed choice of school). This may be because parents conform to socially accepted norms when questioned about their reasons for school choice (Jacob and Lefgren, 2007), suggesting that parents' revealed preferences are more informative. Alternatively, this inconsistency may be because revealed preferences are subject to constraints (for example the expected probability of admission) which would mean that preferences for academic standards are not necessarily overstated. Using households' actual choice of school, we show the extent to which choices are correlated with academic quality of the school.

Is achieving the first choice always best? There is a distinction between a household's first choice and preferred school. For example, Taylor, 2000 finds that 91% of respondents got their first choice, but 17% would have preferred to choose an alternative school in the absence of constraints. In a nationally representative later sample, Burgess et al., 2015 find that 7% of parents would have preferred to choose a different primary school. These constraints typically include the lack of places at popular schools, with priority given to those living closest. Indeed, Taylor and Gorard, 2001 note the 'enduring link between area of residence and the socioeconomic composition of local schools' as school choice is not free of geographical considerations. We therefore discuss the relevance of this indicator of success of the system.

The paper is structured as follows. Section 3.2 offers a summary of the school choice system in England and the resulting incentives for household choices. Section 3.3 describes the new dataset. Section 3.4 details the results, and we offer an overview of the findings and broader discussion in section 3.5.

3.2 The school choice process in England

Parents in the English state education system have the right to express a preference for the school that they would like their child to attend. Parents provide a ranking of their preferred choices of school on a form that is submitted in a centralised system to their Local Authority (LA).[7] All government funded schools (regardless of type) use this common application system.

[7] There are 152 LAs in England. LAs in urban areas are typically geographically small, for example coinciding with London Boroughs in London. Rural LAs tend to be geographically larger. LAs do not raise funding for schools locally, or fund schools directly. All school funding derives from central government.

Private schools are outside this system, although parents can apply to both private and state schools simultaneously. On the LA form parents can provide up to 3-6 choices of school in rank order, depending on the LA. Most LAs ask parents to list up to 3 schools. A set of published school prioritisation criteria are used where a school is over-subscribed. Typically, these include: whether the child has a statement of special educational need; whether the child is looked-after by the local authority; whether the child has a sibling at the school already; the distance of the family home from a school; and less commonly, the faith or aptitude of a child.[8] 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 by definition less popular).

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.[9] For example, listing one 'safe' school may be advantageous to avoid allocation to a school with spare capacity.[10] The LA is responsible for school allocations, considering parents' choices and school priorities and published admission numbers. This allocation is done using an algorithm (student optimal stable allocation, see Pathak and Sönmez, 2013) that is weakly truth revealing, meaning that parents can do no better than by reporting their true preferred schools. 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 preferred 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.

The School Admissions Code defines acceptable over-subscription criteria for schools.[11] More autonomous types of schools (now around 62% of all secondary schools) determine their own admissions criteria within the School Admissions Code. An interview with a parent, for example, is not an acceptable criterion. If a child is refused a place at a school, there is the right of appeal

[8]This is explicitly intended not as a measure of general ability, but a specific aptitude such as music, sport or maths for example.

[9]Prior to 2007, different Local Authorities in England used either a first preference first or an equal preference allocation mechanism. The first preference first system was outlawed in the 2007 admissions code since it prioritises students based on the rank order of parents' choices.

[10]The possibility to list more choices may mitigate this to some extent and allow more 'ambitious' choices in terms of school academic quality, which we explore.

[11]See the latest admissions code (2014).

to an independent panel. In 2015/16 the percentage of admissions resulting in an appeal was 3.7%, of which around one fifth found in favour of the parent(s).[12]

Parents can also devise strategies to maximise their chances of getting into their preferred school, for example by choosing a nearby home. Parents may therefore appear to value school proximity highly, but in fact the distance between home and school is driven by the admissions criteria. The higher demand for homes close to popular schools has been studied empirically and is acknowledged to increase house prices around 'good' schools (Black, 1999, Barrow, 2002, Gibbons and Machin, 2003, Kane, Riegg, and Staiger, 2006, Bayer, Ferreira, and McMillan, 2007, Fack and Grenet, 2010, Gibbons, Machin, and Silva, 2013, Machin and Salvanes, 2016).

3.3 Data

We use globally unique administrative data on parents' school choices. Most school choice analyses use either partial information on choices or full data from a particular locality or city. Unusually, our data covers the whole cohort of pupils who sought admission to any English state secondary school in the school year 2014/15. The parental choice data contain for each pupil: the ID of each nominated school (e.g. first, second and third choices in some areas and up to 6 choices in others), and the identity of the school that the child was offered, which may differ from the school that the pupil was finally enrolled in. Our dataset also links to the National Pupil Dataset (NPD), a census of all pupils in the English education system. Students whose families made a choice but don't enter the state-sector are included in the data. Access to these data was provided by the Department for Education, through the NPD application process.

The two datasets together provide us with: the characteristics of pupils and the detailed characteristics of all the schools they applied to (not just the one they enrol in), and their home location in relation to all their preferred schools and to their allocated school.[13] The sample is large (over 526k pupils) which permits fine-grained analysis. We analyse the whole cohort with only a few exceptions. We exclude middle school areas as there are two school moves rather than one. We include selective areas in which students must pass an examination to get into some schools (grammar schools). We compare LAs where more than three or only three school choices are allowed, and areas with higher and lower numbers of schools in the local area as a measure of population and school density.

For pupil characteristics, we focus on eligibility for Free School Meals (FSM), as a marker of poverty, and aggregate ethnic groups.[14] We also consider whether a pupil has English as an

[12] Department for Education (2017)

[13] The family's postcode is taken from the NPD – at the closest point to when the choice was made. If this is not available, the postcode recorded in secondary school is used.

[14] 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 Back-

Additional Language (EAL), and IDACI as a measure of neighbourhood deprivation. Appendix Table A2.1 shows the sample size for each subgroup.

The most important limitation is that we do not know the nature of the priority of each pupil for each of their school choices. In particular, we do not know whether the child has an older sibling at the school or whether the child is a 'Looked After Child', both of which have high priority in over-subscribed schools, generally overriding proximity. Not having this information complicates the analysis of parents' choices. If having an older sibling was evenly distributed through the cohort, then this problem should not bias our analyses, but this is unlikely to hold. In the Millennium Cohort Study, the presence of an older sibling of secondary school age is strongly correlated with household income: 67% of children in the lowest income decile have an older sibling of school age at the point relevant for school admission, compared to 33% in the highest income decile. Priority to a preferred school (which we do not observe) may therefore be correlated with household characteristics, such as eligibility for free school meals. This point needs to be borne in mind when considering the results.

3.4 Results

We present results relating to our two research questions exploring the functioning and the effectiveness of school choice. For the first research question, the extent and variation in active school choice, we present and discuss key indicators: the number of choices made by households, whether the first-choice school is the nearest, and variation by household and neighbourhood type. For the second research question, we explore whether households value academic standards of a school (with the implication that this leads to higher standards in England's schools), and whether school places reflect parents' preferences: the likelihood of receiving an offer from a first choice school. We also interpret this measure of success of the system. To explore the variation across households, results in all tables are shown separately by personal characteristics: FSM, ethnicity, EAL status and neighbourhood poverty level. We also show neighbourhood characteristics based on where the family making the choices lives: the number of choices families can make, and density. We postpone a broad discussion of the findings to the final section of the paper.

3.4.1 Active use and understanding of school choice

3.4.1.1 Number of choices made

Table 3.1 shows the number of school choices made on the LA form. The overall average number of choices made is 2.4, but there is wide dispersion. 35% of households make only one choice, while at the other extreme, 27% make the maximum number of choices permitted. There is very little difference between the number of choices made by richer and poorer households, as measured

ground') and 'Any Other Ethnic Group'.

by the child's FSM. Similar proportions of FSM and non-FSM households make only 1 choice and indeed similar proportions make the maximum number of choices. There are much bigger differences by ethnicity and EAL status, however. For example, 41% of White British households only make 1 choice, compared to 17% of Asian households and 12% of Black households. At the other extreme, 37% of these latter groups make as many choices as they can, compared to 24% of White British households. These differences are reflected in the split by EAL status, with a much higher fraction of EAL households making all choices possible and a higher mean number of choices.

There are striking differences across neighbourhood characteristics. First, population density (represented by the number of schools within 20km) is positively correlated with the number of choices: people make more choices in dense neighbourhoods, with far fewer making just one choice. We can illustrate this looking at specific places. In Hackney in central London for example only 9% of people make just one choice, while 27% make the maximum allowable six choices. Similarly, in Birmingham, 35% make six choices and over half make at least four choices. By contrast, in Cornwall, 77% of parents make just one choice. Second, the maximum number of choices allowed is correlated with the number of choices made. Almost twice as many choices are made in LAs where more choices are allowed, on average. This is partly related to population density, as urban areas are more likely to allow more than three choices, but there is evidence of a frustrated demand to make more choices for some parents, particularly in urban areas where only three choices are permitted.[15] For example, 71% of parents make the maximum number of three school choices in Brighton and Hove and presumably given the option, many parents would have made more choices.

Finally, as with the individual poverty measure, FSM, there are relatively slight differences in the number of choices made by neighbourhood poverty, the mean number of choices made being slightly higher in poorer areas.

Of course, many of these factors are strongly correlated, for example urban density with neighbourhood poverty. For this reason, we run a simple multivariate regression to control for these factors simultaneously. The results are in Appendix Table A2.2. Columns 1 to 4 show the results relevant to the number of choices made. The points made above from the raw data in Table 3.1 are confirmed by regression analysis, with all the highlighted patterns remaining.

3.4.1.2 First-choice school is the nearest school

Table 3.2 shows the percentage of households that nominate the nearest school as their first choice. Strikingly, this percentage is only 39%. These households appear to value proximity highly, but some will have moved home precisely to make their preferred school their nearest school. Around 3.5% of households in the Millennium Cohort Study report moving to a new

[15] In most areas of the country households can make a maximum of three choices. Exceptions are London (Pan-London co-ordinated admissions) and the surrounding area, Manchester and surrounding LAs, and Birmingham (among others). See the map in Appendix Figure 1.

house as a step to ensure the child is admitted to their preferred school (whether there is an older sibling of secondary school age in the household) but many more are likely to have considered local school quality in their choice of home. Even if we widen the definition to ask, 'is the nominated first-choice school within 20% distance of the nearest one', that is still only true for less than half of all families. This implies that most families do not prioritise distance from the school above all else.

There are important differences in this statistic by area characteristics. Unsurprisingly, households in less dense areas, with fewer schools and longer commute times, are more likely to choose their nearest school. Households in areas where more choices are allowed are also less likely to pick the nearest (though as London is the largest area with more than three choices, this fact may drive this relationship). This would seem to confirm that there is a degree of caution being exhibited, whereby parents are more likely to put down the school that they have the greatest chance of their child being admitted to (often their nearest) when they are only permitted 3 choices. Households living in more affluent neighbourhoods are more likely to choose their closest school, which may reflect the overall quality of schools there rather than preferences for distance.

The differences across sub-groups of families reflect the patterns seen for the number of choices made. There is essentially no difference in the proportion of families choosing their nearest school by FSM-eligibility, 38% versus 39%. By contrast, there are substantial differences by EAL-status and by ethnicity. On the former, 42% of non-EAL pupils put the nearest school top of their list, compared to 27% of EAL pupils.

There is a marked decline in the proportion nominating their closest school by the number of choices made. Only 20% of those making at least four choices nominate the nearest, and even among those that make one choice, only 55% nominate their closest school. This suggests that proximity is not the most important consideration for secondary school choice, even for those who make only one choice.

Appendix Table A2.2 (columns 5 and 6) confirm that these observed patterns are evident when accounting for other factors in a multivariate regression. As such, the finding that ethnic minority pupils are less likely to choose their closest school, for example, is true conditional on population density.

3.4.2 Effectiveness of school choice

3.4.2.1 Value of academic standards

Figure 3.1 shows the likelihood that a family nominates their nearest school as first choice relative to the academic attainment of that school. Households whose closest school is in the lowest quartile of attainment are least likely to choose their closest school. That is, households are more likely to bypass their closest school if it has unsatisfactory academic attainment. The proportion nominating their closest school as first-choice school declines with the number of nearby schools, irrespective of the closest schools' attainment. Less than 15% of households in very dense urban areas whose nearest school is in the bottom quartile of attainment nominate that school as top choice, suggesting most households are actively choosing an alternative school with higher academic standards. We note that higher academic attainment does not necessarily indicate a higher quality of teaching at the school, as academic attainment is also a function of the peer group at the school. It is, however, a commonly used metric for parents.

Are there limits to parents choosing highly attaining schools? One such limit is the number of choices that parents can make, which is binding for many households in some, particularly urban, areas. This means that the first choice school may be 'safe' rather than 'ambitious'. The data show that the quality of parents' first choice school is higher in LAs where more choices are permitted (which is true even taking account of higher school quality in London). These results are reported in a summary regression in Appendix Table A2.2, columns 11 and 12, which accounts for pupil and neighbourhood characteristics as in other regressions. This indicates that where parents are given a greater number of choices they use it to make ambitious choices.[16]

Figure 3.2 shows that, on average, academic attainment is highest for the first choice school and declines with later choices, whether the households make 2, 3, 4, 5, or 6 choices. For example, in London, for households that make 6 choices, the first choice has 71% of students achieving 5A*-C, compared with 70% for the second choice. This declines for each choice, to 63% for the sixth choice. In LAs where three choices are permitted, for those making the maximum number of choices, the first choice school has 62% of students achieving 5A*-C, compared with 59% for the second choice and 57% for the third choice. This suggests that, on average, the most preferred schools have higher academic attainment than lower choices, which is consistent with households valuing this attribute.

3.4.2.2 Admission to first-choice school

Table 3.3 reports the fraction of households that are observed in their first-choice school in the Spring term after school entry. As the table shows, this is slightly different to the fraction receiving an offer from their first-choice school, suggesting some offers are not taken up, some individuals get put on waiting lists and then secure a place at their school of choice, and some successfully appeal decisions. We focus on the receipt of an offer but note any interesting discrepancies.

The overall fraction of households receiving an offer from their first-choice school is 85%. That most parents get their first choice of school suggests, at face value, that the system is effective. With a restricted choice list there is a distinction between the first choice and preferred

[16]This simple regression suggests that for households eligible for free school meals there is not a significant positive relationship between the quality of first choice school and number of choices permitted. This suggests that increasing the number of choices would not necessarily reduce inequality in access to good schools, but a more comprehensive analysis is required for such a conclusion.

school, however, as households may make pragmatic choices based on the probability of admission at each school. This 85% may therefore be viewed more negatively as reflecting constraints on households' choices. A successful system with active and ambitious choices by parents may result in a lower percentage of households achieving their first choice.

There is little difference between FSM and non-FSM families, with respectively 84% and 86% being successful. Comparing offers and attendance, for FSM pupils 81% attend their first-choice school, compared to 84% who received an offer, whereas is reversed for non-FSM pupils - more attend their first choice than receive an initial offer, perhaps due to successful appeals.

Differences are larger between ethnic groups and by EAL status. Black and Asian households are less likely to have an offer from their first-choice school than White British households; similarly, EAL households are less likely to have an offer from their first choice (73% relative to 88%).

There are also significant differences between types of area. In densely populated urban areas (with an above median number of schools within 20km) applicants have a lower probability of receiving an offer from their first-choice school. This may reflect a wider variation in school quality, more schools within a feasible travel distance, more competition for places at popular schools and parents being less able to predict the demand for each school.

Households in LAs where only three choices are allowed are also more likely to receive an offer from their first choice. This may be because these LAs are typically more rural or that households are more cautious when choices are limited. The number of choices made is also strongly correlated with the percentage that receive an offer from their first choice. 97% of households that make only one choice receive an offer from this school. This suggests that their offer was almost guaranteed, perhaps due to proximity or strong priority due to a sibling or other characteristic. For those making the maximum number of choices, 77% received an offer from their first-choice school. This may be because those with a low probability of admission make more choices, or because more sophisticated users of the system make all the choices they can, including an ambitious nomination as first choice.

Overall, Table 3.3 shows that pupils have a lower chance of getting an offer from their first-choice school if: they live in dense urban areas; they live in areas where more choices are permitted; they live in poorer neighbourhoods; they apply to high performing schools; or they are from minority ethnic backgrounds. The largest differences arise from density, school quality and the maximum number of choices permitted. Of course, all these factors are correlated. Because of the importance of population density in affecting the outcome, Appendix Table A2.2 (columns 7 to 10) report the results from a multivariate regression (including the number of schools within 20km as a proxy for population density together with other variables). The key points we make above are confirmed.

Should we therefore conclude that school choice is less effective in these urban areas and for ethnic minority households? The key indicator of interest is the percentage of households offered a place at their most preferred school, with a more effective system increasing this percentage. As the most preferred school may not necessarily equate to the first choice school, it is not possible to conclude whether school choice is more meaningful in particular areas or for particular households. Indeed, as we have argued, ambitious school choices to a preferred school (which should be encouraged) would result in a lower percentage of households allocated to their first choice.

3.5 Discussion and summary

The school a pupil attends can affect their attainment and enjoyment of school, ultimately affecting their life chances. Understanding the functioning of school choice and how pupils are allocated to schools is therefore critical if we are to understand and improve educational equality and social mobility. In this paper, we use a new and comprehensive dataset to study how school choice works in England.

Our analysis shows that a large proportion of parents use the school choice system proactively. Previous studies have explored the process for of choice for relatively small samples of parents. Our contribution is to show the extent and variation in active school choices across household and neighbourhood types. First, significant numbers make the maximum number of school choices. Second, perhaps contrary to expectations, only a minority of parents choose their nearest school. Third, the local school is more likely to be bypassed where the quality is low. In other words, the data suggest that many parents are making active choices for schools and appear to value academic attainment. There are differences across more and less competitive markets, and household types, which we discuss below.

Further, there is clear evidence of strategic choice, which implies that many parents understand the school choice system as it currently operates. For example, we show, for the first time, that when parents make multiple choices, their first choice tends to be more ambitious. Equally, people make more cautious choices when constrained to 3 options. This suggests that they would have benefited from having more choices, consistent with theoretical and experimental evidence (Haeringer and Klijn, 2009, Calsamiglia, Haeringer, and Klijn, 2010), which is an important note for policy. We provide the first evidence that Local Authorities could improve the percentage of households allocated to a preferred school simply by offering the possibility of making more choices on the common application form. This would be relatively costless and would easily reduce the need for a strategic or 'safe' school choice.

While some households clearly engage with the school choice process, which may act to improve standards, choice is curtailed for others by the predominant current school admissions criteria. Allocating places to over-subscribed schools by proximity means that some households have negligible chance of admission to the best schools. Given the limited choices permitted, these households may therefore decide that making such an 'ambitious' choice would be wasteful.

Goldhaber, 2000 states that 'the rules in place governing the structure of any particular school choice program are likely crucial in determining the outcomes of the program.' In England, the dominant over-subscription criteria for secondary schools, straight line distance, is likely to induce strategic school choices, residential mobility and unequal access to the highest quality schools.

Considering how the school choice system operates for different families, we show that FSM and non-FSM households are similar in the number of choices made, the proximity of first-choice, and admission to first choice school. This contrasts with earlier qualitative research which found marked class differences in the process of school choice. However, non-FSM households still access better schools due to their proximity to higher performing schools. In London, students registered for FSM attend a school where 59% of pupils achieve 5A*-C grades at GCSE, on average, compared to 65% for non-FSM students. This gap widens to 8 percentage points outside London. Would the choice behaviour of FSM and non-FSM households differ if choice was not constrained by the probability of admission to good schools? This is a central topic for our future research.

There is a striking difference between White and Black and Asian families (and relatedly between EAL and non-EAL families). Black or Asian families (or EAL families) make far more school choices and prefer higher performing schools. This is consistent with previous research suggesting the high value of education for aspirant ethnic minorities and immigrants and could be consistent with lower cultural capital than the White British population. Building on this research, we are the first to show evidence that these differences in values may lead to varying engagement with school choice. Without more in-depth study we cannot conclusively say why what drives these differences, which is a subject for future research. For example, indicators of lower cultural capital for new immigrants could be explored by comparing households that are newly observed in the national data with less recent migrants. Perhaps these families have a greater focus on education or a better understanding of the school choice system. The trade-off made between academic quality and choosing the 'right' social and ethnic mix should be the subject of future work, which will inform important policy conclusions about ethnic segregation. Indeed, the data used in this research could complement the qualitative literature that finds strong preferences for particular peer groups.

Our conclusion is therefore that the school choice system is being actively used, though more so in urban areas than in rural areas and more so by Black, Asian and EAL students. Certainly, only a minority of families make just one choice or choose their local school. Further, taken at face value, the system appears to be working well. Most parents get their first choice of school. However, given that most schools are at capacity, this finding may equally reflect a degree of realism in parents' assessments of admission. Our data also indicate that where the number of choices is limited, parents make safer options.

We have used the data to understand parental choices and the allocation of pupils to schools.

We have been able to describe some key features of the English secondary school choice system. Whilst we have certainly been able to document the national picture of the pattern of choices that parents make, we have not been able to determine the relative importance of parental choice and constraints (e.g. not living in close enough proximity to their preferred school) in school allocation. In subsequent papers we intend to undertake further statistical modelling to better understand these relationships.

3.6 Tables

Table 3.1: Number of choices made (secondary)

	1 (%)	2 (%)	3 (%)	4+ (%)	Max (%)	Mean
All	34.55	21.58	26.89	16.98	26.88	2.41
Pupil characteristics						
FSM	36.86	20.49	25.98	16.67	24.31	2.39
Non-FSM	34.15	21.77	27.05	17.03	27.32	2.42
EAL	18.62	16.66	29.81	34.91	34.25	3.20
Non-EAL	38.15	22.69	26.23	12.93	25.22	2.24
White British	40.78	23.52	25.94	9.76	24.03	2.10
Asian	17.06	16.51	31.45	34.97	37.79	3.24
Black	11.84	13.13	26.96	48.07	37.12	3.69
Above median SES (neighbourhood)	37.72	22.89	25.94	13.45	25.67	2.24
Below median SES (neighbourhood)	31.49	20.28	27.81	20.43	28.04	2.58
Local area characteristics						
Above median number of schools within 20km	25.55	19.93	26.67	27.85	26.88	2.85
Below median number of schools within 20km	43.73	23.26	27.11	5.89	26.88	1.97
3 choices allowed	44.61	23.62	31.64	0.14	31.78	1.87
More than 3 choices allowed	23.43	19.32	21.64	35.61	21.46	3.01

Source: National Pupil Database linked to national parents' preferences data, made available by the Department for Education. IDACI: *https : //www.gov.uk/government/statistics/english − indices − of − deprivation* − 2015
Note: Note: Overall sample size is 526,329. 'FSM' denotes free school meals, a binary indicator for pupil income disadvantage. 'EAL' denotes English as an additional language. 'Above median SES (neighbourhood)' denotes neighbourhood disadvantage (defined by the Income Deprivation Affecting Children Index) is the more affluent half in the sample. 'Above median number of schools within 20km' denotes the number of schools within 20km above the median in the sample. '3 choices allowed' denotes the maximum number of choices parents are able to express is 3.

Table 3.2: Number of choices made (secondary)

	% with offer to first choice	% attend first choice
All	85.36	85.21
Pupil characteristics		
FSM	84.13	80.64
Non-FSM	85.57	86.01
EAL	72.80	72.97
Non-EAL	88.17	87.91
White British	90.26	89.75
Asian	72.12	73.58
Black	66.34	66.22
Above median SES (neighbourhood)	88.68	89.36
Below median SES (neighbourhood)	82.23	81.08
One choice	97.05	95.31
Two choices	87.31	86.42
Three choices	81.16	81.66
At least four choices	65.54	68.28
Maximum number of choices	77.00	78.17
Local area characteristics		
Above median number of schools within 20km	80.07	80.55
Below median number of schools within 20km	90.72	89.90
3 choices allowed	91.38	90.24
More than 3 choices allowed	78.65	79.58

Source and note: See Table 3.1.

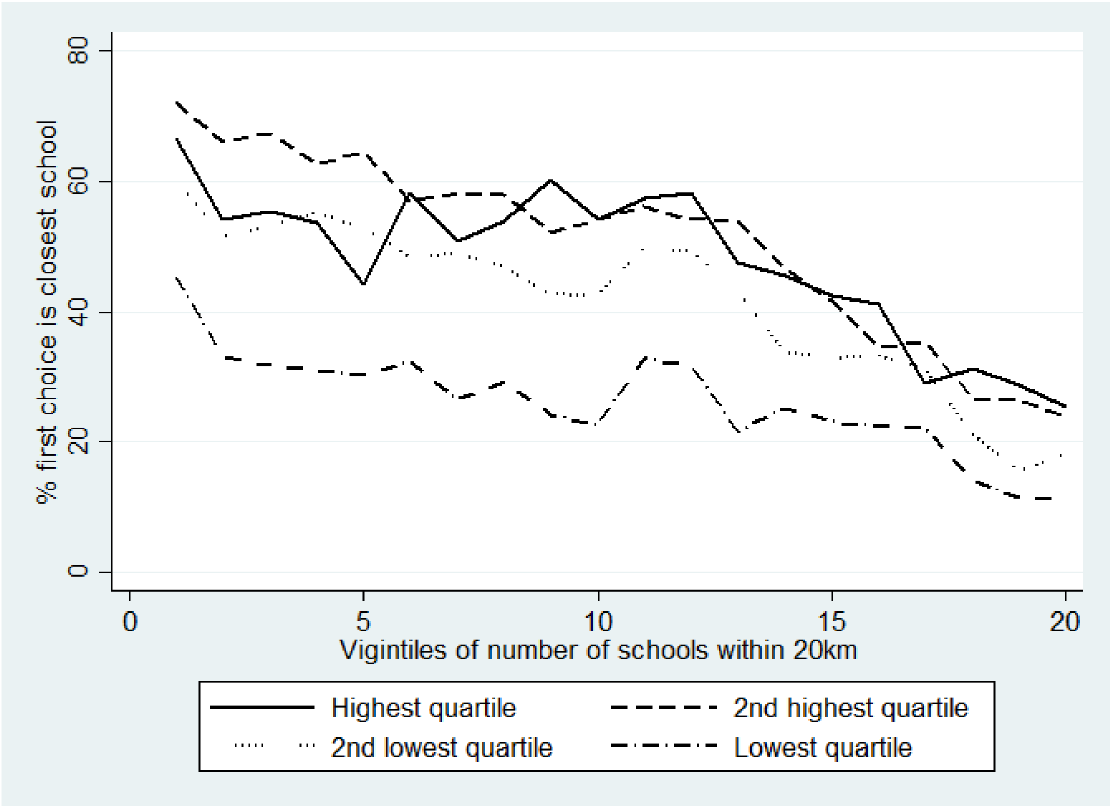
Table 3.3: First choice school is closest school

	% First choice is closest school	% First choice is closest school (within 20% tolerance)	% First choice is closest school in LA
All	38.98	45.95	39.56
Pupil characteristics			
FSM	38.03	45.03	38.60
Non-FSM	39.15	46.11	39.73
EAL	27.28	33.47	27.57
Non-EAL	41.63	48.77	42.28
White British	43.90	51.28	44.59
Asian	27.73	34.39	27.88
Black	18.85	23.51	19.23
Above median SES (neighbourhood)	43.73	51.07	44.47
Below median SES (neighbourhood)	34.35	40.95	34.78
One choice	55.43	62.89	56.53
Two choices	37.59	45.25	38.09
Three choices	31.03	37.88	31.30
At least four choices	19.88	25.15	20.01
Maximum number of choices	28.01	34.43	28.17
Local area characteristics			
Above median number of schools within 20km	31.66	37.98	32.36
Below median number of schools within 20km	46.43	54.05	46.89
3 choices allowed	45.54	52.95	46.23
More than 3 choices allowed	31.73	38.20	32.18

Source and note: See Table 3.1.

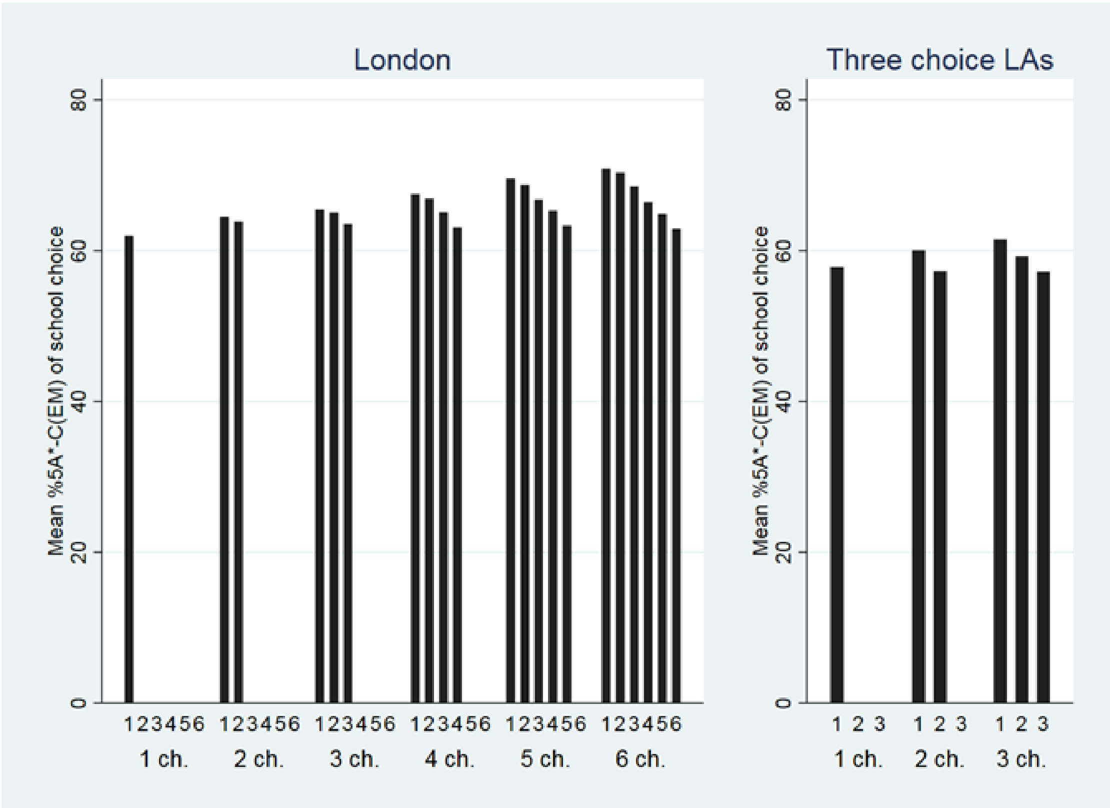
3.7 Figures

Figure 3.1: First choice is closest school, by number of schools within 20km (vigintiles) and school quality of closest school (measured by % 5A*-C) in quartiles



Source and note: See Table 3.1.

Figure 3.2: Academic attainment (measured by % 5A*-C) of school choices, by the number of choices made



Source: See Table 3.1.
Note: The bars show the %5A*-C (including English and maths) for each choice (1 to 6), by the number of choices made (where "1 ch." denotes "1 choice" and so on).

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

Preface: *This chapter is co-authored with Iftikhar Hussain (University of Sussex). The project is the result of my idea, that I invited Iftikhar to co-author, given his research agenda on school inspections and our relevant previous work together (Greaves et al., 2021). I conducted all data analysis and wrote almost all the chapter.*

Acknowledgement: This work was produced using statistical data from ONS. The use of the ONS statistical data in this work does not imply the endorsement of the ONS in relation to the interpretation or analysis of the statistical data. This work uses research datasets which may not exactly reproduce National Statistics aggregates.

4.1 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.[17] 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 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.[18] We show that the treatment and control schools are balanced, and therefore that the timing of inspection is

[17]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.

[18]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.

as good as random within the school year.[19] 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 decline in rating (from 'good' to 'bad') but revealed before and after the school choice deadline, the total number of choices made to the school decreases immediately by 8%. The same comparison for schools with an improvement in rating (from 'bad' to 'good') leads to an immediate increase in total school choices by 3%. The total number of first choices made to the school respond similarly, decreasing by 10% in response to a down-rating and increasing by 2% in response to an up-rating (although this is statistically insignificant). For individual parents, the probability that they choose their nearest school as first choice decreases by 6% in response to a down-rating and increase by 2.5% in response to an up-rating (although this is statistically insignificant). Overall, there appears to be asymmetry in parents' school choice response to information, with a stronger reaction to negative news than positive news revealed before the school choice deadline. 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.[20] 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.

[19]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.

[20]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. In chapter 6, we 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.

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 respond less to the immediate release of information, however, suggesting that there may be some information frictions in the nationwide setting for this group.

Our second contribution is to study the effect of an established school inspection regime on school choices in the context of an entrenched school choice system.[21,22] 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., 2018b, Neilson, Allende, and Gallego, 2019, Ainsworth et al., 2020, Houston and Henig, 2021, Cohodes et al., 2022). 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., 2018b) or the importance of school choice in general (Neilson, Allende, and Gallego, 2019). Facchetti, Neri, and Ovidi, 2021 instead study the provision non-test score 'hard-to-find' information about public sector schools in London, England. 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., 2018b caution

[21]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.

[22]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.

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, Cohodes et al., 2022).

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).[23] 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 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, except for 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

[23] 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. Friesen et al., 2012 study the effect of school test score information on pupils' likelihood of leaving their school.

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.[24]

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 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 system of school choice and accountability measures. Sections 4.3 and 4.4 describes the data and empirical strategy we employ. Section 4.5 presents the results at the school and pupil-level, in the short and medium-term. Section 4.6 concludes.

4.2 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.[25] For this stage of education, the

[24] 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).

[25] 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

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.[26] 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).[27] This algorithm is weakly truth revealing, meaning that parents can do no better than reporting their truly preferred schools. As described in chapter 3 (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 preferred 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 fund-

excluded from test-based school performance measures.

[26]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.

[27]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.

ing 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).

4.3 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.

4.3.1 Parents' school choices

We use administrative data on parents' school choices for the whole cohort of pupils entering primary (or infant) school in two cohorts: September 2014, September 2015.[28] 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.

[28]The cohort entering in September 2014 is the first cohort where national school choices are available to researchers. Future cohorts are available, but for reasons discussed in relation to Ofsted inspections, not used here.

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.

4.3.2 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.[29] 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.[30] 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

[29]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'.

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

of local education markets with a mean of four schools per market.[31] Schools within each 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).

4.3.3 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. We observe in the data that the pattern of school inspection results changes after September 2015, with significantly fewer schools moving from a 'bad' to 'good' rating. We therefore choose to focus our analysis on the pre-reform period. Combined with the availability of the school choice data, we are left with two academic years: schools inspected between September 2013 and July 2014, schools inspected between September 2014 and July 2015.

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 4.1 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. For previously 'Good' schools this is 4 years. In contrast, the mean and median for 'Inadequate' and 'Requires Improvement' schools is around 2.

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

4.3.4 Final sample selection

Appendix Table A3.1 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. The final sample is 15,236 primary schools, implying a loss of 8% of the initial sample. We make only essential sample restrictions. Appendix Table A3.1 shows that the largest drops in the sample size are due to incorrect school identifiers in the school choice data (meaning merging to school inspection ratings would be impossible) and missing information about prior Ofsted inspection rating (meaning defining a change in rating is not possible). 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 few schools that are 'special schools', 'independent', or 'other' schools as these schools have distinct markets. We choose to define a common sample of schools where all outcome variables are observed to be sure that differences across dependent variables are not driven by a changing sample.

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 section 4.4.2.

4.3.5.1 School choices

Figures 4.1 and 4.2 describe the dependent variables. At the pupil-level, Figure 4.1 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 4.1 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 4.2 shows the average number of choices to schools with different Ofsted ratings. Consistent with the pupil-level dependent variable in Figure 4.1, 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 4.2 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 4.2 shows the summary statistics for the pupil-level school choice data in our final sample. Across school years, between 42% and 43% of households choose their closest school as first choice. This rises for 'Outstanding' schools (between 51% and 54% across years) and is lowest for 'Inadequate' schools (between 34% and 35% across years).[32]

Around 88%/89% of pupils receive an offer from their first choice school, while only slightly fewer (around 86%) eventually attend this school in the Spring following admission in September. In chapter 3, we 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 15% of pupils are eligible for free school meals (FSM) in our sample of primary school applicants. Slightly more (around 18%) are recorded as having English as an Additional Language (EAL). Pupils that are White British are around 65% of pupils, with pupils that are 'White Other', 'Asian', 'Mixed' and 'Black' making up between less than 10% each.

4.3.5.2 Ofsted ratings

Schools' Ofsted ratings vary across inspection rounds. Table 4.3 shows that there is movement between previous and current Ofsted rating, for schools with each previous rating. For example, 3% of schools previously rated as 'Inadequate' remain so, 49% move to 'Requires Improvement', 45% to 'Good' and even 3% to 'Outstanding'. In the other direction, there is also movement from 'Outstanding' to 'Inadequate', by around 3% of schools. The most likely current inspection rating is 'Good', for each previous inspection rating aside from 'Inadequate'. This is most true for schools previously 'Good', where 62% remain so, and schools previously 'Requires Improvement', where 61% become so.

Although it would be interesting to explore the response of parents' school choices to movement across the transition matrix, for example from 'Outstanding' to 'Inadequate', defining a continuous treatment for change in Ofsted rating would have a problematic interpretation in our empirical specification (Callaway, Goodman-Bacon, and Sant'Anna, 2021). We therefore define two binary treatment indicators. First, equal to one if a school moves from a 'good' to 'bad'

[32] These patterns were also shown in Figure 4.1, which shows the variation in choosing the closest school by Ofsted rating and distance to the home.

rating and zero otherwise. Second, equal to one if a school moves from a 'bad' to 'good' rating and zero otherwise. 'Bad' is defined as the 'Inadequate' or 'Requires Improvement' categories, while 'good' is defined as the 'Good' or 'Outstanding' categories.

Panel (a) of Figure 4.3 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 more clearly that the percentage rated 'Requires Improvement' decreased in the academic year 2014/2015.

Table 4.4 shows school-level summary statistics for Ofsted ratings. The decrease in the percentage of schools rated as 'Inadequate' and 'Requires Improvement' over school years, and slight increases in the percentage of schools rated as 'Outstanding' and 'Good' mirrors Figure 4.3. The percentage of schools inspected early (the treatment schools) varies only slightly across school years, from 42% in 2013/2014 to 44% in 2014/2015. The percentage of schools that receive a decline, the same, or improvement in Ofsted category relative to their previous rating is roughly constant across school years, although slightly more schools move from 'bad' to 'good' in the later year (29% to 25%).

The following section outlines our empirical approaches and the identifying assumptions necessary to estimate the causal effect of information provision rather than underlying characteristics of the schools. This identifying assumptions are then interrogated in section 4.4.2.

4.4 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 challenge 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 by exploiting 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. These leads to a triple difference design, explained fully in the following section.

4.4.1 Triple difference design

A simple difference-in-differences design in this context would compare school choices submitted to schools that change their rating (say from 'good' to 'bad') to schools with no change in their rating, before and after the inspection takes place.[33] The before and after comparison would aim to account for pre-existing difference in the *level* of school choices across these groups, relying on the parallel trends assumption that the evolution of school choices would have been the same across these groups in the absence of the 'experiment'. It could be that schools that change their rating from 'good' to 'bad' are systematically different from those that have no change in their rating, however, which would violate the parallel trends assumption. For example, schools that change from 'good' to 'bad' might have declining test scores over time that might lead to declining school choices.

The triple difference design introduces a 'placebo' group or 'control' group that are unaffected by the experimental condition. In this case, we use schools that are inspected after parents have submitted their school choices as the control group. One would expect schools that change from 'good' to 'bad' to have similar characteristics whether they are inspected early or late in the school year (tested in the following section). Late inspected schools that change from 'good' to 'bad' therefore become the control group for schools that change from 'good' to 'bad' early in the school year, before parents submit their school choices. This circumvents the concern that schools that change rating are systematically different from schools that don't.

The regression equation for the triple difference design has the following form:

$$\begin{aligned} Y_{ijt} = \alpha &+ \beta_1 X_{ijt} + \beta_2 \tau_t + \beta_3 \delta_j \\ &+ \beta_4 TREAT_i + \beta_5 (\delta_j * \tau_t) \\ &+ \beta_6 (\tau_t * TREAT_i) \\ &+ \beta_7 (\delta_j * TREAT_i) \\ &+ \beta_8 (\delta_j * \tau_t * TREAT_i) \end{aligned} \tag{1}$$

Following the notation in Gruber, 1994, i indexes schools, j indexes the experimental condition (1 if 'good' to 'bad' and 0 if no change) and t indexes time (1 if post and 0 if pre). Y_{ijt} is the dependent variable of interest, for example the number of school choices submitted to school i in experimental condition j in year t. δ_j is an experimental group fixed effect, τ_t is a year effect (pre vs post), and $TREAT_i$ is a binary variable for the treatment group (1 if inspected early). Our main specification excludes time varying school covariates X_{ijt}, but these are included here for consistency with Gruber's specification. The parameter of interest is β_8, the effect of parents

[33] Using the notation of Gruber, 1994, credited with the first use of the triple differences design (Olden and Møen, 2022) the schools that change rating would be the 'experimental' schools while the schools with no change would be the 'non-experimental' schools.

observing a change in the school's rating.

Given our rich data and large sample size, we can also include school fixed effects in this specification. This means that variables and interaction terms that are constant within school over time are absorbed. The specification therefore becomes:

$$\begin{aligned} Y_{ijt} = \alpha &+ \beta_1 X_{ijt} + \beta_2 \tau_t \\ &+ \beta_5(\delta_j * \tau_t) \\ &+ \beta_6(\tau_t * TREAT_i) \\ &+ \beta_8(\delta_j * \tau_t * TREAT_i) \\ &+ \beta_9 S_i \end{aligned} \tag{2}$$

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 experimental schools versus schools experiencing no change is given by β_5. 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 $\beta_5 + \beta_8$. The difference between these two differences, β_8, identifies the effect of parents' *receiving information* about the change in the rating.

In practice, we include two experimental conditions to capture heterogeneity in the response of parents' school choices to a down (d) versus up (u) rating. In this specification, β_{8u} represents the effect of an up-rating from 'bad' to 'good' on parents' school choices, while β_{8d} represents the opposite: a down-rating from 'good' to 'bad'.

$$\begin{aligned} Y_{ijt} = \alpha &+ \beta_1 X_{ijt} + \beta_2 \tau_t \\ &+ \beta_{5u}(\delta u_j * \tau_t) + \beta_{5d}(\delta d_j * \tau_t) \\ &+ \beta_6(\tau_t * TREAT_i) \\ &+ \beta_{8u}(\delta u_j * \tau_t * TREAT_i) + \beta_{8d}(\delta d_j * \tau_t * TREAT_i) \\ &+ \beta_9 S_i \end{aligned} \tag{3}$$

Note that our triple difference design isolates the short-term effect of *information* about school quality within the school year. This leaves parents little time to respond to information that may lead to a lower estimated response than in the medium term, but has the advantage that we capture school choices conditional on a fixed location.

4.4.2 Identifying assumptions for the triple difference design

The identifying assumption for the triple difference design in the general case requires "the relative outcome of group B and group A in the treatment state to trend in the same way as the relative outcome of group B and group A in the control state in the absence of treatment" (Olden and Møen, 2022). In our context, this translates to the number of school choices submitted to schools that change rating versus stay the same to evolve in the same way across early and late inspected schools.

One concern is that schools inspected early and late in the academic year are systematically different. Figure 4.5 shows how the inspection ratings change within (rather than across school years, as shown in Figure 4.3). The percentage of schools rated 'Good' is relatively stable across months, with no obvious seasonal patterns across months.

We formally examine the assumption that month of inspection is as good as random by testing the balance of school characteristics across treatment and control groups. Table 4.5 shows that schools characteristics are generally balanced across treatment and control schools. There are a handful of significant differences across treatment and control groups, but these are small in magnitude. We note that our main 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).[34]

4.5 Results

This section presents results at the pupil and school-level in the short-term, namely how school quality information revealed between September and mid-January affects school choices made by 15th January. As explained in section 4.4.1, the control group of schools are those inspected after the school choice deadline in the same school year, or that receive no inspection. 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. Subsection 4.5.4 considers the effect on actual enrolment in contrast to school choices, while subsection 4.5.5 shows the overall effect of school quality information on pupil segregation.

4.5.1 Main effects

Results from the triple differences model specified in equation 3 are presented in Table 4.6. Focusing first on the main effects, that are presented in the fourth and sixth rows, the overall pattern is that parents' school choices immediately respond to changes in Ofsted rating. At the school-level, both the total number of choices and number of first choices significantly decrease for schools that move from 'good' to 'bad' Ofsted ratings when it is revealed to parents. For

[34]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.

total choices, the coefficient is -7.9, meaning that a school that parents know is down-rated from 'good' to 'bad' receives around 8 fewer choices than equivalent schools where the information is not revealed to parents. This is equivalent to around 8% of the baseline mean (or 10.5% of the baseline standard deviation). In response to the same information, the number of first choices also decreases by 4.5 on average, which is equivalent to around 10% of the baseline mean (or 15.5% of the baseline standard deviation). 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 decrease in first choices of around 4.5 therefore equates to around £22,500 additional funding per cohort, or 85% of a newly qualified teacher's salary. At the pupil-level, the response to a decline in the Ofsted rating that is revealed to parents leads to a 2.7 percentage point decrease in the probability that the closest school is listed as the first choice school. This is a 5.9% decrease from a baseline probability of 0.46.

Parents' responses are more muted to information revealing an up-rating from 'bad' to 'good'. Total choices increase by around 3, equivalent to around 3% of the baseline mean, significant at the 1% level. While the direction of the effect for first choices and the probability of choosing the closest school as first choice is positive as expected, these results are not statistically significant. The comparison of the response to up and down rating therefore reveals that parents' school choices are immediately more responsive to negative than positive shocks.

To give some explanation for the remaining rows, the first row ('Post') is the year effect. This is positive and statistically significant, suggesting demographic changes in the total number of pupils applying to state-funded schools. The second row ('Early#Post') is the interaction between being a school inspected early and year. As expected from our discussion of identifying assumptions (where schools inspected early and late have largely the same characteristics) schools inspected early receive the same number of choices as schools inspected later in the academic year. The third and fifth rows show how school choices evolve over time for 'experimental' schools that change rating after the school choice deadline. Most coefficients in these rows are not significantly different from zero, which would be expected if parents are not able to predict the change in rating. For schools that move from 'bad' to 'good', the total number of school choices is significantly *lower* than for schools with no change in rating, however. This suggests that parents do not correctly anticipate the positive change. At the pupil level, the probability of choosing the nearest school as first choice is significantly lower for schools that move from 'good' to 'bad' in the post year, relative to schools that stay the same. In this case, parents can predict, to some extent, a movement in rating. This might be because those that live close to the school might have inside information about quality. 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.

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/resist declining, 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 summary for this chapter (section 4.6).

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 (chapter 3) and the low percentage of households that choose their closest school as first choice (around 43%, as shown in Table 4.6). 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.

4.5.2 Robustness checks

Table 4.7 presents a set of comprehensive robustness checks for the main specification shown in Table 4.6. 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 4.7 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 extremely 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. As discussed in section 4.4.2, 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. The effects are slightly higher (but not significantly) for the school-level dependent variables.

In column 5, 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 so) for the pupil-level dependent variable.

4.5.3 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 4.8 reveal that households eligible for Free School Meals (FSM) respond similarly to households not eligible for FSM. This is consistent with evidence from chapter 3 where 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 4.8 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 chapter 3, where 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., 2018b who find that the 'benefits of simplified information may be greater for families with limited English proficiency', but in line with Friesen et al., 2012.[35] In our triple difference setting, 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 about a down-rating 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. Non-white British households do respond immediately to an up-rating in school inspection rating,

[35]See also Cohodes et al., 2022.

however, which is not the case for British households. This is an interesting asymmetry, perhaps reflecting constraints in access, that could be explored in future work.

At the pupil-level, we also experiment with alternative distance from school to home radii (second panel in Table 4.8). 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 55% to 41% across distance bands. This implies that the percentage change from the mean of the dependent variable is larger than the point estimate for more distant households. Overall, the pattern suggests that households are more responsive to information when they live further from the school. This could reflect less 'inside information' about a school further from the household.

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 4.10 and 4.11, respectively. The effect size is almost universally consistent with the main effects 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 and with the expected sign for schools in the 2nd lowest KS2 quintile. Indeed, for higher performing schools the effect size is the opposite sign. For the pupil-level dependent variable, the change in Ofsted rating does not appear to outweigh the information from high test-scores. Information appears to be more important for infant compared to primary schools, however, where no test score information is available to parents. 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.

Total choices and first choices are equally responsive across the distribution of KS2 scores (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.

The effect across FSM quintiles is more consistent across the pupil-level and school-level dependent variables. In each case, the most disadvantaged schools gain from an up-rating in Ofsted information that is revealed to parents. 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). These results suggest that information from independent school inspections might outweigh this preference for peer composition for some parents.

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.[36] There is interesting heterogeneity across market position by up or down-rating. All schools receive fewer school choices after a down-rating is revealed, independent of market position. Only schools initially with a more advantaged peer group and higher test scores gain from an up-rating, however. This suggests that the positive signal from the school inspection does not outweigh other school attributes that parents value for these schools.

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). The picture is clearest considering the local definitions of school characteristics that are most pertinent to parents. All schools face demand-side pressure not to decline from 'good' to 'bad', no matter their initial position. This is in contrast to earlier experimental work that found that there was disparate pressure in more affluent areas (Hastings and Weinstein, 2008). Only relatively advantaged schools (defined by peer group and test scores) gain from improving from 'bad' to 'good', however. This is in contrast to Andrabi, Das, and Khwaja, 2017, who find greater effects for initially lower performing schools. 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 maintain education standards.

4.5.4 Demand versus supply: Does choice engender competition?

Table 4.12 reports the results for the outcome 'number of places offered' by the school in the short-term. 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 (and statistically insignificant) relationship between up and down-ratings and offers from the school, despite the increase in total choices and first choices shown in section 4.5.1.

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

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 4.12 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 4.12 shows the intuitive result that school offers respond more to the revelation of negative information where there is excess capacity in the local market: -13% in 'Q2' and -7% in 'Q3'. There is no significant change in offers as a result of the revelation school choice information in 'Q4', as schools with fewer choices are filled by those without a place elsewhere. Indeed, there is a positive and marginally significant coefficient in 'Q5'. 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'.

4.5.5 Market-level effects: segregation

Results presented in sections 4.5.1 and 4.5.4 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 chapter 5:

> 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 4.13 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 4.13 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.

4.6 Summary and discussion

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., 2018b).

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 an up or down rating in Ofsted rating. 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, although recent experimental evidence suggests that the provision of school effectiveness measures alone (without test scores) has 'clear and consistent desegregating choices' (Houston and Henig, 2021). 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. Less affluent households have access to lower quality schools close by in England (Burgess et al., 2011) and Barcelona (Scandurra, Zancajo, and Bonal, n.d.), for example. 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.

4.7 Tables

Table 4.1: Timing of Ofsted inspections

			Summary statistics			
Previous Ofsted rating	N	Mean	S.D.	25th	50th	75th
Inadequate	375	1.77	0.74	1	2	2
Requires Improvement	2,991	1.93	0.67	2	2	2
Good	3,805	4.11	1.05	3	4	5
Outstanding	580	5.18	1.65	4	5	6

Source: Ofsted management information.
Note: The sample is all schools included in the final sample (see Appendix Table A3.1). This table shows summary statistics for the number of school years between the current and previous inspection.

Table 4.2: Pupil-level summary statistics

Variable	2014	2015
Prob choose closest school	0.43 (0.49)	0.42 (0.49)
Prob choose closest school: closest outstanding	0.54 (0.50)	0.51 (0.50)
Prob choose closest school: closest good	0.45 (0.50)	0.45 (0.50)
Prob choose closest school: closest requires improvement	0.39 (0.49)	0.38 (0.49)
Prob choose closest school: closest inadequate	0.34 (0.47)	0.35 (0.48)
Offer from first choice school	0.89 (0.32)	0.88 (0.32)
Attend first choice school	0.86 (0.35)	0.86 (0.35)
FSM	0.15 (0.36)	0.15 (0.35)
EAL	0.18 (0.39)	0.19 (0.39)
Ethnic group: White British	0.66 (0.47)	0.65 (0.48)
Ethnic group: White Other	0.06 (0.24)	0.07 (0.25)
Ethnic group: Asian	0.09 (0.29)	0.09 (0.29)
Ethnic group: Black	0.05 (0.21)	0.04 (0.21)
Ethnic group: Chinese	0.00 (0.07)	0.00 (0.07)
Ethnic group: Mixed	0.05 (0.23)	0.06 (0.23)
Ethnic group: Other	0.01 (0.12)	0.01 (0.11)

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. The sample is all schools included in the final sample (see Appendix Table A3.1). 'FSM' refers to free school meals, a marker of pupil economic disadvantage. 'EAL' refers to English as an additional language.

Table 4.3: Transition matrix

	Current Ofsted rating			
Previous Ofsted rating	Inadequate	Requires improvement	Good	Outstanding
Inadequate	0.03	0.49	0.45	0.03
Requires improvement	0.06	0.31	0.61	0.02
Good	0.04	0.25	0.62	0.10
Outstanding	0.03	0.16	0.57	0.24

Source: Ofsted management information.
Note: The sample is all schools included in the final sample (see Appendix Table A3.1) that are inspected in the academic years 2013/2014 and 2014/2015.

Table 4.4: School-level summary statistics

Variable	2014	2015
Panel (a): Ofsted rating		
Outstanding	6.7	8.3
Good	57.2	64.8
Total 'good'	63.9	73.1
Requires Improvement	30.9	23.3
Inadequate	5.2	3.6
Total 'bad'	36.1	26.9
Panel (b): Treatment		
Control	57.8	55.7
Treatment	42.2	44.3
Panel (c): Changes		
Bad to Good	24.7	29.3
Good to bad	16.4	14.0
No change	58.9	56.8

Source: Ofsted management information.
Note: The sample is all schools included in the final sample (see Appendix Table A3.1) that are inspected in the academic years 2013/2014 and 2014/2015.

Table 4.5: Balance across treatment and control schools: Observable characteristics in the year of, and prior to, inspection

Panel (a): All schools in final sample: Good to bad			
Variable (lag 1)	Treatment	Control	Difference
# total choices	90.42 (56.63)	88.12 (61.34)	2.3
# first choices	42.20 (23.11)	40.26 (23.27)	1.94
# offered	42.55 (22.92)	41.07 (21.87)	1.48
KS2 (std.)	70.83 (12.48)	69.34 (12.64)	1.49
% EAL (std.)	16.64 (22.78)	17.46 (25.01)	-0.81
% FSM (std.)	30.78 (16.52)	30.37 (17.67)	0.41
Infant school	0.16 (0.37)	0.06 (0.25)	0.10**
Religious	0.33 (0.47)	0.39 (0.49)	-0.06
Panel (b): All schools in final sample: No change			
Variable (lag 1)	Treatment	Control	Difference
# total choices	99.18 (79.08)	91.38 (71.48)	7.80**
# first choices	42.22 (28.27)	40.38 (25.72)	1.84
# offered	41.41 (25.64)	40.80 (24.06)	0.61
KS2 (std.)	81.99 (12.91)	76.86 (14.39)	5.13***
% EAL (std.)	18.25 (22.79)	16.76 (22.31)	1.49
% FSM (std.)	25.33 (17.77)	27.38 (18.60)	-2.04**
Infant school	0.17 (0.37)	0.10 (0.30)	0.07***
Religious	0.43 (0.50)	0.41 (0.49)	0.02
Panel (c): All schools in final sample: Bad to good			
Variable (lag 1)	Treatment	Control	Difference
# total choices	80.19 (56.61)	74.15 (50.92)	6.05**
# first choices	35.37 (21.22)	34.00 (20.79)	1.37
# offered	39.12 (21.39)	37.95 (22.40)	1.17
KS2 (std.)	76.02 (11.76)	75.36 (13.05)	0.66
% EAL (std.)	19.30 (23.31)	15.03 (20.54)	4.27***
% FSM (std.)	29.86 (18.14)	29.84 (17.18)	0.02
Infant school	0.08 (0.27)	0.08 (0.27)	0
Religious	0.37 (0.48)	0.39 (0.49)	-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 sample is all schools included in the final sample (see Appendix Table A3.1)that are inspected in the academic years 2013/2014 and 2014/2015. The table shows group means, with standard deviations in parentheses. * $p<0.1$, ** $p<0.05$, *** $p<0.01$. 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 4.6: Short-term response to the revelation of Ofsted ratings

	Total choices	First choices	Choose closest
Post	2.668***	0.601***	-0.003***
	(0.160)	(0.088)	(0.001)
Early#Post	0.820	0.252	-0.001
	(0.625)	(0.341)	(0.004)
Post#To good	-1.717***	-0.151	-0.006
	(0.556)	(0.335)	(0.004)
Early#Post#To good	3.122***	0.871	0.011
	(1.109)	(0.635)	(0.007)
Post#To bad	-0.645	-0.322	-0.012*
	(0.998)	(0.580)	(0.006)
Early#Post#To bad	-7.878***	-4.151***	-0.027***
	(1.548)	(0.885)	(0.010)
Year F.E.	Y	Y	Y
School F.E.	Y	Y	Y
School covariates	N	N	N
Pupil covariates	NA	NA	N
N	30,440	30,415	1,169,133
N schools	15236	15236	15236
Mean dep. var.	94.6	40.5	0.46
S.D. dep. var.	75.1	27.0	0.50
R^2	0.02	0.01	0.16

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 sample is all schools included in the final sample (see Appendix Table A3.1). Standard errors in parentheses. * p<0.1, ** p<0.05, *** p<0.01.

Table 4.7: Short-term response to the revelation of Ofsted ratings: Robustness

Total choices	Main	Pupil X	School X	Doughnut	!Religious
Post#To good	-1.717***		-1.781***	-1.879***	-2.120***
	(0.556)		(0.556)	(0.532)	(0.770)
Early#Post#To good	3.122***		3.221***	3.445***	2.941*
	(1.109)		(1.112)	(1.141)	(1.537)
Post#To bad	-0.645		-0.473	-0.269	-1.059
	(0.998)		(0.999)	(1.032)	(1.413)
Early#Post#To bad	-7.878***		-7.989***	-8.748***	-8.083***
	(1.548)		(1.554)	(1.660)	(2.114)
N	30,440		30,377	29,944	17,325
N schools	15,236		15,236	15,236	8,723
Mean dep. var.	94.57		94.58	94.60	110.33
R^2	0.02		0.03	0.02	0.03
First choices	**Main**	**Pupil X**	**School X**	**Doughnut**	**!Religious**
Post#To good	-0.151		-0.224	-0.114	-0.536
	(0.335)		(0.336)	(0.339)	(0.459)
Early#Post#To good	0.871		0.969	0.789	1.100
	(0.635)		(0.638)	(0.661)	(0.872)
Post#To bad	-0.322		-0.267	-0.182	-0.516
	(0.580)		(0.582)	(0.594)	(0.793)
Early#Post#To bad	-4.151***		-4.090***	-4.344***	-4.846***
	(0.885)		(0.889)	(0.943)	(1.229)
N	30,415		30,352	29,919	17,319
N schools	15,236		15,236	15,236	8,723
Mean dep. var.	40.51		40.51	40.51	46.36
R^2	0.01		0.01	0.01	0.01
Choose closest	**Main**	**Pupil X**	**School X**	**Doughnut**	**!Religious**
Post#To good	-0.006	-0.005	-0.006*	-0.005	-0.007
	(0.004)	(0.004)	(0.004)	(0.004)	(0.005)
Early#Post#To good	0.011	0.011*	0.011*	0.009	0.017**
	(0.007)	(0.007)	(0.007)	(0.007)	(0.008)
Post#To bad	-0.012*	-0.012**	-0.010*	-0.012*	-0.017**
	(0.998)	(0.580)	(0.006)	(0.007)	(0.008)
Early#Post#To bad	-0.027***	-0.027***	-0.027***	-0.026**	-0.029**
	(0.010)	(0.010)	(0.010)	(0.010)	(0.012)
N	1,169,133	1,169,133	1,169,133	1,148,402	755,692
N schools	15,236	15,236	15,236	15,236	8,503
Mean dep. var.	0.46	0.46	0.46	0.46	0.49
R^2	0.16	0.16	0.16	0.16	0.14

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 sample is all schools included in the final sample (see Appendix Table A3.1). Standard errors in parentheses. * p<0.1, ** p<0.05, *** p<0.01. '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 4.8: Short-term response to the revelation of Ofsted ratings: Heterogeneity in the probability of choosing the closest school as first choice by pupil characteristics

Pupil char.	FSM=0	FSM=1	EAL=0	EAL=1	White=0	White=1
Post#To good	-0.003	-0.016	0.000	-0.007	-0.010	-0.001
	(0.004)	(0.010)	(0.004)	(0.010)	(0.007)	(0.005)
Early#Post#To good	0.009	0.028	-0.005	0.020	0.026**	0.000
	(0.007)	(0.018)	(0.008)	(0.017)	(0.011)	(0.008)
Post#To bad	-0.014**	-0.002	-0.020***	-0.007	0.000	-0.020***
	(0.007)	(0.016)	(0.007)	(0.017)	(0.011)	(0.007)
Early#Post#To bad	-0.025**	-0.042*	-0.028**	-0.002	-0.025	-0.028**
	(0.010)	(0.025)	(0.011)	(0.025)	(0.017)	(0.012)
N	1,006,699	162,434	845,412	193,494	399,552	769,581
N schools	15,236	13,116	15,184	11,084	14,295	15,101
Mean dep. var.	0.46	0.44	0.46	0.44	0.41	0.48
R^2	0.18	0.20	0.20	0.20	0.17	0.19
Distance	0-200m	200-400m	400-600m	600-800m	800-1000m	
Post#To good	-0.004	-0.005	-0.007	-0.015	-0.042**	
	(0.010)	(0.006)	(0.007)	(0.010)	(0.017)	
Early#Post#To good	0.023	0.017	-0.002	0.005	0.089***	
	(0.017)	(0.011)	(0.013)	(0.019)	(0.030)	
Post#To bad	-0.019	-0.029***	0.021*	-0.025	0.022	
	(0.015)	(0.010)	(0.012)	(0.017)	(0.027)	
Early#Post#To bad	-0.022	0.003	-0.051***	-0.013	-0.091**	
	(0.025)	(0.016)	(0.018)	(0.027)	(0.044)	
N	183,272	404,902	298,814	145,517	58,269	
N schools	14,163	14,429	13,634	11,235	7,472	
Mean dep. var.	0.55	0.48	0.43	0.41	0.41	
R^2	0.24	0.21	0.23	0.29	0.36	

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 sample is all schools included in the final sample (see Appendix Table A3.1). Standard errors in parentheses. * p<0.1, ** p<0.05, *** p<0.01. '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.

Table 4.9: Short-term response to the revelation of Ofsted ratings: Heterogeneity in the probability of choosing the closest school as first choice by school characteristics

FSM quintile	Lowest	2nd	3rd	4th	Highest		
Post#To good	0.001	-0.032**	-0.011	-0.013	-0.001		
	(0.017)	(0.013)	(0.010)	(0.008)	(0.007)		
Early#Post#To good	0.019	0.040*	0.022	0.029**	0.023*		
	(0.028)	(0.023)	(0.021)	(0.015)	(0.014)		
Post#To bad	0.001	0.009	0.022	-0.019	-0.009		
	(0.022)	(0.024)	(0.018)	(0.013)	(0.012)		
Early#Post#To bad	-0.080**	-0.058*	-0.078**	-0.007	-0.008		
	(0.040)	(0.034)	(0.032)	(0.021)	(0.019)		
N	169,180	188,563	213,346	260,975	273,843		
N schools	3,737	3,955	3,693	3,403	3,018		
Mean dep. var.	0.59	0.54	0.48	0.40	0.35		
R^2	0.21	0.17	0.14	0.11	0.08		
KS2 quintile	Lowest	2nd	3rd	4th	Highest	Infant	Primary
Post#To good	0.018**	-0.003	-0.021	-0.002	-0.009	-0.004	-0.006
	(0.009)	(0.015)	(0.015)	(0.028)	(0.047)	(0.013)	(0.004)
Early#Post#To good	-0.024	0.009	0.043	-0.006	-0.054	0.042*	0.006
	(0.019)	(0.027)	(0.031)	(0.047)	(0.083)	(0.022)	(0.007)
Post#To bad	-0.013	-0.024	-0.019	-0.107*	-0.060	0.007	-0.014**
	(0.015)	(0.024)	(0.032)	(0.057)	(0.081)	(0.019)	(0.006)
Early#Post#To bad	-0.003	-0.061*	-0.035	0.144*	0.172	-0.048*	-0.026**
	(0.023)	(0.036)	(0.052)	(0.082)	(0.179)	(0.029)	(0.010)
N	211,897	188,815	207,391	174,551	142,679	194,200	974,933
N schools	3,951	4,081	4,545	4,291	3,643	1,967	13,269
Mean dep. var.	0.38	0.42	0.45	0.47	0.49	0.55	0.44
R^2	0.12	0.14	0.16	0.19	0.21	0.13	0.16

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 sample is all schools included in the final sample (see Appendix Table A3.1). Standard errors in parentheses. * p<0.1, ** p<0.05, *** p<0.01. 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 4.10: Short-term response to the revelation of Ofsted ratings: Heterogeneity in total choices

FSM quintile	Lowest	2nd	3rd	4th	Highest		
Post#To good	0.468	-4.220***	-1.666	-2.626**	-1.544		
	(2.266)	(1.612)	(2.292)	(1.233)	(0.994)		
Early#Post#To good	-3.538	4.917*	1.956	6.878**	2.725		
	(3.502)	(2.981)	(3.710)	(2.858)	(2.159)		
Post#To bad	-3.220	0.982	0.178	1.512	-0.485		
	(3.165)	(2.087)	(3.804)	(2.507)	(1.857)		
Early#Post#To bad	-1.842	-8.307**	-8.322	-12.301***	-4.042		
	(4.799)	(3.933)	(5.992)	(3.807)	(3.076)		
N	6,118	5,942	5,837	5,723	5,755		
N schools	3,946	4,166	3,958	3,675	3,251		
Mean dep. var.	93.37	99.03	99.83	103.24	84.33		
R^2	0.02	0.04	0.04	0.02	0.01		
KS2 quintile	Lowest	2nd	3rd	4th	Highest	Infant	Primary
Post#To good	1.938	-0.822	-4.653*	-3.917	-0.791	-2.736	-1.582***
	(1.422)	(1.984)	(2.693)	(3.160)	(2.527)	(1.932)	(0.581)
Early#Post#To good	2.622	-1.859	5.772	13.596***	5.306	3.861	2.842**
	(3.203)	(3.965)	(4.469)	(4.947)	(4.326)	(3.932)	(1.157)
Post#To bad	-1.786	2.784	-0.403	3.335	-0.867	2.071	-0.863
	(1.916)	(3.701)	(3.827)	(6.479)	(5.520)	(3.919)	(1.027)
Early#Post#To bad	-6.097*	-10.460*	-4.597	-6.346	-16.127*	-5.244	-8.180***
	(3.404)	(5.488)	(5.785)	(8.048)	(8.599)	(5.171)	(1.621)
N	5,001	5,229	5,397	5,086	4,843	3,989	26,451
N schools	3,954	4,488	4,639	4,368	3,721	2,005	13,251
Mean dep. var.	69.76	87.97	97.53	102.14	98.63	123.74	89.94
R^2	0.02	0.03	0.02	0.02	0.03	0.03	0.02
Market position	Popular	Not popular	Low FSM	High FSM	High KS2	Low KS2	
Post#To good	-1.031	-2.373*	-1.682**	-1.634	-0.969	-2.086	
	(0.740)	(1.388)	(0.814)	(1.277)	(0.840)	(1.291)	
Early#Post#To good	3.471**	4.358*	5.122***	1.375	4.280***	2.518	
	(1.403)	(2.379)	(1.502)	(2.308)	(1.577)	(2.314)	
Post#To bad	-0.802	-0.574	-1.356	0.440	-0.780	-1.450	
	(1.337)	(1.908)	(1.338)	(2.014)	(1.305)	(2.474)	
Early#Post#To bad	-8.410***	-8.684***	-7.366***	-9.549***	-8.392***	-8.621**	
	(2.077)	(3.058)	(2.136)	(3.157)	(2.084)	(4.086)	
N	15,142	12,531	15,064	12,229	13,643	9,922	
N schools	7,574	6,267	7,536	6,115	6,823	4,962	
Mean dep. var.	76.49	116.48	86.07	107.12	86.48	99.15	
R^2	0.02	0.03	0.03	0.02	0.01	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 sample is all schools included in the final sample (see Appendix Table A3.1). Standard errors in parentheses. * p<0.1, ** p<0.05, *** p<0.01. '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 4.11: Short-term response to the revelation of Ofsted ratings: Heterogeneity in first choices

FSM quintile	Lowest	2nd	3rd	4th	Highest		
Post#To good	1.827	-1.184	-0.574	-0.454	-1.055		
	(1.255)	(0.968)	(0.980)	(0.804)	(0.731)		
Early#Post#To good	-1.231	0.803	-0.934	0.359	3.168**		
	(2.062)	(1.847)	(1.949)	(1.751)	(1.339)		
Post#To bad	-2.512	-0.604	1.842	0.180	1.191		
	(2.282)	(1.369)	(2.051)	(1.299)	(1.210)		
Early#Post#To bad	-0.894	-4.021	-4.612	-5.470**	-4.347**		
	(3.057)	(2.500)	(2.897)	(2.216)	(1.765)		
N	6,113	5,939	5,829	5,719	5,752		
N schools	3,945	4,163	3,955	3,673	3,249		
Mean dep. var.	38.14	41.18	43.04	44.00	39.44		
R^2	0.00	0.01	0.01	0.01	0.01		
KS2 quintile	Lowest	2nd	3rd	4th	Highest	Infant	Primary
Post#To good	1.577*	1.414	-1.637	-0.169	1.246	1.661	-0.313
	(0.932)	(1.190)	(1.794)	(1.988)	(2.229)	(1.333)	(0.346)
Early#Post#To good	1.379	-4.666*	-1.028	5.261	1.670	-1.805	0.989
	(2.145)	(2.526)	(2.550)	(3.498)	(3.930)	(2.412)	(0.658)
Post#To bad	-1.765	1.472	1.519	-6.955**	-5.133**	1.894	-0.597
	(1.148)	(1.672)	(3.118)	(3.394)	(2.313)	(2.472)	(0.588)
Early#Post#To bad	-2.267	-8.852**	-4.690	4.036	1.223	-2.834	-4.219***
	(2.371)	(3.443)	(3.703)	(5.048)	(3.272)	(3.333)	(0.918)
N	4,996	5,228	5,397	5,083	4,841	3,987	26,428
N schools	3,949	4,487	4,639	4,366	3,720	2,005	13,251
Mean dep. var.	33.06	38.83	41.08	41.69	38.96	56.04	38.05
R^2	0.01	0.02	0.01	0.01	0.01	0.00	0.01
Market position	Popular	Not popular	Low FSM	High FSM	High KS2	Low KS2	
Post#To good	-0.105	-0.683	-0.332	-0.232	-0.413	0.145	
	(0.412)	(0.757)	(0.451)	(0.698)	(0.454)	(0.711)	
Early#Post#To good	0.412	2.650**	1.759**	0.540	2.208***	-0.460	
	(0.781)	(1.298)	(0.831)	(1.262)	(0.852)	(1.274)	
Post#To bad	-0.031	-0.276	-0.281	-0.124	-0.423	-1.110	
	(0.746)	(1.041)	(0.742)	(1.101)	(0.706)	(1.362)	
Early#Post#To bad	-3.950***	-5.240***	-3.752***	-5.106***	-4.101***	-3.999*	
	(1.158)	(1.668)	(1.182)	(1.726)	(1.126)	(2.249)	
N	15,123	12,530	15,051	12,226	13,637	9,919	
N schools	7,574	6,267	7,536	6,115	6,823	4,962	
Mean dep. var.	31.88	50.45	37.28	44.88	36.75	41.49	
R^2	0.01	0.01	0.01	0.00	0.01	0.01	

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 sample is all schools included in the final sample (see Appendix Table A3.1). Standard errors in parentheses. * p<0.1, ** p<0.05, *** p<0.01. '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 4.12: Short-term response of the number of school placed offered to the revelation of Ofsted ratings

			Quintiles of pupils to school places				
Offers	DIDID	DIDID	Q1 (fewest)	Q2	Q3	Q4	Q5 (most)
Post#To good	-0.210	-0.222	-0.309	0.939	-0.242	0.689	1.053
	(0.303)	(0.303)	(0.542)	(0.724)	(0.643)	(0.892)	(0.831)
Early#Post#To good	0.126	0.242	-1.024	-1.758	0.083	0.181	-1.807
	(0.540)	(0.543)	(1.004)	(1.316)	(1.169)	(1.608)	(1.603)
Post#To bad	-0.304	-0.309	-0.412	2.481**	0.085	-2.761*	-1.595
	(0.487)	(0.485)	(0.886)	(1.191)	(1.077)	(1.518)	(1.375)
Early#Post#To bad	-0.746	-0.770	0.079	-4.605**	-3.261**	0.490	4.006*
	(0.718)	(0.721)	(1.382)	(1.836)	(1.633)	(2.331)	(2.377)
Year F.E.	Y	Y	Y	Y	Y	Y	Y
School F.E.	Y	Y	Y	Y	Y	Y	Y
School covariates	N	Y	N	N	N	N	N
Pupil covariates	N/A	N/A	N/A	N/A	N/A	N/A	N/A
N	30,419	30,356	4,858	5,786	9,116	6,086	4,573
N schools	15,236	15,236	2,841	4,126	6,268	4,661	3,062
Mean dep. var.	39.85	39.85	18.69	35.31	41.56	49.77	50.93
R^2	0.01	0.01	0.01	0.02	0.01	0.01	0.01

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 sample is all schools included in the final sample (see Appendix Table A3.1). Standard errors in parentheses. * p<0.1, ** p<0.05, *** p<0.01. 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.

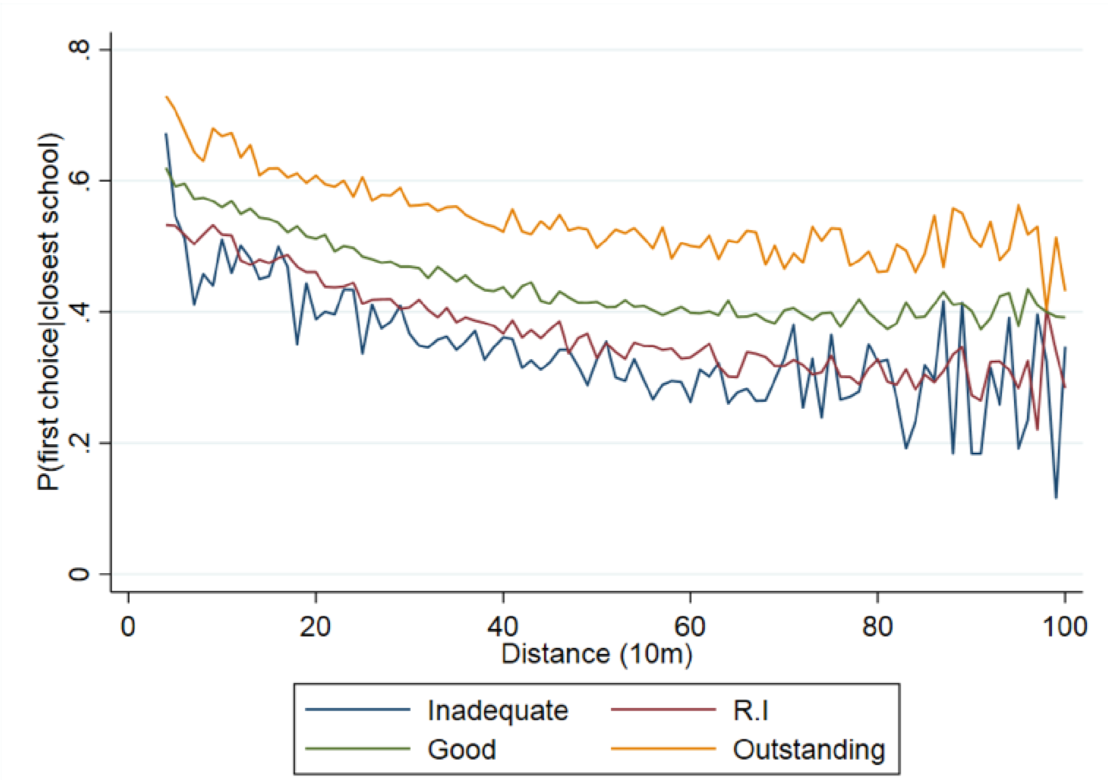
Table 4.13: Relationship between market-level variation in Ofsted ratings and variation in market-level market shares and market-level segregation

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)	0.015** (0.006)	0.038*** (0.011)	-0.009 (0.010)
N	6,940	6,933	6,934
N markets	3,479	3,477	3,477
Mean dep. var.	-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)	0.002 (0.004)	-0.005 (0.005)	0.006 (0.004)
N	6,936	6,908	6,910
N markets	3,479	3,458	3,478
Mean dep. var.	0.29	0.30	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: The sample is all schools included in the final sample (see Appendix Table A3.1). Standard errors in parentheses. * p<0.1, ** p<0.05, *** p<0.01. All variables are created at the local education market-level. D denotes the index of dissimilairy (Duncan and Duncan, 1955).

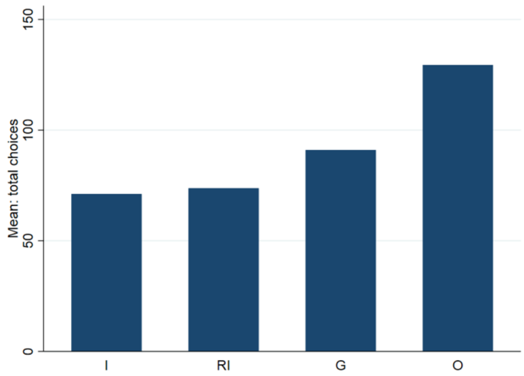
4.8 Figures

Figure 4.1: Percentage of households choosing their closest school as first choice, by Ofsted rating and distance to the school

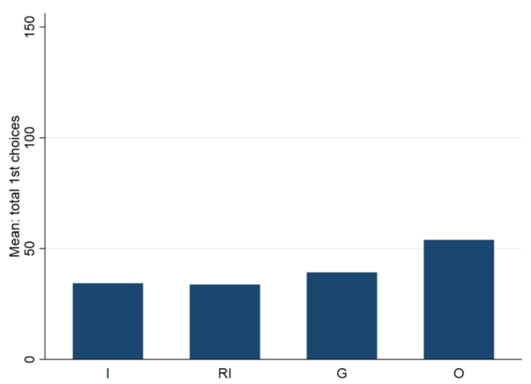


Source: Ofsted management information and National data on school choices/preferences linked to the National Pupil Database, provided by the Department for Education.
Note: The sample is all school choices submitted in January 2014 and January 2015, for pupils and schools included in the final sample (see Appendix Table A3.1).

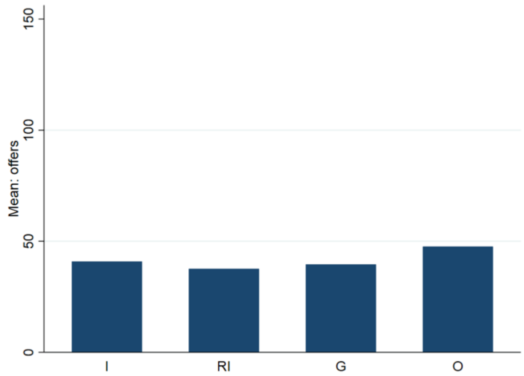
Figure 4.2: Mean school-level dependent variables



(a) Total choices



(b) Total first choices

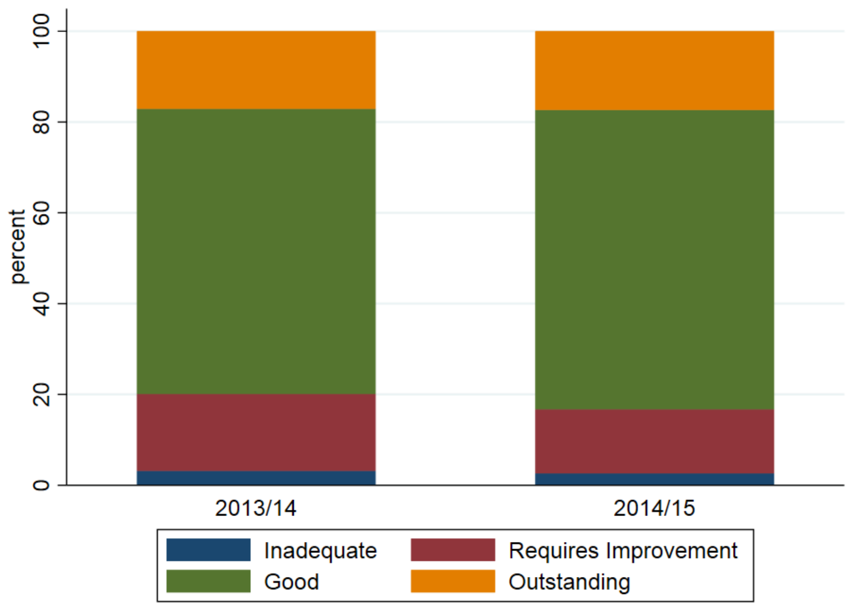


(c) Total offers

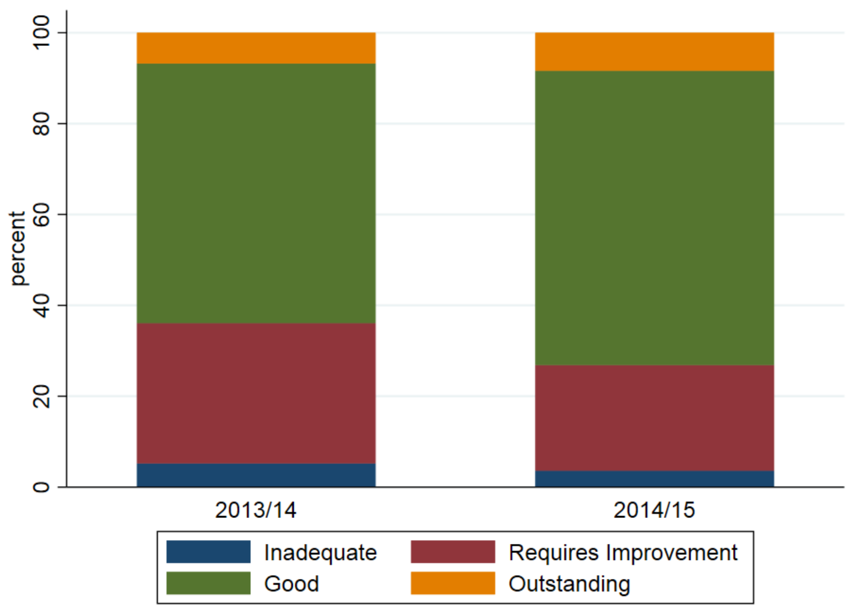
Source: Ofsted management information and National data on school choices/preferences linked to the National Pupil Database, provided by the Department for Education.

Note: The sample is all school choices submitted in January 2014 and January 2015, for pupils and schools included in the final sample (see Appendix Table A3.1). ‘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.

Figure 4.3: Ofsted rating over time



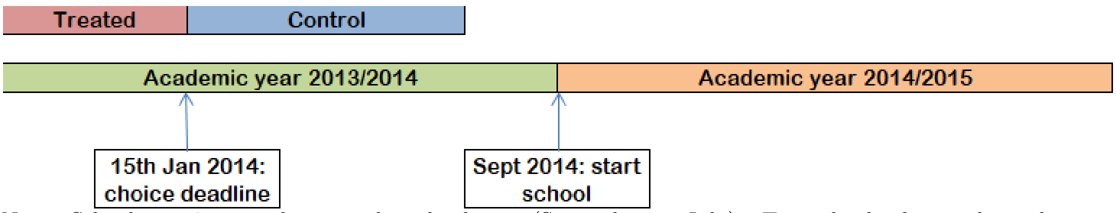
(a) Stock



(b) Ofsted rating in each year

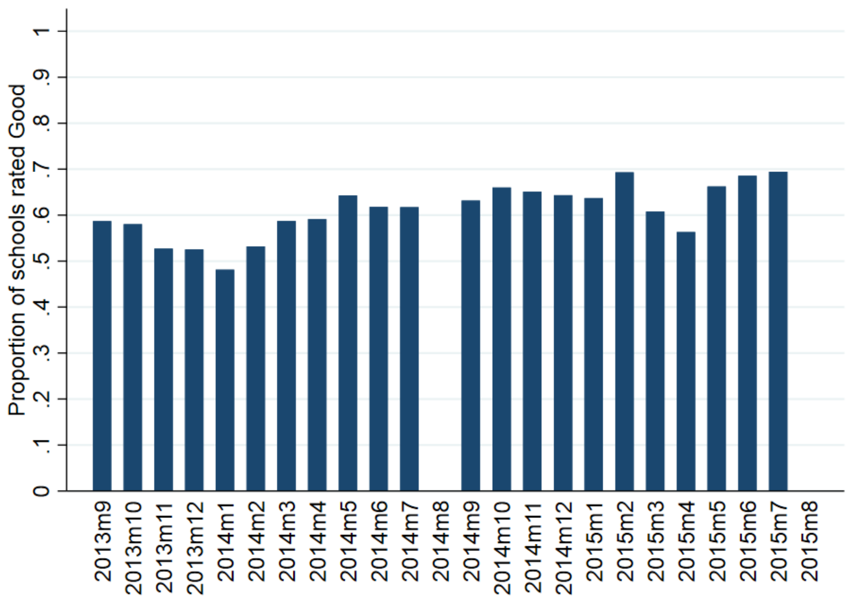
Source: Ofsted management information and National data on school choices/preferences linked to the National Pupil Database, provided by the Department for Education.
Note: The sample is schools included in the final sample (see Appendix Table A3.1). Panel (a) shows the stock of Ofsted ratings in each 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 4.4: Research design for one cohort of pupils

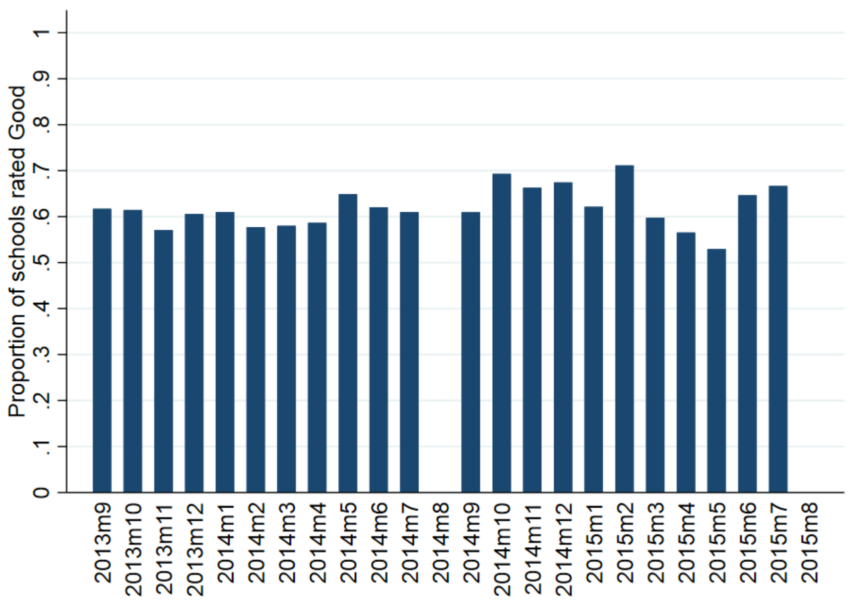


Note: Schools are inspected across the school year (September to July). Treated schools are those that are inspected early, so that their Ofsted inspection rating is released before the school choice deadline on 15th January. Control schools are those that are inspected late, so that the information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

Figure 4.5: Ofsted rating over time within school years



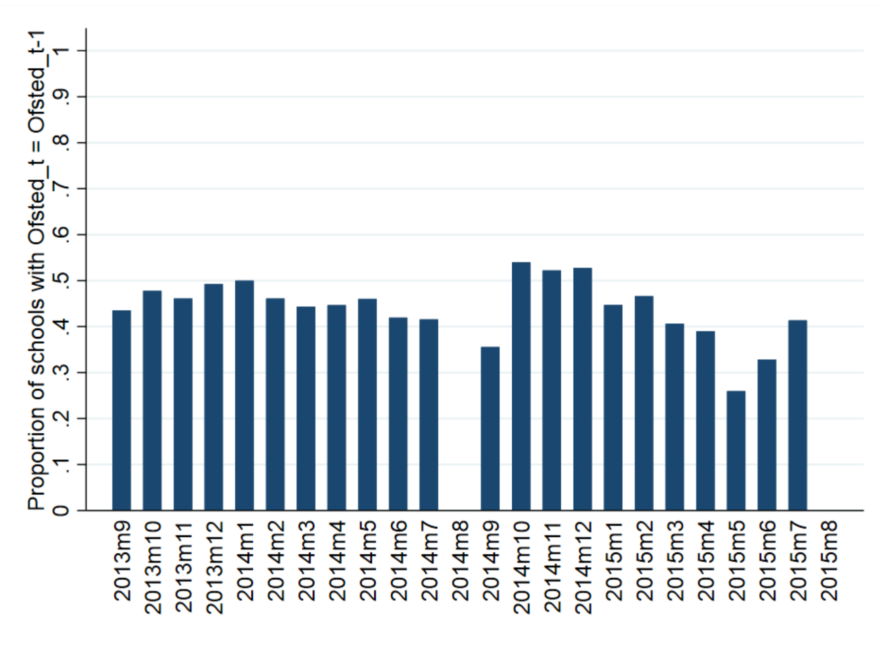
(a) All schools



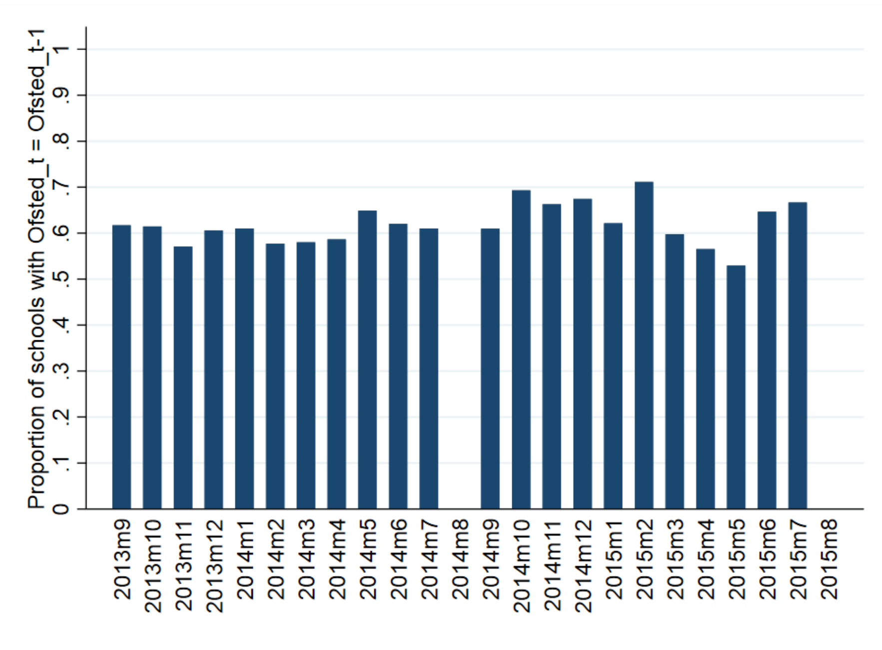
(b) Previously rated 'Good'

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 sample is all schools included in the final sample (see Appendix Table A3.1). 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'.

Figure 4.6: Change in Ofsted rating over time within school years



(a) All schools



(b) Previously rated 'Good'

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 sample is all schools included in the final sample (see Appendix Table A3.1). 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'.

5 Segregation by choice? School choice and segregation in England

Preface: *This chapter is co-authored with Mat Weldon (Department for Education, formally University of Lancaster). The project is the result of general discussions about school choice. Mat was responsible for the data analysis for the counterfactual experiments. I was responsible for the data analysis for the correlates with segregation. I took the lead on writing most sections, in particular integrating our research with the previous literature.*

5.1 Introduction

School choice – broadly defined as any system in which parents' preferences over schools partly determine allocation to school – has theoretical benefits: increasing competition between schools drives productivity (Friedman, 1955, Hoxby, 2003a) and freedom of choice promotes equality of access to 'good' schools (Cantillon, 2017). There is long-standing concern, however, that allowing households school choice will increase school segregation between groups of different social class, income level and ethnicity, which may be problematic for educational outcomes (Guryan, 2004, Reber, 2010, Hanushek, Kain, and Rivkin, 2009, Lutz, 2011, Johnson, 2011, Billings, Deming, and Rockoff, 2014) and wider outcomes such as crime (Billings, Deming, and Rockoff, 2014, Billings, Deming, and Ross, 2019). It is also likely be problematic for society, as integrated schools 'offer the opportunity to enhance intergroup relations' (Burgess and Platt, 2020) and pro-social behaviour (Rao, 2019) while reducing the perception of discrimination (Oberti, 2021).

This paper focuses on whether school segregation is *by choice*, rather than due to constraints in school access or residential segregation. This is important, because to design policies to reduce segregation 'knowledge about its driving forces is indispensable' (Oosterbeek, Sóvágó, and van der Klaauw, 2021). We use data from parents' submitted school choices, under-explored due to their recent availability. This improves upon using school allocations, as these are the product of school choices *and* assignment to school, considering schools' over-subscription criteria and capacity constraints.

Consider an example of a high-attaining state school that has few low-income pupils, neighbouring a low-attaining state school with the reverse. This segregation could be due to households' preferences: preferences for a peer-group 'like us' (Clark, Dieleman, and Klerk, 1992, Schneider and Buckley, 2002, Karsten et al., 2003, Elacqua, Schneider, and Buckley, 2006, Noreisch, 2007, Byrne, 2009, Hastings, Kane, and Staiger, 2009, Bunar, 2010, Saporito, 2014, Abdulkadiroğlu, Agarwal, and Pathak, 2017, Glazerman and Dotter, 2017), the 'right mix' (Byrne, 2006, Hollingworth and Williams, 2010, Vowden, 2012) or for distance over academic quality (Weekes-Bernard, 2007, Hastings, Kane, and Staiger, 2009, Burgess et al., 2015, Borghans, Golsteyn, and Zölitz, 2015, Glazerman and Dotter, 2017, Beuermann et al., 2018, Harris and Larsen, 2019, Walker and Weldon, 2020, Abdulkadiroğlu et al., 2020, Bertoni, Gibbons, and Silva, 2020). Alternatively, this could be driven by constraints in access: all households could prefer the high-attaining school, but if places are rationed by distance to the school and prices rise accordingly (Black and Machin, 2011, Nguyen-Hoang and Yinger, 2011), only the higher-income households are able to gain admission through residential sorting. In England, around 15% of households each year are not admitted to their first-choice school because of capacity constraints (chapter 3), with this percentage higher in urban areas. Using the current allocation of pupils to schools to infer the role of preferences in driving segregation is therefore problematic: segregation could be driven by differences in preferences or differences in constraints across groups.

We provide the first evidence on whether segregation is by choice in a national school choice environment where school choice is not typically constrained by academic ability. Using national administrative data for England, we find that school choices are segregating across most Local Authorities (LAs). A counterfactual exercise that allocates all pupils to their first-choice school, removing all constraints in access, leads to equally highly segregated schools than the current allocation in most areas. This is true for segregation by ethnic group, income deprivation and prior-attainment. In contrast, a counterfactual that assigns pupils to their closest school reduces segregation across all groups in most areas. This final finding would be reversed if only a small percentage of households changed their residential choice in response to such a reform, however.

England is an excellent laboratory to study the functioning of school choice. The right for parents to express a preference for their child's school has been enshrined since the 1988 Education Reform Act. School choices are collected by a central authority and the allocation mechanism used to assign pupils to schools is truth-revealing. National, complete and high-quality data on each pupil in a state-funded school is collected by Government, including their school choices. We exploit the variation in density, ethnic and social composition of LAs to explore how school choice may or may not contribute to segregation in different areas.

The next section summarises the previous literature on school choice and segregation across research disciplines. We then describe the system of school choice in England in more detail, followed by our data and methodology. The results follow, including robustness checks. The final sections provide discussion, including possible policy options to reduce segregation, and our conclusion.

5.2 Previous literature

A significant body of qualitative research across fields studies households' school choices. Choices are often driven by the reproduction of cultural capital (Ball, Bowe, and Gewirtz, 1995, Reay and Ball, 1998, Ball, 2003, Butler and Robson, 2003, Bridge, 2006, Byrne, 2006) as the middle-classes fear the 'destructive and contaminating effects of going to the local comprehensive' (Reay and Lucey, 2004).

This process leads to 'white flight' from some schools (Reber, 2005, Noreisch, 2007, Brunner, Imazeki, and Ross, 2010, Baum-Snow and Lutz, 2011, Vowden, 2012, Zancajo and Bonal, 2020) or neighbourhoods (Kauppinen, Ham, and Bernelius, 2021, Rogne, Borgen, and Nordrum, 2021) and consequently segregated 'idealized' and 'demonized' schools (Reay and Lucey, 2004) while working-class households are traditionally characterised as 'disconnected' choosers (Gewirtz, Ball, and Bowe, 1994). Research focusing on the interaction between class and ethnicity has also shown preference against 'white' schools for some ethnic minority households to avoid the possibility of racism/bullying and enhance community (Reay and Lucey, 2004, Byrne, 2009, Weekes-Bernard, 2007, Bunar, 2010).

One may suppose, therefore, that observed levels of segregation in England's schools are through choice, but in fact there are also 'structural constraints on the choices available to parents in economically deprived areas' (Weekes-Bernard, 2007). This is because over-subscribed schools must choose a way to ration places, which is often by proximity. Admission to a school at the top of the local hierarchy (dependent on a 'fragile equation of colour, ethnicity and social class' (Reay and Lucey, 2004) therefore typically depends on parents' willingness and means to afford higher property prices in the desirable catchment area. This is one of the 'circuits of schooling' identified by Ball, Bowe, and Gewirtz, 1995. Burgess et al., 2011 and Hamnett and Butler, 2011 describe how school catchment areas limit access for economically disadvantaged households, who have only the illusion of choice to over-subscribed schools. Hamnett and Butler, 2013 conclude that 'distance-based rationing of supply of school places in the face of high demand serves to reinforce and reflect existing patterns of residential social segregation and to indirectly undermine the principles underlying the policy of greater school choice'.

Previous empirical research has used either event analysis or counterfactual simulation to study segregation under school choice. In a meta-review of research, Wilson and Bridge, 2019 conclude that 'school choice is associated with higher levels of segregation of pupils between schools', remarkably consistent across school choice systems and contexts.

In the former strand of research, the consensus is that introducing school choice has not led to markedly higher segregation between social groups in England (Allen and Vignoles, 2007, Goldstein and Noden, 2003, Gorard and Fitz, 2000, Noden, 2000) or German primary schools (Schneider et al., 2012, Makles and Schneider, 2015).

Elsewhere worldwide, school choice is typically related to increases in segregation across schools. In Chile, by ability (Hsieh and Urquiola, 2006), New Zealand and the US by race (Ladd and Fiske, 2001, Bifulco and Ladd, 2007), Sweden by ability and family background (Söderström and Uusitalo, 2010, Böhlmark, Holmlund, and Lindahl, 2016) and South Korea by ability (Oh and Sohn, 2019).

In the latter, counterfactual, strand of research, segregation is typically found to decrease under 'neighbourhood' allocation to schools (Allen, 2007, Taylor, 2009, Östh, Andersson, and Malmberg, 2013, Bernelius and Vaattovaara, 2016, Glazerman and Dotter, 2017, Boterman,

2019) although Harris and Johnston, 2020 find that school intakes generally reflect the surrounding neighbourhoods. Glazerman and Dotter, 2017 find that a neighbourhood schools policy would decrease segregation by race but increase segregation by income.

Our paper builds most closely on Allen, 2007, who assesses the allocation of pupils in earlier administrative data for England, relative to counterfactual simulations. Allen, 2007 concludes that where pupils sort into non-proximity schools it tends to increase segregation by ability and income. The difference between the current and counterfactual simulations is largest in urban areas, presumably as pupils have a more diverse choice set of schools within a reasonable commuting time. We extend this analysis by using administrative data on households' school choices in addition to allocation to explore the role of preferences relative to constraints.

Recent work studying Amsterdam is the first to quantitatively isolate the effect of parents' school choices (preferences) on segregation. Using administrative data from the secondary school match, Oosterbeek, Sóvágó, and van der Klaauw, 2021 find that 70% of school segregation (within school tracks) is driven by preference heterogeneity across groups. In Amsterdam, pupils are free to choose any school which offers their ability track. As Amsterdam is relatively small and school density is high, the authors suggest that residential segregation should not necessarily be a main driver of school segregation. The results may not therefore generalise to cities without free school choice, larger cities, or cities with less developed public transport.

5.3 School choice in England

School choice is England is broadly defined as 'open enrollment' as opposed to 'opt-out' (see Wilson and Bridge, 2019). That is, households are not assigned to a default school which they can opt-out from (as is common in the US, Scotland, and some areas in Germany) and instead can submit preferences for any preferred school(s), locally or further afield. This is similar to the system in many European countries - for example Sweden and the Netherlands - New Zealand, and South America - Chile and Brazil (Wilson and Bridge, 2019). Butler and Hamnett, 2007 characterise the English education system as sitting between 'a choice-driven North American model of educational allocation and a more geographically driven allocation model traditionally favoured in Europe.'

Parents in or entering the English state education system provide a ranking of their preferred choices of school on a form that is submitted in a centralised system to their LA. All government funded schools participate in this common system. Applications made to private schools are not co-ordinated, although parents can apply to both private and state schools simultaneously. Depending on the LA, parents can provide up to three to six choices of school in rank order, with three being the mode (Table 5.1).

A set of published school admissions criteria are used where a school is over-subscribed. Typically, these include: whether the child has a statement of special educational need, whether the child is looked-after by the LA; whether the child has a sibling at the school already; the distance of the family home from a school; and less commonly, the faith or aptitude of a child (chapter 3). Each child is allocated to their highest ranked school where they are admitted according to the criteria of each school. This allocation is done using an algorithm (student optimal stable allocation, see Pathak and Sönmez, 2013) that is weakly truth-revealing, meaning that parents can do no better than by reporting their true preferred schools, although the short list implies that low probability schools may be omitted (Haeringer and Klijn, 2009, Calsamiglia, Haeringer, and Klijn, 2010, Fack, Grenet, and He, 2019). Parents may also have erroneous beliefs, and for this reason we treat the list of chosen schools as 'stated' preferences rather than necessarily parents' true underlying preferences. If a pupil is not allocated to any preferred school they are assigned to a school with spare capacity (which is by definition less popular).

This process is the same for entry for primary schools (at age four) and secondary schools (at age 11). Some (16 out of 152) LAs have additional middle schools, which we exclude from this analysis. We include LAs that have selective state schools known as grammar schools, where admission depends on an additional test taken at the end of primary school.

This system leads to a level of segregation in England's schools which is around the middle of OECD countries, typically lower than countries where selection by ability is the norm (Jenkins, Micklewright, and Schnepf, 2008).

5.4 Data

Data on each household's secondary school choice(s) covers the whole cohort of pupils seeking admission to any English state secondary school in the school-year 2014/2015. Access to these data was provided by the Department for Education, through the National Pupil Database (NPD) application process.

We derive the closest, or neighbourhood, school from pupils' home location. The data include the following pupil-level characteristics which we use to study segregation: eligibility for Free School Meals (FSM), a binary variable as a marker of poverty; ethnic group, which we group into White and non-White; prior-attainment according to performance in nationally set and externally examined assessments at the end of primary school (KS2). This measure is grouped into the top 20% of prior-attainment and bottom 80%. These are coarse measures, which miss subtlety across the distributions of socio-economic status and prior attainment, and across ethnic groups. Taylor, 2018 shows that there is a meaningful proportion of children 'who could be described as disadvantaged' who are not recorded as eligible for FSM, for example. Hobbs and Vignoles, 2010 show that FSM children are more likely to be in low-income households than non-FSM children, but between 50% and 75% of FSM children are not in the lowest income households. Additionally, FSM does not capture a broader definition of social class that is shown to influence the process of school choice in other contexts (see for example Boterman, 2021).

Table 5.1 shows the descriptive statistics for our final sample at the LA-level. The initial population of students is all students who didn't choose and weren't allocated to a Special school (catering for children with special educational needs). The median LA has 79% of secondary school pupils that are white, 38% FSM and 28% high prior-attainment. In the median LA, 87% of pupils are assigned to their first-choice school, although there is wide variation across LAs, with the minimum at 65% and maximum at 99%. In the median LA, 40% choose their closest school as first-choice, and 43% attend their closest school. Again, there is variation across LAs, from 19% to 77%.

These data are supplemented with area-level characteristics, such as pupil composition, school density (schools per km^2), rural/urban category, and whether the LA has a selective (Grammar school) system or allocates pupils according to 'fair-banding' where an equal proportion of pupils from across ability bands is admitted to the school. These final two policy variables are binary, defined according to whether a substantial percentage of schools in the LA has the relevant admissions criteria (10% for selective schools and 20% for fair-banding). Information on school admissions criteria is drawn from schools' self-reported admissions type (selective or not) and data collected as part of a wider project on secondary schools' admissions criteria led by Simon Burgess at the University of Bristol.

5.5 Method

5.5.1 Measuring segregation

A common measure of segregation is the Index of Dissimilarity (D) (Duncan and Duncan, 1955). For two disjoint sub-groups of the population indexed by $t \in \{0, 1\}$ representing, for example, white and minority pupils, and G non-overlapping geographical units (for example neighbourhoods or schools), the index is defined as

$$D = \frac{1}{2} \sum_{g=1}^{G} \left| \frac{n_{g,0}}{N_0} - \frac{n_{g,1}}{N_1} \right|$$

where $n_{g,t}$ is the number of group t in unit g, and N_t is the total population of group t across all units. At the LA level, N_0 would be the total population of White pupils in the LA, for example.

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.

Although popular in the segregation literature, D is known to be upward-biased for finite samples. This problem is especially acute when numbers of one or both groups are small in some geographical units. To address this, we use the bias correction method in Allen et al., 2015, which is described in Appendix A4.1.

5.5.2 Counterfactual simulation

Segregation under first-choices is calculated using each household's first-choice rather than actual school in the measure of D. School capacities are allowed to vary, with popular schools expanding and unpopular schools contracting. By removing capacity constraints, this simulation isolates the effect that parents' school choices (interpreted as preferences) have on school segregation. We discuss caveats to this interpretation in section 5.7.

An advantage of D as a measure of segregation is that it is additive, which means that the observed D and D resulting from the counterfactual estimates can be differenced. For example, post-residential sorting would be $D_{\text{allocation}}$ - D_{nearest}. In order to assess the degree to which D reflects post-residential rather than underlying residential segregation, we compute segregation under an additional simulated scenario in which all children attend their nearest school. In this scenario, *all* segregation is residential, so the difference between the actual measured levels of segregation, and the levels measured under this counterfactual indicates the extent of post-residential segregation.

This method is very similar to that described by Allen, 2007. The main difference is the way that school capacities are treated in the counterfactual. In order to treat school capacities as exogenously fixed, Allen uses a 'Boston' matching mechanism where some pupils are therefore allocated to schools far away. In contrast, we remove fixed capacities, so each pupil is allocated to his/her nearest school: the de facto catchment area of each school is its Voronoi neighbourhood.

5.6 Results

Figure 5.1 presents the distribution of segregation of secondary schools across LAs in our sample. The first column shows segregation by FSM. The second column shows segregation by prior-attainment and the third by ethnic group. Each row represents a different school choice environment. From top to bottom: current allocation, allocation by first-choice, and allocation by nearest school.

5.6.1 Segregation under current allocation

The current distribution of segregation in England's secondary schools is shown in the first row of Figure 5.1. The average level of segregation by FSM and prior-attainment is around 0.2: 20% of pupils would have to be re-allocated across schools for there to be an even spatial distribution across schools within the LA. This is slightly lower than found for the cohort of pupils entering secondary school in 2000 (Allen, 2007). There is variation in segregation across LAs, with some having levels of segregation close to zero and some close to 0.4. The inter-quartile range is narrow, however (0.08, Table 5.2). The average segregation by ethnic group is higher (0.28) and has a wider distribution (inter-quartile range 0.15). Some LAs have levels of segregation close to zero while others are close to 0.6, meaning that over half of pupils would have to re-allocated across schools for there to be no segregation within the LA.

England's secondary schools are therefore more segregated by ethnic group than income deprivation and prior-attainment. A high level of segregation by ethnic background is consistent with earlier research, dis-aggregated by ethnic group (Burgess, Wilson, and Lupton, 2005). The cause may be the interaction between class and race in white middle-class school choices (Byrne, 2009) and 'tipping points' in the acceptable percentage of 'other' students (Noreisch, 2007, Vowden, 2012), but could also reflect active choices by non-White ethnic groups. For example, chapter 3 and Walker and Weldon, 2020 show, using the nationally representative data we employ here, that the school choice patterns of non-White households are consistent with active engagement in the school choice process and 'ambitious' school choices consistent with valuing school quality. Households from the non-majority ethnic group in England may also seek a safe and respectful place for their child, free of bullying (Weekes-Bernard, 2007, Bunar, 2010) and low teacher expectations (Weekes-Bernard, 2007). Harris and Johnston, 2020 also explore the interaction between ethnic and socio-economic segregation, finding that, for some groups in some LAs, apparent ethnic segregation has 'underlying socio-economic causes'.

The current level of segregation across LAs is correlated with several observable LA characteristics (Table 5.3). These vary across groups. Whether an LA has a significant fraction of selective schools is positively correlated with segregation by KS2, but not by FSM and ethnic group. The percentage of White students in the LA is negatively correlated with segregation by FSM and KS2, but positively with segregation by ethnic group. Segregation by ethnic group is higher where there is a higher proportion of non-White pupils, even conditional on geographical characteristics such as school density.

Other variables have a similar effect across categories. For example, as in Allen, 2007, urban LAs have higher segregation relative to rural LAs, although only significantly so for FSM and ethnic group.

Several area-level characteristics are notably uncorrelated with the current level of segregation, however, such as whether households are permitted to express more than three school choices.

5.6.2 Segregation under first choice allocation

The distribution of segregation across LAs would change only marginally if all pupils in England were assigned to their first-choice school rather than the actual allocation. The second row of Figure 5.1 shows the distribution for segregation by ethnic group, FSM and prior-attainment. The distributions for FSM and prior-attainment are very similar to the distributions under the current allocation. For example, the mean level of segregation for FSM is 0.23 under the current allocation and 0.22 under the simulated allocation using first-choice school. For prior-attainment, the mean level of segregation changes from 0.20 to 0.21 (Table 5.2). The inter-quartile ranges are also almost identical. The mean and inter-quartile range for segregation by ethnic group are also largely unchanged, although the shape of the distribution is altered, with weight moving from the very lowest to slightly higher levels of segregation. These patterns are perhaps unsurprising given the high percentage of households that are allocated to their first-choice school. At the median, 87% of households are allocated to their first-choice school, so the first-choice counterfactual would be affected by only 13% of households.

The lower panel of Table 5.2 shows the distribution of the *difference* in segregation between current and simulated allocations across LAs. There is no difference in segregation by ethnic group, FSM or prior-attainment for the majority of LAs: the mean, mode and median are close to zero in each case. There are LAs where allocating all pupils to their first-choice would increase or decrease segregation, however, up to around 0.1: constraints in some LAs change school-level segregation by around 10%.

At face value, these results suggest that segregation from school choice is a result of parents' preferences rather than any constraints in accessing preferred schools. Whether this is true depends on whether parents' school choices reflect their true preferences, or whether, instead, parents' stated choices are influenced by other factors such as the probability of admission to each school. We return to this in the robustness section.

5.6.3 Segregation under proximity allocation

In contrast, the distribution of segregation across LAs changes when all pupils are assigned to their closest school. The mean level of segregation typically decreases: by prior attainment, 0.12 under neighbourhood assignment, compared to 0.20 under the current allocation; by FSM, 0.20 compared to 0.23; by ethnic group, the mean is unchanged (0.28) but the median decreases from 0.27 to 0.25 (Table 5.2).

At the LA-level, moving to neighbourhood schools would reduce school segregation in most LAs. This is most obvious for segregation by prior-attainment, where the mean difference across LAs is -0.05: moving from a system of school choice to neighbourhood allocation would reduce segregation by 5% in the mean LA. The distributions are wide, however, with segregation *increasing* in around 18%, 13% and 46% of LAs under the neighbourhood allocation compared to the current allocation, for FSM, prior-attainment and ethnic group, respectively.

These results imply that, overall, school choice exacerbates rather than reduces school segregation arising from residential segregation. This does not take into account that residential choices are endogenous pupil assignment, however. For example, parents' choice of residential neighbourhood becomes even more important when it entirely determines access to schools, as we discuss in the robustness section.

5.6.4 Who chooses segregation?

This section explores the circumstances where allocation by first-choice exacerbates or reduces segregation, presenting results from a multivariate regression with the difference in segregation under the first-choice allocation and current allocation ($D_{[pref]} - D_{[alloc]}$) as the dependent variable. The interpretation of $D_{[pref]} - D_{[alloc]}$ is the difference in sorting due to school choices when admission constraints are removed. The current allocation $D_{[alloc]}$ is included as a covariate, as the current allocation is likely to reflect some structural features of the LA that cannot be captured by other observable characteristics. A negative coefficient means that the covariate lowers $D_{[pref]} - D_{[alloc]}$, which is equivalent to decreasing segregation under the first-choice allocation relative to the current allocation. The first three columns of Table 5.4 show that the coefficient for $D_{[alloc]}$ is negative and significant for each category: when households face a more segregated school system, their school choices tend to be less segregating.

Geographical characteristics of the LA are not correlated with the dependent variable, with the exception that in London, allocation by first-choice lowers segregation across ethnic groups compared to the current allocation.

Policy characteristics of the LA are correlated with the difference between current and counterfactual segregation. Areas with fair-banding have slightly higher segregation by FSM under the first-choice allocation than current allocation, and with a similar effect size (but insignificantly) by prior attainment. Areas with selective schools also have higher segregation under the first-choice allocation by prior attainment, which suggests that parents choose more ability segregation, in selective areas, than the system actually delivers. Finally, whether an LA allows more than three school choices is positively correlated with increasing segregation under the first-choice allocation relative to the current allocation. These three factors suggest that when given more freedom to choose schools, households have more segregating choices.

In summary, there are no clear or universal characteristics of LAs that are correlated with school choices that are highly segregating or desegregating. The strongest relationship is between the current level of segregation in the LA, which is correlated with first-choices being desegregating. Other than that, policy options have the clearest correlation. Where policy options give parents more freedom in their choices, their choices appear to be slightly more segregating for some groups.

This broad pattern is also observed when comparing the actual allocation to the nearest-school counterfactual. In the final three columns of Table 5.4, the dependent variable is $D_{[near]} - D_{[alloc]}$, and again, $D_{[alloc]}$ is an important covariate. Unsurprisingly, the nearest-school counterfactual is more desegregating where schools are currently more segregated. Allocating pupils to the nearest school significantly decreases segregation in policy environments where first-choices increased segregation. For example, segregation relative to the current allocation is lower under neighbourhood assignment in selective LAs and fair-banding LAs, although only significantly for

KS2 in fair-banding LAs.

Segregation by ethnic group increases in the neighbourhood allocation relative to the current allocation where the LA has a higher percentage of White pupils and a higher percentage of pupils with low prior attainment. This might be due to higher levels of residential segregation in these LAs.

Overall, there is an emerging pattern that LAs which currently allow parents more freedom in their school choices are more segregated under first-choices and less segregated under neighbourhood assignment. This strengthens the case that the current level of segregation in England's schools is driven by households' preferences in addition to schools' capacity constraints, rather than solely by schools' capacity constraints.

5.6.5 Robustness

Throughout, we have assumed that school choices reflect parents' true preferences for schools. This is justified in part by the truth-revealing allocation mechanism used to assign pupils to schools in England. The restricted list length for school choices means that parents may rationally be strategic in their school choices, omitting schools with zero/low probability of admission (Haeringer and Klijn, 2009, Calsamiglia, Haeringer, and Klijn, 2010, Fack, Grenet, and He, 2019). If so, then school choices do not map perfectly to preferences, but instead incorporate constraints (through the expected probability of admission) to some extent.

To explore this, we first interpret the results that school choices are segregating (and neighbourhood allocation desegregating) where households' school choices are currently less bound by residential location. These are areas for which a meaningful proportion of schools allocate pupils according to prior ability, either from the top (selective LAs) or an equal distribution (fair-banding LAs) or households have a less constrained school choice list. Across these areas where it is most likely that school choices to equate to preferences, school choices are generally more segregating than the current allocation.

To give a concrete example, the number of school choices permitted varies across the 10 LAs within Greater Manchester, from three to six. Within Greater Manchester, first-choices are more segregating where six choices are permitted, although not significantly so, perhaps given the small sample size. This is most notable for segregation by ethnic group, where segregation is 0.016 higher under first-choice compared to allocation in LAs with six rather than three choices. This is equivalent to 1.6% more White students changing school to achieve a balanced distribution. While this example cannot be given a causal interpretation, as there may be differences between LAs that choose to allow more or fewer school choices, it certainly does not contradict the pattern that emerges from the multivariate regressions.

A limitation of all counterfactual studies to date is that households' location is fixed, rather than responsive to changes in school assignment mechanisms. Local school quality currently informs households' residential decisions, as admission is often implicitly tied to location, for example through ‘catchment areas’ or distance. A substantial body of literature finds that house prices, indicative of demand, respond to local school quality (Black, 1999, Leech and Campos, 2003, Bayer, Ferreira, and McMillan, 2007, Gibbons and Machin, 2008, Fack and Grenet, 2010, Gibbons, Machin, and Silva, 2013, Machin and Salvanes, 2016). Exploring the variation in this response, He, 2017 finds heterogeneous effects across cheaper and more expensive school districts and Cheshire and Sheppard, 2004 show the house price premium is responsive to the ‘suitability’ of potential homes for families and local constraints in the supply of housing.

How would households’ choice of neighbourhood change if it were explicitly (rather than implicitly) tied to school choice? Theory suggests that households’ location responds to changes in school admission policies (Nechyba, 2000, Epple and Romano, 2003, Bayer, Ferreira, and McMillan, 2007, Ferreyra, 2007, Calsamiglia, Martínez-Mora, and Miralles, 2015, Calsamiglia, Fu, and Güell, 2020). This has been confirmed through empirical work using changes in school choice environments over time (Thrupp, 2007, Baum-Snow and Lutz, 2011, Brunner, Cho, and Reback, 2012, Chakrabarti and Roy, 2015). The re-introduction of school zones (catchment areas) in New Zealand indicates that residential segregation is likely to increase in response to neighbourhood schooling (Thrupp, 2007). Baum-Snow and Lutz, 2011 find a strong migration response of White households in response to desegregation policies in the US, while Reber, 2005 estimates that ‘white flight’ reduced the effects of desegregation plans by about one-third.

We find that neighbourhood allocation decreases income (FSM vs non-FSM) segregation compared to the current allocation by 3% at the mean. This implies that, for the mean LA, more segregating residential choices of 3% of non-FSM households would reverse this finding. This seems modest, given the findings in the literature.

A related limitation is that households may respond to a change in school admissions policies by exit to the private sector (Clotfelter, 1976, Clotfelter, 2001, Reardon and Yun, 2003, Reber, 2005, Saporito, 2014, Saporito and Sohoni, 2007, Söderström and Uusitalo, 2010, Baum-Snow and Lutz, 2011, Calsamiglia and Güell, 2018, Calsamiglia, Fu, and Güell, 2020) which is one method of admission to a preferred school (Butler and Robson, 2003, Rangvid, 2007, Maloutas, 2007, Cordini, Parma, and Ranci, 2019, Bonal, Zancajo, and Scandurra, 2019, Nielsen and Andersen, 2019, Boterman, 2021). Again, for the mean LA, a relatively small exit response by more affluent households would reverse the finding that neighbourhood assignment is desegregating.

Like previous counterfactual analysis, we can therefore conclude that neighbourhood allocation would decrease segregation in schools if households have no endogenous responses. Incorporating households’ changes in residential location and exit to the private sector is likely to mean that neighbourhood allocation is no longer desegregating, as in empirical studies of real-world cases.

Finally, our results are similar in an alternative specification where no density correction is applied to the Dissimilarity Index (not shown due to space constraints).

5.7 Discussion

The aim of school choice policies, to meaningfully improve educational attainment for all, has limited empirical support. The second aim, to improve equality of access to popular schools by breaking down the selection-by-mortgage effect, has long been questioned by research across disciplines that has recognised the classed and raced differential access to meaningful choice. This article contributes to this research field by providing nationally representative evidence about whether school choice segregates by choice, or due to barriers such as lack of economic resources necessary to navigate the 'circuits of schooling'.

We find that school choices are at least as segregating as the current allocation in most areas in England, suggesting that segregation is by choice (subject to the caveats discussed in the robustness section). Segregating preferences mean that if policy-makers and schools are concerned about the current level of segregation by income, prior-attainment and ethnic group, alternative or adjusted policies need to be explored.

First, at the extreme, neighbourhood or 'zoned' schooling could be re-introduced, as in New Zealand following their experimentation with neo-liberal school choice policies. Our counterfactual simulation shows that residential segregation is typically lower than school segregation. At face value, assigning all pupils to their local school would decrease school segregation. Indeed, the Geography of Education literature recommends that educational policies should also be integrated with spatial policies (Boterman, 2019). Frankenberg, 2013 notes the 'integrative potential of housing policies' and the potential of 'legal and policy options for how residential integration efforts might affect school segregation'. As discussed in section 5.6.5, however, policy-makers must recognise that residential segregation is also partly an outcome of the school choice environment. Admission to schools is shown to drive residential sorting particularly in areas where location largely determines school admission, such as Finland (Bernelius and Vilkama, 2019) and Paris (Oberti and Savina, 2019). At the other extreme, Boterman, 2021 finds that free school choice in Amsterdam has allowed neighbourhood integration, as parents need no longer move to gain admission to their preferred school. (See also Rangvid, 2007 and Rangvid, 2009 for Copenhagen, Söderström and Uusitalo, 2010 for Stockholm, and Schachner, 2021 for suburban processes in Los Angeles.) This policy would therefore be likely to lead to increased sorting across neighbourhoods.

Second, within a system of school choice - following parents' preferences to some extent, there are options for non-geographical rationing of places. Fair-banding, where an equal proportion of pupils from across ability bands is admitted to the school, could be expanded across England, as considered by Hamnett and Butler, 2011 and Hamnett and Butler, 2013. Hamnett and Butler, 2013 state that fair banding 'appears to have much to commend it in terms of overcoming the role of distance-based allocational systems', although Weekes-Bernard, 2007 notes the potential for parents (particularly those with English as an Additional Language) to misunderstand fair banding as a selective, 11+ style, admissions test.

There are other non-geographical admissions arrangements, such as lotteries for over-subscribed schools. This policy has been trialled in Brighton and Hove (see Allen, Burgess, and McKenna, 2013), and was vehemently opposed by residents in formerly desirable catchment areas. Local pressure meant that the lottery became district-wide rather than LA-wide, with 'district' arguably gerrymandered to retain segregation between higher and lower attaining schools.

These political problems are noted by Hamnett and Butler, 2013 as a significant barrier to implementation. Parents may also be opposed to the uncertainty created by lotteries (Vowden, 2012) and mourn the loss of local schools which serve as a community for many parents, particularly of primary school age children (one reason for attaining the 'right mix' of children is to form friendships with the 'right mix' of parents). Burgess, Greaves, and Vignoles, 2020 consider the feasibility of 'marginal ballots' – where a substantial proportion of school places would be allocated as normal, and the remaining places would be reserved for a random draw among unaccepted applicants - and a simple priority for disadvantaged families, or reserved places for applicants from less well-off backgrounds. Burgess, Greaves, and Vignoles, 2020 state that:

> Our personal view of the evidence is that there is much to recommend a marginal ballot approach, with perhaps 10% or 20% of places reserved for non-priority applicants. However, how the ballot is communicated to potential applicants is also key to avoid a rejection of a 'postcode lottery' approach, which is perceived to be a major problem in other public services.

Another, mainly theoretical, literature studies the effect of affirmative action policies, where seats at popular schools are reserved for certain groups of pupils (Hafalir, Yenmez, and Yildirim, 2013, Ehlers et al., 2014, Echenique and Yenmez, 2015, Doğan, 2016, Klijn, Pais, and Vorsatz, 2016, Dur et al., 2018, Escobar and Huerta, 2021). Escobar and Huerta, 2021 find that affirmative action is an effective tool for reducing segregation, while Dur et al., 2018 find that the ordering of the precedence matters for the eventual assignment and Kojima, 2012 notes the potential perverse effects.

For all potential policy reforms to school admissions under school choice, careful consideration would need to be given to 'tipping points' or 'white flight' from the area or from the state sector entirely (Reber, 2005, Noreisch, 2007, Baum-Snow and Lutz, 2011, Vowden, 2012), which could lessen integration overall. Considering this, Vowden, 2012 states that:

> Even a relatively modest reform – such as a controlled choice system designed to ensure that the proportion of children eligible for free school meals in every Hammersmith & Fulham primary school fell between 25 and 50% – might prompt a significant exodus of middle-class parents from the local state system. The most popular schools in the study area had lower proportions than that, and for many respondents this was an important part of their appeal.

Evidence suggests that segregated school systems have particularly detrimental effects for those in "Minority" schools. In the US, some of these disadvantages stem from the local financing of schools, and so the effects of segregation on pupils' educational outcomes could be partially offset by compensatory resources to schools (Billings, Deming, and Rockoff, 2014, Gamoran and An, 2016). Progressive funding in England, enhanced by the Pupil Premium, is therefore likely to mitigate the effects of segregation, although broader outcomes such as social cohesion are unlikely to be resolved by additional funding.

5.8 Summary

Segregation in schools between different groups of pupils has been shown to have negative short-term and long-term consequences. This paper extends previous research to explore whether, under a system of school choice, segregation is *by choice*. This is important, as different policies are required to address segregation depending on the cause: driven by parents' preferences, or constraints in accessing their preferred school. Using national administrative data for England, we find that school choices are segregating across most Local Authorities. Allocating all pupils to their first-choice school, removing all constraints in access, leads to equally highly segregated schools than the current allocation in most areas. This is true for segregation by ethnic group, income deprivation and prior-attainment.

A potential limitation to these results is the interpretation of the first-choice as reflecting parents' preferences. Despite the truth-revealing assignment mechanism used in England, the short list length (between three and six across England) may induce strategic school choices if households 'skip the impossible' where there is no chance of admission (Fack, Grenet, and He, 2019). Our interrogation of the data suggests that this is not the case: school choices are more segregating in areas where school choices are less constrained by the list length or geographically based probability of admission, suggesting that segregation is by choice.

In our second counterfactual, following Allen, 2007, we find that 'neighbourhood' schooling – where all pupils are assigned to their closest school – reduces segregation relative to the current allocation in most Local Authorities. At face value, this suggests that policy-makers must balance the potential advantages and disadvantages (increased segregation) arising from school choice. In practice, however, it is likely that compensating residential movement in response to ending school choice would undo this finding. For example, higher income households currently living in more integrated neighbourhoods may decide to sort into more segregated neighbourhoods around 'good' schools to gain admission in a neighbourhood system. Further research into households' strategic moves in response to local school quality under school choice is necessary to determine the likely effects of policies to encourage integration in schools. Alternative policies for increasing equality of access and reducing segregation in schools should be considered, such as the introduction of fair-banding, marginal lotteries or quotas.

5.9 Tables

Table 5.1: Summary statistics on 136 Local Authorities

	Min.	1st Qu.	Median	Mean	3rd Qu.	Max.
Pupils:						
# pupils	310	1,854	2,646	3,416	3,737	15,246
% white	6	65	79	71	91	97
% FSM	10	26	38	43	63	87
% high KS2	19	25	28	38	56	82
Allocations:						
% at nearest	20	34	43	43	52	76
% at 1st preference	65	79	87	85	94	99
% 1st pref. is nearest	19	31	40	41	50	77
Schools:						
# Schools	4	11	15	20	21	98
% faith sch	0	14	22	26	33	100
% grammar	0	0	0	4	1	37
Mean % 5+ A*-C	39	53	57	57	61	75
LA:						
# choices on list	3	3	3	4	6	6
Selective	0	0	0	15	0	100
Rural	0	0	0	9	13	59
Urban city and town	0	0	43	43	87	100
Urban conurbation	0	0	38	47	100	100
School density	0.01	0.03	0.11	0.16	0.23	0.86

Source: Author's calculations from national preference data and the National Pupil Database. School characteristics from publicly available information provided by the Department for Education and collection of secondary school admissions criteria.
Note: 'Selective' means that at least 10% of schools in the LA select pupils according to an academic test. 'Fair-banding' means that at least 20% of schools in the LA select pupils according to an academic test to have a equal balance of academic ability in the intake. An LA is classified as 'rural' if at least one third of schools are classified as 'rural'. An LA is classified as 'town' if the modal school is classified as 'urban city and town' and less than one third of schools as 'rural'. An LA is classified as 'city' if the modal school is classified as 'urban conurbation' and less than one third of schools as 'rural'. 'School density' is the number of schools per km^2 (/1,000,000).

Table 5.2: Distribution of dissimilarity indices of 136 Local authorities

	Min.	1st Qu.	Median	Mean	3rd Qu.	Max.
Dissimilarity indices:						
D_{FSM}, allocated	0.03	0.19	0.22	0.23	0.27	0.37
D_{FSM}, pref	0.02	0.19	0.23	0.22	0.26	0.38
D_{FSM}, near	0.04	0.15	0.20	0.20	0.24	0.38
D_{KS2}, allocated	0.01	0.14	0.17	0.20	0.21	0.60
D_{KS2}, pref	0.01	0.15	0.18	0.21	0.23	0.60
D_{KS2}, near	0.04	0.10	0.12	0.12	0.14	0.25
D_{eth}, allocated	0.03	0.21	0.27	0.28	0.36	0.56
D_{eth}, pref	0.07	0.21	0.27	0.29	0.36	0.60
D_{eth}, near	0.06	0.17	0.25	0.28	0.35	0.62
Dissimilarity differences:						
$D_{\text{FSM,pref}} - D_{\text{FSM,alloc}}$	-0.07	-0.01	-0.00	-0.01	0.00	0.06
$D_{\text{FSM,near}} - D_{\text{FSM,alloc}}$	-0.16	-0.06	-0.02	-0.03	-0.00	0.10
$D_{\text{FSM,pref}} - D_{\text{FSM,near}}$	-0.11	-0.00	0.02	0.02	0.06	0.15
$D_{\text{KS2,pref}} - D_{\text{KS2,alloc}}$	-0.03	-0.00	0.01	0.01	0.03	0.09
$D_{\text{KS2,near}} - D_{\text{KS2,alloc}}$	-0.48	-0.09	-0.05	-0.07	-0.01	0.04
$D_{\text{KS2,pref}} - D_{\text{KS2,near}}$	-0.05	0.02	0.06	0.09	0.12	0.51
$D_{\text{eth,pref}} - D_{\text{eth,alloc}}$	-0.08	-0.00	0.01	0.01	0.02	0.10
$D_{\text{eth,near}} - D_{\text{eth,alloc}}$	-0.27	-0.03	-0.00	-0.00	0.03	0.15
$D_{\text{eth,pref}} - D_{\text{eth,near}}$	-0.13	-0.03	0.01	0.01	0.05	0.29

Source: Author's calculations from national preference data and the National Pupil Database.

Table 5.3: Correlates with dissimilarity indices in 136 Local authorities

	FSM	KS2	Ethnicity
Fair-banding	-0.015	0.048	-0.031
	(0.025)	(0.028)	(0.047)
Selective	0.029	0.144***	0.003
	(0.017)	(0.019)	(0.032)
More than 3 choices	0.007	0.009	-0.025
	(0.014)	(0.015)	(0.026)
Urban city and town	0.057***	0.033	0.032
	(0.016)	(0.018)	(0.029)
Urban conurbation	0.065***	0.041	0.087*
	(0.019)	(0.021)	(0.036)
London	0.007	0.077***	-0.150***
	(0.020)	(0.023)	(0.038)
School density	-0.045	-0.038	-0.145
	(0.062)	(0.070)	(0.118)
% FSM	-0.000	-0.003**	0.000
	(0.001)	(0.001)	(0.001)
% White	-0.101**	-0.081*	0.357***
	(0.036)	(0.041)	(0.068)
% Low KS2	0.083	0.895***	0.308
	(0.110)	(0.124)	(0.209)
% Religious schools	0.109*	0.059	0.005
	(0.049)	(0.056)	(0.094)
N	136	136	136

Source: Author's calculations from national preference data and the National Pupil Database. School characteristics from publicly available information provided by the Department for Education and collection of secondary school admissions criteria.

Note: 'Selective' means that at least 10% of schools in the LA select pupils according to an academic test. 'Fair-banding' means that at least 20% of schools in the LA select pupils according to an academic test to have a equal balance of academic ability in the intake. An LA is classified as 'rural' if at least one third of schools are classified as 'rural'. An LA is classified as 'town' if the modal school is classified as 'urban city and town' and less than one third of schools as 'rural'. An LA is classified as 'city' if the modal school is classified as 'urban conurbation' and less than one third of schools as 'rural'. 'School density' is the number of schools per km^2 (/1,000,000).

Table 5.4: Correlates with the difference in dissimilarity indices under alternative counterfactuals in 136 Local authorities

	Preferences and current allocation			Nearest school and current allocation		
	FSM	KS2	Ethnicity	FSM	KS2	Ethnicity
D. Allocation	-0.125***	-0.100**	-0.048*	-0.201*	-0.792***	-0.131*
	(0.030)	(0.030)	(0.023)	(0.080)	(0.055)	(0.052)
Fair-banding	0.020*	0.017	-0.013	-0.032	-0.038*	-0.035
	(0.008)	(0.009)	(0.012)	(0.022)	(0.017)	(0.027)
Selective	0.006	0.030***	0.006	-0.011	-0.035*	0.002
	(0.006)	(0.008)	(0.008)	(0.015)	(0.014)	(0.019)
More than 3 choices	-0.002	0.011*	0.002	-0.000	-0.002	-0.003
	(0.005)	(0.005)	(0.007)	(0.012)	(0.010)	(0.015)
Urban city and town	0.003	0.007	0.006	0.008	0.016	-0.002
	(0.006)	(0.006)	(0.008)	(0.015)	(0.011)	(0.017)
Urban conurbation	0.008	0.005	0.013	0.005	0.019	0.022
	(0.007)	(0.007)	(0.010)	(0.018)	(0.013)	(0.021)
London	0.002	-0.004	-0.029**	-0.017	-0.033*	-0.070**
	(0.007)	(0.008)	(0.011)	(0.018)	(0.015)	(0.023)
School density	-0.030	-0.032	0.055	-0.010	-0.010	-0.081
	(0.021)	(0.024)	(0.031)	(0.055)	(0.043)	(0.069)
% FSM	-0.000	-0.000	-0.001	-0.000	0.000	-0.001
	(0.000)	(0.000)	(0.000)	(0.001)	(0.000)	(0.001)
% White	-0.015	0.027	0.008	0.020	0.020	0.143**
	(0.012)	(0.014)	(0.020)	(0.033)	(0.025)	(0.044)
% Low KS2	0.065	0.040	0.005	-0.081	-0.139	0.244*
	(0.037)	(0.050)	(0.055)	(0.098)	(0.091)	(0.122)
% Religious schools	0.022	-0.015	-0.031	0.043	-0.021	0.028
	(0.017)	(0.019)	(0.024)	(0.045)	(0.034)	(0.054)
N	136	136	136	136	136	136

Source: Author's calculations from national preference data and the National Pupil Database. School characteristics from publicly available information provided by the Department for Education and collection of secondary school admissions criteria.
Note: 'Selective' means that at least 10% of schools in the LA select pupils according to an academic test. 'Fair-banding' means that at least 20% of schools in the LA select pupils according to an academic test to have a equal balance of academic ability in the intake. An LA is classified as 'rural' if at least one third of schools are classified as 'rural'. An LA is classified as 'town' if the modal school is classified as 'urban city and town' and less than one third of schools as 'rural'. An LA is classified as 'city' if the modal school is classified as 'urban conurbation' and less than one third of schools as 'rural'. 'School density' is the number of schools per km^2 (/1,000,000).

5.10 Figures

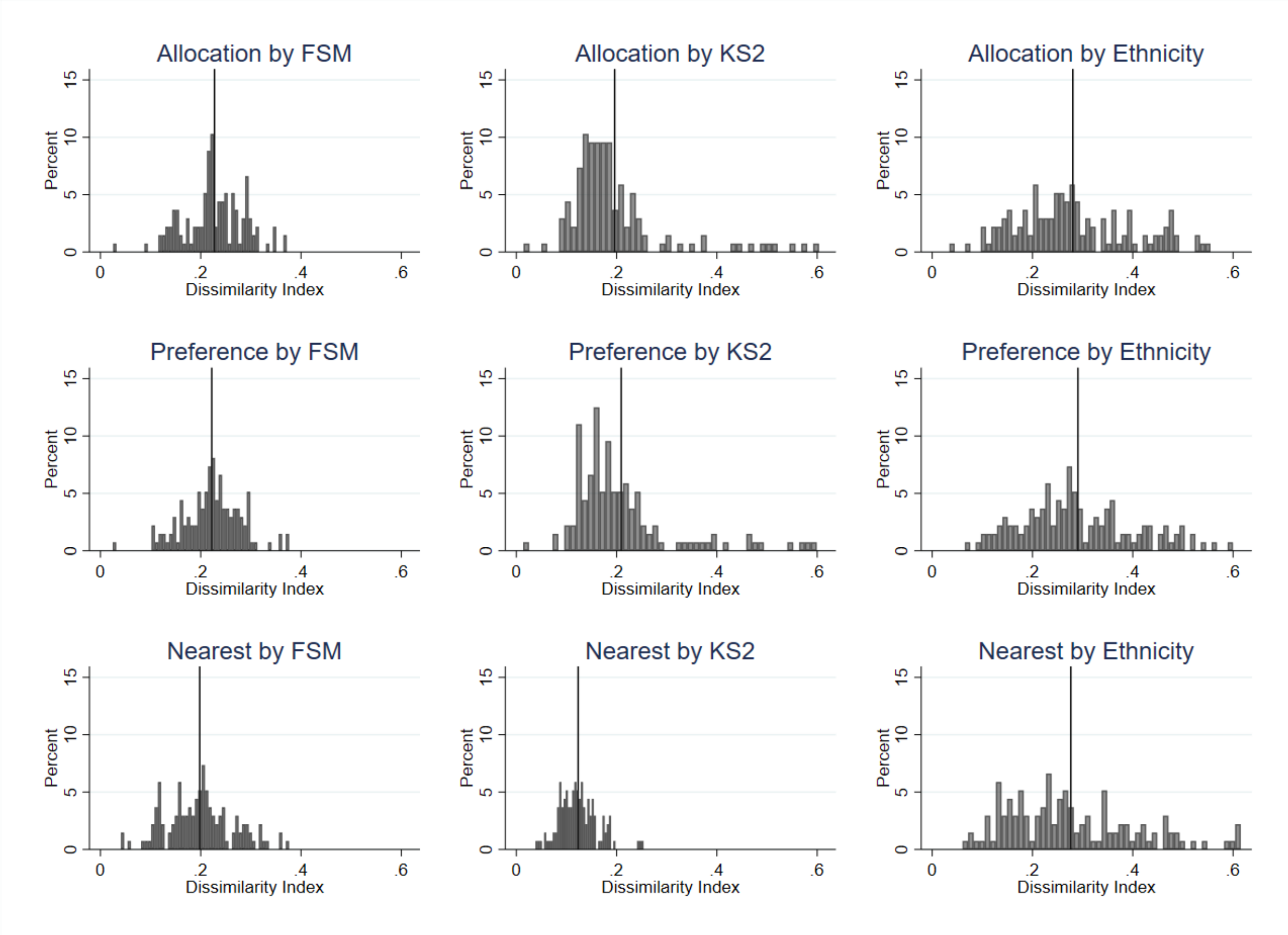


Figure 5.1: Distribution of segregation indices for 136 LAs in England

Source: Author's calculations from national preference data and the National Pupil Database.
Note: The first column shows segregation index (D) by Free School Meals status; the second column by Key Stage 2 prior attainment (top 20% vs bottom 80%); and the third column by White/non-White. The first row shows the observed allocation. The second row shows the counterfactual allocation to first choice school. The third row shows the counterfactual allocation of students to their nearest school. The vertical line shows the mean of segregation in each panel.

6 School Choice and Neighbourhood Sorting

Preface: *This chapter is co-authored with Hélène Turon, and is the result of many years of discussions and iterations between us. I took the lead on the coding, with Hélène quality checking and improving the code in the later stages. I am responsible for all data cleaning and coding. I took the lead on writing, except for the modelling section.*

6.1 Introduction

Wherever location partly determines school access, richer neighbourhoods tend to have 'better' schools, as defined by their pupils' educational attainment. This is both because richer pupils have higher attainment, on average, and because richer households can selectively sort into neighbourhoods close to 'good' schools. This cyclical process might have long-lasting implications for social mobility and inequality, given the importance of education for individual and societal outcomes. Under a system of school choice, households are able to apply to other schools than their local one, which partly breaks the deterministic school assignment given location. Indeed, one objective of providing school choice is to widen access to good schools to pupils from a greater range of socio-economic backgrounds.[37]

The school choice process typically has three steps. First, schools set admissions priorities for if they are over-subscribed: how they rank pupils for admission. This is typically based on geography (a school zone/catchment area or by distance to the school), ability, or random assignment. Second, parents give their school choices in a ranked ordered list. Third, a central authority uses an allocation mechanism (for example Boston or Deferred Acceptance) to allocate pupils to schools, given the admissions priorities for over-subscribed schools. Previous research has focused on the second and third steps. The insight that over-subscribed schools still need to ration places even under school choice is largely neglected in theoretical and empirical research, however, although it has a large effect (Calsamiglia, Martínez-Mora, and Miralles, 2020).

Our paper answers the question of how schools' admissions priorities affect the eventual allocation of pupils to schools *and* households across neighbourhoods. Neighbourhoods are affected by whether the admissions priority is based on geography or not. In the latter case, households may no longer sort to be close (and gain access) to the 'better' schools. Recognising the potential for endogenous sorting of households across neighbourhoods is important, because failure to account for possible residential resorting 'may lead to an incomplete understanding about the distributional consequences of school choice' (Avery and Pathak, 2021) that is evident in the counterfactual simulations in Agostinelli, Luflade, and Martellini, 2021.[38] We quantify the rela-

[37]Among the multiple objectives of school choice, one objective is to encourage competition among schools and thereby increase the overall quality of schooling services. Evidence for this channel of impact is however mixed (see for example Hoxby, 2000 and Rothstein, 2007), and is not a mechanism we will examine in this paper.

[38]Avery and Pathak, 2021 model the residential mobility across districts with and without school choice programmes. The central finding is that school choice (with pupils assigned by lottery) narrows the gap between

tionship between the type of school priority (geographic preference/school zone versus random assignment/lottery) and the social mix within schools and within neighbourhoods, defined as the mix between households of different ages, different income and different family size.

The innovative feature of our approach is that we build a dynamic structural model of household choices across different life-stages, allowing for heterogeneity in family types along completed family size (assuming perfect foresight in fertility). The dynamic components of our framework come from the sibling priority rule applied by schools, whereby the family's younger sibling is guaranteed a place in the school that the older sibling attends, and from the existence of moving costs. Because households are forward-looking and there is a cost of moving across neighbourhoods, households' residential choices exhibit some persistence. That is, households that expect to become parents may choose to move close to a good school before they have children. All households care for local amenities, and parents value school quality and distance to school when there are children travelling to school. Having two rather than one child increases the value placed on school quality. Residential location matters most in the life-stage when parents apply to a secondary school (under a geographic preference priority system) and in the life-stage when the household's children travel to school. This model serves two important purposes. First, we use the model to simulate the effect of changing admissions priorities from geographic preference to lottery on school and neighbourhood composition. Second, we use the model to illustrate mechanisms at work in school and neighbourhood formation, and provide a nuanced interpretation to existing reduced form empirical estimates.

We calibrate our model to data from two neighbourhoods in the city of Bristol, England. One more affluent area has the Above Average school, while one less affluent area has the Below Average school. We find that our model replicates patterns of sorting in schools and neighbourhood across family types and life-stages well. In our setting, geographical school priorities contribute to segregation by household type (age and completed fertility, in addition to income) at the neighbourhood as well as school level. These factors have hitherto been unexplored and not considered, but it is reasonable to assume that segregation by household type is as problematic as segregation by income for societal outcomes. For example, under the current system with geographic preference, the neighbourhood with the Above Average school has 9% more households with dependent children, particularly with two or more children (28%) and at school choice (88%) or secondary school (38%) age. Property prices in the neighbourhood with the Above Average school are 20% higher, which is due to both school and neighbourhood factors. The rate of pupils eligible for free school meals (an indicator for poverty) is 74% lower in the neighbourhood with the Above Average school. Although these are not clean causal estimates,

the highest and lowest quality schools, and therefore equilibrium rents across districts, then high income types *low income types* move away to a 'no choice' district. This is because in the absence of school choice, households can sort into neighbourhoods (and prices) which perfectly match their type. In that model, 'low types' prefer the availability of a 'low-cost, low-quality school' as they have low willingness to pay for school quality, while 'high types' value the provision of a 'high-cost, high-quality public school' that they can gain access to through the housing market.

the pattern in the data is consistent with the model's predictions that the geographic preference system is associated with segregation across neighbourhoods as well as schools.

We find that the simulated effect of an alternative admissions priority by lottery reduces segregation by income at the school level. Schools become more integrated. Neighbourhoods also become more integrated by household age and completed fertility. The effect on income segregation at the neighbourhood level is weaker, however, as neighbourhoods are completely sorted by income in response to preferences for neighbourhood amenities (that are positively correlated with school quality). These results illustrate the complex interaction between neighbourhood and school choices, and the possibly unintended consequences of school choice policies on societal outcomes.

We also use our model to illustrate important channels that have so far been absent in empirical studies of school choice and related property price effects. For example, there is a large empirical literature using boundary discontinuity design, beginning with Black, 1999, on parents' willingness to pay for local school quality. These studies typically find that households are willing to pay a premium of around 3-4% for access to a one standard deviation increase in school average test scores. (See Gibbons and Machin, 2008, Black and Machin, 2011 and Nguyen-Hoang and Yinger, 2011 for detailed summaries.) Our model shows that non-parents and older households dampen the market equilibrium property prices around highly performing schools. As such, our model shows that the reduced form estimates should be interpreted as a weighted average of demand from parents and non-parents, rather than parents' valuation of school quality (as is typically done). Our model also makes clear that differences in empirical estimates for the relationship between school quality and property prices across contexts are not necessarily due to differences in parents' valuation of school quality, but could be due to differences in the market composition.

Illustrating another unexplored mechanism, our model also shows that households without dependent children also dampen the estimated effect of school choice reforms, for example those studied in previous empirical work (Reback, 2005, Bogart and Cromwell, 2000, Ries and Somerville, 2010, Lee, 2015, Machin and Salvanes, 2016). This is because households with child(ren) in the school choice phase are the only households that are directly affected by the reforms, as they can be allocated to a school further afield. These households must therefore calculate whether life-time utility is higher by incurring moving costs but lowering transport costs to school, or vice versa. All other households may be affected by changes in the equilibrium rents or the expectation of allocation to particular schools. The estimated effect of a school choice reform on property prices is therefore affected by the market composition, travel costs and moving costs.

Our first main contribution is to focus on the effect of school priorities rather than the allocation mechanism on equilibrium outcomes for the school and neighbourhood. We complement the only existing model provided by Calsamiglia, Martínez-Mora, and Miralles, 2020, who study segregation by income with a focus on equilibrium sorting under alternative allocation mechanisms (Boston and Deferred Acceptance) rather than school priorities. The baseline model of Calsamiglia, Martínez-Mora, and Miralles, 2020 excludes residential mobility.[39] In extensions, the authors discuss the role of transport costs and geographical priorities. Strict geographical admissions priorities or prohibitively high transport costs across districts leads to Perfect Assortative Matching. In relation, our dynamic model additionally includes lifecycle considerations such as moving costs and different household types. Calsamiglia and Miralles, 2020 assess the role of geographical school admissions priorities on preventing school choice (specifically 'access to better schools') more explicitly than Calsamiglia, Martínez-Mora, and Miralles, 2020, but without the addition of endogenous residential mobility. Calsamiglia and Miralles, 2020 conclude that the choice of allocation mechanism is marginal compared to the role of school zones/catchment areas, concluding that 'Future work should incorporate the design of these priorities as a fundamental part of the mechanism design'.

Our second main contribution is to consider the welfare effects of the school choice environment for households that never have dependent children, or whose dependent children have left home.[40] We find that welfare is unambiguously lower for non-parents under a geographical admissions system compared to a lottery system. These externalities are important to consider, as households without dependent children are a large share of the market (73% of households in our setting). Our dynamic model allows us to consider the effect for households across the lifecycle. Particularly under a system with geographic preferences, non-parents and older households that live in the area with the high-performing school pay higher rents, which lowers their utility. Some households are also displaced to a neighbourhood with lower quality amenities, which also lowers their utility. This suggests that policy-makers should consider the welfare effects across the population when designing school choice systems, not just the welfare of parents and children. Despite a rich body of theoretical research modelling the joint decisions of school and residential location (see for example Nechyba, 2000, Epple and Romano, 2003, Ferreyra, 2007), non-parents have only been incorporated by two (Caetano, 2019, Agostinelli, Luflade, and Martellini, 2021). We complement Caetano, 2019 by focusing on the welfare effects for households without dependent children, rather than using these households for identification. We differ from Agostinelli, Luflade, and Martellini, 2021 in classifying households by their completed fertility, rather than current presence of children, that allows us to model dynamic considerations.

Finally, our rich model includes transport costs (as in Epple and Romano, 2003, Calsamiglia,

[39]The Calsamiglia, Martínez-Mora, and Miralles, 2020 model illustrates that if advantaged families are willing to take greater risks (due the private school outside option) then the Boston Mechanism leads to segregation between student types even with identical preferences for schools. In contrast, the Deferred Acceptance allocation mechanism does not induce such segregation, as there is no benefit to households playing risky school choice strategies under this truth-revealing mechanism.

[40]Agostinelli, Luflade, and Martellini, 2021 include households that are *currently* not parents in their model to study the welfare effects and reduction in policy effectiveness from households that currently gain no flow utility from school quality.

Martínez-Mora, and Miralles, 2020 and Agostinelli, Luflade, and Martellini, 2021) but no other theoretical models of school and neighbourhood choice).[41] We show that transport costs affect residential choices in addition to school choices (see also Agostinelli, Luflade, and Martellini, 2021). This suggests that policy-makers should consider the specific context for households' choices when designing school choice systems, in particular the quality and cost of the local transport system.

Possible extensions to our current model include a private school outside option, endogenous school quality and heterogeneous preferences. Appendix A5.2 sketches these extensions.

Section 6.2 presents our model. Section 6.3 illustrates the mechanisms of the model, at the same time presenting the intuition of the model. This could be a substitute for the formal details of the model in section 6.2 for some readers. Section 6.4 then presents some stylised facts of our chosen neighbourhoods, which clearly show the model's predictions. Section 6.5 shows the results of our calibration and the simulation of an alternative school admissions priorities which breaks the link between residential choice and school access. Section 6.6 concludes.

6.2 Model of dynamic neighbourhood choice

This section gives a formal description of our model of neighbourhood and school choice and its solution. Section 6.3 describes the intuition of the model discursively and illustrates key mechanisms that shape equilibrium outcomes.

6.2.1 Environment

Our aim is to set up a very simple environment in order to show the consequences of school priority rules on the allocation of school places and residence dwellings across households of different ages, income, and family status – i.e. parents vs. non-parents, one-child families vs two-children families. This will allow us to revisit the interpretation of the rent difference between neighbourhoods hosting schools of different quality as well as our understanding of the sensitivity of housing prices to school priority rules.

We assume our environment to have reached a steady state where all variables of interest are fixed and no changes, except ageing and childbirth, are being anticipated by households. These lifecycle events happen in a deterministic manner.

We consider a universe of only two neighbourhoods n, denoted H and L, offering different levels of amenities, $a_H > a_L$. The housing market consists in N dwellings overall, which

[41] Epple and Romano, 2003 model the introduction of district-wide open enrolment, from traditional neighbourhood enrolment. In the equilibrium with neighbourhood enrolment, the model predicts that income stratification implies school quality stratification because the access to neighbourhoods with better peer groups is rationed by higher housing prices. In the equilibrium with open enrolment and no transport costs, the model predicts equal school qualities and equal property prices across neighbourhoods. Finally, in the equilibrium with open enrolment and transport costs, only higher income households are able to choose schools, while lower income households live and attend the school in the poorer neighbourhood.

are available to rent. These dwellings are identical within each neighbourhood and the rent in neighbourhood H is denoted r_H and in neighbourhood L is r_L. The number of dwellings in neighbourhood H is N_H and is fixed exogenously. There is one secondary school in each neighbourhood and they differ in quality: the school in the H neighbourhood is rated by all as above average (AA) while the school in the L neighbourhood is below average (BA).[42] School quality is thus correlated with neighbourhood 'quality' as measured by amenities.

The total number of school places P equals the size of the global children cohort and each school has a fixed number of places, P_{AA} and P_{BA} respectively. Since all households value the AA school more than the BA school, some rationing will take place - even though, as we will see below, distance to school and higher rents will also play a role in curtailing demand for the AA school. We will examine two possible allocation mechanisms to deal with the excess demand: priority to closer applicants ('geographic preference') with a random draw among applicants from the same neighbourhood, or a random draw ('lottery') among all applicants (regardless of place of residence). We also assume that schools operate a 'siblings priority' rule whereby younger siblings are guaranteed a place in the school of the elder child. This will affect the decisions of households with more than one child. The probabilities of being granted a place for the eldest child in the AA school are denoted π_H and (respectively π_L) for households residing in the neighbourhood H (respectively L).

There are N households living across the two neighbourhoods, which we follow over the lifecycle. These households are unitary and forward-looking. They can be of three 'fertility types' f, i.e. their completed fertility can be 0, 1 or 2 children. Households know their type throughout the lifecycle and have no uncertainty over the timing (and number) of births. We rule out divorce and premature death. Households also differ by income y. For now, income is assumed constant over the lifecycle.[43] Note that both these dimensions of household heterogeneity are known to the household from the beginning of the lifecycle and exogenous, so neither fertility nor income respond to our policy experiments.

The lifecycle is decomposed into four stages T_t defined around the period that is key for our purposes, i.e. the time when households apply for a place in secondary school. This period is denoted T_1. The preceding period, T_0 starts when households enter adult life and need to make a choice of residential location and ends at the age where they need to apply for a place in a secondary school for their eldest child (if they have any). All households with children attend a public (non-private) secondary school.[44] The third period T_2 relates to the years when the household has children going to a secondary school and the last period T_3 is the remaining lifetime

[42]Measures of school quality are discussed in section 6.4.

[43]In practice, non-parents and parents have different income distributions to reflect the higher disposable income for non-parents. The shape of these income distributions are taken from a UK panel study, Understanding Society, using households where the main respondent is between 30 and 50, and 'parent' defined by whether there is a dependent child currently in the household.

[44]Appendix A5.2 sketches an extension of the model where households have the option to send their child(ren) to a private school.

of the household after the children have finished secondary school. In our stylised representation of the timing of these events, we assume that the duration of each period is 15, 2, 8 and 15 years, with adult life starting at the age of 25.

Preferences are assumed to be homogeneous across households. They value consumption, school quality, neighbourhood amenities and proximity to school in a separable manner. In utility terms, the commute to school across neighbourhoods is costly but the commute to work is the same from both neighbourhoods. When moving across neighbourhoods, households incur a one-off utility cost, m_t, which can vary over the lifecycle. For simplicity, we assume that households of different ages and sizes rent similar dwellings and all pay the same rent, i.e. r_H in neighbourhood H and r_L in neighbourhood L.

In this framework, households only make one choice per life-stage: the choice of neighbourhood in which to reside. Indeed, we rule out saving and borrowing. All agents have perfect information but are not able to coordinate. There are externalities through congestion on the housing market through the rent and on the market for school places through the probabilities of admission to the AA school, π_H and π_L. Endogenous outcomes of our model are these two quantities, as well as the composition of each neighbourhood in terms of income distribution, age distribution and (completed) family size distribution.[45]

Both the housing market and the market for school places clear at the global level by definition, since there are N households and N dwellings in total, and P children in each cohort for P school places across the two schools.

6.2.2 Model solution

We provide here the formal specification of the model ingredients described above, as well as the model solution. The time period is one year and the flow utility for a household of fertility type f, income y and in life-stage t has the following expression when it chooses to reside in neighbourhood n and their child(ren), if any, attend secondary school s:[46]

$$\begin{aligned} U(f,t,y,s,n_t) &= \frac{\left[y - r_H \cdot \mathbb{1}_{(n_t=1)} - r_L \cdot \mathbb{1}_{(n_t=0)}\right]^{(1-\gamma)}}{(1-\gamma)} + \alpha \cdot n_t \\ &\quad + \mathbb{1}_{(k>0)} \cdot \left[s \cdot (\theta + \eta \cdot \mathbb{1}_{(k=2)}) - d \cdot \mathbb{1}_{(s \neq n_t)}\right], \qquad (4) \end{aligned}$$

This household currently has $k = k(f,t)$ children of secondary school age. If k is positive it experiences a disutility d if the children commute to a secondary school in a different neighbourhood and a utility θ (respectively $\theta + \eta$) from having one (respectively two) child(ren) attending the

[45] To be precise, income, age and fertility are all exogenous processes but the selection of different groups across the two neighbourhoods is endogenous.

[46] The neighbourhood and school variables are defined as follows: $n = 1$ if the household resides in the H neighbourhood and 0 otherwise; $s = 1$ if the child(ren) in the household attend the AA school and 0 otherwise. Note that s does not vary across life-stages since we rule out the event of children moving schools.

above average secondary school relatively to the below average one.[47] It also derives a utility α from the amenities in the high-quality neighbourhood relative to the low-quality neighbourhood and utility from consumption which equals all income minus expenditure on rent, which is neighbourhood specific. Finally, the household incurs a cost m_t if it moves across neighbourhoods between life-stage $t-1$ and life-stage t.

Since our environment is in steady-state, the fraction of the population of households in the life-stage t and of fertility type f is constant and equal to $\tau_t \cdot \phi_f$, where τ_t is the fraction of total adult life spent in phase t and ϕ_f is the fraction of households of fertility type f.[48]

Looking at the specification of the utility function (4), we see that the only term in which neighbourhood choice interacts with income is in the utility of consumption. Given the strictly diminishing marginal utility of consumption in (4), there is a unique income threshold $\tilde{y}(f,t,r)$ above which households of type f and in life-stage t will choose neighbourhood H. For the same reason, this threshold will be increasing in r_H. We can thus formalise the market clearing of housing in neighbourhood H as:[49]

$$N_H = \sum_{f=0}^{2} \sum_{t=0}^{3} \tau_t \phi_f \overline{F}\left(\tilde{y}(f,t,r)\right) \tag{5}$$

where we denote $F(.)$ the c.d.f. of the distribution of income y across households, which in this exposition we assume is the same across household types.[50] Since the right-hand side decreases with r_H, this defines a unique rent level $\tilde{r_H}$ at which the housing market clears in both neighbourhoods. Note that since the set of the two neighbourhoods comprises as many properties as households, when one market clears, the other does too.

The size of a children cohort is $N\tau_1(\phi_1 + 2\phi_2)$. Of these, $N\tau_1\phi_2$ are younger siblings, who either have a place reserved in the AA school because their older sibling was awarded one, or choose not to apply to the AA school because their older sibling already attends the BA school.[51]

The total number of applicants to the AA school *without a guaranteed place* is hence A, of which A_H (respectively A_L) reside in neighbourhood H (respectively L), where these numbers

[47] Families with two children always send their children to the same school. We justify this assumption by conjecturing that commuting to two different schools incurs a cost that is larger than the benefit of having the youngest child attending the above average school when the youngest attends the below average school.

[48] Note that we have $\sum_{t=0}^{3}\sum_{f=0}^{2}\tau_t\phi_f = 1$.

[49] In fact, these thresholds also depend on the neighbourhood in which the family resided in the previous period since there is a moving cost, and for families with two children in life-stage T_2, the threshold will depend on whether the elder child has been given a place in school AA or not. For ease of exposition, we summarise the demand of properties in neighbourhood H with a single threshold per family type, but we relax this in the full model derivation below.

[50] $\bar{F} = 1 - F$

[51] We have ruled out households commuting to two different schools by assumption, which is reasonable as there are likely to be large monetary (uniform/travel) and non-monetary (administration/travel time) costs of siblings attending different schools. Our assumption is that these costs outweigh the utility of one child attending the AA school when another child attends the BA school.

have the following expressions:

$$A = N\tau_1(\phi_1 + \phi_2) \quad (6)$$

$$A_H = N\tau_1 \left[\phi_1 \overline{F}\left(\tilde{y}(1,1,r)\right) + \phi_2 \overline{F}\left(\tilde{y}(2,1,r)\right)\right] \quad (7)$$

$$A_L = N\tau_1 \left[\phi_1 F\left(\tilde{y}(1,1,r)\right) + \phi_2 F\left(\tilde{y}(2,1,r)\right)\right] \quad (8)$$

The total number of school places P per cohort across the two schools AA and BA is equal to the size of the children cohort. The school AA has P_{AA} places and the school BA has P_{BA} places. These numbers are fixed by policy and do not respond to excess demand in our model.

$$P = P_{AA} + P_{BA} = N\tau_1(\phi_1 + 2\phi_2) \quad (9)$$

The number of households of type $f = 2$ who are successful in their application to the AA school with their eldest child and thus have a place reserved for their second child is denoted S and depends on these families' neighbourhood choices:

$$S = N\tau_1\phi_2 \left[\overline{F}\left(\tilde{y}(2,1,r)\right).\pi_H + F\left(\tilde{y}(2,1,r)\right).\pi_L\right] \quad (10)$$

The number of places available in the AA school, i.e. not reserved for younger siblings of existing pupils, is $P_{AA} - S$. Thus, in case of lottery allocation, the probability of being granted a place in the above average school for households without a sibling priority does not depend on the place of residence:

$$\pi_H = \pi_L = \frac{P_{AA} - S}{A} \quad (11)$$

In the case of allocation with geographic preferences, these probabilities become:

$$\pi_H = min\left(1, \frac{P_{AA} - S}{A_H}\right) \quad (12)$$

$$\pi_L = \frac{P_{AA} - S - \pi_H.A_H}{A_L} \quad (13)$$

The two dynamic components of our framework are the following. First, there is a cost m of moving across neighbourhoods. Second, for families of fertility type 2, the school for the younger child is determined by the school place granted to the older child, which may depend on neighbourhood choice.

The dynamic choice of a household of type f and income y over the life cycle is a sequence $\{n_t\}_{t=0..3}$ of neighbourhood choices that maximises:

$$\sum_{z=0}^{T-1} \beta^z \cdot EU(n_t) - \sum_{t=1}^{3} m_t \mathbb{1}_{(n_t \neq n_{t-1})}, \quad (14)$$

where β is the yearly discount rate of households, T is the total number of years in an adult life and z refers to the current year. Note that EU does not depend on z as it is constant within life-stages and that m_t is only occurred once in a life-stage if the household moves neighbourhood between life-stage $t-1$ and life-stage t. Consistent with the specification defined above, EU also depends on family type f, income y, and school attended by the children if in life-stage 2. The only stochastic component in the household environment is the allocation of school places between life-stages 1 and 2.

This dynamic choice problem will be solved by backward iteration:

$$V_3(f,y,n_2) = \max_{n_3}\left[\left(\sum_{z=0}^{\tau_3-1}\beta^z\right)U(f,3,y,0,n_3) - m_3\mathbb{1}_{(n_3\neq n_2)}\right] \tag{15}$$

$$V_2(f,y,s,n_1) = \max_{n_2}\left[\left(\sum_{z=0}^{\tau_2-1}\beta^z\right)U(f,2,y,s,n_2) - m_2\mathbb{1}_{(n_2\neq n_1)} + \beta^{\tau_2}V_3(f,y,n_2)\right] \tag{16}$$

$$V_1(f,y,n_0) = \max_{n_1}\left[\left(\sum_{z=0}^{\tau_1-1}\beta^z\right)U(f,1,y,n_1) - m_1\mathbb{1}_{(n_1\neq n_0)} + \beta^{\tau_1}E_s\{V_2(f,y,s,n_1)|n_1\}\right] \tag{17}$$

$$V_0(f,y) = \max_{n_0}\left[\left(\sum_{z=0}^{\tau_0-1}\beta^z\right)U(f,0,y,n_0) + \beta^{\tau_0}V_1(f,y,n_0)\right] \tag{18}$$

$$\tag{19}$$

where the choice in life-stage 1 is the only choice under uncertainty that the household has to make and its expected continuation value is:

$$E_s\{V_2(f,y,s,n_1)|n_1\} = \pi_{n_1}V_2(f,y,AA,n_1) + (1-\pi_{n_1})V_2(f,y,BA,n_1) \tag{20}$$

The model solution consists of a set of equilibrium rents and admission probabilities from each neighbourhood such that, at a given set of preference parameters, demand equals supply in the housing markets and in school places. At this stage, we find a local minimum. Future work will ensure that we find a global minimum.

6.3 Mechanisms

This section illustrates the mechanisms of the model and shows how it deepens understanding of existing empirical reduced form results. This section is intentionally discursive to clearly describe the channels through which residential and school choices affect equilibrium outcomes, such as the rents across neighbourhoods and segregation in neighbourhood and schools.

This section is intended to stand alone, but key ideas from section 6.2 are useful. These are that: (i) there are different household types, some of which gain utility from schooling and neighbourhood amenities (parents with younger children) while others gain utility only from neighbourhood amenities (those without children or with post school age children); (ii) each household type has a rent threshold above which they choose to live in the neighbourhood with high rather low quality than amenities (H rather than L); (iii) parents with school aged child(ren) gain utility from attending the Above Average (AA) school compared to the Below Average (BA) school; (iv) households make optimal decisions taking into account their whole lifecycle, incorporating the expected probability of admission to the AA school and moving costs across periods, which makes their problem dynamic.

First, we explore the body of literature relating property prices to local school quality, specifically the interpretation that parents' valuation of school quality can be inferred from the causal increases in property prices around popular schools. Second, we illustrate some underlying mechanisms that affect the new equilibrium when geographic preference/school zones are replaced with freer school choice, taking Machin and Salvanes, 2016 as a case study. Third, we explore how the cost of transport to school interacts with the school choice environment to shape equilibrium outcomes.

6.3.1 School quality and property prices

The introduction described the large empirical literature on the willingness to pay for local school quality. Since Black, 1999, boundary discontinuity design has typically been used to estimate the causal effect of local school quality on property prices, where the identifying assumption is that neighbourhood attributes are continuous at the boundary while school quality jumps discontinuously. Across studies in this literature, households are typically willing to pay a premium of around 3-4% for access to a one standard deviation increase in school average test scores, although there is variation across contexts, from 1.4% in Paris to 8% in Oslo.[52]

This reduced form effect has been interpreted as parents' valuation of school quality. For example, Black, 1999 states that the 'value that parents place on school quality' is given by 'calculating how much more people pay for houses located in areas with better schools'. Our model illustrates that the reduced form effect on price is in fact a weighted average of the demand effect from parents and non-parents at different life-stages. The resulting premium for property prices around AA schools is in fact a lower bound on parents' underlying valuation of local school quality, which is dampened according to the composition of the market, namely the share of non-parent and older households.

To illustrate this in more detail, Figure 6.1 shows the equilibrium outcomes around the AA school as the share of parents in the population increases from 0.1 to 0.9. The number of households is fixed, while the number of school places adjusts to meet the number of pupils, keeping the share of pupils in the AA and BA school constant. The blue dots show the equilibrium

[52] The existing empirical estimates (in order of magnitude) are: Fack and Grenet, 2010 (Paris): 1.4% for school test scores, 2.1-2.4% for school peer-group; Bayer, Ferreira, and McMillan, 2007 (San Francisco Bay): 1.8%; Black, 1999 (Massachusetts): 2.1%; Harjunen, Kortelainen, and Saarimaa, 2018 (Helsinki): 3.3%; Davidoff and Leigh, 2008 (Australia): 3.5%; Gibbons, Machin, and Silva, 2013 (England): 3.5%.

when parents' utility from the AA school is relatively low (0.06) compared to utility from the amenities in the high quality neighbourhood (0.056). The orange dots show the equilibrium when parents' utility from the AA school is relatively high (0.082).

The first panel shows that the equilibrium rent in the neighbourhood with the AA school increases as the share of parents increases. This is because when there is a high proportion of parents, who value both the amenities in H and the AA school, there is increased pressure upwards on the rent in H. At the same time, there is a lower share of non-parents who dampen the rent increase. This is because non-parents only value the amenities in H (they gain no flow utility from proximity to the AA school) and therefore choose to move out of neighbourhood H when the rent rises above their threshold. This dampening effect is weaker when the proportion of non-parents is lower.

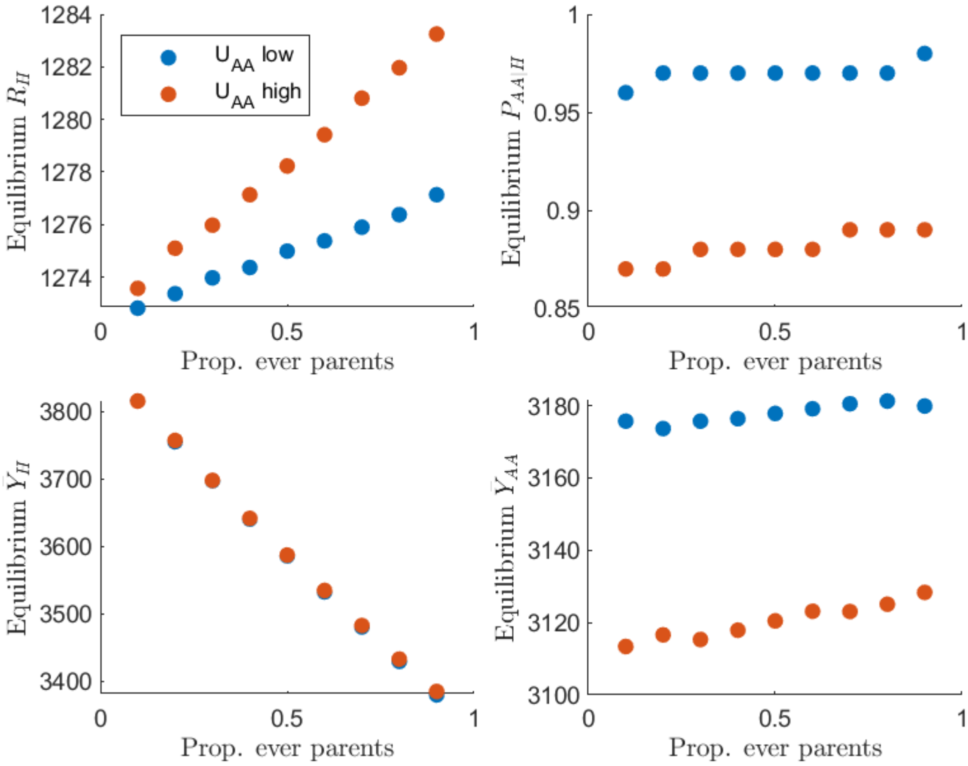
This first panel illustrates that the rent premium depends on the population composition in addition to parents' utility from schools. The variation according to the share of parents is most marked when the utility from the AA school is higher (orange dots). This means that the bias due to population composition when interpreting reduced form causal effects as parents' valuation of school quality is larger in contexts when parents' utility from school quality is higher.

The second panel on the first row shows the probability of admission to the AA school from the high quality neighbourhood. As the proportion of parents increases (and the rent increases), a smaller *share* of parents choose to live in H, which increases the probability of admission to the AA school from within the high quality neighbourhood. The probability of admission to the AA school is high when parents' utility from schooling is low, as fewer parents crowd into H. In contrast, more parents crowd into H than there are school places available when there is high utility from school quality, which lowers the probability of admission to the AA school. For these parents, the expected utility of living in H (with the *chance* of admission to the AA school) is higher than L (where there is no chance of admission).

Population composition has the opposite effect on average income in neighbourhoods (first panel on the second row) and schools (second panel on the second row). As the share of parents increases, mean income in the AA school *slightly increases* while mean income in H *decreases.* This may jar with first intuition for some readers. What explains the opposing effects in the housing and school markets? In essence, this is because the income level in the school is affected only by the income level of parents, while the income level in the neighbourhood is affected by the income levels of parents *and* non-parents.

In the absence of schooling in our stylised model, neighbourhoods would be perfectly sorted according to income. Neighbourhood H would be home to the highest income households due its high quality amenities. Introducing the school market decreases income segregation across neighbourhoods, as parents' threshold to live in H is lower than non-parents. Non-parents whose threshold is below the new equilibrium rent choose to live in L, while parents (with lower income

Figure 6.1: Equilibrium outcomes as the proportion of households that ever have children (parents) increases.



Note: 'Proportion of parents' is the proportion of households that ever have children. 'Equilibrium R_H' is the rent in the High quality neighbourhood in equilibrium. 'Equilibrium $P_{G|H}$' is the probability of admission to the Above Average school from the High quality neighbourhood in equilibrium. 'Equilibrium $\bar{Y}_H$' and 'Equilibrium $\bar{Y}_{AA}$' are the mean income in the High quality neighbourhood and Above Average school in equilibrium, respectively.

than these displaced non-parents) choose to live in H.[53]

A higher share of parents in the population depresses mean income in H. The opposite is the case for schools. As the share of parents increases, mean income in AA slightly increases. That is, when parents are a larger share of the population (and rents around the AA school are higher) then the AA school becomes increasingly selective by income.

The AA school becomes more selective by income when parents care less about school quality (the blue dots are higher than the orange dots) which contradicts first intuition. This is because when the utility from attending the AA school is low, only the richest parents choose to live in H (partly due to the high neighbourhood amenities) and so the school becomes exclusive. When the utility from schooling increases, parents' income threshold to live in H decreases, so a broader range of parents choose to live in H and gain (a positive probability of) access to the AA school.

Overall, this discussion has shown that the equilibrium effect of local school quality depends on the market composition, as non-parents dampen the price response to local school quality. The effect of population composition may therefore help to reconcile differences in reduced form estimates across contexts. For example, prices rise by 1.4% for a one-standard deviation in test scores in Paris (Fack and Grenet, 2010) compared to 3.5% in England (Gibbons, Machin, and Silva, 2013). Our model makes clear that these differences across contexts are not necessarily due to differences in parents' valuation of school quality. Differences in the reduced form causal effect could be driven by population composition, even if the valuation of school quality is the same.[54]

It is clear from this illustration that parents' valuation of local quality can not be inferred directly from the causal reduced form increase in price around AA schools. This intuition extends to papers which study the effect of school quality information (score cards and/or inspections) on parents' demand, inferred through local property prices. In Figure 6.1, the difference between the blue and orange dots can be interpreted as an information shock that changes perceptions about local school quality. The price response to this information shock is increasingly muted as the share of non-parents increases.

6.3.2 Change in the probability of admission

We now consider the underlying mechanisms that our model reveals when school admissions priorities change. This change could be from a geographic preference system - where pupils living in a particular district/zone, or closer to the school, have priority at the school - to any

[53]Lower income parents have a higher marginal utility of consumption than higher income non-parents, but choose to live in H because they gain utility from the school amenity.

[54]Other potential mechanisms that reconcile results across studies that are not included in our model are differences in the elasticity of housing supply (Cheshire and Sheppard, 2004, Harjunen, Kortelainen, and Saarimaa, 2018), the supply of 'family homes' (Ries and Somerville, 2010), the availability of private school outside options (Fack and Grenet, 2010), the availability of school quality information, stage of education, exact empirical specification and the time period.

other system that weakens the link between home and school attended. For example, Machin and Salvanes, 2016 study the house price effects of a reform in Olso county that moved from a geographic preference system to allocation by ability (where applicants were ordered by ability in line for their preferred schools).[55] Machin and Salvanes, 2016 find that the house price premium around 'good' schools decreased by at least 50% following the reform, which is interpreted as showing that 'parents substantially value better performing schools'.

As discussed in section 6.3.1, it is problematic to infer this price change as parents' valuation of access to local school quality. Primarily, this is because the overall price change is a weighted average of the demand of different groups in the population, for example older and younger households and households with and without children. Only a sub-set of these households receive the flow utility of priority at the local school, and so only a subset of households are directly affected by the reform.

Other households are affected indirectly by the reform. Some households may change residential location in response to changes in equilibrium prices across neighbourhoods. Parents of child(ren) who are unsuccessful in admission to their local high school under the ability allocation system can choose to pay higher transport costs or move closer to their child's allocated school. Parents of child(ren) or expectant parents who anticipate being unsuccessful in admission to their local high school may also change residential location in advance.

Machin and Salvanes, 2016 observe that the house price premium does not fall to zero after the Olso reforms. The authors suggest that the most likely explanation is that 'persistent neighborhood differences induced by the former catchment areas remain', such as unobserved differences in neighbourhood quality and neighbourhood peers from the pre-reform days. Our model is consistent with this explanation, as the presence of moving costs generates persistence in neighbourhood composition that the authors describe. This is despite our assumption that households gain no flow utility from neighbourhood composition - in our model moving costs alone generate the persistence.

Our model also offers further potential explanations for the remaining premium, however. The intuition for these explanations is that all households maximise their expected life-time utility. After the reform, households will calculate their expected life-time utility from moving or remaining in their current location. Moving costs mean that not all households will choose to move from their pre-reform location, even if they lose priority at their local school, or equilibrium rents increase. Households with child(ren) in the school choice phase are the only households that are directly affected by the reforms, as they can be allocated to a school further afield. These households must therefore calculate whether life-time utility is higher by incurring moving costs but lowering transport costs, or vice versa. Households with younger child(ren) or those

[55] We simulate the effect of a change from geographic preference to lottery admissions in section 6.5. Rehm and Filippova, 2008 study the re-introduction of school zones in New Zealand in 2000, after the country's experiment with school choice. Lee, 2015 studies the introduction of school districts in South Korea (away from an exam based entry system), while Reback, 2005 studies the introduction of school choice in Minnesota.

that expect to become parents must make a similar calculation, additionally accounting for the uncertainty about allocation to school. Uncertainty will be lower for households when there is a higher correlation between income and ability.

Appendix A5.3 shows that equilibrium rents in H fall most in response to the reform when moving costs are low and transport costs are low. The intuition is that when moving costs are low, it is more likely that households re-optimise location in response to the reforms, with parents moving away from H. When transport costs are low, it is less likely that households relocate to the neighbourhood where their child(ren) is assigned. Discussion of external validity of studies of this kind must therefore consider the moving costs and transport costs in the study setting, in addition to the population composition.

6.3.3 Changes in transport costs

Transport costs to schools vary across and within countries, depending on the quality and cost of the public transport system, provision of school buses, and density and location (relative to neighbourhoods) of schools. Differences in the cost of travel to school may affect households' school choices. For example, free bus travel provided (until recently) to pupils in London may expand the choice set of schools considered feasible to attend. Outside of London in England, free transport is only provided to pupils attending their 'nearest suitable school' and living a set distance from it.[56] Differences in the cost of travel to school may be one factor that contributes to differences in the patterns of school choice across England, where households in rural areas are much more likely to choose and attend their closest school (Burgess, Greaves, and Vignoles, 2019).

There are few empirical papers that directly assess the role of transport on school choice. Trajkovski, Zabel, and Schwartz, 2021 study the provision of free school buses in New York city, using cut-offs in eligibility by distance to school to provide causal estimates of the effect of free transport on choice of school. Trajkovski, Zabel, and Schwartz, 2021 conclude that, overall, 'bus eligibility plays a significant role in school choice decisions and increases the likelihood of attending a school'. To be more specific, the authors find that longer distance to school significantly deters choice, but that 'school bus eligibility increases the likelihood of choosing a school by 1.4–4 percentage points (or 12-30 percent)'. The policy conclusion is that 'expanding access to school buses or relaxing the bus eligibility rules would induce more students to attend a school other than their zoned school'.

Our model illustrates that in addition to affecting school choices, reduced transport costs also affect residential choices. When residential choices do not influence the probability of admission, reducing transport costs allows more households to choose to live in a lower-cost neighbourhood while attending an alternative school. In line with this, Boterman, 2021 recognises the combined

[56]These distance cut-offs are 2 miles from the school if a pupil is under 8 and 3 miles from the school if a pupil is 8 or older. Source: https://www.gov.uk/free-school-transport.

role of free school choice, high school density and low transport costs in Amsterdam (in this case commuting by bike) in facilitating residential integration in the city. The intuition is that free (non-geographical) school choice and low transport costs separate the residential choice and school choice, while geographical school choice and high transport costs bind them.

To illustrate this, Figure 6.2 shows equilibrium rents in H as transport costs change, under geographical preference (in blue) and non-geographical (lottery, in orange) school admissions priorities. Reducing transport costs decreases the equilibrium rent in H under non-catchment based admissions priorities (orange dots). This is because households no longer find it optimal to move close to their child(ren)'s school to reduce transport costs, which decreases demand in H. In contrast, under the geographic preference system the equilibrium rent in H increases slightly as transport costs decrease (blue dots). This is because, under the geographic preference system, households with child(ren) have a strong incentive to live in H to gain admission to the AA school. Demand in H remains high when transport costs are low, as those unsuccessful in admission (when the number of children in H is greater than supply of school places) are less likely to move to L to be closer to the BA school.

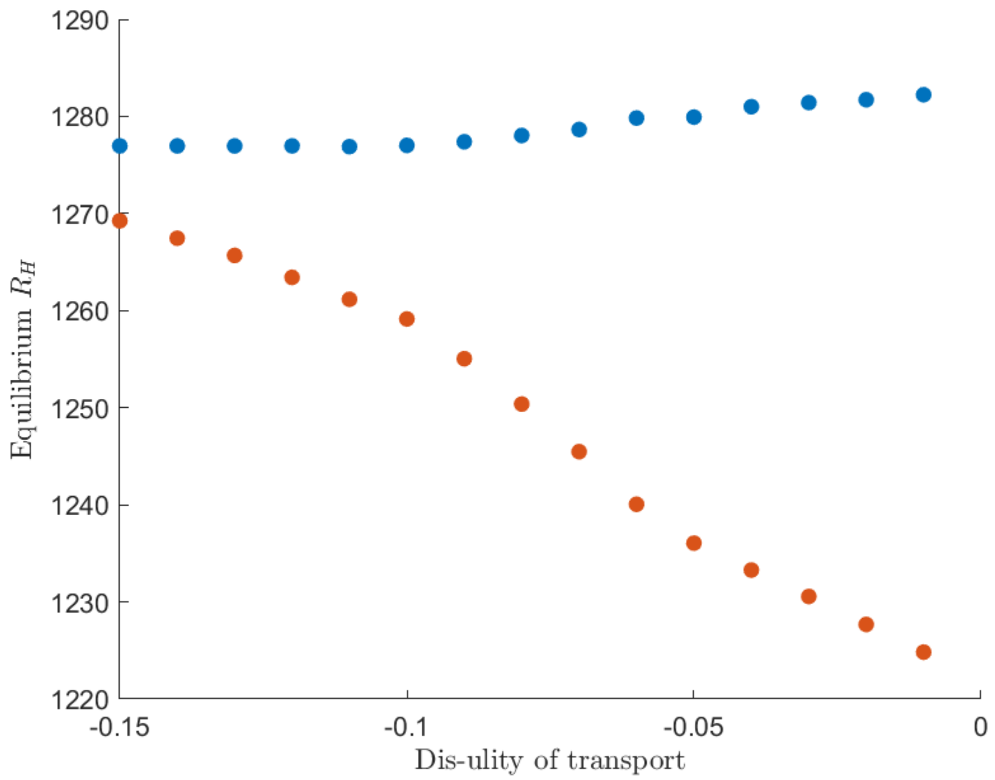
How do transport costs and the school choice environment affect neighbourhood composition? Whether the share of households that ever have children is evenly distributed across neighbourhoods strongly depends on the cost of transport and school admissions priorities. Under the lottery admissions priorities with low transport costs (most similar to the case described by Boterman, 2021 for Amsterdam), the share of households of different types in H is close to the population share. When transport costs are higher, then the proportion of households that ever have children increases in H. This is because households find it optimal to live close to their allocated school (and the AA school has a higher share of school places than the BA school).

Under the geographic preference system, the transport costs have a more marginal effect on neighbourhood composition. Decreasing travel costs slightly increase the proportion of households that ever have children in H, for the same reason discussed in relation to the equilibrium rents: under the geographic preference system, households with child(ren) have a strong incentive to live in H to gain admission to the AA school. If they are unsuccessful in admission (when the number of children in H is greater than supply of school places), transport costs affect whether unlucky households move to L to be closer to the BA school.

In contrast, the variation of income of households in H is largely unaffected by changing the cost of transport. This is because higher income households sort into H because of the high neighbourhood amenities, regardless of the school choice system in place.

Overall, our model reveals that transport costs affect residential choices in addition to school choices, which has largely been absent from discussion in the empirical literature, only recently explored in Agostinelli, Luflade, and Martellini, 2021. When designing school choice systems, policy-makers should consider the specific context for households' choices, in particular the quality and cost of the local transport system. Transport for London has recently had to remove free

Figure 6.2: Equilibrium outcomes as the dis-utility (cost) of travel reduces.



Note: 'Dis-utility of transport' is the dis-utility associated with travel across neighbourhoods to school. 'Equilibrium R_H' is the rent in the High quality neighbourhood in equilibrium. Blue dots show the equilibrium under geographical priorities, and orange dots show the equivalent in non-geographical (lottery).

bus travel for school children in London (in response to funding cuts related to Covid-19), which may dramatically affect households' school choices in the coming years. London, and other cities worldwide, should evaluate the effect of transport policies on school choices and allocations, and potential spillovers to the housing market. Our model shows that these policies are likely to have the largest effect where school choice is 'free' - that is, not geographically constrained.

6.4 Stylised facts

This section presents the key features of differences across two contiguous neighbourhoods in a city in the South West of England, Bristol. The neighbourhoods are chosen to represent one area containing an above average school and high neighbourhood quality (corresponding to school AA and neighbourhood H in the model), and one area containing a below average school and reasonably comparable neighbourhood quality (school BA and neighbourhood L in the model). These short-hand names do not truly reflect the school and neighbourhood characteristics, but are used for convenience to be consistent with the model. 'Neighbourhood quality' in this context could best be described as proximity to the city centre and amenities such as tree-lined streets. We choose one school to represent the 'above average' school as it has the highest academic attainment (and is the only school far above the national average for state schools in England in our sample).[57] The schools have comparable levels of 'value-added' or average pupil progress, however, in our chosen time period.

The characteristics of these schools are presented in Table 6.1. Pupils attending Redland Green have the highest attainment in both 2011 and 2012.[58] For example, in 2011, 83% of pupils achieved at least 5 GSCEs above grade C, compared to 50% in Fairfield. There are similarly large differences in the percentage of pupils achieving the English Baccalaureate (EBACC) with 50% in Redland Green in 2011, compared to 17% in Fairfield. These large differences in the attainment of pupils are not necessarily the result of school quality, as final test scores are the result of school, child and parent inputs. Measures of Value-Added, the average progress pupils make from entry to exit, is therefore not always consistent with the raw attainment data. In 2012, for example, Fairfield has the highest. The most recent Ofsted grade, which provides an independent 'snapshot' summary measure of school quality remains higher in Redland Green than Fairfield.[59]

In practice, parents have preferences for the peer composition as well as school quality (Hastings, Kane, and Staiger, 2009, Burgess et al., 2015, Borghans, Golsteyn, and Zölitz, 2015, Abdulkadiroğlu, Agarwal, and Pathak, 2017, Glazerman and Dotter, 2017, Beuermann et al., 2018, Abdulkadiroğlu et al., 2020). The percentage of peers eligible for Free School Meals is a proxy for the intake of the school. This is lowest in Redland Green, where only 4% of pupils in 2011

[57]In 2011 the national average for state schools in England was 58%
[58]This pattern holds up until the latest year of attainment data.
[59]Redland Green became 'Outstanding' in the subsequent inspection round.

are eligible for Free School Meals, compared to 22% in Bristol. Fairfield is more comparable with the Bristol average, with 24% of pupils eligible for Free School Meals.

Redland Green is most likely to be chosen by pupils living in the catchment area/school zone. 86% of pupils living in the Redland Green catchment area name the school as first choice, compared with only 18% in the Fairfield catchment area. These are not perfect measures of parents' demand for schools, as first choices may reflect the perceived chance of admission ('skipping the impossible', coined by Fack, Grenet, and He, 2019) or the presence of siblings at the school (which almost guarantees admission). This pattern suggests, however, that Redland Green is the most popular school, retaining the majority of pupils residing in the catchment area.

Table 6.1: Descriptive statistics for two neighbouring secondary schools in Bristol

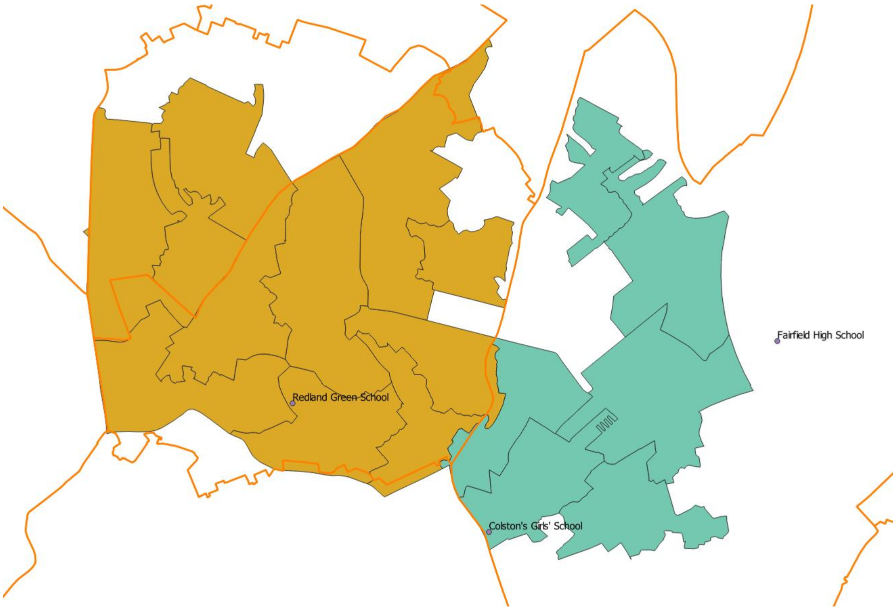
	Bristol Mean	Redland Mean	Fairfield Mean
School performance (2011)			
% 5A*-C (including English and Maths)[1]	53	83	50
% EBACC[2]	14	50	17
% Value-Added[3]	999.9	1023.7	1002.8
Ofsted grade[4]		Good	Satisfactory
% FSM[5]	22	4	24
School performance (2012)			
% 5A*-C (including English and Maths)	55	84	52
% EBACC	13	56	11
% Value-Added	1002.9	1024.0	1031.4
Ofsted grade		Good	Satisfactory
% FSM	37	6	46
School choices (2014)			
% Choose catchment school		86	18

[1] 5A*-C is the percentage of pupils that achieve at least 5 GCSEs at high grades (A*-C) including English and mathematics. This was the benchmark measure of attainment used to compare schools until 2016.
[2] EBACC is the percentage of pupils that achieve the English Baccalaureate, which requires at least 5 A*-C grades in English, mathematics, two sciences, a foreign language and history or geography at GCSE level.
[3] Value-Added is the average progress made by pupils at the school from the end of primary school to the end of secondary school.
[4] Ofsted is the Office for Standards in Education, Children's Services and Skills. Ofsted provides inspection reports for schools at regular intervals, with a summary measure of school quality.
[5] FSM is the percentage of pupils eligible for Free School Meals, which is a proxy for income disadvantage.

Data to illustrate the differences across neighbourhoods are primarily from the 2011 Census, measured at a low level of geography (lower level super output area, LSOA).[60] To create a proxy

[60] LSOAs are homogeneous small areas of relatively even size (around 1,500 people).

Figure 6.3: Catchment areas/school zones: Chosen Lower level Super Output Areas in two contiguous secondary school catchment areas



Note: Redland Green school corresponds to the Above Average school in the model, while Fairfield school corresponds to the Below Average school in the model. Colston's Girls' School operates a lottery for admissions and so does not have a catchment area.

for household income, area-level data on the percentage of household reference people of each National Statistics Socio-Economic Classification (NSSEC) and age group is combined with the average total net weekly earnings of households across age groups and NSSEC group observed in the Labour Force Survey. Property prices are from the Land Registry database of all sale prices. The variation in property prices due to neighbourhood and school (and all other) factors is calculated by taking the residual of property prices conditional on the total floor area, presence of an open fireplace and total floor area interacted with the number of habitable rooms and whether the property is a flat/maisonette. The observable characteristics of the properties are taken from the 87% of properties with a perfect match between the price paid dataset and Energy Performance Certificate database. See Appendix A5.4 for full details of the data construction.

Figure 6.3 shows the selected neighbourhoods, with Redland (H) shown in yellow and Fairfield (L) in green. The orange lines show boundaries in school catchment areas/school zones, while the black lines show boundaries of LSOAs.

Table 6.2: Descriptive statistics for Bristol and two adjacent catchment areas/school zones within Bristol

	Bristol		Redland		Fairfield (close)	
	Mean	S.D.	Mean	S.D.	Mean	S.D.
Income and occupation						
% Professional occupation+1	36.6	16.9	65.6	2.7	55.5	3.3
% Routine occupation+1	26.91	13.3	7.87	1.44	12.14	2.04
Low income score+2	0.15	0.11	0.04	0.01	0.08	0.05
Mean net weekly income (£, imputed)[3]	453.1	65.6	568.1	29.4	495.6	25.1
% Free school meals	20.3		2.2		8.5	
Household composition						
% F0 (never dependent children)*			2.7		6.6	
% Dependent child(ren)*+	44.0	9.6	49.1	6.5	45.1	3.2
% Dependent child(ren) primary age*	31.7	7.9	33.4	5.0	33.5	4.4
% Dependent child(ren) school choice age*+	2.3	1.0	3.4	0.0	1.8	0.0
% Dependent child(ren) secondary age*	12.6	3.2	17.2	3.0	12.5	3.0
Property prices (£1000s)[5]						
Mean price +	199.6	95.4	360.2	70.3	277.4	51.3
Mean price, 3 bedroom+	149.5	47.0	203.7	47.3	172.1	20.1
Mean price, 4 bedroom+	174.5	67.5	295.8	72.8	223.2	28.8
Mean price, residual*	-6.8	49.2	69.8	23.7	19.4	14.5
School access						
Implied probability of admission to AA*6			0.89		0.00	

All characteristics are measured at the lower level super output area (LSOA). Columns present the average and standard deviation across LSOAs, weighted by LSOA population. Columns 1-2 show the average and standard deviation across all LSOAs in Bristol. Columns 3-4 shows the equivalent for the 8 LSOAs in the immediate Redland Green catchment area/school zone, Columns 5-6 the 6 LSOAs in the Fairfield catchment area (contiguous to the Redland Green catchment area).

[1] Occupation classifications are the National Statistics Socio-Economic Classification of the household reference person (HRP).

[2] Low income score is the 2010 Index of Multiple Deprivation (Income Domain), which classifies small areas according to the proportion of the population in an area experiencing deprivation according to low income.

[3] Imputed income is derived as the average net weekly household income within, given the NSSEC of the household reference person. Average income (by age group and NSSEC) is calculated using the Labour Force Survey.

[4] Household life-stage t indicates time period consistent with the age of households in our model. t0 denotes that the Household Reference Person is between 25 and 40; t1 is between 40 and 42; t2 is between 42 and 50; and, t3 is between 50 and 65.

[5] Property prices are taken from the universal Land Registry Database and aggregated to LSOA level. The residual prices are the variation in price not accounted for by total floor area, the number of rooms and presence of an open fireplace.

[6] The implied probability of admission to AA is the calculated probability of admission for non-sibling applicants. 94.1% of applicants to Redland Green from within the catchment area are successful. Some of these will be siblings, so have actual probability of 1. From the British Household Panel Survey, 48.6% of children have an older sibling. The implied probability of admission for non-sibling applicants is (0.941-0.4861)/0.5139 = 0.8851.

* Denotes used as a moment to match the model to the data.

\+ Denotes observed directly in a data source. Otherwise, the measure is derived in some way.

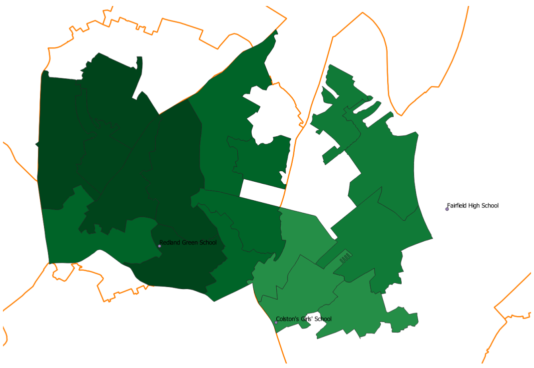
Differences across LSOAs in different school catchment areas/school zones are summarised in Table 6.2. The overall picture from the first panel ('Income and occupation') is that these two neighbourhoods are relatively affluent compared to Bristol as a whole, which is particularly true for Redland. Both neighbourhoods have a much higher percentage of households with a 'managerial, administrative and professional' occupation: 65.6% of households in Redland and 55.5% of households in Fairfield, compared to 36.6% in Bristol as a whole. There are also fewer households with 'semi-routine and routine' occupations. Mean net weekly income, derived from households' occupation combined with average income by occupation group in the Labour Force Survey, is, as expected, higher in both neighbourhoods than Bristol as a whole. Similarly, there are far fewer households eligible for free school meals than the Bristol average, and small 'low income scores' from national classifications. Figure 6.4 shows the variation in average income across LSOAs in our three areas, with particularly high representation of those in professional occupations, and, by construction, income, in the LSOAs to the North-West of Redland Green school. The LSOA to the South-East in Fairfield catchment area is distinct in having a higher percentage of those in routine occupations and a higher 'low income score' in the national classification.

Household composition in the life-stage of households is similar in the two neighbourhoods, and similar to Bristol as a whole. The demographics vary according to family type, however, with a lower percentage of households that never have children in Redland compared to Fairfield. Households in Redland are more likely to have dependent child(ren): 49.1% compared to 45.1% in Fairfield and 44.0% in Bristol overall. This particularly true for households with a child at school choice age (10): 3.4% in Redland compared to 1.8% in Fairfield and 2.3% in Bristol. The percentage is also higher for secondary school aged children, although not for primary school aged children. Figure 6.5 show the share of households with dependent children and dependent children of each stage age across LSOAs, respectively. The share is higher for the majority of LSOAs inside the Redland catchment area/school zone than outside, but particularly so in some cases.

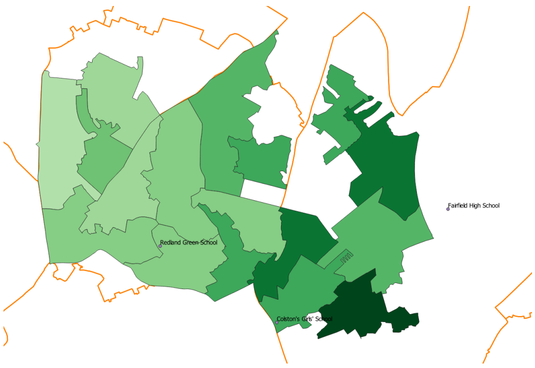
We consider the residual property price as our main measure of demand to account for any variation in property size across areas. The residual property price abstracts from variations in price that are due to total floor size, the number of habitable rooms and the presence of an open fireplace (as a proxy for period property features). The residual price best captures the property price due to neighbourhood and school characteristics, with the caveat that other features unobservable to us, such as the decoration and presence/size of garden, may also affect property prices. As expected, the mean residual price in Bristol as a whole is close to zero.[61] The mean residual property price in Redland is £69,800, compared to £14,500 in Fairfield. Property prices are around 20% higher in Redland compared to Fairfield for the mean price, mean price for

[61] It is not exactly zero as the initial regression was at the property level. The residuals were then aggregated to the LSOA level. The table shows the average of these LSOA averages.

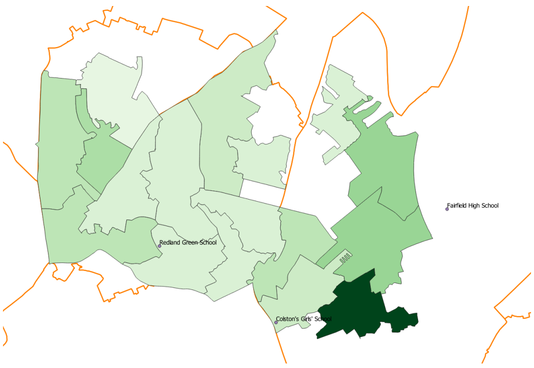
Figure 6.4: Income and occupation across LSOAs



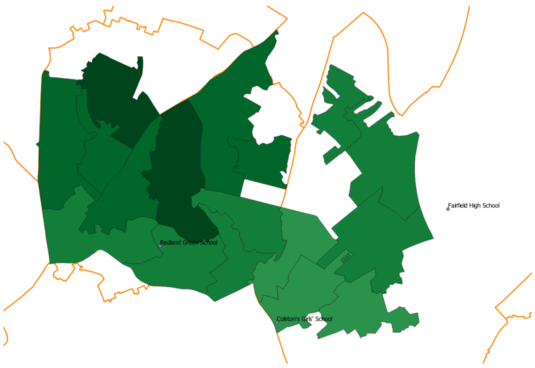
(a) Professional occupation



(b) Routine occupation



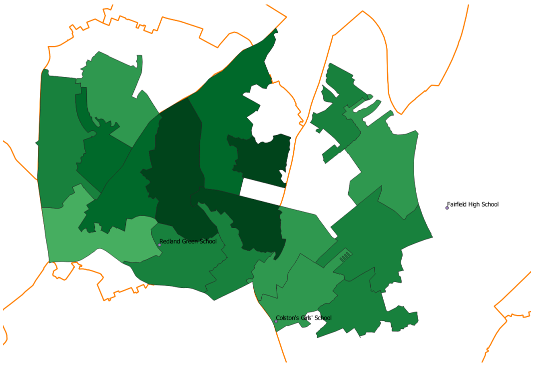
(c) Low income score



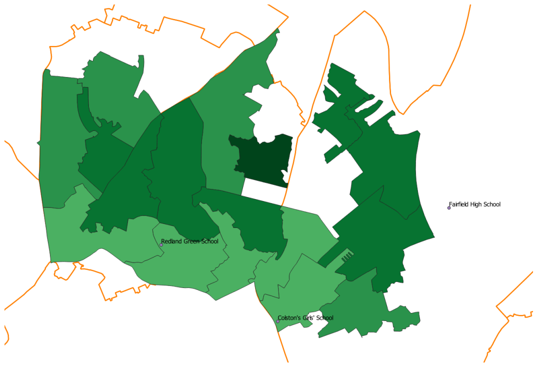
(d) Imputed income

Source: NOMIS, Labour Force Survey, 2010 Index of Multiple Deprivation (Income Domain). Note: Darker colours mean higher values.

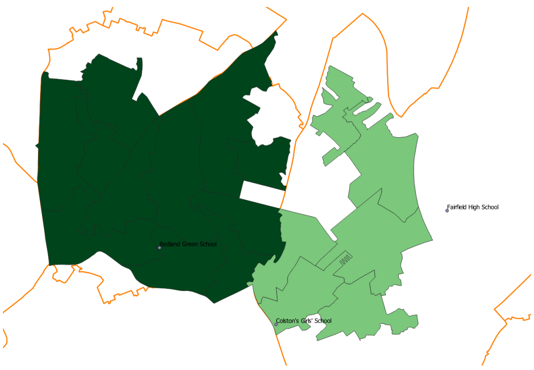
Figure 6.5: Household composition across LSOAs



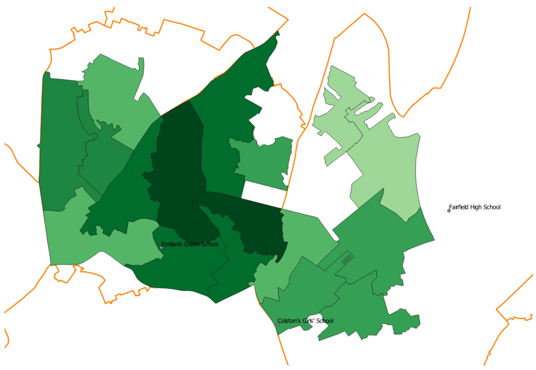
(a) Dependent child(ren)



(b) Dependent child(ren): primary



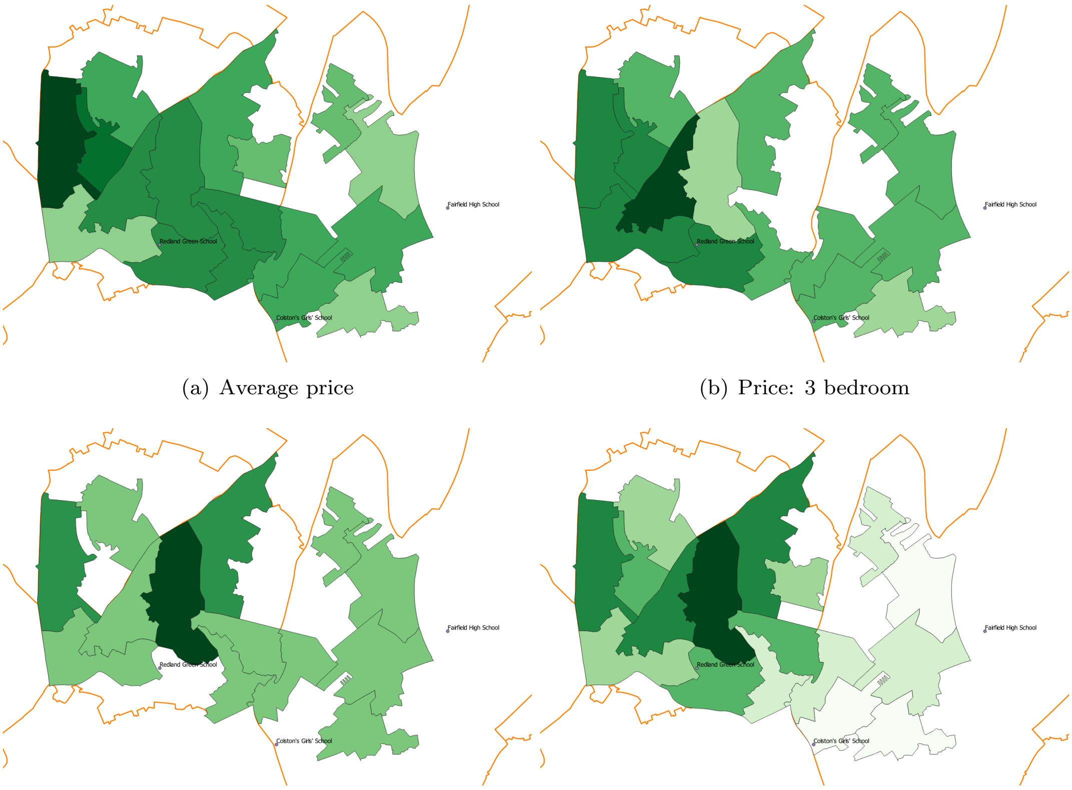
(c) Dependent child(ren): choice



(d) Dependent child(ren): secondary

Source: NOMIS. Note: Darker colours mean a higher proportion.

Figure 6.6: Property prices across LSOAs



(a) Average price (b) Price: 3 bedroom

(c) Price: 4 bedroom (d) Price: residual

Source: Price Paid Data from HM Land Registry and Energy Performance of Buildings Data: England and Wales. Note: Darker colours mean higher values.

a 3 bedroom property and mean price for a 4 bedroom property. Figure 6.6 shows this variation across LSOAs within catchment areas/school zones. Residual prices are typically higher across LSOAs inside the Redland Green catchment area than in the Fairfield catchment area.

6.5 Results

This section presents the results of the model, calibrated to the contiguous neighbourhoods in Bristol described in section 6.4.

6.5.1 Empirical and model moments

The equilibrium preference parameters are found through calibration. The moments we choose to match are the proportion of households of each family type and life-stage that choose to live in neighbourhood H.[62] We choose to match the *proportion* of households that choose to live in H to relax the assumption that there is no in or out migration from our neighbourhoods across the lifecycle. That is, the moments we match ask, given the households that choose to live in H or L, what proportion choose to live in H?

We target the following five moments: non-parents ($F0$) in period one ($T1$); parents with one child ($F1$) in each period; parents with two children ($F2$) in the school choice phase ($T1$). Overall, the model matches the empirical moments well. Roughly the same proportion of non-parent households choose to live in H in the first and second life-stage. The model is not able to capture the higher proportion of $F0$ that choose to live in H in the third life-stage, however. This may be due to these households, in reality, having more disposable income later in life, and therefore a lower marginal utility of consumption, but this is not yet modelled. Given that we target these moments, the model matches the proportion of $F1$ that choose to live in H well, with an increase from 55% to 64% of households choosing to live in H between the first and second life-stage. This increase is due to the additional benefit of living in H in the school choice phase, as it increases the probability of admission to the AA school. Some households choose to move in the first life-stage as the utility cost of moving would be greater than the disutility of higher rent in this period. The model has a similar pattern for $F2$, where a higher proportion choose to live in H in the school choice phase as they gain additional utility from their second child attending the school. The model is unable to capture the higher proportion of $F2$ households that choose to move a stage earlier, however.

Future refinements will improve the fit of the model. With that caveat in mind, the following section describes the simulated results of moving from a geographic preference system to a lottery system for these two neighbourhoods, using the calibrated preference parameters.

Table 6.3: Empirical and model moments

	Empirical moments			Model moments		
	T0	T1	T2	T0	T1	T2
% F0	0.46	0.45	0.59	0.45	0.45	0.45
% F1	0.55	0.64	0.69	0.55	0.66	0.66
% F2	0.67	0.73	0.74	0.55	0.73	0.72

Empirical moments are derived from area-level statistics from the 2011 Census (NOMIS). Model moments are found through calibration.

[62] Note that these proportions are constructed from area-level census data and will contain some measurement error. We exclude the final life-stage (the age consistent with children having left home) as we can not identify households that once lived with children in the data.

6.5.2 Simulation of lottery in admissions

This section shows the likely equilibrium outcomes if the AA school moved from geographic preference to lottery admissions priority. We use the calibrated preference parameters found for the geographic preference case, and simulate the effect of breaking the link between residence in H and admission to the AA school. In practice, this means replacing the probability of admission to be even across areas, rather than dependent on the demand for housing from different household types. The equilibrium outcome under admission by lottery is shown in Table 6.4. As expected, the probability of admission to the Above Average school, given application, is the same across neighbourhoods in this simulation.[63]

The implied rent difference and mean income of households across neighbourhoods decreases. This reflects the loss in demand from households with/expecting dependent children that no longer prefer H to L under equilibrium prices. Although falling by 5%, the price remains higher in H than L. This is due to the difference in neighbourhood 'quality', which remains, and which households have strong preferences for.[64] This explains the remaining difference in mean income across neighbourhoods, that falls by only 1%: access to higher neighbourhood quality is still rationed by income. In contrast, sorting by income across schools falls: there is an 8% fall in mean income in the AA school, implying more equality of access to the AA school. This is an important point, discussed in section 6.3, that admissions reform can have opposing effects on neighbourhood and school segregation. In this case, neighbourhood income segregation reduces marginally, as neighbourhoods become perfectly sorted by income, rather than lower income parents displacing higher income non-parents. School income segregation decreases, as admission is no longer determined by parents' willingness or ability to choose higher rent to improve the probability of admission.

Household composition across neighbourhoods is dramatically changed when the priority by lottery replaces the geographic preference system. For example, the proportion of households that never have children in H increases from 0.16 to 0.19, increasing by 19%. In fact, under the lottery system, the composition of neighbourhoods roughly mirrors that in the population, as all household types gain similar utility from living in H. Only households with children gain higher utility from living in H when their child(ren) are allocated to the AA school by the lottery, as this reduces their commuting costs. The fraction of households that never have dependent children in H is slightly higher than in the population, as these households have more disposable income and therefore a lower marginal utility of consumption. The age distribution in H also mirrors that in the population under the lottery simulation. The most striking contrast is for the

[63] Recall that we assume all households with applicant children choose the AA school, as the utility from attending it would outweigh the disutility cost of travel across neighbourhoods.

[64] Household demand is very sensitive to change in the rent in H, that means that small changes in the rent induce large changes in demand. We have explored this sensitivity, and believe it is because there is a mass of households around the equilibrium threshold level. This means that small changes in the rent change the equilibrium decisions of many households. Future work will explore the sensitivity of our results to alternative income distributions.

proportion of households in the school choice phase, which is 38% higher under the geographical admissions priority system. This is because parents in this life-stage derive utility from gaining admission to the AA school, are more willing to forgo consumption by paying higher rents to improve the probability of admission.

In summary, neighbourhoods have a more equal distribution of households across life-stages and completed family type when choice of school is not determined by location. Whether this is desirable is a question for policy-makers. We posit that integration has benefits for society if it promotes tolerance, but there may be costs if households have preferences for homogeneous neighbourhoods.

Table 6.4: Simulated moments

	Catchment	Lottery
r_H [1]	1,282	1,224
Probability of admission (AA from H)[2]	0.89	0.63
Probability of admission (AA from L)[2]	0.00	0.63
$\bar{Y}_H$	3,424	3,382
$\bar{Y}_G$	3,126	2,870
$F0_H$	0.16	0.19
$F1_H$	0.18	0.18
$F2_H$	0.66	0.63
$T0_H$	0.48	0.39
$T1_H$	0.08	0.05
$T2_H$	0.17	0.19
$T3_H$	0.26	0.38

Note: This table shows the equilibrium outcomes under two alternative school admissions systems. First, the catchment system, where pupils within the high quality neighbourhood have priority in admission to the above average school. These equilibrium outcomes are derived using the calibrated model parameters. r_H denotes the rent in the high quality neighbourhood, that is calibrated to match the observed premium in the data. The probability of admission to the above average (AA) school is also calibrated to match the observed patterns in the data. $\bar{Y}_H$ denotes the average income for households that choose to live in H, and $\bar{Y}_G$ for average household income for pupils allocated to the AA school. $F0_H$, $F1_H$ and $F2_H$ denote the share of households that choose to live in H with completed fertility size of zero, one and two. $T0_H$, $T1_H$, $T2_H$ and $T3_H$ are the share of households that choose to live in H in each life-stage.

6.5.3 Spillovers to Non-Parents

The inclusion of non-parents in our model allows us to consider externalities of the school system for non-parents.[65] If schooling was irrelevant for all households in our model, then neighbourhoods would be perfectly sorted by income. Rents would rise in H until the demand for properties in H equalled the supply. Higher income households would sort into H as they have a lower marginal utility of consumption (their welfare is less affected by paying higher rents as they have higher income).

Including schooling in the model creates externalities for non-parents in two ways. First, the utility of non-parents living in H decreases and rents in H increase (as they now have lower consumption).[66] Second, non-parents whose threshold for living in H is lower than the new higher rent are displaced to L. This is true when there is a non-geographical (eg lottery) admissions system, as parents value proximity to their allocated school. As the AA school has a larger share of school places, this increases rents in H. The externalities created by the school system are even larger when there is a geographical admissions system, however. This is because parents' want to gain admission to the AA school by living in H, which increases demand in H and therefore the equilibrium rent in H.

Welfare is unambiguously lower for non-parents under a geographical admissions system compared to a non-geographical system. Using the calibrated parameter values, our model shows that 10% of non-parents are displaced from H when the admissions system changes from lottery to geographic preference. The displaced non-parents are around the middle of the income distribution (the 9th to 11th vigintiles of income) and choose L instead of H when the equilibrium rent in H rises as a result of the geographic preference system. Welfare falls for these households, by around 200%. Welfare also falls for the non-parents that remain in H under the geographic preference system, by around 100%, due to the rent premium.

More generally, how does the school admissions system affect the welfare of parents? As for non-parents, those that always live in H regardless of the admission system are affected by the changing rent in H. For parents, however, any increase in rent under the geographic preference system might be offset by the increased probability of admission to the AA school under the geographic preference system. Welfare will also change for parents displaced from H to L once the rent in H rises above their threshold level.

The school admissions system distorts parents' residential choices dramatically. Under our calibrated parameter values, only around one third of parents with two children (those with the lowest income) choose to live in L under the geographic preference system from the school choice stage of life onward, compared to around two thirds of parents with two children under

[65] Caetano, 2019 includes non-parents in his model of school and residential choice, but does not use this to consider spillovers between groups and welfare calculations. Agostinelli, Luflade, and Martellini, 2021 include non-parents defined in a static way: those currently with or without children.

[66] Note that in reality, rents in L may also fall, which would affect utility for those living in L. In our model, rents in L are fixed, however.

the lottery system. Almost all these parents have higher utility under the lottery system, aside from in the school phase when parents allocated to the AA school have higher utility under the catchment system.

6.6 Summary

School choice has the potential to increase access to 'above average' schools for children from less advantaged backgrounds. Whether school choice achieves this aim depends crucially on the *design* of school choice. The innovation of this paper is to consider school priorities (the ranking of applicants if oversubscribed) rather than the allocation mechanism (used by the central authority to assign places) on the equilibrium outcomes for schools and neighbourhoods, and to consider overall welfare effects for all household types.

We quantify the relationship between the type of school priority (geographic preference versus lottery) and the social mix within schools and within neighbourhoods, defined as the mix between households of different ages, different income and different family size. To do this, we build the first dynamic structural model of household choices across different life-stages allowing for heterogeneity in family types (completed family size). Households care for local amenities, school quality and distance to school when there are children travelling to school. Residential location matters most in the life-stage when parents apply to a secondary school (if there is neighbourhood sorting) and in the life-stage when the household's children travel to school. Because households are forward-looking and there is a cost of moving across neighbourhoods, households' residential choices exhibit some persistence.

The model illuminates important mechanisms underlying reduced form estimates of causal effects. For example, the model shows that it is not possible to interpret the estimated relationship between school quality and house prices as parents' valuation of school quality, as the estimated effect will be dampened by the presence of non-parents and older households in the market. A similar logic applies to reduced form estimates of school choice reforms and improving information about school quality. The model also shows the interacting effects of transport costs and moving costs in school and neighbourhood choice, which are therefore factors for academics and policy-makers to consider in evaluation and design of school choice environments.

We calibrate our model to data from three neighbourhoods in the city of Bristol, England, and find that it replicates patterns of sorting in schools and neighbourhood across family types and life-stages well. The area around the 'above average' school in high demand is characterised by a larger proportion of households of secondary school phase age, and a larger proportion of households with dependent children (particularly households with more than one child, who have more to gain from admission to the 'above average' school). Residual property prices are higher than in the area with similar neighbourhood quality but lower school quality (according to attainment data).

Comparative statics from the model illustrate the potential trade-off between integration in neighbourhoods and schools. When residential location does not affect priority to the 'above average' school neighbourhoods are perfectly sorted according to income, with an equal proportion of family types, while the 'above average' school has a more integrated composition. In the alternative 'geographic preference' case where living in the High quality neighbourhood increases the probability of admission to the 'above average' school, neighbourhoods become more integrated by income but less integrated by family type (with a higher concentration of households with children) and schools become less integrated according income. These results suggest a potentially difficult trade-off for a social planner wishing to increase integration between household types.

Our model has assumed that households have preferences for school quality rather than the school peer group. Peer preferences would alter the equilibrium outcomes under lottery allocation, as strong peer preferences would equalise desired attendance at each school. More generally, peer group preferences have been shown theoretically to weaken schools' incentives to exert effort under school choice (Barseghyan, Clark, and Coate, 2019). Empirical evidence generally finds relatively strong peer preferences (see for example Burgess et al., 2015), but has been unable to account for endogenous location of households and other unobservable attributes of the school.

There is the potential to apply our model to other contexts. We will also develop extensions to the model, most importantly incorporating a private school outside option which has been shown to affect the equilibrium outcomes of school choice systems by providing an 'outside option' to richer households (Epple and Romano, 2003, Calsamiglia, Fu, and Güell, 2020, Calsamiglia, Martínez-Mora, and Miralles, 2020). Despite this, we believe our existing model provides important insights into the relationship between school priorities, school choice and neighbourhood and school sorting. That the welfare of households without children is affected by the school choice environment is an important but previously understudied fact. Such a model could be calibrated to other cities in England and elsewhere in the world to illuminate the impact of alternative school priorities on sorting into neighbourhoods and schools.

7 How do schools shape neighbourhoods? Endogenous residential location in response to local school quality

Preface: *This chapter is sole authored.*

Acknowledgement: This work contains statistical data from ONS which is Crown Copyright. The use of the ONS statistical data in this work does not imply the endorsement of the ONS in relation to the interpretation or analysis of the statistical data. This work uses research datasets which may not exactly reproduce National Statistics aggregates. The permission of the Office for National Statistics to use the Longitudinal Study is gratefully acknowledged, as is the help provided by staff of the Centre for Longitudinal Study Information & User Support (CeLSIUS). CeLSIUS is funded by the ESRC under project ES/V003488/1. This work also contains publicly available data licensed under the Open Government Licence v3.0. The author alone is responsible for the interpretation of the data.

7.1 Introduction

Households weigh up the costs and benefits of moving to a particular neighbourhood. For some households, this decision may include the quality of the local school. Households might wish to move close to their preferred school to reduce their commuting time, and, in some contexts, improve their probability of admission. This means that the design of the education system in how pupils are allocated to schools is consequential for neighbourhood composition in addition to school composition. There is wide variation in policy across and even within countries. In some contexts, places at popular over-subscribed schools are decided by lottery, in others by aptitude (test scores) and finally by geography (for example school zones or distance rank). There is little evidence, however, about how these policy choices affect households' residential choices and therefore the formation of, and segregation within, neighbourhoods. This is important evidence to determine the role of school admissions priorities in households' overall welfare, and aggregate sorting across neighbourhoods that may affect societal outcomes.

This paper studies the influence of geographical admissions priorities on the frequency and timing of residential moves. The context is England, where there are two dominant school admissions priorities in use. For the majority of areas, admission to over-subscribed schools is largely determined by geography - either catchment areas (elsewhere known as 'school zones' or 'la carte scolaire') where those resident inside have priority, or distance between home and school, where closer pupils have a higher rank. A minority of areas historically retained the previous non-geographical selective system (otherwise dismantled in the 1960s and 1970s), where pupils are admitted according to test scores rather than location.

The methodology is to compare households' residential choices across these two types of area - those with geographical or non-geographical admissions priorities. In areas with geographical admissions, the hypothesis is that valuing admission to a 'good' local school leads to endogenous residential sorting to a neighbourhood that guarantees admission (or, more precisely, improves the probability of admission). In areas with non-geographical admissions, households that have children value admission in the same way, but the residential location is not strategic, as factors other than location (test scores) determine admission. To account for the possibility of non-random selection into alternative admissions arrangements, a difference-in-differences design is used. The second difference is between households that ever have children and households that never have children, that are defined using nationally representative longitudinal data. The intuition is that 'ever parents' value admission to the local school while 'never parents' gain no flow utility from the local school, but may value neighbourhood attributes that are correlated with local school quality, such as public or private amenities, the housing stock and neighbourhood composition. This comparison therefore isolates the effect of local school quality on residential choices.[67] This intuition is similar to that discussed in Bayer, Ferreira, and McMillan, 2007, that neighbourhood attributes, such as school quality and peer composition, can be correlated or causally related. This paper uses households that are never parents to control for these (observable and unobservable) factors that are correlated with local school quality.

The first finding is that the extensive margin - whether households decide to move home or not - is largely unaffected by whether the school admissions priorities are geographical or non-geographical (selective). Younger households that ever become parents are more likely to move than younger households that never become parents, but this pattern is the same across geographical and non-geographical admissions areas. This implies that geographical admissions priorities do not impose additional moving costs for households across the lifecycle. For the intensive margin - the quality of the local school conditional on moving - there is no effect on average across all households. There is some limited evidence that households with high social class (defined by occupation) are slightly more likely to move, and move closer to higher quality schools at younger ages if they ever become parents and live in geographical admissions areas, however. This suggests that the observed property price increases around popular oversubscribed schools (discussed below) are due to strongly increased demand for a minority of households. This is in contrast to the received wisdom that schools are a driving factor for many households' residential decisions. These results present the picture of endogenous residential choices for only a limited proportion of (more affluent) households.

The paper contributes to four research themes. First, in relation to the large, well-established and robust empirical literature on the effect of popular over-subscribed schools on local property prices, this paper delves into the 'black box' to uncover *when* households move and *which* households move for local school quality. This is important to determine whether geographical

[67]Measurement error induced by uncertainty in the likelihood of ever or never becoming a parent is discussed in section 7.4.1. A full description of the methodology and identifying assumptions follow in sections 7.3 and 7.5, respectively.

admissions impose costly additional moves, affecting households' welfare, and whether inequality in school access is exacerbated by geographical admissions priorities. This paper finds that geographical admissions priorities do not reduce households' welfare, on average, by imposing additional moves, but do increase inequality in access by social class. Knowing whether households move away from 'good' schools after their children have left school also helps to interpret the existing empirical literature, as the estimated reduced form effect is a combination of flows in and out of neighbourhoods around good schools. This paper shows that households tend to move earlier in the lifecycle, so there will be limited dampening of the empirical house price premium through households exiting shortly after the school choice phase.[68]

The identification problem for this strand of literature is to separate the effect of school quality on property prices from the effect of other neighbourhood attributes that are correlated with school quality, such as proximity to amenities. Fack and Grenet, 2010 state the additional problem of reverse causality clearly:

> The estimation is complicated by the endogeneity of school performance in the housing price equation, since better schools tend to be located in wealthier neighborhoods and pupils drawn from privileged socio-economic backgrounds generally have higher academic achievement.

Beginning with Black, 1999, Boundary Discontinuity Design is typically used to estimate the causal effect of local school quality on property prices, where the identifying assumption is that neighbourhood attributes are continuous at the boundary while school quality jumps discontinuously. Across studies in this literature, households are typically willing to pay a premium of around 3-4% for access to a one standard deviation increase in school average test scores, although there is variation across contexts, from 1.4% in Paris to 7-10% in Olso.[69] The mechanism is likely to be the higher probability of admission to the desirable school (Bonilla-Mejía, Lopez, and McMillen, 2020.

Boundary Discontinuity Design is appropriate when there are clear school zones that determine school admission. Another research theme studies how the introduction of school choice

[68]Chapter 6 shows that the reduced form effect is also dampened by the presence of households that gain (or will gain in the future) no flow utility from local school quality, for example households that never have children or households where the dependent children have moved on.

[69]The existing empirical estimates (in order of magnitude for a one standard deviation increase in test scores) are: Fack and Grenet, 2010 (Paris): 1.4% for school test scores, 2.1-2.4% for school peer-group; Bayer, Ferreira, and McMillan, 2007 (San Francisco Bay): 1.8%; La, 2015 (Boston): 2-4%; Black, 1999 (Massachusetts): 2.1%; Harjunen, Kortelainen, and Saarimaa, 2018 (Helsinki): 3.3%; Davidoff and Leigh, 2008 (Australia): 3.5%; Gibbons, Machin, and Silva, 2013 (England): 3.5%; Kane et al., 2003 (North Carolina): 5%; Machin and Salvanes, 2016 (Oslo): 7-10%. Bayer, Ferreira, and McMillan, 2007 note that school quality induces sorting of neighbourhood peers across the boundary. Accounting for this factor roughly halves the estimated effect of school quality. This is also noted by Kane, Riegg, and Staiger, 2006. Dhar and Ross, 2012 find slightly lower estimates using school district boundaries rather than school zone boundaries. Goldstein and Hastings, 2019 relate differences in property price premiums to differences in inequality, with the hypothesis that inequality increases the perceived need to access a 'good' school. Wong and Deng, 2021 study changes in property prices in China following catchment area mergers, finding that house prices appreciate by 1.3 to 4.1% in areas that suddenly gain access to high ranking schools.

(or ‘choice schools’ in the US) weakens the relationship between school quality and property prices, as it breaks the deterministic link between home location and school. Schwartz, Voicu, and Horn, 2014 find that new ‘choice schools’ reduce the property price premium of traditional ‘zoned’ schools by approximately one third. Following the introduction of a system of school choice, property prices around ‘good’ schools reduce by around half in Olso (Machin and Salvanes, 2016) and 10-30% in Seoul (Chung, 2015a). In a related contribution, Reback, 2005 finds that property prices across Minnesota school districts respond to whether students are able to transfer from and to the district, and Brunner, Cho, and Reback, 2012 use variation across US states in the adoption of school choice to find a similar pattern.[70] Taken together, this large body of research in economics is to date silent about the types of households that move in response to school quality, and at what life-stage.[71] Recent work in sociology explores how school choice expedites the gentrification of inner-city neighbourhoods, particularly by college-educated white households (Pearman and Swain, 2017). This paper shows that it is largely high social class households that make endogenous residential choices in response to the school choice environment.

The second contribution is to inform the likely bias in discrete choice models that estimate parents’ preferences for school attributes. In these models, parents’ preferences are inferred from the observed attributes of their school choice(s) relative to the observed attributes of other schools in their choice set. Universally, in this large literature, these models are estimated assuming that households have fixed location.[72] Bias is introduced to the estimation if some households move close to their preferred school to improve the probability of admission. In this case, when choosing the closest school reflects preferences for other school attributes (for example the peer group and/or school quality) that induced the residential move, the estimated preference for proximity would be upward biased. In fact, with this non-linear estimation method, all coefficients would be biased. The common finding in this literature that more advantaged households have different preferences for school attributes than less advantaged households may also be due to the bias induced by residential sorting. This paper shows that endogenous residential sorting to guarantee school admission is limited to the highest social classes. This implies that comparisons of preferences across social classes is problematic. Positively, estimated preferences for school attributes for lower social classes should be largely free of bias.

The third contribution is to expand the research on the selective grammar school system

[70] See also Bogart and Cromwell, 2000 for Ohio, US, Lee, 2015 and Chung, 2015b for Seoul, South Korea and Batson, 2021 for the Rockford School District, US. Danielsen, Fairbanks, and Zhao, 2015 provide a comprehensive review of this literature.

[71] An exception is that Bayer, Ferreira, and McMillan, 2007 estimate heterogenous preferences for local neighbourhood preferences across household types, which is informative, but does not explicitly study the frequency and location of residential moves.

[72] In chronological order, see Hastings, Kane, and Staiger, 2009, Burgess et al., 2015, Borghans, Golsteyn, and Zölitz, 2015, Abdulkadiroğlu, Agarwal, and Pathak, 2017, Glazerman and Dotter, 2017, Ruijs and Oosterbeek, 2019, Beuermann et al., 2018, Oh and Sohn, 2019, Harris and Larsen, 2019, Ajayi and Sidibe, 2020, Walker and Weldon, 2020, Abdulkadiroğlu et al., 2020, Bertoni, Gibbons, and Silva, 2020).

to include the effect on residential sorting. Previous research has documented little evidence of improved test scores for pupils that are marginally admitted vs rejected (Clark, 2010)[73] although stronger effects for longer-run outcomes (Clark, 2010, Clark and Del Bono, 2016). Other studies are externally valid to the whole population of grammar school pupils (rather than only those close to the admission threshold) but suffer from omitted variable bias from unobservable pupil characteristics that may differ between pupils at selective and non-selective schools, including pre-existing attainment trajectories (Pischke and Manning, 2006, Coe et al., 2008). Typically, findings from these papers are that attainment is marginally higher for pupils at selective schools (relative to the counterfactual) but lower for pupils at the remaining schools (Atkinson, Gregg, and McConnell, 2006). Overall, this increases inequality in attainment, that translates into inequality in earnings (Burgess, Dickson, and Macmillan, 2019). This paper instead focuses on the important general equilibrium consideration of households' residential choices and therefore neighbourhood formation.

The final contribution is to the literature that estimates households' valuation of neighbourhood amenities through discrete choice models. School quality is often (although not universally) included as an independent variable in discrete choice models of residential choices (see, for example Kim, Pagliara, and Preston, 2005 and Brasington and Hite, 2005).[74] The estimate is given the interpretation of 'willingness-to-pay' for school quality, although without overcoming the problem that school quality is an endogenous variable. This paper's empirical strategy overcomes the problem that school quality is correlated with other neighbourhood attributes, and in fact can be causally related in both directions as stated by Fack and Grenet, 2010, above. That is, school quality can influence neighbourhood amenities/peers and neighbourhood amenities/peers can influence school quality. This is because school quality is defined as the combined effect of the incoming school cohort and value added by the school, and there is a positive correlation between academic ability and household income, for example.[75] This paper provides evidence that local school quality is important for a minority of households' residential choices, and that urban choice models should allow heterogeneity in the effect of local school quality across households. This heterogeneity could be due to differences in preferences or constraints in income and/or information (Bergman, Chan, and Kapor, 2020).

The remainder of the paper is structured as follows. Section 7.2 provides further detail on the education system in England and the incentives induced for parents. Section 7.3 describes the methodology to identify the causal effect of geographical admissions priorities on households' neighbourhood sorting. Section 7.4 details the data employed, including data construction and

[73]This is consistent with most evidence from marginal students at 'elite' schools worldwide (Abdulkadiroğlu, Angrist, and Pathak, 2014, Dobbie and Fryer, 2014, Lucas and Mbiti, 2014, Dee and Lan, 2015, Zhang, 2016), although some studies find positive (but small) effects (Pop-Eleches and Urquiola, 2013, Deming et al., 2014, Ding and Lehrer, 2007).

[74]Couture and Handbury, 2020 study the role of schools in contributing to urban revival.

[75]On-going work by the author seeks to provide an instrument for local school quality that could be used in urban discrete choice models.

summary statistics. Section 7.5 presents evidence that the identifying assumptions hold in the data. Section 7.6 shows the descriptive and main results for the role of school priorities on neighbourhood sorting, before section 7.7 concludes.

7.2 Context

Everywhere in England, parents submit a ranking of preferred schools to their Local Authority of residence (LA).[76] Each LA then runs a truth-revealing assignment mechanism to allocate pupils to schools, given parents' preferences, schools' capacity and admissions criteria. The assignment mechanism is known as 'equal preferences' and is equivalent to the Gale-Shapley deferred acceptance mechanism with a short list length (between 3 and 6 choices, depending on the LA). This short list length means that some parents have an incentive to misreport their true preferences to include a safe school, as they would be allocated to a school with spare places (by definition unpopular) in the event of being unassigned to any of their ranked schools. Misreporting preferences by omitting some schools is coined 'skipping the impossible' by Fack, Grenet, and He, 2019. The allocation mechanism therefore permits parents to make multiple school choices with limited distortions to their preferences, but in practice, successful admission to a chosen school depends on the school's admissions priorities.

The following sub-sections present the two most common admissions priorities used in England, 'outside options' available to parents, and nuances to the two main systems.

7.2.1 Main admissions priorities

There are two main school admissions criteria in England. The predominant form is a comprehensive system with geographical admissions priorities. If a school is over-subscribed, then the priority ranking to determine which pupils are admitted is normally ordered by location. That is, whether pupils live inside or outside a catchment area (equivalent to a 'school zone' or 'la carte scolaire') or by distance between the home and school. The second admissions system is (largely) non-geographical. Instead, it is a 'selective' or 'grammar' system, where pupils must pass an entry test to gain admission to certain prestigious schools in an LA.[77] Pupils that do not pass the test, known as the '11-plus', attend 'comprehensive' schools (previously called 'secondary modern' schools).

The key distinction between these systems, for this paper, is the induced residential incentives for parents. Parents have an incentive to move close to their preferred school (or schools) in an LA with predominantly geographically based admissions criteria to maximise the probability of admission. This incentive is absent in an area where schools select by test scores, as geography is largely irrelevant for admission to these schools. Geography is 'largely irrelevant' in selective

[76] Parents can choose schools from other LAs by nominating them on their LAs list. The capital, London, has a single coordinated system.

[77] 'Non-geographical', 'selective' and 'grammar' will be used interchangeably.

areas as 'catchment areas', where present, are very wide.[78] Geography remains relevant to reduce commuting time to school in both areas.[79]

Figure 7.1 shows the number of grammar (selective) schools in England over time. Grammar schools were first introduced in 1944, following the extension of free education to all state secondary schools and introduction of the 'tripartite system' for secondary education. This 'tripartite system' included three types of schools: Grammar, technical and secondary modern. In practice, most pupils who did not pass the selective test at age 11 attended a secondary modern secondary school, as few technical schools opened (Kerckhoff et al., 1996).

The 'tripartite' (or effectively 'bipartite') system was largely dismantled starting from 1965, when 'circular 10/65' was issued by the Ministry of Education (under a Labour government). This circular encouraged LAs to move to non-selective or comprehensive education, providing six options for change. These options were partly informed by existing experimentation in some LAs. Indeed, Kerckhoff et al., 1996 note that two-thirds of pupils attended secondary schools in LAs that were already implementing or planning a comprehensive schools policy in 1965. See Morris and Perry, 2017 for an excellent summary of evidence for the public dissatisfaction with the tripartite system before 1965, including the lack of resources for secondary modern schools, the crudeness of the entry test, the separation of siblings and wider concerns about segregation and social justice.[80]

Figure 7.1 shows that the move away from selective education took place in LAs across England, as the number of grammar schools dramatically declines through the 1960s and 1970s. The creation of new grammar schools was outlawed in 1998 (again under a Labour government) but in practice the number of grammar schools had been largely constant since the 1980s.

What types of areas chose to maintain the selective system of education against the tide? The leading explanation, discussed further in section 7.5.1, is that Conservative controlled areas were more likely to retain the selective system, although Kerckhoff et al., 1996 conclude that it was 'far from axiomatic' (p164). Section 7.5.2 explores the characteristics of these selective areas in comparison to non-selective areas in recent years.

Although historic, it is possible that areas self-selected into alternative school admissions priorities according to observed or unobserved characteristics. This means that it is not possible to identify the effect of school admissions priorities on households' behaviour from a simple comparison across these two systems. The following section describes the methodology to identify the causal effect of geographical school admissions priorities on households' residential choices.

[78] Further evidence is presented in the following sub-section.

[79] This dis-utility of distance to school is modelled in chapter 6 across two adjacent neighbourhoods.

[80] Griffiths, 1971 describes the government reports and public discontent that led to the creation and dismantling of the tripartite system.

7.2.2 'Outside options'

Everywhere across England, private schools are an 'outside option' to the state sector, typically available to households with high incomes.[81] Table 7.1 shows the distribution of private secondary school share (unweighted by pupil numbers) across LAs in England. The mean share of private schools within an LA is around a quarter (14% at the 25th percentile and 35% at the 75th percentile). Only 10 (from 151) LAs contain no private secondary school. This type of outside option is therefore readily available for most households across England with sufficient means.

Table 7.1 shows that LAs with geographical admissions priorities have, on average, a larger share of private secondary schools (25% compared to 20%). Although descriptive rather than causal evidence, this could suggest that households use private schools as an alternative to residential mobility to gain access to a desirable school. One could hypothesise that demand for private schools is lower in non-geographical (selective) LAs as higher income households are likely to gain admission to the selective (desirable) school, which is a close substitute to a private school, at least in peer group.

Another form of 'outside option' are religious state-funded schools, that typically prioritise pupils according to religious rather than geographic/test-score criteria.[82] Across LAs in England, on average, 22% of state secondary schools have a religious denomination of some form (Table 7.1).[83] Not all of these schools will be feasible outside options for all pupils, for example those of atheist households, or households practising an alternative religion. There is evidence that some households misrepresent or strategically enhance their religious affiliation to gain entry to a desirable school, however.[84] Only 6 (from 151) LAs contain no religious state secondary school. Like private schools, therefore, this form of school is a feasible alternative option for many households across England.

Table 7.1 shows that LAs with geographical admissions priorities also have, on average, a larger share of religious state secondary schools (22% compared to 17%). There is less variation in the share across LAs with non-geographical (selective) admissions priorities, however. This is unlikely to be an endogenous response to the admissions system (decided in the 1960s and 1970s) as the presence and location of religious schools depend on more historical factors. Allen and Vignoles, 2016 describe the development of faith schools as best described as 'a late nineteenth-

[81] Some private schools offer scholarships or discounted fees. From the Independent Schools Council, only 14% of means tested bursaries and scholarships from member private schools cover fees completely, however, and 54% cover only 50% or less (Parkes, Chan, and Chan, 2021.

[82] Unlike private schools, religious state-funded schools are part of the LA application and allocation process described above. This means that private schools can act as an outside option for those unsuccessful at their preferred state school(s) after the coordinated allocation run by the LA. Religious schools can not be used in this way, but may represent and alternative option to moving home for schools in areas with geographical admissions, or passing the test in selective LAs.

[83] Around half of religious secondary schools are Roman Catholic, and one third are Church of England. The remainder are largely another Christian denomination, with a handful of Jewish, Muslim and Sikh schools.

[84] For example, a recent survey of parents found that between 20% and 37% of households across social classes knew households that had "attended church/religious services so that their child(ren) could enter a church/religious school" (Montacute and Cullinane, 2018).

century expansion, then a financially induced stagnation to 1950, followed by a final moderate growth (principally in RC [Roman Catholic] schools) in the 1950s and 1960s.'

7.2.3 Nuances in 'geographical' and 'non-geographical' areas

The paper has so far characterised LAs as having 'geographical' or 'non-geographical' (selective) admissions priorities. The discussion around outside options has already softened this distinction across areas, as households have outside options that may limit strategic incentives in response to their admissions system. A further nuance is that geography plays some role even in 'non-geographical' LAs. This is because schools in selective LAs may choose to adopt a catchment area and/or distance based tie-breakers. Table 7.2 shows that 47% of schools in 'non-geographical' LAs actually have a catchment area in their admissions policy. This compares to 53% in 'geographical' LAs.[85] Selective schools in selective LAs have catchment area as a prominent criteria - 61% have catchment area in their first three priorities, compared to 37% of non-selective schools in selective LAs. Only 6% of selective schools in selective LAs have distance so prominently, compared to 21% of non-selective schools in selective LAs.

The overall pattern in Table 7.2 is that selective schools are more likely to have catchment areas than non-selective schools in the same LAs, but less likely to prioritise pupils according to proximity. This implies that households have some incentive to reside in a particular catchment area to access their preferred selective school, but not necessarily very close to the school. To what extent can 'non-geographical' areas therefore be considered free from geographical incentives? First, note that the catchment areas for selective schools are typically very large. For example, for Bournemouth and Poole, the catchment areas for the selective schools are the size of the whole Borough. In Kent, many selective schools reserve a proportion of school places for pupils outside the catchment area, that are typically large, for example, within a 9-mile radius of the school. In Reading, the catchment areas are much larger than the city of Reading (shown in Appendix Figure B6.1). In Wirral, five selective schools have no catchment area, and one has a catchment area that is 'Wirral, Cheshire West and Chester and any other areas within 5 miles'.[86]

[85]Proximity is more often used as a tie-breaker - to decide admission between pupils that are equal according to all other criteria. This is evident from Table 7.2, as proximity rarely features in schools' first three admissions criteria (16% in 'non-geographical' LAs and 18% in 'geographical' LAs).

[86]For the other non-geographic/selective areas: Bexley, Medway, Plymouth and Torbay: there are no catchment areas for the selective schools; Buckinghamshire: the catchment areas for selective schools are around one-third of the LA; Kingston upon Thames: one selective school has a catchment area of 14km from the school, and the other school's catchment area includes 11 electoral wards and 44 postcode districts; Lincolnshire: three selective schools have catchment areas defined by a radius from the school - 6.5 miles, 12 miles, 9 miles. Also, two selective schools have no catchment area, and five are difficult to classify; Slough: one selective school has no catchment area, one has a radius of 4 miles, and one contains 42 postcode districts; Southend-on-Sea: two selective schools have catchment areas of 10 postcode districts, and two are difficult to classify; Sutton: three selective schools have no catchment area, and two reserve places for pupils outside the catchment area; Trafford: one school reserves places for pupils outside the catchment area, four schools have a catchment area of between 4 and 8 postcode districts, one school has a radius of 8 miles, and one school is difficult to classify; Warwickshire: difficult to classify as catchment areas normally constructed from parishes. Future research will geocode this catchment areas and provide quantitative comparisons across admissions areas.

Second, geography is not the primary consideration in these areas: residence in the catchment area does not facilitate admission if the pupil fails the test. Incentives for residential mobility to access non-selective schools in selective areas are muted, as the distribution of school quality for these schools is more even than in geographical LAs (presented in section 7.6.1).

7.3 Methodology

The causal effect of geographical school admissions criteria on households' neighbourhood sorting is estimated through a difference-in-differences design. The goal is to estimate the effect of geographical school admissions criteria on the frequency and location of residential choices. The hypothesis is that geographical admissions criteria increase the lifetime moves per household (as households may move into a neighbourhood for a school, and then out again once the child is admitted or has left the school) and the quality of local schools (as households have an incentive to move close to preferred schools to gain access).

The first difference is between households residing in LAs that use geographical vs non-geographical school admissions priorities. It is useful to note why this simple comparison across areas would not be sufficient to identify the causal effect. This is because, as explored in the previous section and further in section 7.5, there could have been historical non-random selection into (retaining) the non-geographical school admissions criteria. There may still be observable and unobservable differences between areas with geographical and non-geographical school admissions priorities, that are correlated with their population's residential choices. As an example, it may be that geographical LAs have lower housing costs. This might lead to more frequent residential moves, perhaps into relatively more expensive areas with better schools, and so a positive correlation between the error term and dependent variable (the frequency and location of residential moves).

A second difference is therefore used to account for the potential observable and unobservable differences between LAs with geographical and non-geographical school admissions priorities. This difference is between individuals that ever become parents vs never become parents. The reasoning is that parents value local school quality, while non-parents gain no direct flow utility from being close to good schools. 'Ever parents' are defined rather than current parents to allow households to be forward-looking and make expectations about the probability of becoming a parent. This second difference means that any systematic differences across LAs with geographical and non-geographical catchment areas are accounted for, as long as these differences are common between 'ever parents' and 'never parents' within areas.

The main estimating equation is of the general form:

$$Y_{in} = \alpha + \beta_1 GEOG_{in} + \beta_2 PARENT_i + \beta_3 GEOG_{in} * PARENT_i + \epsilon_{in} \tag{21}$$

Where Y_{in} is the dependent variable of interest for household i in neighbourhood n. The dependent variable is either at the extensive margin: the probability of moving (which implies the frequency of moves) or the intensive margin: local school quality, conditional on moving. $GEOG_{in}$ is a binary variable equal to one if the LA household i lives in around the age of 40 has geographical school admissions. $PARENT_i$ is a binary variable equal to one if individual i ever becomes a parent. More details on data construction follow in section 7.4.1. $GEOG_{in} * PARENT_i$ is the interaction of these two binary variables. The coefficient of interest is therefore β_3, the effect of ever being a parent in an area with geographical admissions on the frequency/location of residential moves. Note that $GEOG_{in}$ and $PARENT_i$ are time invariant, so there is no time subscript. The difference-in-differences design is therefore across treatment and control areas and household types ('ever' and 'never' parents) rather than time.

To give further intuition for this model, it is assumed that all households care about local rents and amenities, such as proximity to parks, shops and leisure facilities. All 'ever parents' care about proximity to school to reduce commuting costs. All 'ever parents' also value access to a good school. The key difference is that in LAs with geographical school admissions this leads 'ever parents' to place weight on location to gain school access, whereas in LAs within non-geographical admissions access is (largely) independent of location. Finally, 'ever parents' value property size more than 'never parents' given the additional space required for children.

To explain clearly how equation (21) estimates β_3, the preferences for the four different household groups are presented. First, for 'ever parents' (ep) under geographical admissions (G):

$$Y_{in}^{G_{ep}} = \alpha + \delta_1 A_n + \delta_2 R_n + \delta_3 SQC_n + \delta_4 N_n + \delta_5 SQA_n + \delta_7 SEL_n + \epsilon_{in}^{G_{ep}} \tag{22}$$

Where $Y_{in}^{G_{ep}}$ is the dependent variable of interest for household i in neighbourhood n, for 'ever parents' (ep) under geographical admissions (G). Local amenities in n are denoted by A_n and rents by R_n. These 'ever parents' also care about the commute to the local school, SQC_n, and access to the local school (dependent on location), SQA_n. 'Ever parents' also value property size, N_n. SEL represents the potential non-random selection into retaining the non-geographical admissions criteria - shorthand for all factors that are correlated with retaining the selective Grammar system and the dependent variable of interest. For example, taken from the literature, areas that retained the selective system are more likely to have been under Conservative party control. This factor could be correlated with the frequency and location of residential moves if, for example, Conservative voters are more likely to move/move to desirable locations than Labour voters. The second difference removes this selection term if it affects households that ever and never become parents equally. In this example, that both sets of households are influenced in the same way by the local political party control.

'Never-parents' (np) under geographical admissions also value local amenities, A_n, and rents, R_n, and are affected by selection into geographical admissions areas (SEL):

$$Y_{in}^{G_{np}} = \alpha + \delta_1 A_n + \delta_2 R_n + \delta_7 SEL_n + \epsilon_{in}^{G_{np}} \tag{23}$$

The equivalent equations for 'ever parents' and 'never parents' in areas with non-geographical admissions areas are the same (see equations (24) and (25), respectively), except that 'ever parents' do not value proximity to school for access and there is no selection term.

$$Y_{in}^{NG_{ep}} = \alpha + \delta_1 A_n + \delta_2 R_n + \delta_3 SQC_n + \delta_4 N_n + \epsilon_{in}^{NG_{ep}} \tag{24}$$

$$Y_{in}^{NG_{np}} = \alpha + \delta_1 A_n + \delta_2 R_n + \epsilon_{in}^{NG_{np}} \tag{25}$$

For clarity, taking the difference between 'ever parents' across admissions areas (equation (22) - (24)) and assuming strict exogeneity (the explanatory variables are uncorrelated with the idiosyncratic error across both groups) in the error terms gives:

$$Y_{in}^{G_{ep}} - Y_{in}^{NG_{ep}} = \delta_3 SQA_n + \delta_7 SEL \tag{26}$$

Taking the difference between 'never parents' across admissions areas (equation (23) - (25)) and again assuming strict exogeneity in the error terms gives:

$$Y_{in}^{G_{np}} - Y_{in}^{NG_{np}} = \delta_7 SEL \tag{27}$$

Taking the difference between the differences (equation (26) - (27)) therefore leaves only SQA_n, the effect of access to a good school through location.

The required assumptions are that 'never parents' have the same value of amenities and rents across admissions areas (δ_1 and δ_2 are common across areas for 'never parents') and that 'ever parents' have the same value of amenities, rents, commuting to school and property size across admissions areas (δ_1, δ_2, δ_3 and δ_4 are common across areas for 'ever parents'). Note that these assumptions allow 'ever parents' and 'never parents' to have different preferences for local amenities and rents. For example, 'never parents' might place greater weight on inner-city amenities such as proximity to restaurants and nightlife, while 'ever parents' might value proximity to child-centred amenities such as play parks. Regarding rents, 'ever parents' might respond differently to local rents if children reduce disposable household income and the marginal utility of consumption is non-linear. Examples such as these do not violate the assumptions of this model. To reiterate, the model requires only common preferences between 'never parents' across admissions areas, and, separately, 'ever parents' across admissions areas. Also, that any area-level factors that affected retaining the non-geographic admissions system affect 'ever' and 'never' parents in the same way.

To estimate the residential choices of households across the lifecycle, the coefficients are interacted with binary variables for five-year age bands (denoted by AGE):

$$\begin{aligned} Y_{int} = \alpha + \gamma_1 AGE_{it} + \gamma_2 GEOG_{in} + \gamma_3 PARENT_i + \gamma_4 GEOG_{in} * PARENT_i + \\ \gamma_5 GEOG_{in} * AGE_{it} + \gamma_6 PARENT_i * AGE_{it} + \gamma_7 GEOG_{in} * PARENT_i * AGE_{it} + \epsilon_{int} \end{aligned} \tag{28}$$

In this equation there is a time subscript t to allow the household's age to vary across the lifecycle. This specification makes it possible to estimate how school admissions criteria affect households' residential decisions across the lifecycle. The hypothesis is that residential choices might be particularly sensitive to admissions priorities when dependent children approach secondary school age, although moves might happen at earlier stages if households are forward-looking and they try to reduce the number of (costly) moves.[87]

It is novel to estimate whether there is movement *away from* school quality later in life. This unstudied fact is important to interpret the existing estimates of the property price premium around good schools, because the estimates are muted by exits from the neighbourhood after the child is successfully admitted or has left the school. The regression is also run for particular sub-groups to explore *who* makes endogenous residential choices in response to geographical admissions criteria.[88] Again, this is important to interpret existing estimates of the property price premium around good schools: is this driven by a minority of households or households across the distribution?

Threats to identification include the violation of strict exogeneity. One potential concern is that there are other neighbourhood attributes that households value that are correlated with school quality. This would induce a correlation between the error term and the independent variable of interest. For example, popular schools may attract child-centred businesses to the area, that are valued by parents. This is not problematic as long as the relationship between unobserved amenities and school quality is the same across areas with geographical and non-geographical admissions priorities, which seems plausible.

Another potential threat is that 'ever parents' have different preferences across areas with geographical and non-geographical admissions, for amenities, rents, commuting to school and property size. This assumption could be violated if there is non-random selection by households

[87]Chapter 6 studies these dynamic considerations in depth, through a structural model of neighbourhood and school choices with forward-looking households, that anticipate moving and travel costs.

[88]In this case, the identifying assumptions have the same structure: households within each sub-group must have the same preferences within 'never parents' and 'ever parents', as described above.

into admissions areas, or if there are differences in incomes across areas that reflect the historical selection into geographical admissions. Differences in average incomes across admissions areas, combined with non-linear marginal utility of consumption, would lead households to have different preferences for local rents and property size. This potential threat to identification is considered in section 7.5, following a description of the data used.

Note that in this specification, the 'treatment' is a time invariant attribute. This has the advantage of avoiding 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). The difference-in-differences specification is run without the inclusion of covariates, that require additional assumptions (Sant'Anna and Zhao, 2020).

7.4 Data

A cohort study is used to track residential choices across the lifecycle. The Office for National Statistics (ONS) Longitudinal Study (LS) is a longitudinal study of a 1% sample of the whole population of England and Wales.[89] England has had a national Census every 10 years since 1801, except for in 1941 during the Second World War. The LS began in 1971 and covers the following four Censuses (1981, 1991, 2001 and 2011) for people usually resident in England and Wales, and born on one of four birth dates. Sample members can enter the LS at each Census through birth and immigration and exit through emigration or death (leavers' existing data is retained). Only LS sample members are linked across Censuses, but information about other members of the household is collected at each Census.[90] From this information, variables are constructed at the household rather than individual level, for example the maximum level of education of parents in the household. These are described in section 7.4.1.

The LS is well-suited to this study because it is large (over 500,000 people in each Census and over 1,000,000 people across the whole dataset), nationally representative, and longitudinal. The longitudinal design means that residential moves can be studied after children have left school, before children start school, and even before children are conceived. This is important if households have expectations about the future, for example if they expect to have children and therefore care about local school quality.

To investigate the link between residential mobility and local school quality, local area characteristics are merged to the LS. These are the quality of local schools, the local school system (geographical or not), and local property prices. To preserve the anonymity of the LS sample members, these local area characteristics must be non-disclosive, in that any combinations of the area characteristics do not uniquely identify any small areas. These variables are described

[89] The ONS LS also contains linked life events data (for example births to sample members) but these are not used in this paper.

[90] The linkage rate between Censuses is high, ranging from 87.7% between the 2001 and 2011 Censuses and 91.3% between the 1971 and 1981 Censuses (Lynch et al., 2015). In the final sample used in this paper, around 72% of LS sample members are present across all five Censuses.

fully in section 7.4.2. A key point to note here is that these area characteristics of interest are available from the 1991 Census. It is therefore possible to study household moves in relation to area characteristics only from this Census. Earlier Census years are used to define household attributes.

7.4.1 Derived variables in the ONS Longitudinal Study

Three cohorts of interest are defined, and presented in Table 7.3. The first cohort is LS sample members aged between 20 and 30 in the 1991 Census, so ranging between 0 and 50 across the longitudinal data. This cohort is used to study household moves in early life, for example as households form and children are born and start school. The second cohort is LS sample members aged between 30 and 40 in the 1991 Census, so ranging between 10 and 60 across the longitudinal data. The third and final cohort is those aged between 40 and 50 in 1991, so ranging between 20 and 70 in the longitudinal data. This cohort is used to study moves later in life, as dependent children leave secondary school, for example.[91] Focusing on these three cohorts selects 58% of the total available sample. The final sample also excludes those with missing key covariates and focuses only on the later Census years. Appendix Table A6.1 shows the sample restrictions applied for the final analysis sample in detail.

The following derived variables at the household level are created to have consistent categories across Censuses: completed fertility; social class; immigrant status; education; ages of dependent children. For each variable in each Census, information from only the LS sample member is used where the LS sample member is single, and information from two household members is used if the LS sample member is part of a couple (married or cohabiting). Figure 7.2 shows the time-varying derived variables over Census years and cohorts, for each cohort starting when the LS sample member is an adult. Only completed fertility and social class are used in the empirical analysis. The other variables are used to describe the sample only.

The presence of a partner is coded as whether a household member is recorded as a spouse or cohabiting partner. Panel (a) of Figure 7.2 shows that LS sample members in each cohort are increasingly likely to live with a partner as they age. For a given age, younger cohorts are less likely to live with a partner.

Panel (b) of Figure 7.2 shows that the households of LS sample members are increasingly likely to be owner occupiers as they age, particularly to own outright.[92] Again, this is less likely for younger cohorts at a given age. The proportion living in socially rented housing decreases across cohorts and Censuses.

[91] Some cleaning of ages recorded in the Censuses is required. For example, age does not increment by 10 years across Censuses for 13% of LS sample members. This is re-coded where possible, where there is a clear pattern and clear anomaly to be fixed. Only 0.04% of cases are dropped from the final sample as it is not possible to infer the correct age sequence across Censuses - see Appendix Table A6.1 for full details.

[92] Note that it is not possible to separate the categories of 'owns outright' and 'owns with mortgage' for the 1971 Census.

Immigrant status is aggregated into 'born in the UK' and 'not born in the UK'. Where the LS sample member is part of a couple, three categories are defined: 'both born in the UK', 'one born in the UK' and 'none born in the UK'. Note that 'both born in the UK' and 'one born in the UK' therefore combine the effects of couple formation and origin of birth. Panel (c) of Figure 7.2 shows that most households contain a couple where both were born in the UK. This percentage is increasing across Censuses and as cohorts age, which could be driven by couple formation. The youngest cohort are most likely to have one individual born outside the UK.

Education is defined coarsely to be consistent across Censuses, to be whether the individual has a degree. As for social class, this measure is defined to be the highest of the couple where the LS sample member is part of a couple. Panel (d) of Figure 7.2 shows that the percentage of households that have at least one adult with a degree increases across Censuses. This is true for each cohort, so that in 2011, around 40% of all households have at least one adult with a degree.

Social class is the Standard Occupational Classification (SOC), that has categories defined consistently across Censuses. Where the LS sample member is part of a couple, the highest SOC is taken. Panel (e) of Figure 7.2 shows that the percentage of households with each social class category is roughly constant across Censuses, with slight increases in the percentage of households with 'Intermediate' occupations and slight decreases in the percentage with 'Skilled manual' occupations.

Finally, panel (f) shows that as households in cohort 3 age, they are increasingly likely to have only one or no adults in work, particularly as they reach retirement age in 2011. Cohort 1 shows that households are increasingly likely to have two adults in work as they age, partly due to household formation and partly due to employment status.

Turning to a time-invariant variable, completed fertility is coded as whether the LS sample member is ever observed living with a dependent child in a Census. This definition applies to male as well as female LS sample members, as the dependent child(ren) can be born to another household member, and includes step-children. This definition will have some measurement error, as some parents may never live with their dependent child on a Census date, but it will correctly classify most sample members as 'ever' or 'never' parents. There is also measurement error in the expectation of becoming an 'ever parent', as some households would like/expect to become parents and are unable to, while others become parents unexpectedly.[93]

The age of dependent children is grouped for ease of interpretation to focus on secondary school aged children. That is, at each Census date, whether the household has dependent children below, at, or above secondary school age. Figure 7.3 shows the proportion of households with a dependent child (panel (a)) and dependent child of secondary school age (panel (b)) by age of

[93] The National Institute for Health and Care Excellence in England states that 'infertility affects 1 in 7 heterosexual couples in the UK'. For these couples, the success rate of IVF varies by the age of the women, decreasing from 32% for women under 35 to 4% for women aged over 44 (NHS). Other couples will choose to adopt. In the alternative case, Wellings et al., 2013 estimate that around 15% of pregnancies are unplanned, but these unplanned pregnancies may only shift the timing of birth rather than the overall likelihood of becoming a parent.

the LS sample member. The average age for LS sample members to be first observed as a parent is 31-32 (with only slight variation across cohorts, where younger cohorts have children later, on average). This in turn means that children first start secondary school when the parent is 41-42, on average.

To allow comparison between 'ever parents' and 'never parents', a 'key age' is defined at which most households that are 'ever parents' have children. All the variables described above are recorded at this 'key age', which is the closest Census to when the LS sample member is aged 40. For example, if an individual was 34 in 1991 and 44 in 2001, then the variables from the 2001 census would be used to define their 'key age' variables.

Table 7.4 shows summary statistics for the 'key age' variables in the ONS LS for the three cohorts of interest. As for Figure 7.3, social class around age 40 is largely constant across cohorts, with a minority classified at the tails ('professional' and 'unskilled') and between 19% and 25% across other categories. The percentage with a degree at age 40 is higher for younger cohorts, ranging from 39% for cohort 1 to 24% for cohort 3. Younger cohorts are less likely to live with a partner at age 40 (83% compared to 90% for cohort 3) or ever become parents (77% compared to 88% for cohort 3). Younger cohorts are also less likely to have all adults in the household born in the UK (76% compared to 81% for cohort 3). Local school quality and property prices (defined below) are roughly constant across these cohorts around age 40.

7.4.2 Local area characteristics

All local area characteristics are ultimately defined at the lower level super output area (LSOA), based on the 2011 LSOA boundaries. There are over 30,000 LSOAs in England, each with a minimum of 400 households and maximum of 1,200 (ONS). These small geographical units are used as the building blocks for classifying small area characteristics across Census years. A Geographic Information System (GIS), QGIS, is used to make consistent look-ups between boundaries over time and differently sized geographies.

For school quality, the LSOA is classified according to the school closest to the population-weighted mid-point of the LSOA. For property prices, the LSOA is assigned the mean property price of properties sold in the LSOA. For the admissions system, each LSOA is assigned to the LA that it has the largest overlap with. 6.6% of observations have no linked area characteristics, and so are dropped from the final sample. Each data source is now described in turn.

7.4.2.1 School quality

Publicly available information on secondary school performance is downloaded from the official statistics available on the 'Find and compare schools in England' website. The only consistent school accountability measure across the census years is the percentage of pupils that achieve at least 5 GCSE grades at A* to C (including English and Maths). The average school performance across academic years close to each census year is recorded for each school.[94] This measure of school quality - raw academic results - includes the contribution of the school's intake and school effectiveness, rather than a measure of pure 'value-added' by the school. In other words, a more advantaged pupil intake leads to higher school quality for the measure used in this paper. This is especially relevant in a selective admissions system, as the intake of the school is by definition the highest ability. It is also relevant in a geographical admissions system, however, as sorting into 'good schools' through residential location by higher-income households would amplify differences in measured school quality.

School quality measures are converted into deciles to preserve the anonymity of the ONS LS. Another version of the school quality data is also transformed to be the ranking (in deciles) of the school within the Local Authority. This allows finer distinctions between local schools. For example, historically, secondary schools in London had lower performance, on average, than other regions in England. In this era, this would mean that many schools in London would be recorded as being in the lowest deciles of national performance. Since the 2000s, however, schools in London have had higher performance, on average, and so now would disproportionately be recorded as being in the highest deciles of national performance. A local measure of school quality would instead identify schools as relatively higher or lower performing within their area, rather than being grouped together using a definition at a national level.[95]

Figure 7.4 shows school quality, defined at the national level, for 2011. Panel (a) shows school quality across LSOAs in England, while panel (b) focuses on LSOAs in London and the South East. Figure 7.5 shows the equivalent figures using the local measure of school quality: the academic ranking of the school *within* the LA. This second measure is used in the empirical analysis.

7.4.2.2 Admissions system

The admissions system is classified at the Local Authority level, to be geographical or non-geographial. For this paper, 'non-geographical' equates to 'selective' and 'grammar', that are used interchangeably. The classification is derived from the school performance tables described in the previous sub-section, that record whether each schools' admissions policy is 'selective', 'modern' or 'non-selective'. 'Modern' is the equivalent school to the 'selective' in a grammar school system that takes the pupils that do not pass the selective test. LAs are defined as non-geographical if at least 25% of schools are classified as 'selective' or 'modern'. To preserve anonymity of the ONS LS and for ease of interpretation, four (out of 152) LAs were re-coded to have consistent classifications of selective or not across Census years, by selecting the modal

[94]For the 2011 Census, academic years 2009/2010 and 2010/2011 are taken. For the 2001 Census, academic years 1999/2000 and 2000/2001 are taken. For the 1991 Census, academic years 1993/1994 and 1994/1995 is taken, as data for earlier academic years is missing or largely missing.

[95]To ensure anonymity, 14% of values were further re-coded to the closest alternative value. This introduces some measurement error into the variable.

classification across years.[96] So few LAs are re-classified as the grammar school system has remained largely unchanged since the 1960s, and new grammar schools were banned in 1998, as described in section 7.2.

The LAs classified as selective are shown graphically in Figure 7.6 and listed in the notes to this figure.[97] These selective LAs are spread across England, but are primarily in the South. Section 7.5 provides a description of these two types of admissions areas in relation to the methodology described in section 7.3.

As for household characteristics described in section 7.4.1, a time invariant measure of the admissions system is coded at the 'key age' around 40. This classifies households as living in a geographic or non-geographic LA in the Census year that is closed to their 40th birthday.

7.4.2.3 Property prices

Property prices are taken from the Consumer Data Research Centre, that in turn are derived from the Price Paid Data from HM Land Registry, which covers all property sales in England and Wales that are sold for full market value and are lodged with HM Land Registry for registration. These Consumer Data Research Centre data contain median property prices for all properties sold in the LSOA in a given quarter. As for school quality, data is collected for the period around the Census year, taking the mean price across the calendar year. In this case, the calendar years are 1991, 2001 and 2011, respectively.

Figure 7.7 shows property prices at the LSOA level across England in 2011. Prices are typically higher in the South and in urban areas, but there is variation within each LA.[98]

7.4.2.4 Census 2011

Publicly available data from the 2011 Census at the Lower Level Super Output Area (LSOA) from nomis are also used to describe selective and non-selective Local Authorities, but is not linked to the ONS LS to preserve anonymity. These data include: age composition, social class, and the presence of dependent children.

[96]Some LA boundaries were re-drawn between the 1991 and 2001 Censuses. Using GIS to classify postcodes into consistent LA boundaries leads to some re-classification of areas into selective or not. Half of ten LAs' classification as selective or not are affected by changing boundaries.

[97]This classification is very similar to previous research on the grammar school system in England. For example, almost all LAs have the same classification as that in Burgess, Crawford, and Macmillan, 2018, which is based on the percentage of pupils in 1983, rather than the percentage of schools in later (Census) years. Burgess, Crawford, and Macmillan, 2018 include Liverpool as selective and exclude Reading and Warwickshire. In a later unpublished exercise, also based on pupil numbers, Crawford includes Wirral as selective and excludes North Yorkshire, but the other Local Authorities are consistent with the definition used here. Cribb, Sibieta, and Vignoles, 2013 classify LAs as selective if at least 10% of pupils attend a grammar school. This definition leads to largely to the same classification, the only differences being the exclusion of Warwickshire and inclusion of Gloucestershire and Calderdale.

[98]To ensure anonymity, 28% of values were further re-coded to the closest alternative value. This introduces some measurement error into the variable.

7.4.2.5 Dependent variables

There are two main dependent variables. First, for the extensive margin, the variable of interest is a binary variable equal to one if the LS sample member moves between Censuses and zero otherwise. This variable has some measurement error. First, the LS sample member might move more frequently between Census years. That is, the Censuses reveal a maximum of one move every 10 years, but households might in fact move multiple times within those 10 years. The total number of moves observed across the lifecycle across the data is therefore a lower bound to the actual total number of moves. Second, the definition of 'move' varies slightly across Census years.[99] In future work, with collaboration from ONS, it may be possible to create a consistent variable across Censuses, using movement across LSOAs.

Figure 7.8 shows the percentage of LS sample members that move between Censuses, by age and by cohort. For all cohorts, moving at least every 10 years (the time between Censuses) is very common. Over 80% of households are observed to move around age 30. This monotonically declines with age, to around 40% around age 50. Descriptively, it seems that households are more likely to move around the time children are born or start school than later in life.

The second dependent variable of interest is the local school quality of the LSOA of residence (described in section 7.4.2.1).

7.5 Identifying assumptions

Section 7.3 outlined the identifying assumptions required to estimate the causal effect of geographical admissions priorities on households' residential location decisions in the difference-in-differences design. Having presented the data in section 7.4, this section now examines the plausibility of these identifying assumptions. First, it is required that area-level selection into retaining the non-geographic admissions system affects 'ever' and 'never' parents in the same way. This is examined in subsection 7.5.1. Second, it is required that the group of 'ever parents', on average, have the same preferences across admissions areas, and likewise for the group of 'never parents'. Differences in preferences within groups across areas could be driven by two factors. First, whether there was historically non-random selection at the area-level into geographical admissions priorities that lead to differences in residents' choices, for example due to differences in political control or property prices. This is examined in subsection 7.5.1. Second, whether there is non-random selection by households into admissions areas that is correlated with preferences, for example stronger preferences for school quality. This is examined in subsection 7.5.2.

[99] Between 1971 and 1981, 'move' is coded to one if the LS sample member's address is different. Between 1981 and 1991, 'move' is coded to one if the LS sample member moved more than 500m. Between 1991 and 2001, the definition is more vague: 'move' is coded to one if the LS sample member is 'assumed to have moved'. Finally, between 2001 and 2011, 'move' is coded to one if the LS sample member moved more than 250m.

7.5.1 Historical evidence of non-random selection

This section discusses evidence drawn from elsewhere about the potentially area-level non-random selection into retaining the non-geographical (selective) admissions system. Recall that the chosen research design for this paper does not require balance between all residents in geographical vs non-geographical admissions areas, only that the selection factors into non-geographical admissions areas are the same between 'ever' and 'never' parents, and that historical selection doesn't alter current preferences within 'ever' and 'never' parent household types. This is helpful, as Pischke and Manning, 2006 conclude that the selection problem is not solved through 'careful choice of treatment and control areas' or 'using political control of the county as an instrument for early implementation of the comprehensive regime'.[100]

Writing in 1971, at the time of transition, Griffiths, 1971 summarises that:

> It is no accident that comprehensive schools have development most quickly in urban areas which suffered extensive war damage necessitating radical rebuilding of shattered schools, and in thinly populated rural areas where the provision of the full range of secondary education required large multi-purpose schools in which teachers and facilities could be most effectively and economically deployed.

In their detailed study of the transition to comprehensive education across Local (Education) Authorities between the 1940s and 1970s, Kerckhoff et al., 1996 agree that structural factors affected the pace of change, such as the extent of bomb damage that determined the need for new school building, perhaps in combination with housing policy. The level of funding/resources at the local level could also delay the implementation of plans, as the move to comprehensive education was not funded by central government.

The other leading explanation is the local political control, with more 'progressive' Labour controlled areas more likely to give support to comprehensive education. From Galindo-Rueda and Vignoles, 2004, where 'LEA' refers to Local Education Authority, equivalent to Local Authorities today:

> Kerckhoff et al. (1996) showed that the political orientation of the LEA was crucially important. Specifically, LEAs that had Conservative political control experienced slower change towards mixed ability schooling than LEAs under Labour control. Furthermore, LEAs under Labour control initially but that then switched to Conservative control, appeared to have been able to reverse or slow plans to move towards comprehensive schooling.

Kerckhoff et al., 1996 conclude that although local political control was an important pre-

[100]Looking at the data retrospectively, Pischke and Manning, 2006 find that areas that moved to geographical admissions priorities are 'systematically poorer, and have students with lower previous achievement'.

dictor of the pace of change, it was 'far from axiomatic' (p164), however.[101] There were also idiosyncratic factors such as the role of individuals, for example the persuasion of the local Chief Education Officer (that may or may not align with political party affiliation). The presence of local pressure groups and views of the public were also factors, that were in turn influenced by the views of the local press (and perhaps vice versa). Where the local press had vested interests in the preservation of the grammar schools (perhaps as successful products of the system) they were more likely to present negative information about the transition to a comprehensive system. Kerckhoff et al., 1996 summarise that each of the ten Local Education Authorities in their study had a unique combination of these factors that affected the speed of implementation.

Burgess, Dickson, and Macmillan, 2019 discuss the issue of non-random selection in depth in order to motivate the matching variables for their research design. These authors choose the following variables to find the nearest neighbour match for each selective LA. First, political control at the local level, that is cited as one of the most important determinants of the pace and nature of re-organisation of the schooling system during the 1960s and 1970s. Burgess, Dickson, and Macmillan, 2019 also match according to area characteristics in the 1981 Census (the proportion of residents with a degree, high social class and in employment) and time-varying economic characteristics (local unemployment rate and local male hourly wage rate). Cited in Galindo-Rueda and Vignoles, 2004, Kerckhoff et al., 1996 find that, surprisingly, such economic variables explain 'less than 5% of the variation in the proportion of LEA state supported schools that were comprehensive'. Only the pupil teacher ratio is a significant predictor of transition in some specifications, with better resourced areas shifting towards mixed ability schooling sooner.

This sub-section has discussed the existing evidence for potential area-level characteristics that determined area-level selection into geographical (comprehensive) or non-geographical (selective/grammar) admissions priorities. The historical evidence presents some reasoning for differences in the speed of implementation across areas, but concludes that there were numerous independent and varying factors across LAs. As noted above, any historical differences between areas that removed versus retained selective admissions are *not* challenges to the identification strategy used in this paper unless they *differentially* affect households that ever vs never become parents.[102] Or, that they might lead 'ever' or 'never' parents to have different preferences across admissions areas. The discussion in the literature to date reveals no such concern, with the caveat that the most detailed studies focus on the pace of change from the old selective system, rather than (most relevant to this study) whether LAs largely or completely retained it.

The following sub-section conducts new tests to study non-random selection of households into and out of geographical admissions areas in recent years. Selection of this kind would be problematic if it implies that 'ever parents' have different preferences (for school quality or

[101] For example, the Labour Prime Minister, Harold Wilson, was reported to have said that grammar schools would be abolished 'over my dead body'. See footnote 6 on page 26 of Kerckhoff et al., 1996.

[102] Differential selection by either 'ever' or 'never' parents would mean that the selection term, SEL, in equations (22) and (23) would not cancel, leading to bias in the main estimates.

household size, for example) across admissions areas.

7.5.2 Current non-random selection

Figure 7.9 shows the balance of detailed characteristics across areas. Overall, there are few cases where the population of geographical LAs appears different from non-geographical LAs, on average. The exception is the age structure across areas, as non-geographical areas have fewer young people (25-29 and potentially 20-24) than geographical areas. Recall, however, that balance in all observed and unobserved characteristics across admissions areas is not necessary for identification. Imbalance is problematic only if it implies that 'ever parents' differentially select into non-geographical admissions areas compared to 'never parents', or that 'ever parents' inside non-geographical areas have different preferences to those outside.

To explore this, Table 7.5 shows the characteristics of households that are always resident in geographical or non-geographical admissions areas, and the characteristics of households that move between admissions areas over the life-course. The majority (around 80%) of households are always resident in LAs with geographical admissions priorities. Around 7% of households are always resident in LAs with non-geographical (selective) admissions. Roughly equal proportions of households move into and out of LAs with non-geographical admissions over their life-course (6% and 5%, respectively) and a minority move both in and out (1%).

There are few significant differences between households that move in and out of geographical admissions areas (columns 3 and 4). The exception is that households that switch *to* non-geographical (selective) areas are more likely to be 'ever parents' than those that switch *from* non-geographical areas for the first and third cohorts. Overall, the population that moves to and from non-geographical admissions areas is similar, which provides encouraging evidence that there isn't systematic selection into admissions areas according to observable household characteristics. The small population that move in both directions are also largely comparable, with a few exceptions.

There are some observable differences between households that move between admissions areas versus always live within one type of admissions area over their life-course. For example, households that are always in one admissions area are less likely to have professional and intermediate social class, and less likely to have a degree than those that switch, on average. Households that are 'stable' in one admissions area are more likely to be 'ever parents', however, suggesting that the patterns are not driven by households with children.

This might reflect the characteristics of more mobile households in general, rather than households that move across admissions areas, however. Appendix Table A6.2 shows the summary statistics for the groups of households, conditional on moving at least twice. This removes the potential effect of characteristics associated with households that move often. For this sample, there are very few differences across the population always remaining within geographical and non-geographical areas, moving in either direction, or moving in both directions. The exception is the social class of the household, where there are larger differences between households that are stable versus move across admission area types. For example, the proportion of households with 'Intermediate' social class in cohort 1 is 0.23 and 0.19 for households that always live within geographical and non-geographical areas, respectively, and 0.31 for households that move in either direction across admissions areas. The balance between those that move in either direction suggests that there is not systematic selection into non-geographical areas according to observable characteristics, however.

Unfortunately, it is not possible to rule out that there are no unobservable differences between these households. For example, parents with strong preferences for school quality and a child with high ability may select into a selective admissions area to access the grammar school. This assumption therefore remains untestable, but plausible given the overall balance in observable characteristics.

7.6 Results

This section presents the main findings for the causal effect of geographical admissions priorities on households' residential decisions. First, descriptive evidence on the relationship between property prices and school quality across LAs with geographical and non-geographical admissions priorities is shown. The effect of selective admissions criteria on the distribution of schools quality within an area is also presented.

7.6.1 Descriptive evidence

There is a strong correlation between local school quality and local property prices in LAs with geographical admissions criteria. Table 7.6 shows that an increase of one decile in local school quality is associated with around a 0.4 increase in the decile of local property prices. One explanation for this is that there is higher demand for properties around 'good' schools, as parents try to secure access to them through proximity. The relationship in Table 7.6 can not be given a causal interpretation, however, as there is likely to be reverse causality. That is, higher local property prices imply higher incomes for local residents, which in turn imply higher ability local children, on average.[103] Recall that the raw measure of 'school quality' used in this paper is a combination of the school's pupil intake/ability and the school's value added.

Table 7.6 shows that the correlation between local school quality and local property prices declines with the share of schools that have non-geographic/selective admissions priorities. In areas with the highest concentration of selective schools, for example, the correlation is less than 0.1. The interpretation is that a one decile increase in local school quality is associated with an increase in property prices of around one tenth of a decile. This is preliminary suggestive

[103] See the introduction and Gibbons and Machin, 2008, Black and Machin, 2011 and Nguyen-Hoang and Yinger, 2011 for detailed summaries of causal evidence of the relationship.

evidence that sorting across neighbourhoods in response to school quality is less prevalent in areas with non-geographical admissions.

Figure 7.10 shows the distribution of school quality across areas with geographical and non-geographical (selective) admissions. As school quality is defined by test scores, and selective schools admit pupils with higher initial test scores, it follows that the distribution of school quality is more unequal in LAs with selective admissions. Note that the uneven distribution of measured school quality is likely due to the uneven allocation of pupils by ability, rather than differences in school-effectiveness. For example, recall that the causal evidence on test scores for the effect of being marginally admitted to a selective school is close to zero (Clark, 2010).

In non-geographic/selective LAs, more than 25% of schools are in the top decile of national school attainment. That is, around a quarter of schools in these LAs are among the highest attaining in England. The distribution is bi-modal, however, with just under 20% of schools in these LAs in the *bottom* decile. The implication of the selective system is that a pupil will attend a school with either among the highest or lowest test scores in the country. This in turn provides incentives for households' behaviour. If a child does not pass the '11-plus' exam to gain entry to a prestigious secondary school, the child will be assigned to a school with low-performing peers, or exit to the private sector. This pattern is also evident in panel (b) of Figure 7.4, where non-geographical admissions areas (highlighted in red) have more inequality in school quality across LSOAs (represented by very light and very dark blue) than geographical areas.

In contrast, the distribution of school quality in LAs with geographical admissions is more even, with roughly 10% of schools in each decile defined nationally. The exception is the top decile, where fewer schools in geographical admissions areas feature, given the dominance of schools in non-geographical (selective) areas. In these areas, parents can choose to make strategic residential moves to gain access to a higher-attaining school. The following section tests whether geographical admissions priorities have a causal effect on households residential choices, at which stage of life and for which households.

Figure 7.11 shows the difference in the probability of moving across Censuses for 'ever parents' and 'never parents', by age and by cohort. For each cohort, the percentage of 'ever parents' with a child of secondary school age is shown in the lower panel. These patterns show that 'ever parents' are more likely to move than 'never parents' at younger ages, typically before most households have a child of secondary school age. This is suggestive that households who ever have children make different choices at the extensive margin. The following section will interrogate whether this difference is causal - due to the geographical admissions priorities - or explained by other factors that differ between 'ever parents' and 'never parents', for example demand for property size.

7.6.2 Causal evidence

The estimated difference-in-differences models are shown in full in Appendix Tables A6.3 and A6.4 and marginal effects are shown in Appendix Figures B6.2 and B6.3.

Table 7.7 shows the interaction effects of interest for the extensive margin: the probability of moving between Censuses. For each cohort, the reference group is households aged 41 to 45. For all cohorts, there are no significant effects of living in a geographical admissions area and being an 'ever parent' on the probability of moving between censuses. The point estimates are also small. For example, the coefficients of 0.002 and 0.020 for households aged 31-34 for cohort 1 and 2, respectively, imply a 0.2 and 2 percentage point increase in the likelihood of moving between censuses, relative to households aged 41 to 25. Overall, there is little evidence that households that ever have children move more frequently in areas with geographical admissions priorities.

For the intensive margin (the school quality of the nearest school) the interaction effects shown in Table 7.8 reveal no significant differences. That is, on average, households in areas with geographical admissions priorities do not live closer to schools that are relatively highly ranked. The full model shows that, overall, local school quality is significantly higher for those living in areas with geographical admissions priorities (Appendix Table A6.4). This may be due to the bi-modal distribution of school quality in non-geographical (selective) areas shown in Figure 7.10. That is, school quality in geographical areas has less variation (as measured by raw test scores) than in non-geographic/selective areas, where local school quality is typically at the tails of the distribution. On average, this leads to higher local school quality in geographical admissions areas.

These results reveal that, overall, there is little evidence of household sorting across neighbourhoods to a greater extent in areas with geographical compared to non-geographical (selective) admissions. This is true at the extensive margin, considering whether households move at all, and at the intensive margin, considering the local school quality of the destination moved to. This is perhaps unsurprising. First, for the extensive margin, over 80% of households move between Censuses at young ages. At such a high baseline level, it may be difficult to detect additional moves due to an area's school admissions priorities. Second, at the intensive margin, the average effect includes all movements for all households. If population density is similar around schools with higher and lower quality, it must be the case that some households move near to higher quality and some to lower quality schools. It is therefore important to test for heterogeneity in the response across household types.

The sample is split according to social class, as this variable is stable over time and across cohorts (Table 7.4). Tables 7.9 and 7.10 present the equivalent results for the sample of households with 'Professional' and 'Intermediate' social class only. The full results are shown in Appendix Tables A6.5 and A6.6. Table 7.9 shows that the probability of moving is unaffected by geographical admissions priorities for the oldest cohort, even for households of the highest social class.

Households in cohort 1 are more likely to move between Censuses at ages 26 to 30, relative to 41 to 45, if they are ever parents and resident in an LA with geographical school priorities. The coefficient for 31 to 35 is also of similar magnitude, although not statistically significant. This pattern could suggest that for this younger cohort, moves are shifted close to average childbirth age for those that expect to become parents. This cohort (and cohort 2) are also more likely to move between Censuses at ages 46-50, which for many households would be as children leave secondary school. This could be tentative evidence that this high social class sub-group make more frequent moves after the school phase, although this pattern is not evident for the oldest cohort.[104] Overall, these results imply that considerations about school access largely do not affect *whether* a household chooses to move, even for relatively advantaged households.

Table A6.6 presents some limited evidence that the location of moves is influenced by the admissions system for households with high social class. This implies that this group might consider the location of moves in relation to local school quality, if not the timing of moves. The evidence is not overwhelming, however. Households in cohort 2 aged 36 to 40 are more likely to live close to schools with higher school quality than households in the reference age group in geographical admissions areas (around 0.7 of a decile) than households in non-geographical admissions areas, with borderline statistical significance. For most households, this is just before their first child begins secondary school. Cohort 3 shows suggestive evidence that older households move to areas with lower school quality later in life. This is consistent with lower flow utility from local school quality as children age and exit secondary school. Despite the large sample size, these effects are not strongly significant or economically meaningful.

The findings therefore appear to contradict well-established and robust existing literature that finds evidence of property price premiums around good schools, that it is assumed are driven by greater demand for properties by parents seeking admission for their child. Three factors might reconcile the findings. First, the measurement error in the dependent and independent variables used in this paper could cause downward bias in the estimates. The standard errors are large, despite the large sample size. Further research using continuous variables, in collaboration with the ONS, would test the sensitivity of these estimates to measurement error induced by the requirement to make the data anonymous.

Second, the existing literature uses Boundary Discontinuity Design to estimate the local price premium immediately around the catchment area boundary for high performing schools. This paper, instead, uses variation across all neighbourhoods, inside and close to catchment area boundaries, using 'never parents' and non-geographical admissions areas as the counterfactual, rather than neighbouring areas. This has the advantage of having higher external validity, if households that select to be marginally inside the boundary are different to those choosing to locate closer to the school. The disadvantage is that this approach might abstract from very

[104] In fact, for cohort 3, there is a marginally negative coefficient for this age, suggesting that 'ever parent' households are less likely to move at this stage of life in geographical admissions areas.

local effects at the boundary.

Finally, this paper studies the whole of England rather than particular over-subscribed schools. For example, Bayer, Ferreira, and McMillan, 2007 estimate boundary discontinuities for the subset of boundaries where the 'test score gap comparing low and high sides is in excess of the median gap (38.4 points)', reasoning that 'if schools were identical on either side, there would be little reason to expect to see sorting'. Similarly, Kane, Riegg, and Staiger, 2006 choose boundaries where there is at least a 0.25 student-level standard deviation difference in mean test scores. Ries and Somerville, 2010 note that their identification of school effects is from 'a limited number of areas where the changes in school quality were significant'. It could be that the positive effect for a minority of popular schools is muted by the majority. This would imply that sorting across neighbourhoods in response to local school quality is not a widespread phenomenon, but instead concentrated around particularly high-performing schools. Gibbons and Machin, 2006 find evidence consistent with this, finding a price premium for properties close to primary schools at the top of the attainment distribution. Chan et al., 2020 and Agarwal et al., 2016 also find a non-linear school quality premium in urban China and Singapore, respectively.

In addition to occurring for a minority of schools, the property premiums could be driven by a minority of households with strong demand for school quality. This is in contrast to the received wisdom that schools are a driving factor for many households' residential decisions, although note that this minority of households could still drive equilibrium outcomes. Bayer, Ferreira, and McMillan, 2007 conclude that heterogeneous preferences for neighbourhood composition, in addition to heterogeneous preferences for school quality, would generate strong 'second-round 'social multiplier' effects on prices', that are potentially stronger than direct effects of school quality.

7.7 Summary and discussion

The design of an education system has important implications for students' learning, later life outcomes, and ultimately total welfare in society. This is because different design choices influence efficiency and equity in public schools, and the level of segregation or integration between groups of pupils. This paper addresses another important, and understudied, consequence of education system design choices: neighbourhood formation. The intuition is that how schools choose to admit pupils if oversubscribed affects parents' residential choices. In particular, geographical admissions priorities can induce households to move close to their preferred school to gain access. As demand around desirable schools increases, so do property prices, increasing inequality in school access between more and less affluent households.

Previous empirical work has clearly, consistently and robustly demonstrated this property price premium around popular schools with high pupil attainment. But less is known about how this process ultimately affects neighbourhoods, the costs imposed on households, and how widespread the phenomenon is. These are all important questions to move towards assessing the overall welfare calculations of different policy choices.

This paper explicitly compares two alternative school admissions priority arrangements in England. Most areas use some form of geography - a catchment area/school zone or distance ranking - to order pupils if a school is oversubscribed. A minority of areas retained a non-geographical admissions system, where spots at the 'top' schools are awarded primarily by test score. A second difference between households that ever become parents versus never become parents accounts for any area-level selection into retaining the non-geographical admissions system, isolating the effect of geographical admissions criteria on the probability of moving home and the characteristics of the destination.

This paper finds that only a minority of households (those with high social class) make endogenous residential choices in response to geographical school admissions priorities. This implies that households typically incur minimal additional moving costs through the presence of a geographical admissions system. Residential mobility is high for young families (around 80% of these households move within the 10-year interval of the panel data) but mobility is no more likely in areas with geographical compared to non-geographical school admissions. On the intensive margin, households with high social class are very slightly more likely to move to areas with higher school quality. Moves later in life are not affected significantly, tentatively suggesting that moving costs (either monetary or non-monetary) are larger at this life-stage.

These results are useful for primarily two strands of literature. First, these results give interpretation to the 'black box' of the estimated house price premium around 'good' schools. The results suggest that rather than being a widespread phenomenon, moving for schools occurs for a minority of households, for a minority of schools. It would be informative for forthcoming estimates of the property price premiums around 'good' schools to include the distribution of the premium, including the percentage of schools that have a non-zero premium. Confirming the intuition presented in this previous literature, these households are more affluent (in this paper measured by social class). This paper shows that the previous empirical estimates are not dampened by subsequent movement away once children have gained admission or left school. On this point, chapter 6 shows that the estimates *are* dampened by the presence of households that don't gain flow utility from school quality (such as non-parents), but value neighbourhood attributes that are correlated with school quality.

Why is endogenous residential location confined to higher-social class households? Two explanations are differences in budget constraints and differences in preferences for school quality. Budget constraints bind more tightly for households with lower socio-economic status, and may prohibit moving to the area with the most desirable school (and therefore the highest premium). In a natural experiment, holding location fixed, chapter 4 finds that all households respond to information revealed about school quality, suggesting that preferences are similar across groups. Previous literature has found differences in preferences between socio-economic groups, however, when estimated from discrete choice models of parents' school choices.

Turning to this second strand of literature, discrete choice models to estimate parents' preferences have, to date, uniformly assumed a fixed residential location from which households make their school choices. Any endogenous residential location causes bias to the estimated preferences. While acknowledged as problematic, the scale of this problem has remained unquantified. This paper reveals that residential choices are endogenous for a limited set of households with high social class. This is a positive result for the literature, in that estimating discrete choice models for lower social class households should be free of this form of bias. Less encouragingly, however, it is not possible to infer differences in preferences between groups from these models, where location is endogenous for one group.

These results are based on the comparison of two existing admissions systems in England. Are the results externally valid for other contexts, and alternative admissions arrangements, such as lotteries, that also break the near deterministic link between residential location and school access? School assignment clearly depends on alternative admissions arrangements, for example whether students are sorted randomly or by ability. Exit to the private sector may also depend on the admissions arrangements, eventual distribution of 'school quality' and peer group. Both these factors (school assignment and exit to the private sector) are likely to affect the efficiency and equity of the resulting education system. The residential location decisions may behave similarly under both systems, however, as both remove the incentive for residential choice decisions to factor in the probability of admission to a desirable school. (Commuting costs to school may still play a role in households' decisions.)

The role of the private sector is an important factor in the generalisability of the results. Private schools are an outside option for parents under geographical and non-geographical admissions criteria. For geographical admissions criteria, parents might choose to pay for school, rather than pay more for a house close to a desirable state school. Contexts with more limited provision of private schooling might therefore see larger endogenous residential movement in response to geographical admissions arrangements. Heterogeneity in the response in England according to local market characteristics (such as the private school provision or other outside options, such as faith schools) is left to future research, as it would need to incorporate the potential endogeneity of private school provision to local market characteristics.

One limitation of this empirical paper is the measurement error induced by anonymisation, namely using discrete rather than continuous variables for local school quality. There is also measurement error in the classification of 'ever' and 'never' parents, and inconsistency in the measurement of a residential move over time. These factors suggest that the results are a lower-bound on the differences between admissions systems. Future work, in collaboration with ONS, could reduce this measurement error problem.

The role of school admissions priorities, and the design of the education system more generally, on neighbourhood formation in addition to school composition is an important area of study.

Individuals are shaped by their environment at home and school. Re-designing education systems to shift either or both of these factors could economically affect future generations of pupils. Further research is needed to evaluate reforms and provide evidence on the overall total welfare effects of alternative school choice policies, including the general equilibrium effects on residential choices.

7.8 Tables

Table 7.1: The share of private and state-funded religious secondary schools across Local Authorities in England

	Mean	S.D.	10th %ile	25th %ile	Median	75th %ile	90th %ile
Private	0.25	0.16	0.06	0.14	0.22	0.35	0.45
Private: geog.	0.25	0.16	0.06	0.14	0.22	0.36	0.47
Private: non-geog.	0.20	0.15	0.07	0.14	0.20	0.29	0.40
Religious	0.22	0.14	0.08	0.13	0.20	0.29	0.38
Religious: geog.	0.22	0.11	0.08	0.13	0.16	0.27	0.39
Religious: non-geog.	0.17	0.06	0.11	0.13	0.18	0.20	0.27
N LAs	151	151	151	151	151	151	151

Source: School Performance Tables (Department for Education), 2011. Note: The sample excludes one LA (the City of London) that has no state secondary schools, and only one private school. Colleges and Special Schools (that cater for pupils with some Special Educational Needs) are excluded from the sample. The share of religious schools is for state-funded secondary schools only.

Table 7.2: The prevalence of geographical admissions priorities across 'geographical' and 'non-geographical' Local Authorities

	Geographical	Non-geographical		
		All	Non-selective	Selective
Priority ever mentioned				
Catchment	0.53	0.47	0.43	0.61
Proximity	0.58	0.50	0.55	0.35
Catchment | Proximity	0.87	0.84	0.84	0.85
Priority mentioned in first 3				
Catchment	0.48	0.41	0.37	0.61
Proximity	0.18	0.16	0.21	0.06
Catchment | Proximity	0.64	0.56	0.56	0.61
Priority mentioned in first 5				
Catchment	0.53	0.47	0.43	0.61
Proximity	0.46	0.46	0.52	0.28
Catchment | Proximity	0.83	0.83	0.84	0.83

Source: Data collection by Min Zhang, funded by the Keynes Fund (PI Anna Vignoles and Simon Burgess). Collection of all secondary schools' admissions policies for entry in the school year 2020/2021. Note: The data are collapsed to the Local Authority (LA) level. The sample excludes Hartlepool. 'Priority ever mentioned' means that the priority (catchment or proximity) is mentioned at any point in the schools' admissions priorities. 'Priority mentioned in first 3' means that the priority is given as one of the top three admissions priorities. 'Priority mentioned in first 5' means that the priority is given as one of the top five admissions priorities.

Table 7.3: Cohorts of interest in the ONS Longitudinal Study

Census	Cohort 1	Cohort 2	Cohort 3
1971	0-10	10-20	20-30
1981	10-20	20-30	30-40
1991	20-30	30-40	40-50
2001	30-40	40-50	50-60
2011	40-50	50-60	60-70
N	358,309	334,130	351,125
N final sample	248,952	251,542	281,190
N observed in 1991 to 2011	149,389	150,175	166,473

Source: ONS Longitudinal Study. Note: This table shows the age range of LS sample members in three chosen cohorts across Censuses. The number of observations is individual by Census year. The process from unrestricted to final sample is shown in Appendix Table A6.1.

Table 7.4: Summary statistics for three cohorts at the 'key age' of 40 (mean, and standard deviation in brackets)

Census Age in 1991	Cohort 1 20-30	Cohort 2 30-40	Cohort 3 40-50
'Ever parent'	0.77 (0.42)	0.83 (0.37)	0.88 (0.33)
Not born in UK	0.01 (0.12)	0.05 (0.22)	0.05 (0.22)
One born in UK	0.22 (0.42)	0.18 (0.39)	0.14 (0.35)
Both born in UK	0.76 (0.43)	0.77 (0.42)	0.81 (0.39)
Professional	0.02 (0.13)	0.02 (0.12)	0.03 (0.16)
Intermediate	0.22 (0.42)	0.21 (0.41)	0.20 (0.40)
Skilled Non-Manual	0.22 (0.41)	0.21 (0.41)	0.19 (0.39)
Skilled Manual	0.24 (0.43)	0.24 (0.43)	0.25 (0.43)
Partly Skilled	0.23 (0.42)	0.23 (0.42)	0.22 (0.41)
Unskilled	0.07 (0.25)	0.08 (0.27)	0.09 (0.28)
Degree	0.39 (0.49)	0.34 (0.47)	0.24 (0.43)
One in work	0.30 (0.46)	0.31 (0.46)	0.37 (0.48)
Both in work	0.63 (0.48)	0.62 (0.49)	0.61 (0.49)
Partner	0.83 (0.37)	0.87 (0.33)	0.90 (0.30)
Local school quality (LA deciles)	5.67 (2.83)	5.64 (2.81)	5.62 (2.81)
Property price (national deciles)	5.50 (2.78)	5.60 (2.81)	5.77 (2.81)

Source: ONS Longitudinal Study. Note: The final common sample is applied. Variables refer to the LS sample member if the LS sample member is single, or combined characteristics of LS member and partner if the LS sample member is part of a couple (married or cohabiting). The 'key age' of 40 is selected as an age where most LS sample members that ever have children will have dependent children at the household. All the variables in the table are recorded at this 'key age', which is the closest Census to when the LS sample member is aged 40. For example, if an individual was 34 in 1991 and 44 in 2001, then the variables from the 2001 census would be used to define their variables at the key age.

Table 7.5: Characteristics of LS sample members that are resident in or move between Local Authorities with 'selective' or 'grammar' school admissions

Cohort 1 Variable	Always geographic Mean (SD)	Always non-geographic Mean (SD)	Switch to non-geographic Mean (SD)	Switch to geographic Mean (SD)	Switch to and from Mean (SD)
Not born in UK	0.02 (0.13)	0.01 (0.12)	0.01 (0.12)	0.01 (0.09)	0.01 (0.09)
Both born in UK	0.76 (0.43)	0.76 (0.43)	0.78 (0.42)	0.79 (0.41)	0.72 (0.45)
Professional	0.02 (0.13)	0.01 (0.11)	0.03 (0.17)	0.03 (0.17)	0.02 (0.14)
Intermediate	0.22 (0.41)	0.18 (0.38)	0.31 (0.46)	0.31 (0.46)	0.38 (0.48)
Skilled Non-Manual	0.21 (0.41)	0.22 (0.42)	0.23 (0.42)	0.25 (0.43)	0.21 (0.41)
Skilled Manual	0.24 (0.43)	0.26 (0.44)	0.19 (0.39)	0.19 (0.39)	0.21 (0.41)
Partly Skilled	0.23 (0.42)	0.24 (0.43)	0.18 (0.38)	0.17 (0.38)	0.14 (0.35)
Unskilled	0.07 (0.26)	0.08 (0.27)	0.04 (0.20)	0.04 (0.19)	0.03 (0.16)
Degree	0.39 (0.49)	0.32 (0.47)	0.49 (0.50)	0.51 (0.50)	0.53 (0.50)
One in work	0.30 (0.46)	0.32 (0.47)	0.30 (0.46)	0.29 (0.45)	0.29 (0.46)
Both in work	0.63 (0.48)	0.62 (0.48)	0.65 (0.48)	0.67 (0.47)	0.66 (0.47)
Partner	0.83 (0.37)	0.83 (0.38)	0.87 (0.33)	0.88 (0.33)	0.82 (0.39)
'Ever parent'	0.78 (0.41)	0.79 (0.41)	0.76 (0.42)	0.73 (0.44)	0.71 (0.45)
Cohort 2					
Not born in UK	0.06 (0.24)	0.04 (0.19)	0.03 (0.17)	0.03 (0.16)	0.02 (0.13)
Both born in UK	0.76 (0.43)	0.78 (0.41)	0.77 (0.42)	0.77 (0.42)	0.71 (0.45)
Professional	0.01 (0.12)	0.01 (0.12)	0.02 (0.15)	0.03 (0.16)	0.04 (0.20)
Intermediate	0.21 (0.40)	0.19 (0.39)	0.29 (0.45)	0.31 (0.46)	0.30 (0.46)
Skilled Non-Manual	0.21 (0.41)	0.24 (0.43)	0.25 (0.44)	0.26 (0.44)	0.31 (0.46)
Skilled Manual	0.24 (0.43)	0.24 (0.43)	0.19 (0.40)	0.16 (0.37)	0.14 (0.34)
Partly Skilled	0.24 (0.42)	0.23 (0.42)	0.18 (0.38)	0.17 (0.38)	0.15 (0.36)
Unskilled	0.08 (0.28)	0.08 (0.28)	0.04 (0.21)	0.05 (0.22)	0.04 (0.20)
Degree	0.34 (0.47)	0.31 (0.46)	0.43 (0.49)	0.44 (0.50)	0.44 (0.50)
One in work	0.31 (0.46)	0.31 (0.46)	0.34 (0.48)	0.32 (0.47)	0.33 (0.47)
Both in work	0.62 (0.48)	0.64 (0.48)	0.60 (0.49)	0.63 (0.48)	0.63 (0.48)
Partner	0.88 (0.33)	0.88 (0.32)	0.88 (0.33)	0.88 (0.32)	0.84 (0.36)
'Ever parent'	0.84 (0.37)	0.85 (0.35)	0.81 (0.39)	0.80 (0.40)	0.79 (0.41)
Cohort 3					
Not born in UK	0.05 (0.23)	0.04 (0.20)	0.03 (0.17)	0.03 (0.17)	0.02 (0.14)
Both born in UK	0.81 (0.40)	0.81 (0.39)	0.81 (0.39)	0.81 (0.40)	0.85 (0.36)
Professional	0.03 (0.16)	0.02 (0.15)	0.03 (0.16)	0.03 (0.16)	0.03 (0.16)
Intermediate	0.20 (0.40)	0.20 (0.40)	0.25 (0.43)	0.29 (0.45)	0.29 (0.45)
Skilled Non-Manual	0.19 (0.39)	0.20 (0.40)	0.23 (0.42)	0.24 (0.43)	0.19 (0.39)
Skilled Manual	0.25 (0.43)	0.25 (0.43)	0.22 (0.42)	0.20 (0.40)	0.24 (0.43)
Partly Skilled	0.22 (0.42)	0.22 (0.41)	0.17 (0.38)	0.17 (0.38)	0.13 (0.33)
Unskilled	0.09 (0.28)	0.08 (0.27)	0.07 (0.25)	0.05 (0.21)	0.09 (0.29)
Degree	0.24 (0.43)	0.23 (0.42)	0.28 (0.45)	0.32 (0.47)	0.25 (0.43)
One in work	0.37 (0.48)	0.39 (0.49)	0.33 (0.47)	0.39 (0.49)	0.32 (0.47)
Both in work	0.61 (0.49)	0.59 (0.49)	0.64 (0.48)	0.59 (0.49)	0.65 (0.48)
Partner	0.90 (0.30)	0.91 (0.29)	0.91 (0.28)	0.88 (0.32)	0.91 (0.29)
'Ever parent'	0.88 (0.33)	0.89 (0.31)	0.88 (0.33)	0.84 (0.36)	0.90 (0.30)

Source: ONS Longitudinal Study and School Performance Tables (Department for Education). Note: The final common sample is applied. 'Selective' and 'grammar' are used interchangably. Local Authorities are defined as 'selective' if at least 25% of schools are classified as 'selective' or 'modern' in the School Performance Tables around the Census years 1991, 2001 and 2011. The definition is refined by consistent across Census years and accounting for changes in Local Authority boundaries between the 1991 and 2001 Censuses.

Table 7.6: The relationship between local property prices and local school quality at the LSOA level

	Grammar school concentration (LA level)			
	(1)	(2)	(3)	(4)
(a) Unconditional	<10%	10-25%	25-50%	>50%
Local school quality	0.388***	0.212***	0.170***	0.098***
	(0.006)	(0.033)	(0.024)	(0.013)
Urban/rural FE	No	No	No	No
Region FE	No	No	No	No
Observations	27,979	867	892	2,694
(b) Conditional	<10%	10-25%	25-50%	>50%
Local school quality	0.267***	0.180***	0.161***	0.101***
	(0.004)	(0.023)	(0.021)	(0.011)
Urban/rural FE	Yes	Yes	Yes	Yes
Region FE	Yes	Yes	Yes	Yes
Observations	27,979	867	892	2,694

Source: Consumer Data Research Centre and School Performance Tables (Department for Education). Note: + p<0.10, * p<0.05, ** p<0.01, *** p<0.001. The table shows coefficients from an ordinary least squares regression, with standard errors in parentheses. The relationship between local prices and school quality at the LSOA level, estimated using OLS. The dependent variable is the decile of local property prices, where 1 is the lowest price and 10 is the highest price. The independent variable is the decile of local school quality, where 1 is the lowest test scores and 10 is the highest. Panel (b) is conditional on urban/rural (4 categories) and region fixed (9 categories) effects.

Table 7.7: Difference-in-differences estimation for the effect of geographical admissions priorities for secondary schools in England on the probability of moving, by cohort and age band. Interaction effects of interest.

	Cohort 1	Cohort 2	Cohort 3
0-20 # Geog. # 'Ever parent'	0.048		
	(0.055)		
21-25 # Geog. # 'Ever parent'	-0.010		
	(0.030)		
26-30 # Geog. # 'Ever parent'	0.050	-0.043	
	(0.031)	(0.057)	
31-35 # Geog. # 'Ever parent'	0.002	0.012	
	(0.030)	(0.034)	
36-40 # Geog. # 'Ever parent'	0.004	0.020	-0.064
	(0.031)	(0.035)	(0.060)
41-45 # Geog. # 'Ever parent'		*reference group*	
46-50 # Geog. # 'Ever parent'	0.053	0.036	-0.017
	(0.034)	(0.036)	(0.036)
51-55 # Geog. # 'Ever parent'		0.040	0.016
		(0.036)	(0.035)
56-60 # Geog. # 'Ever parent'		0.004	0.001
		(0.040)	(0.037)
61-65 # Geog. # 'Ever parent'			-0.005
			(0.037)
66-70 # Geog. # 'Ever parent'			0.005
			(0.043)
N	149,389	150,175	166,473
R^2	0.096	0.143	0.058

Source: ONS Longitudinal Study linked to School Performance Tables (Department for Education). Note: + p<0.10, * p<0.05, ** p<0.01, *** p<0.001. The final common sample is applied. # refers to the interaction between variables. The table shows coefficients from an ordinary least squares regression, with standard errors in parentheses. Full results from this specification are shown in Appendix Table A6.3.

Table 7.8: Difference-in-differences estimation for the effect of geographical admissions priorities for secondary schools in England on the local school quality of chosen residence, by cohort and age band. Interaction effects of interest.

	Cohort 1	Cohort 2	Cohort 3
0-20 # Geog. # 'Ever parent'	-0.042		
	(0.344)		
21-25 # Geog. # 'Ever parent'	-0.037		
	(0.190)		
26-30 # Geog. # 'Ever parent'	0.317	-0.085	
	(0.194)	(0.345)	
31-35 # Geog. # 'Ever parent'	-0.021	0.059	
	(0.190)	(0.204)	
36-40 # Geog. # 'Ever parent'	0.104	0.228	-0.136
	(0.195)	(0.214)	(0.363)
41-45 # Geog. # 'Ever parent'		*reference group*	
46-50 # Geog. # 'Ever parent'	0.136	0.265	-0.135
	(0.212)	(0.219)	(0.217)
51-55 # Geog. # 'Ever parent'		0.302	-0.192
		(0.216)	(0.212)
56-60 # Geog. # 'Ever parent'		0.077	-0.043
		(0.243)	(0.223)
61-65 # Geog. # 'Ever parent'			0.005
			(0.220)
66-70 # Geog. # 'Ever parent'			-0.097
			(0.258)
N	149,389	150,175	166,473
R^2	0.003	0.002	0.002

Source: ONS Longitudinal Study linked to School Performance Tables (Department for Education). Note: + p<0.10, * p<0.05, ** p<0.01, *** p<0.001. The final common sample is applied. # refers to the interaction between variables. The table shows coefficients from an ordinary least squares regression, with standard errors in parentheses. Full results from this specification are shown in Appendix Table A6.4.

Table 7.9: Difference-in-differences estimation for the effect of geographical admissions priorities for secondary schools in England on the probability of moving, by cohort and age band. Interaction effects of interest. Highest social class only (Professional and Intermediate)

	Cohort 1	Cohort 2	Cohort 3
0-20 # Geog. # 'Ever parent'	0.106		
	(0.095)		
21-25 # Geog. # 'Ever parent'	0.033		
	(0.055)		
26-30 # Geog. # 'Ever parent'	0.098+	-0.013	
	(0.054)	(0.100)	
31-35 # Geog. # 'Ever parent'	0.068	0.024	
	(0.055)	(0.060)	
36-40 # Geog. # 'Ever parent'	0.002	0.024	-0.055
	(0.055)	(0.062)	(0.109)
41-45 # Geog. # 'Ever parent'		*reference group*	
46-50 # Geog. # 'Ever parent'	0.152**	0.124*	-0.091
	(0.059)	(0.063)	(0.070)
51-55 # Geog. # 'Ever parent'		0.030	-0.060
		(0.063)	(0.066)
56-60 # Geog. # 'Ever parent'		-0.008	-0.021
		(0.070)	(0.073)
61-65 # Geog. # 'Ever parent'			-0.069
			(0.068)
66-70 # Geog. # 'Ever parent'			-0.081
			(0.084)
N	35,918	33,747	38,460
R^2	0.099	0.190	0.089

Source: ONS Longitudinal Study linked to School Performance Tables (Department for Education). Note: + p<0.10, * p<0.05, ** p<0.01, *** p<0.001. The final common sample is applied. # refers to the interaction between variables. The table shows coefficients from an ordinary least squares regression, with standard errors in parentheses. Full results from this specification are shown in Appendix Table A6.5.

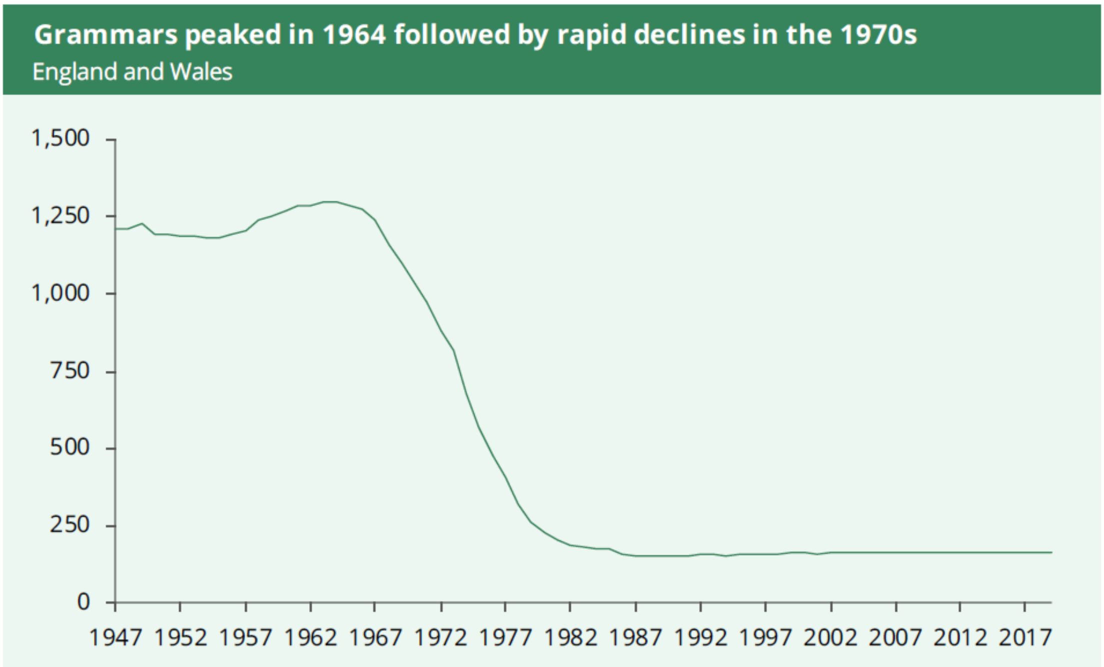
Table 7.10: Difference-in-differences estimation for the effect of geographical admissions priorities for secondary schools in England on the local school quality of chosen residence, by cohort and age band. Interaction effects of interest. Highest social class only (Professional and Intermediate)

	Cohort 1	Cohort 2	Cohort 3
0-20 # Geog. # 'Ever parent'	1.401*		
	(0.628)		
21-25 # Geog. # 'Ever parent'	0.326		
	(0.363)		
26-30 # Geog. # 'Ever parent'	0.588	0.442	
	(0.359)	(0.635)	
31-35 # Geog. # 'Ever parent'	0.372	0.005	
	(0.363)	(0.379)	
36-40 # Geog. # 'Ever parent'	0.361	0.683+	0.232
	(0.362)	(0.390)	(0.638)
41-45 # Geog. # 'Ever parent'		*reference group*	
46-50 # Geog. # 'Ever parent'	0.568	0.548	-0.132
	(0.390)	(0.400)	(0.411)
51-55 # Geog. # 'Ever parent'		0.595	-0.552
		(0.399)	(0.387)
56-60 # Geog. # 'Ever parent'		0.527	-0.118
		(0.445)	(0.427)
61-65 # Geog. # 'Ever parent'			0.243
			(0.398)
66-70 # Geog. # 'Ever parent'			-0.860+
			(0.496)
N	35,918	33,747	38,460
R^2	0.005	0.004	0.006

Source: ONS Longitudinal Study linked to School Performance Tables (Department for Education). Note: + p<0.10, * p<0.05, ** p<0.01, *** p<0.001. The final common sample is applied. # refers to the interaction between variables. The table shows coefficients from an ordinary least squares regression, with standard errors in parentheses. Full results from this specification are shown in Appendix Table A6.6.

7.9 Figures

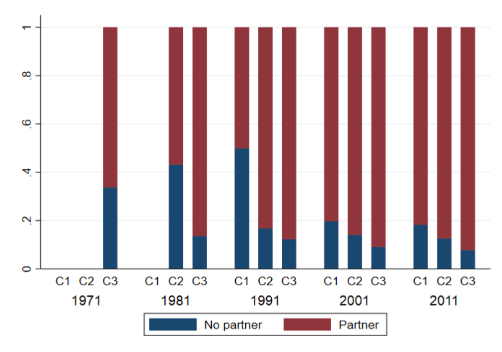
Figure 7.1: The number of grammar schools in England over time



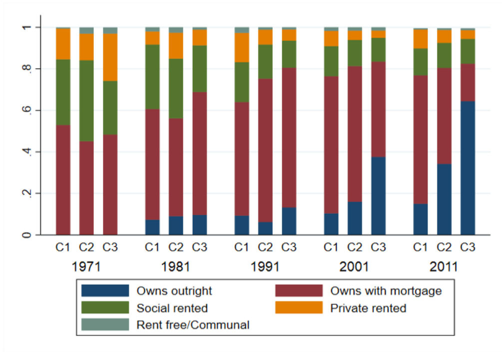
Notes: England and Wales to 1969, England only thereafter. These figures do not include direct-grant grammar schools which continued outside the maintained sector after the 1944 Act.
Sources: Statistics of education schools in England, various years; Schools, pupils and their characteristics: January 2019 (and earlier), DfE

Source: Parliamentary Briefing Paper.

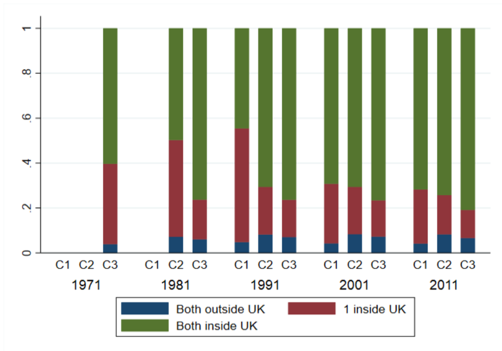
Figure 7.2: Characteristics of Longitudinal Study sample members' households over Census and cohorts



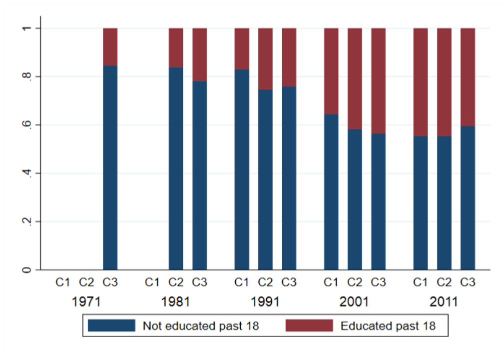
(a) Partner



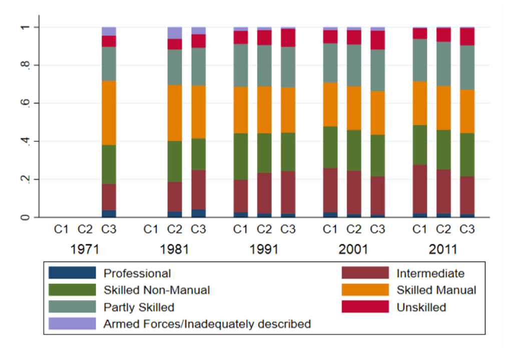
(b) Housing tenure



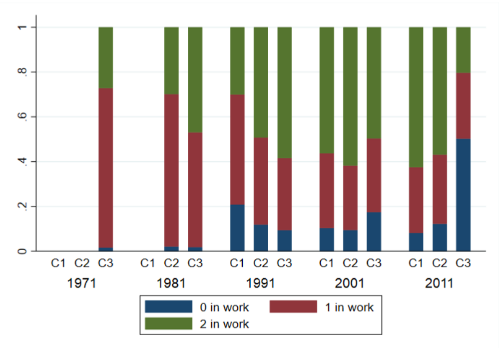
(c) Born in UK



(d) Education (degree)



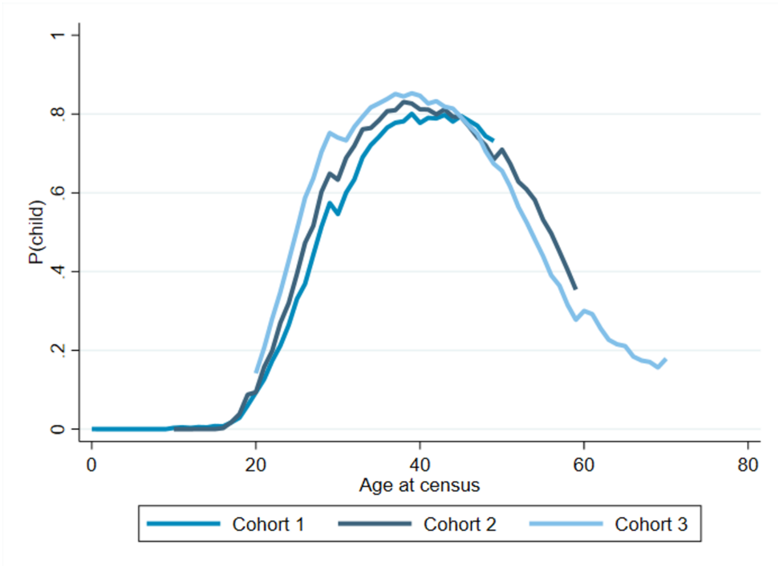
(e) Social class



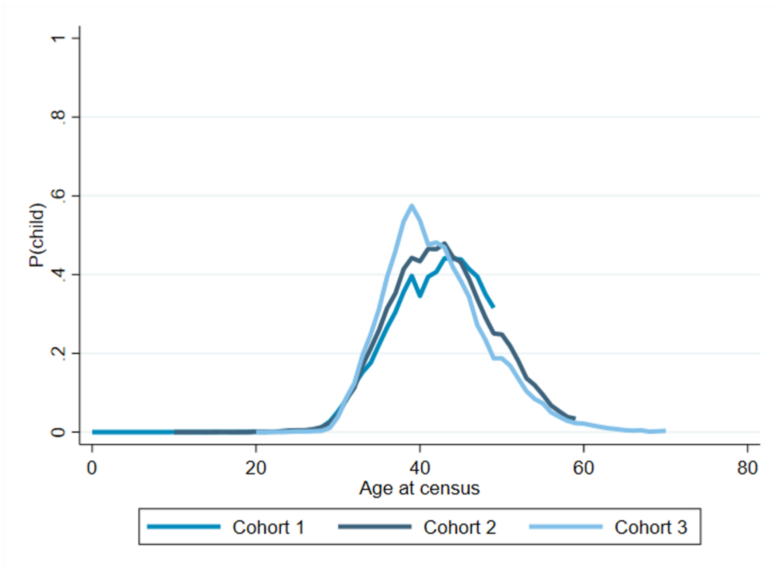
(f) In work

Source: ONS Longitudinal Study. Not that housing tenure is necessarily coded differently in 1971, when there is not separate categories for owning a home with or without a mortgage. All variables are coded at the household level. Information from only the LS sample member is used where the LS sample member is single, and information from two household members is used if the LS sample member is part of a couple (married or cohabiting). This is the maximum value (for education and social class) or count (for born in the UK and in work).

Figure 7.3: The proportion of households with dependent children and dependent children of secondary school age



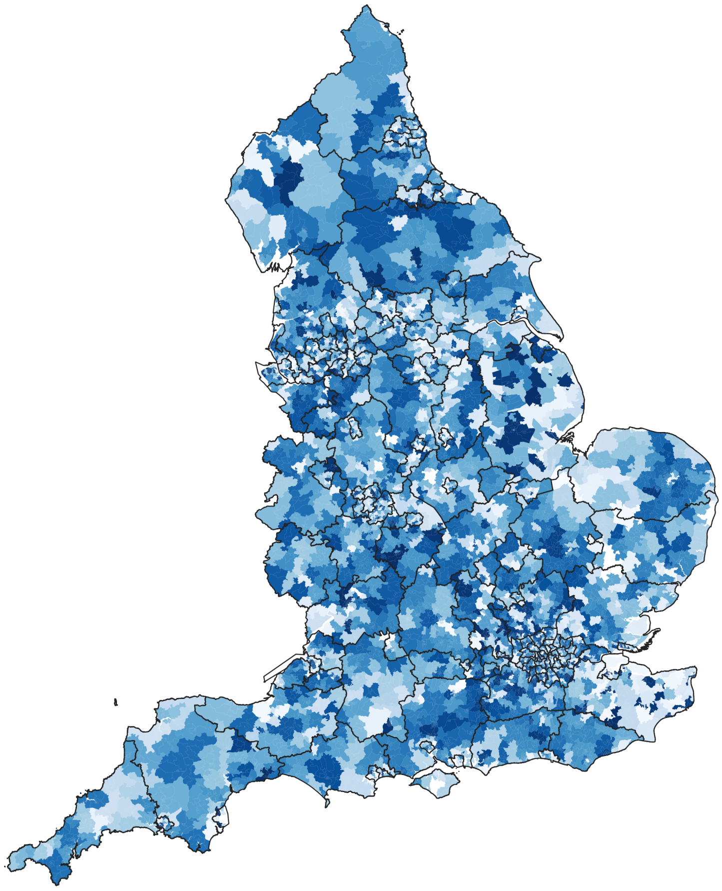
(a) Child in household



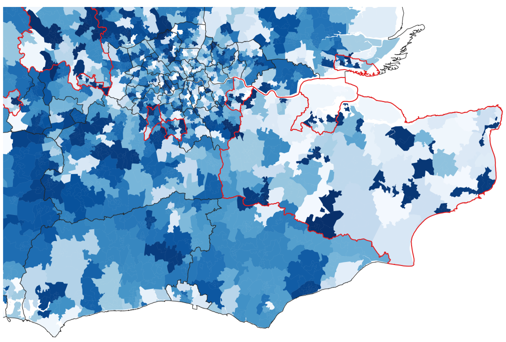
(b) Child of secondary age in household

Source: ONS Longitudinal Study. Note: The final common sample is applied.

Figure 7.4: School quality of the closest secondary school across England at the LSOA level



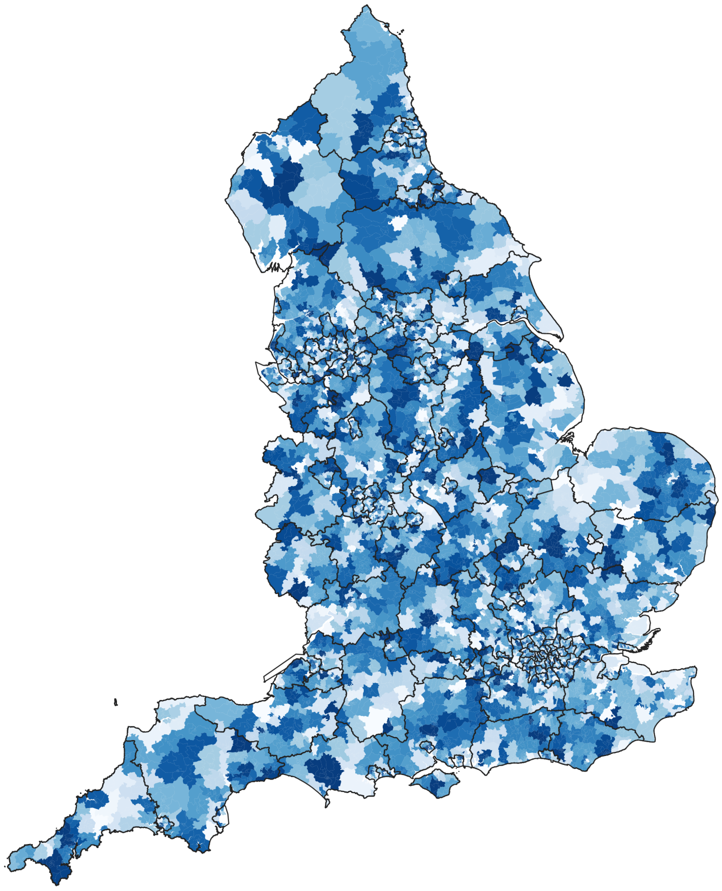
(a) England



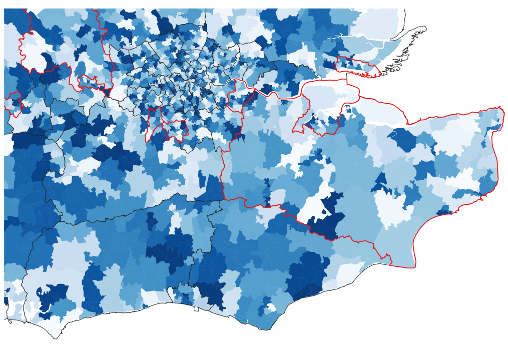
(b) London and South East

Source: School Performance Tables (Department for Education). Note: The school quality in each LSOA is the secondary school performance (5A*-C) of the closest school to the LSOA population-weighted centroid in 2011. Local Authority geographical boundaries rather than LSOA boundaries are shown in black. In panel (b), Local Authorities with non-geographical (selective) admissions are shown with a red boundary. In both panels, a darker shade represents higher school quality.

Figure 7.5: Relative local school quality of the closest secondary school across England at the LSOA level



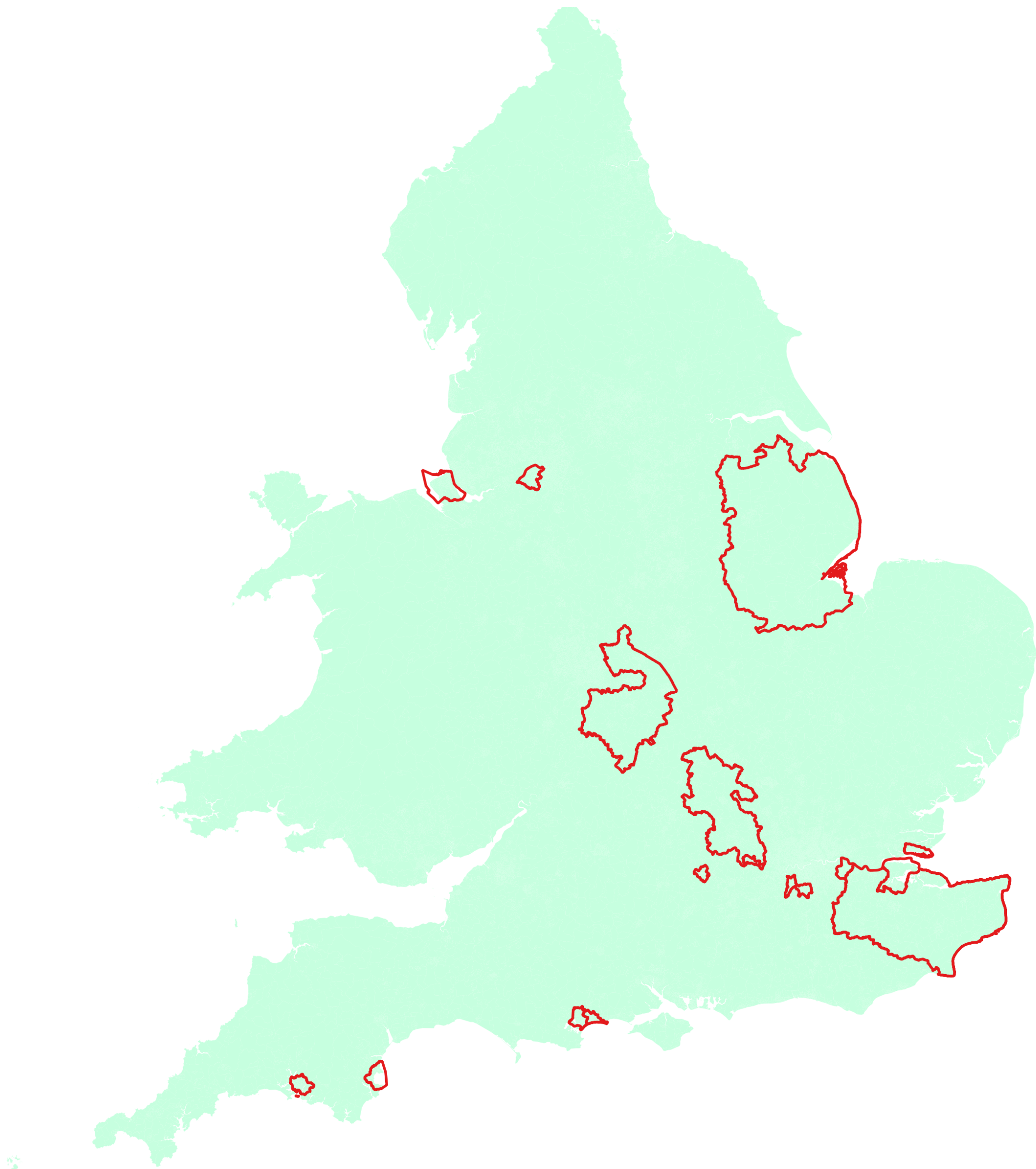
(a) England



(b) London and South East

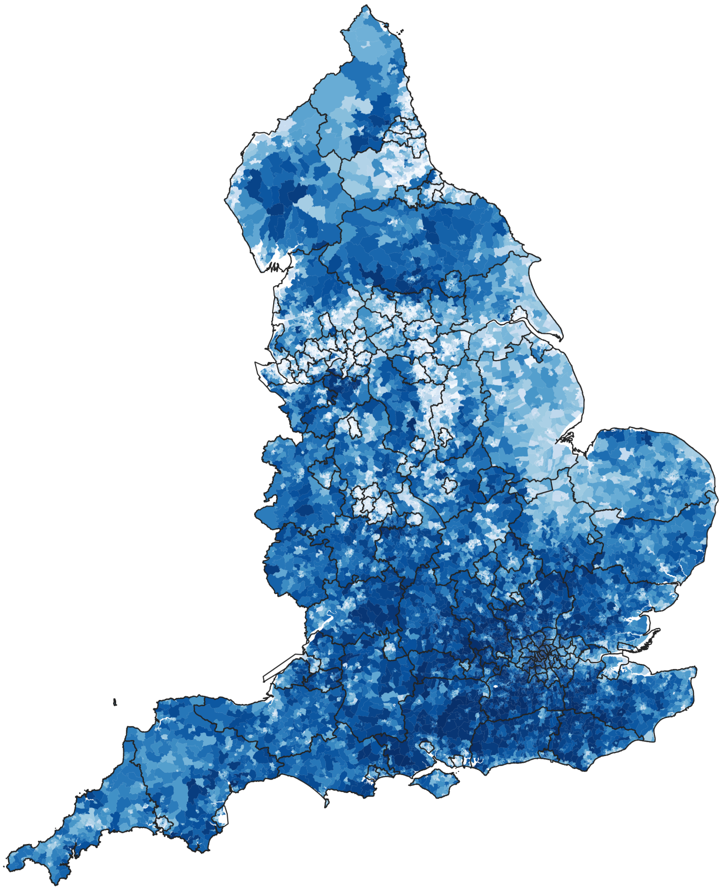
Source: School Performance Tables (Department for Education). Note: The school quality in each LSOA is the secondary school performance (5A*-C) of the closest school to the LSOA population-weighted centroid in 2011, relative to all other schools in the Local Authority. Local Authority geographical boundaries rather than LSOA boundaries are shown in black. In panel (b), Local Authorities with non-geographical (selective) admissions are shown with a red boundary. In both panels, a darker shade represents higher school quality.

Figure 7.6: The location of Local Authorities with at least 25% of schools classified as part of a 'selective' or 'grammar' system

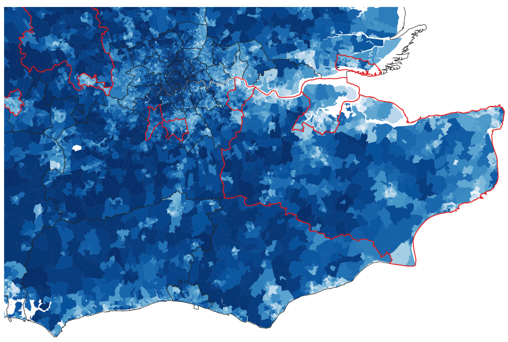


Source: School Performance Tables (Department for Education). Note: 'Selective' and 'grammar' are used interchangeably. Local Authorities are defined as 'selective' if at least 25% of schools are classified as 'selective' or 'modern' in the School Performance Tables around the Census years 1991, 2001 and 2011. The definition is refined by consistent across Census years and accounting for changes in Local Authority boundaries between the 1991 and 2001 Censuses. Local Authorities with bordered highlighted in read have at least 25% selective schools. These areas are, in alphabetical order: Bexley; Bournemouth; Buckinghamshire; Kent; Kingston upon Thames; Lincolnshire; Medway; Plymouth; Poole; Reading; Slough; Southend-on-Sea; Sutton; Torbay; Trafford; Warwickshire; Wirral.

Figure 7.7: Average property prices across England at the LSOA level



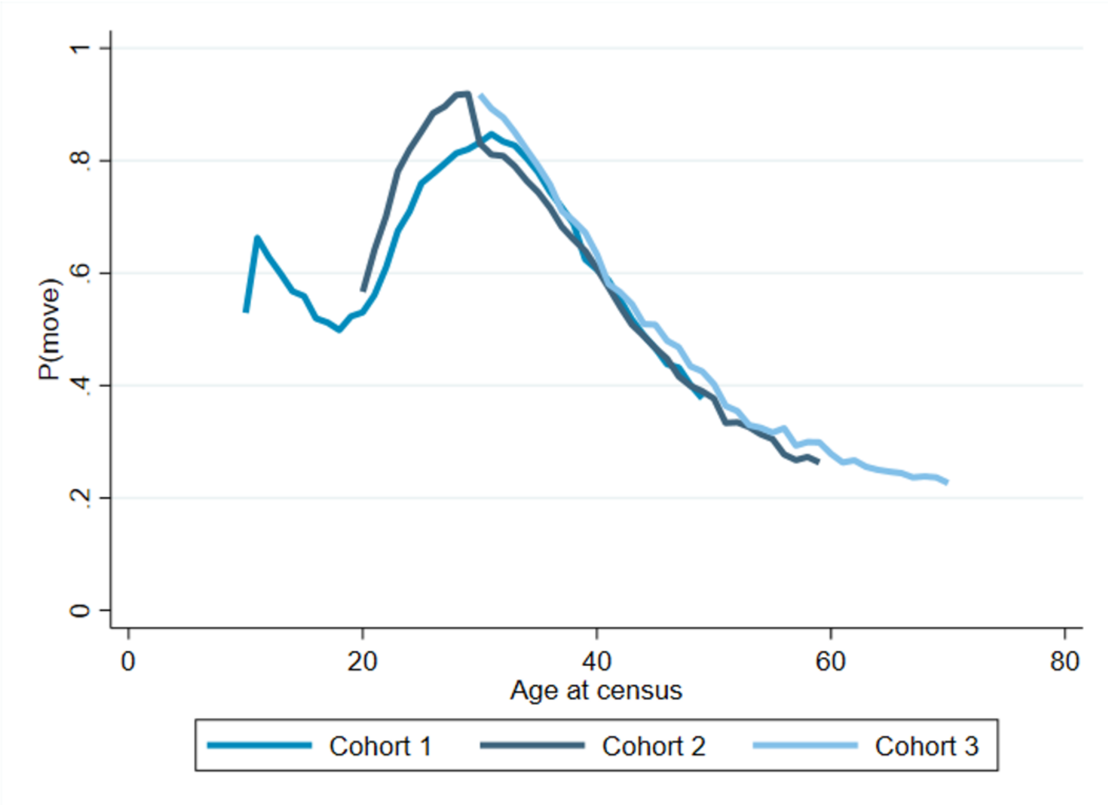
(a) England



(b) London and South East

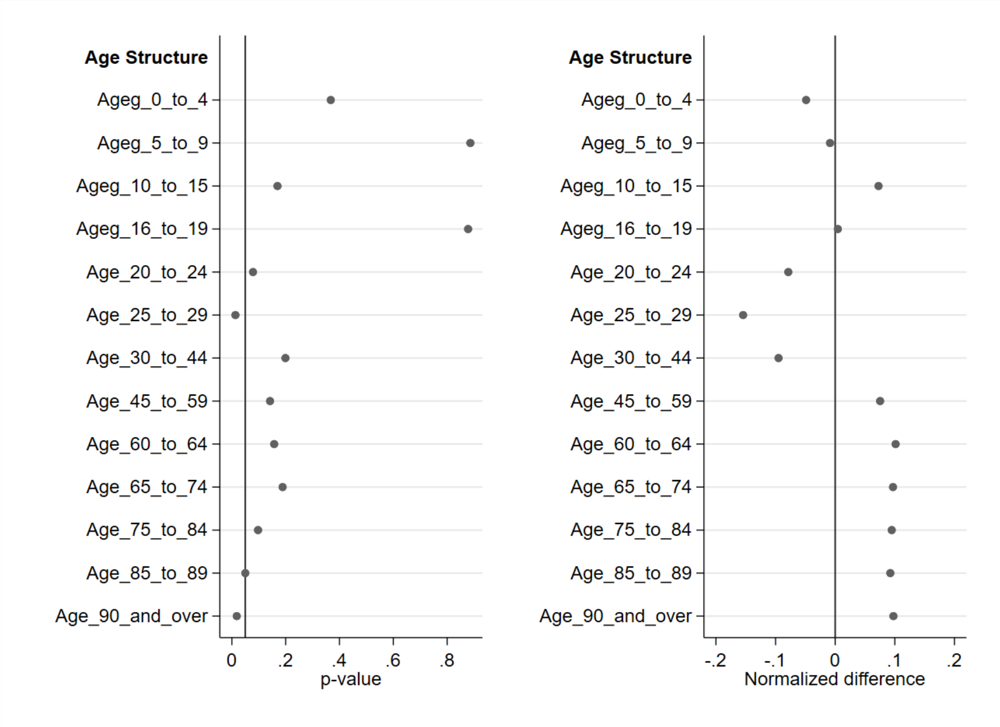
Source: Consumer Data Research Centre. Note: The property price in each LSOA is the average price across median price in the four quarters in 2011. Local Authority geographical boundaries rather than LSOA boundaries are shown in black. In panel (b), Local Authorities with non-geographical (selective) admissions are shown with a red boundary. In both panels, a darker shade represents higher property prices.

Figure 7.8: The percentage of LS sample members that move between Census years, by age and cohort

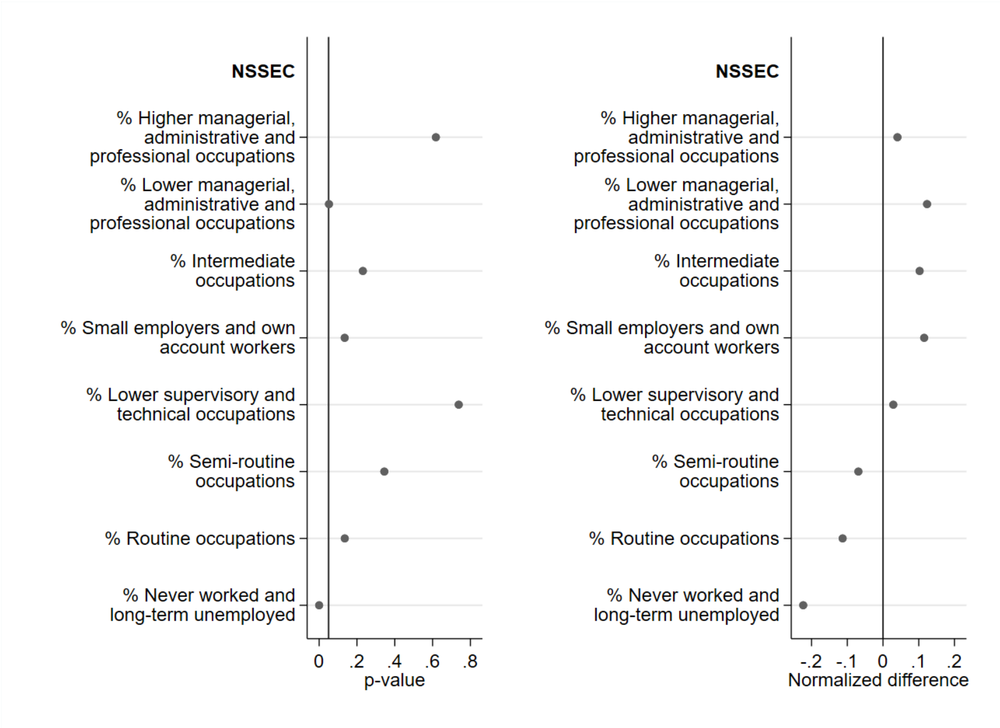


Source: ONS Longitudinal Study. Note: The final common sample is applied.

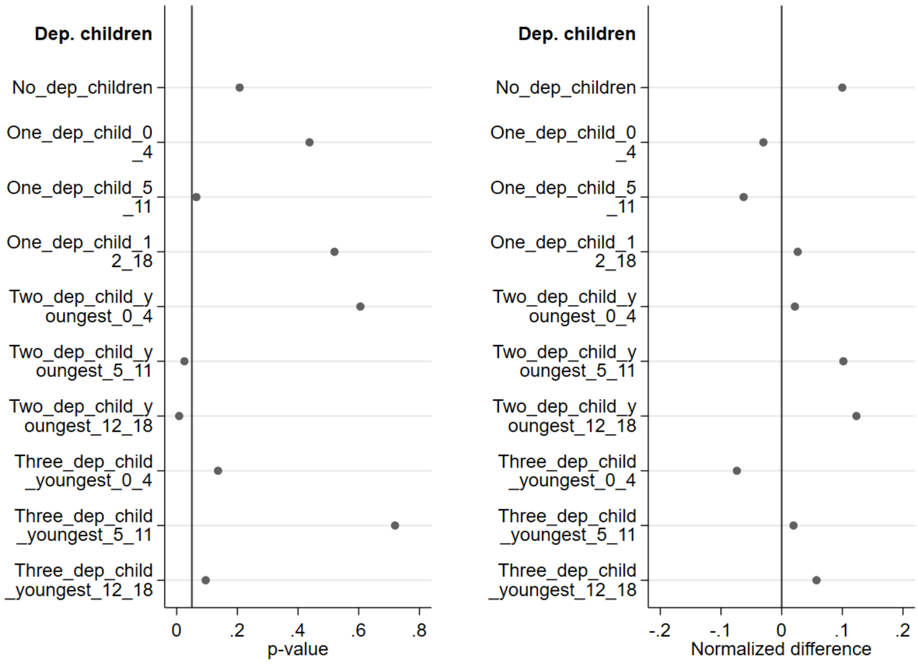
Figure 7.9: The characteristics of Local Authorities with at least 25% of schools classified as part of a ‘selective’ or ‘grammar’ system



(a) Age structure



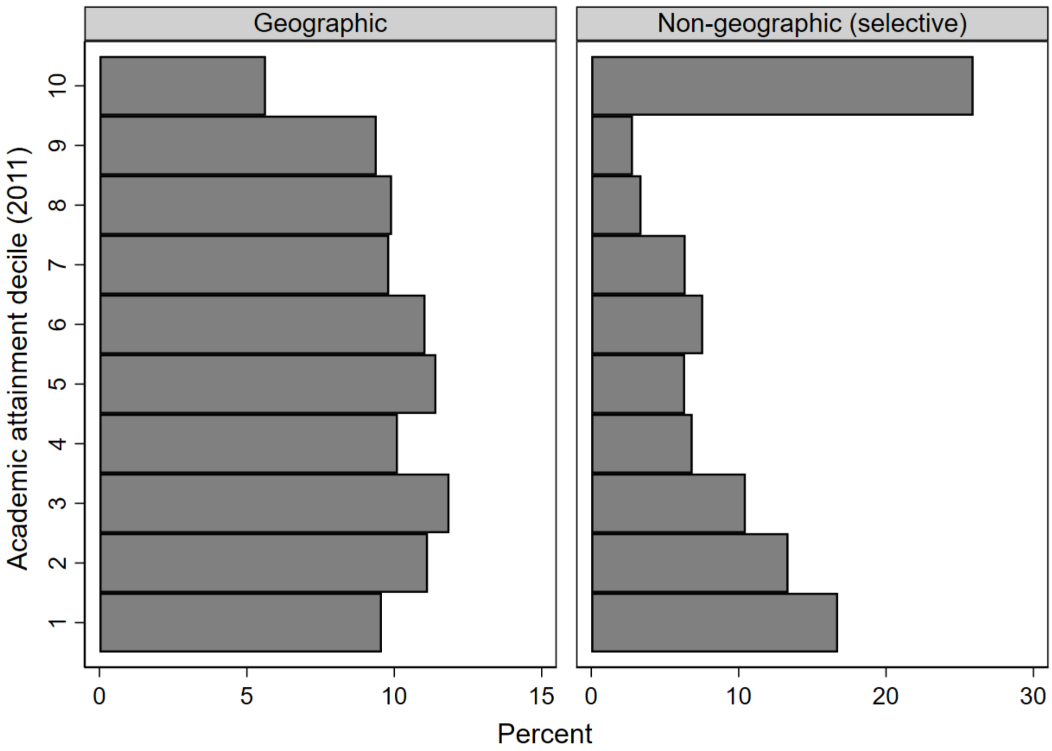
(b) Socio-economic classification



(c) Dependent children

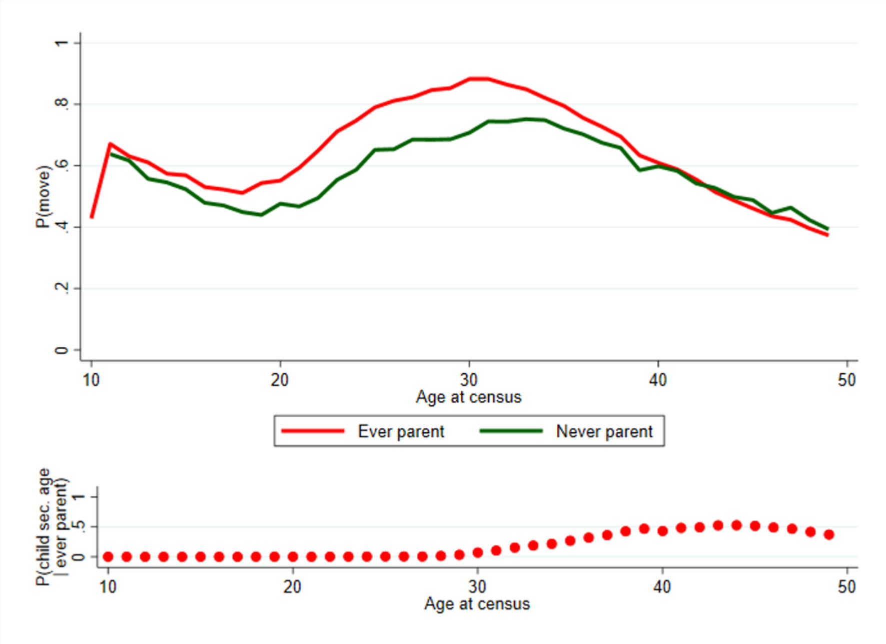
Source: NOMIS (area-level Census data for 2011). Note: The normalised difference is between geographical and non-geographical LAs. A positive difference means that non-geographical LAs have a higher value.

Figure 7.10: The distribution of school quality across Local Authorities with geographical and non-geographical (selective) admissions priorities



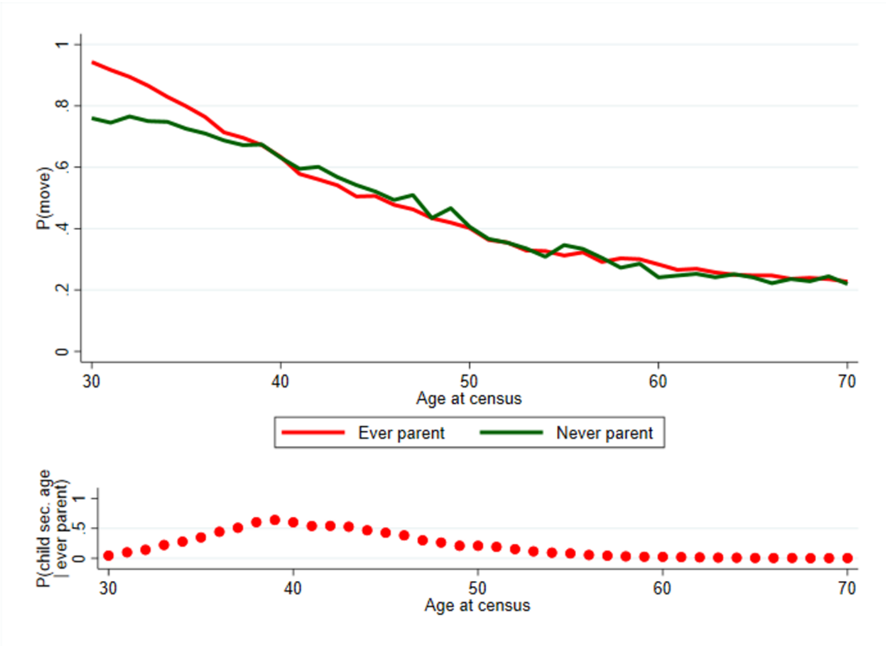
Source: School Performance Tables (Department for Education). Note: 'Selective' and 'grammar' are used interchangably. Local Authorities are defined as 'selective' if at least 25% of schools are classified as 'selective' or 'modern' in the School Performance Tables around the Census years 1991, 2001 and 2011. The definition is refined by consistent across Census years and accounting for changes in Local Authority boundaries between the 1991 and 2001 Censuses.

Figure 7.11: The percentage of LS sample members that move between Census years, by 'ever' and 'never' parent, age and cohort



(a) Cohort 1

(b) Cohort 2



(c) Cohort 3

Source: ONS Longitudinal Study. Note: The final common sample is applied.

8 Conclusion

This thesis studies how the design of school choice affects the composition of schools and neighbourhoods. These are both important dimensions to consider, as segregation between groups has the potential to affect a wide range of societal outcomes, in addition to individuals' own education, employment, health and well-being.

How the design of school choice affects sorting depends on two key inputs. First, parents' preferences for school attributes: what do parents value in schools, and therefore how would they respond to changes in, or information about, school 'quality'? This evidence also has important implications for the ability of school choice and competition to raise education standards across schools. Second, how residential choices interact with school choices under alternative policy environments: how might residential choices work against reforms intended to improve equality of access under school choice? This thesis has studied both aspects. This conclusion brings together research findings across chapters, integrating them into the existing literature.

Turning first to parents' preferences for school attributes, the data presented in chapter 3 suggest that many parents are making active choices for schools (for example bypassing their closest school, particularly when it has lower test scores) and appear to value academic attainment. This is consistent with the large body of evidence studying parents' revealed preferences through their school choices (Hastings, Kane, and Staiger, 2009, Burgess et al., 2015, Borghans, Golsteyn, and Zölitz, 2015, Abdulkadiroğlu, Agarwal, and Pathak, 2017, Ruijs and Oosterbeek, 2019, Glazerman and Dotter, 2017, Beuermann et al., 2018, Oh and Sohn, 2019, Harris and Larsen, 2019, Ajayi and Sidibe, 2020, Walker and Weldon, 2020, Abdulkadiroğlu et al., 2020, Bertoni, Gibbons, and Silva, 2020). There are similar patterns for more and less affluent households, suggesting engagement with the process of school choice across socio-economic lines. This is in contrast to existing qualitative literature, that typically classifies households into more affluent 'active' and less affluent 'disconnected' choosers (Ball, Bowe, and Gewirtz, 1996). Quantitative evidence from revealed preferences also typically concludes that households with lower socio-economic status place less weight on a school's academic standards relative to distance when making school choices (Hastings, Kane, and Staiger, 2009, Burgess et al., 2015, Borghans, Golsteyn, and Zölitz, 2015, Harris and Larsen, 2019, Walker and Weldon, 2020).

How do parents respond to information about school quality when making school choices? Chapter 4 studies how parents' primary school choices respond to information provided in a national system of independent school inspections. Isolating the effect of information, we conclude that parents are more likely to choose schools with higher quality signals. This is consistent with chapter 3 in that households appear to value schools' education standards, and supports previous literature that has estimated a positive effect of information provision largely through experimental design (Hastings and Weinstein, 2008, Ajayi, Friedman, and Lucas, 2017, Andrabi, Das, and Khwaja, 2017, Corcoran et al., 2018a, Neilson, Allende, and Gallego, 2019, Cohodes et al., 2022) although contrasts with information about school effectiveness provided at scale in Chile (Mizala and Urquiola, 2013). For Chile, Mizala and Urquiola, 2013 use a regression discontinuity design to estimate the effect of being labelled as an effective school. In comparison to this context, we study the effect of school quality information for all schools across the distribution, rather than only the 25% identified as high performing. Consistent with chapter 3, more and less affluent households respond similarly to the information shock, suggesting that the awareness and engagement with information is similar across socio-economic lines.

Chapters 3 and 4 both find similar patterns in the engagement with school choice and response to information about school quality across socio-economic groups. That is, households with lower socio-economic status respond similarly to households with higher socio-economic status. This is a rare finding in the existing literature, both qualitative and quantitative, and therefore worth exploring. The quantitative literature on parents' revealed preferences from school choices is estimated through discrete choice models. These models assume that residential location is fixed: given a household's location, what school choices are made? This rules out the potential first stage of a household's decision making: how does the quality of the local schools affect residential location? If households make endogenous residential choices closer to preferred schools, then the estimates from these models are biased. In contrast, in chapter 4, given the short time window we study, residential location is fixed. We therefore reasonably and convincingly ask: given a household's location, how does information about local school quality affect school choices? Chapter 3 abstracts from endogenous residential location by studying the patterns of school choices (for example the number of school choices made, and the probability of choosing the closest school) rather than the absolute level of school quality chosen.

If the distribution of preferences are common across household types, then why is there school segregation in England? Constraints in households' school choices or search are an alternative explanation to preferences. Where there are geographical admissions priorities, for example, lower income households may be priced out of 'better' schools. Variation in local school quality across socio-economic groups has been demonstrated empirically (Oberti, 2007, Denice and Gross, 2016, Burgess et al., 2011, Sartain and Barrow, 2021, Scandurra, Zancajo, and Bonal, n.d.). Chapter 5 assesses the role of such constraints, by simulating school segregation when all households are allocated to their first choice school. Segregation remains as high as the current allocation, suggesting that removing such constraints would not eliminate segregation. This interpretation relies on parents' school choices reflecting their true preferences, however, but in fact households' submitted choices may 'skip the impossible' (Fack, Grenet, and He, 2019) or be a function of 'outside options' (such as private schools) that allow more ambitious choices (Bibler and Billings, 2020, Calsamiglia, Fu, and Güell, 2020, Calsamiglia, Martinez-Mora, and Miralles, 2021). Burgess et al., 2015 note that 7% of parents making school choices in England would have preferred to choose an alternative as their first choice. Chapter 5 finds that segregation remains high even in areas with fewer constraints on parents school choices, however. Even under free school choice without geographical admissions priorities, transport costs may limit access to 'better' schools for lower income households (Laverde, 2022), that improving the public transport system (such as free school bus provision) may help overcome (Agostinelli, Luflade, and Martellini, 2021, Trajkovski, Zabel, and Schwartz, 2021).

The effectiveness of school choice as a system depend on schools' incentives to improve standards of education to attract pupils. Chapter 3 suggests this is the case, while chapter 4 finds convincing evidence that all schools can attract choices by improving their inspection rating, regardless of their prior test scores or pupil composition. This is important evidence in the context of the large body of evidence studying parents' revealed preferences through their school choices, that typically find stronger demand-side incentives for schools in more affluent areas. In fact, chapter 4 finds that the incentives to improve are, if anything, larger for schools initially at the bottom of their local hierarchy. This has positive implications for school choice to improve outcomes for all pupils. An important policy note, however, is that schools' incentives are dampened in areas with little excess capacity, as all (even unpopular) schools are allocated pupils (and therefore funding). This may help to reconcile the strong theoretical arguments for school choice improving overall standards of education and the limited empirical support (Hoxby, 2000, Rothstein, 2007, Hoxby, 2007, Gibbons, Machin, and Silva, 2010, Lavy, 2010, Dijkgraaf, Gradus, and Jong, 2013, De Haan, Leuven, and Oosterbeek, 2016).

A large research literature studies the efficiency and equity implications from alternative school allocation mechanisms. That is, once parents have submitted their school choice(s), what mechanism allocates pupils to schools? The choice of mechanism can affect the allocation of pupils to schools, as certain mechanisms give parents the incentive to consider making strategic school choices. This is evident under variants of the 'Boston' mechanism used around the world (Lai, Sadoulet, and de Janvry, 2009, Lucas and Mbiti, 2012, Ajayi, 2013, Pathak and Sönmez, 2013, He, 2017, Agarwal and Somaini, 2018, Calsamiglia, Fu, and Güell, 2020, Gortázar, Mayor, Montalbán, et al., 2020, Kapor, Neilson, and Zimmerman, 2020). Chapter 3 presents descriptive evidence that even under a truth-revealing mechanism, parents make more cautious choices when constrained to making few school choices (between three and six across Local Authorities in England). This suggests that they would have benefited from having more choices, consistent with theoretical and experimental evidence that shows that parents 'play safe' when choices are constrained (Haeringer and Klijn, 2009, Calsamiglia, Haeringer, and Klijn, 2010). A concrete policy suggestion is therefore to increase the number of options on the school choice form. This would be relatively costless and would easily reduce the need for a strategic or 'safe' school choice. Recent research also suggests that providing information to parents about admission probabilities increases the number of school choices made, by providing a rationale for parents to continue in the costly search process (Arteaga et al., 2021).

Much less attention has been devoted to the important role of admissions priorities in determining access to schools. That is, how do schools rank pupils to determine admission when they are over-subscribed? This thesis contributes to this important area of study. Chapter 3 reminds us that 'the rules in place governing the structure of any particular school choice program are likely crucial in determining the outcomes of the program' (Goldhaber, 2000). In England, the dominant over-subscription criteria for secondary schools - straight line distance or catchment area - is likely to induce strategic school choices, residential mobility and unequal access to the highest quality schools. Chapter 6 models the effects of alternative school admissions priorities on neighbourhood and school sorting, while chapter 7 assesses the empirical evidence for endogenous residential moves using longitudinal data.

School choice might have different implications for segregation at the neighbourhood and school level. For example, Boterman, 2021 finds that free school choice in Amsterdam has allowed neighbourhood integration, as parents need no longer move to gain admission to their preferred school. Modelling in chapter 6 finds evidence of complicated and surprising general equilibrium effects of school choice reform to neighbourhood composition, by incorporating households without children and an initial positive correlation between school and neighbourhood 'quality'.

What options do policy-makers have? Evidence across three US counties suggests that policy choices do affect segregation at the neighbourhood and school level (Taylor and Frankenberg, 2021). As explored in chapter 5, one extreme option is to return to a 'neighbourhood' allocation system, which, at face value, would reduce segregation between schools. Whether this policy would in fact reduce segregation between schools depends on households' resulting residential mobility, for example moving to the assigned neighbourhood of their preferred school. This thesis has not been able to definitively conclude whether compensating residential movement in response to ending school choice would undo the finding in chapter 5. On one hand, chapter 6 finds evidence that where admission to a top performing school is determined by location, households that ever have children sort into this area, increasing income segregation between schools and segregation at the neighbourhood level. This is consistent with the large empirical literature that finds property price premia around 'good' schools with geographical admissions criteria (Black and Machin, 2011, Nguyen-Hoang and Yinger, 2011). On the other hand, chapter 7 presents evidence that this is not a general phenomenon. Only households with high social-class make endogenous residential choices for schools, and to a limited extent. To reconcile these two findings, it is likely that endogenous sorting occurs for a minority of schools at the top of the distribution, and these patterns are difficult to detect in national data covering all schools. That is, the residential choices of higher social-class parents respond to differences in school quality at the top of the distribution, but more generally, parents' residential choices do not respond to differences in quality in the middle and bottom of the distribution. This is consistent with evidence for the response to primary school quality in England (Gibbons and Machin, 2006).

This is related to another benefit of school choice, that *all* households are permitted some choice over their school allocation. This is evident in chapter 4, where households are equally responsive to information about school quality across socio-economic groups. In the alternative 'neighbourhood' allocation, only families who can afford to pay a property price premium can choose the most popular state school options. This is also evident in chapter 6, where even under the assumption of homogeneous preferences, poorer households are excluded from the 'better' neighbourhood and school by the geographical admissions priority.

Another alternative option - random assignment - is modelled in chapter 6, explicitly focusing on the induced residential incentives for households. As expected, after shifting from a catchment area system to a lottery system for the 'popular' school, the school segregation by income decreases, and market clearing property prices in the 'popular' neighbourhood decline, consistent with real-world reforms (Machin and Salvanes, 2016). The model reveals an unexpected effect on neighbourhood segregation by income, however, that is driven by the initial correlation between school and neighbourhood quality. After the reform, households sort by income into the 'popular' neighbourhood to enjoy the greater neighbourhood amenities, displacing lower-income parents who gained utility from the high probability of their children attending the 'good' school. Neighbourhood segregation by income therefore increases, but neighbourhood segregation by family type decreases. This model therefore reveals the potentially complicated general equilibrium effects of reforms to school admissions priorities, and school choice more generally.

As discussed in chapter 5, in addition to nuanced policy implications, there are political economy considerations from moving away from geographical admissions priorities. For example, a lottery for school admissions was trialled in Brighton and Hove (see Allen, Burgess, and McKenna, 2013), and was vehemently opposed by residents in formerly desirable catchment areas. Local pressure meant that the lottery became district-wide rather than Local Authority wide, with 'district' arguably gerrymandered to retain segregation between higher and lower attaining schools. These political problems are noted by Hamnett and Butler, 2013 as a significant barrier to implementation.

Burgess, Greaves, and Vignoles, 2020 consider the feasibility of 'marginal ballots' – where a substantial proportion of school places would be allocated as normal, and the remaining places would be reserved for a random draw among other applicants - and a simple priority for disadvantaged families, or reserved places for applicants from less well-off backgrounds. Burgess, Greaves, and Vignoles, 2020 state that:

> Our personal view of the evidence is that there is much to recommend a marginal ballot approach, with perhaps 10% or 20% of places reserved for non-priority applicants. However, how the ballot is communicated to potential applicants is also key to avoid a rejection of a 'postcode lottery' approach which is perceived to be a major problem in other public services.

Another option, fair-banding, where an equal proportion of pupils from across ability bands is admitted to the school, could be expanded across England, as considered by Hamnett and Butler, 2011 and Hamnett and Butler, 2013. Hamnett and Butler, 2013 state that fair banding ‘appears to have much to commend it in terms of overcoming the role of distance-based allocational systems’. Future research will explore the effect of existing fair-banding systems, retained by three Local Authorities in London (see West, 2005), on equality of access and educational outcomes.

Finally, ‘selective’ allocation, where pupils with the highest ability are admitted to prestigious schools, reduces the link between home location and school access. Drawing on wider research, this system has been found to widen inequalities in educational outcomes (Atkinson, Gregg, and McConnell, 2006), leading to wider inequalities in earnings (Burgess, Dickson, and Macmillan, 2019). Chapter 7 reveals that there are limited effects on neighbourhood formation, despite the weakened residential incentives. This may be because there is correlation between neighbourhood ‘quality’ and school ‘quality’ (as modelled in chapter 6) that maintain residential patterns in areas with selective schools.

The new evidence on endogenous residential location from this thesis is useful for primarily two strands of literature. First, these results give interpretation to the ‘black box’ of the estimated house price premium around ‘good’ schools. Chapter 6 shows that these estimates cannot be interpreted as parents’ demand for school quality, as the premium is dampened by the presence of households that do not gain flow utility from school quality (such as non-parents), but value neighbourhood attributes that are correlated with school quality. Also, the results from chapter 7 suggest that rather than being a widespread phenomenon, moving for schools occurs for a minority of households, for a minority of schools. It would be informative for forthcoming estimates of the property price premiums around ‘good’ schools to include the distribution of the premium, including the percentage of schools that have a non-zero premium. Residential moves are less common later in life, suggesting that moving costs (monetary and non-monetary) are higher at this stage. Referring back to chapter 6, this means that older households who no longer have children do not significantly dampen the observed premium by moving away from ‘good’ schools. Confirming the intuition from the existing literature, these households are more affluent (in this thesis, measured by social class).

Why is endogenous residential location confined to higher-social class households? Two candidate explanations are differences in budget constraints and differences in preferences for school quality. Budget constraints bind more tightly for households with lower socio-economic status, and may prohibit moving to the area with the most desirable school (and therefore the highest premium). Evidence from chapter 4 also points to constraints rather than heterogeneous preferences, as all households respond to information revealed about school quality, suggesting that preferences are similar across groups. As mentioned above, however, previous literature typically finds the opposite. This is a crucial empirical fact to test, properly accounting for endogenous residential choices and varying ‘outside options’ (for example private schools) across household types.

The second strand of literature is parents’ revealed preferences for school attributes from school choices, that has to date uniformly assumed a fixed residential location from which households make their school choices. Any endogenous residential location causes bias to the estimated preferences. While acknowledged as problematic, the scale of this problem has remained unquantified. This thesis reveals that residential choices are endogenous for a limited set of households with high social class. This is a positive result for the literature, in that estimating discrete choice models for lower social class households should be free of this form of bias. Less encouragingly, however, it is not possible to infer differences in preferences between groups from these models, where location is endogenous for one group.

For all potential policy reforms to school admissions under school choice, careful consideration would need to be given to 'tipping points' or 'white flight' from the area or from the state sector entirely (Reber, 2005, Noreisch, 2007, Baum-Snow and Lutz, 2011, Vowden, 2012), which could reduce integration overall. Considering this, Vowden, 2012 states that:

> Even a relatively modest reform – such as a controlled choice system designed to ensure that the proportion of children eligible for free school meals in every Hammersmith & Fulham primary school fell between 25 and 50% – might prompt a significant exodus of middle-class parents from the local state system. The most popular schools in the study area had lower proportions than that, and for many respondents this was an important part of their appeal.

Another, mainly theoretical, literature studies the effect of affirmative action policies, where seats at popular schools are reserved for certain groups of pupils (Hafalir, Yenmez, and Yildirim, 2013, Ehlers et al., 2014, Echenique and Yenmez, 2015, Doğan, 2016, Klijn, Pais, and Vorsatz, 2016, Dur et al., 2018, Escobar and Huerta, 2021). Escobar and Huerta, 2021 find that affirmative action is an effective tool for reducing segregation, while Dur et al., 2018 find that the ordering of the precedence matters for the eventual assignment and Kojima, 2012 notes the potential perverse effects.

Exit from the state sector, and differential access to such 'outside options' across households, is an important channel that this thesis has left unexplored, despite theoretical and empirical evidence for its importance (Epple and Romano, 2003, Calsamiglia, Fu, and Güell, 2020, Bibler and Billings, 2020, Calsamiglia, Martínez-Mora, and Miralles, 2020). This is an important avenue for future research, exploring how private sector choices respond to different school choice policy environments, incorporating endogenous residential and private sector choices. Modelling should also account for externalities to households that do not have children. Chapter 6 reveals, for the first time, the significant spillovers from the general equilibrium effects from the school choice system to this large percentage of households.

Together, these conclusions have useful implications for policy and future research. For policy, re-designing the school choice environment is unlikely to dramatically change neighbourhoods, especially in the short-run, although some movement by higher social class households would be expected. Policy reform may also alter the externalities to non-parents, as demonstrated in chapter 6 but has to date not been considered. The lever of school choice design therefore has a more direct effect on school composition than on neighbourhood composition, although it can serve to integrate both. For future research, it is important to model endogenous residential location in response to policy reforms, but it is unlikely to be a central determinant in the resulting efficiency of the system.

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bibliography

Appendix

Table A2.1: Sample size by pupil and local area characteristics

	Number of observations
All	524,115
Pupil characteristics	
FSM	77,563
Non-FSM	446,552
EAL	96,393
Non-EAL	427,722
White British	369,635
Asian	56,209
Black	29,547
Above median SES (neighbourhood)	260,260
Below median SES (neighbourhood)	260,256
Local area characteristics	
Above median number of schools within 20km	264,598
Below median number of schools within 20km	259,517
3 choices allowed	275,225
More than 3 choices allowed	248,735

Source and note: See Table 3.1.

Table A2.2: Regression estimates

	One school choice		Maximum number of choices		First-choice school is closest school		Attend first choice school		Offer from first choice school		Quality of first choice school	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
FSM	0.06***	0.06***	-0.04***	-0.04***	0.03***	0.03***	-0.02***	0.03**	-0.01***	-0.01**	-4.68***	-4.60***
	(0.00)	(0.00)	(0.01)	(0.01)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.30)	(0.30)
EAL	-0.02***	-0.12***	0.01	0.12***	0.01	-0.08***	-0.02*	-0.10***	-0.01*	-0.11***	-1.62***	3.09***
	(0.00)	(0.01)	(0.01)	(0.02)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.31)	(0.44)
Asian	-0.15***		0.17***		-0.11***		-0.10***		-0.13***		7.16***	
	(0.01)		(0.02)		(0.01)		(0.01)		(0.01)		(0.53)	
Black	-0.20***		0.19***		-0.18***		-0.17***		-0.18***		5.86***	
	(0.02)		(0.02)		(0.01)		(0.01)		(0.01)		(0.59)	
Below median	0.00	-0.01	0.00	0.02	-0.04***	-0.05***	-0.03***	-0.04***	-0.02***	-0.03***	-7.92***	-7.55***
SES	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.00)	(0.00)	(0.00)	(0.00)	(0.35)	(0.34)
(neighbourhood)												
Above median number of schools	-0.08***	-0.09***	0.03	0.05*	-0.07***	-0.08***	-0.02***	-0.03***	-0.03***	-0.04***	1.17*	1.59***
within 20km	(0.02)	(0.02)	(0.03)	(0.03)	(0.02)	(0.03)	(0.01)	(0.01)	(0.01)	(0.01)	(0.47)	(0.47)
More than 3	-0.13***	-0.14***	-0.16***	-0.15***	-0.07*	-0.08*	-0.06***	-0.07***	-0.07***	-0.09***	1.79**	2.23***
choices allowed	(0.03)	(0.03)	(0.05)	(0.05)	(0.03)	(0.03)	(0.01)	(0.01)	(0.01)	(0.01)	(0.56)	(0.55)
School quality											0.35***	0.35***
in local area											(0.02)	(0.02)
Ethnic group included	Y	N	Y	N	Y	N	Y	N	Y	N	Y	N
Number of observations	520,499	520,499	520,499	520,499	515,662	515,662	516,871	516,871	504,583	504,598	458,510	458,510

Source and note: See Table 3.1.

Table A3.1: Final sample selection

Selection criteria	**N**
N schools in school-level choice data	16564
& school is in pupil-level choice data (is a closest school)	16257
& School ID is recognised	15699
& link to Ofsted data	15684
& not split school	15681
& not merging school	15651
& not special independent or other school	15642
& relevant Ofsted data (not ever 9 or ungraded)	15578
& have prior grade	15319
& not inspected twice in the same year	15281
& all outcomes observed in treatment year	15255
& restrict to observed in 2014 or 2015 only	15236

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

A4.1 Bias correction for the Dissimilarity Index

The correction in Allen et al., 2015 begins by calculating, for each geographical unit, the observed proportions $p^{\text{obs}}_{g,t} = n_{g,t}/N_t$ as estimates of the population parameters $\pi_{g,t}$. Since the proportions are modelled as arising from a multinomial distribution, in large samples the random variable $Z_g = |p^{\text{obs}}_{g,0} - p^{\text{obs}}_{g,1}|/\hat{\sigma}_g$ has approximately a folded normal distribution with mean μ_g and variance σ^2_g. The variance is estimated by plugging in

$$\hat{\sigma}^2_g = \frac{p^{\text{obs}}_{g,0}(1-p^{\text{obs}}_{g,0})}{N_0} + \frac{p^{\text{obs}}_{g,1}(1-p^{\text{obs}}_{g,1})}{N_1},$$

and the mean by maximum likelihood:

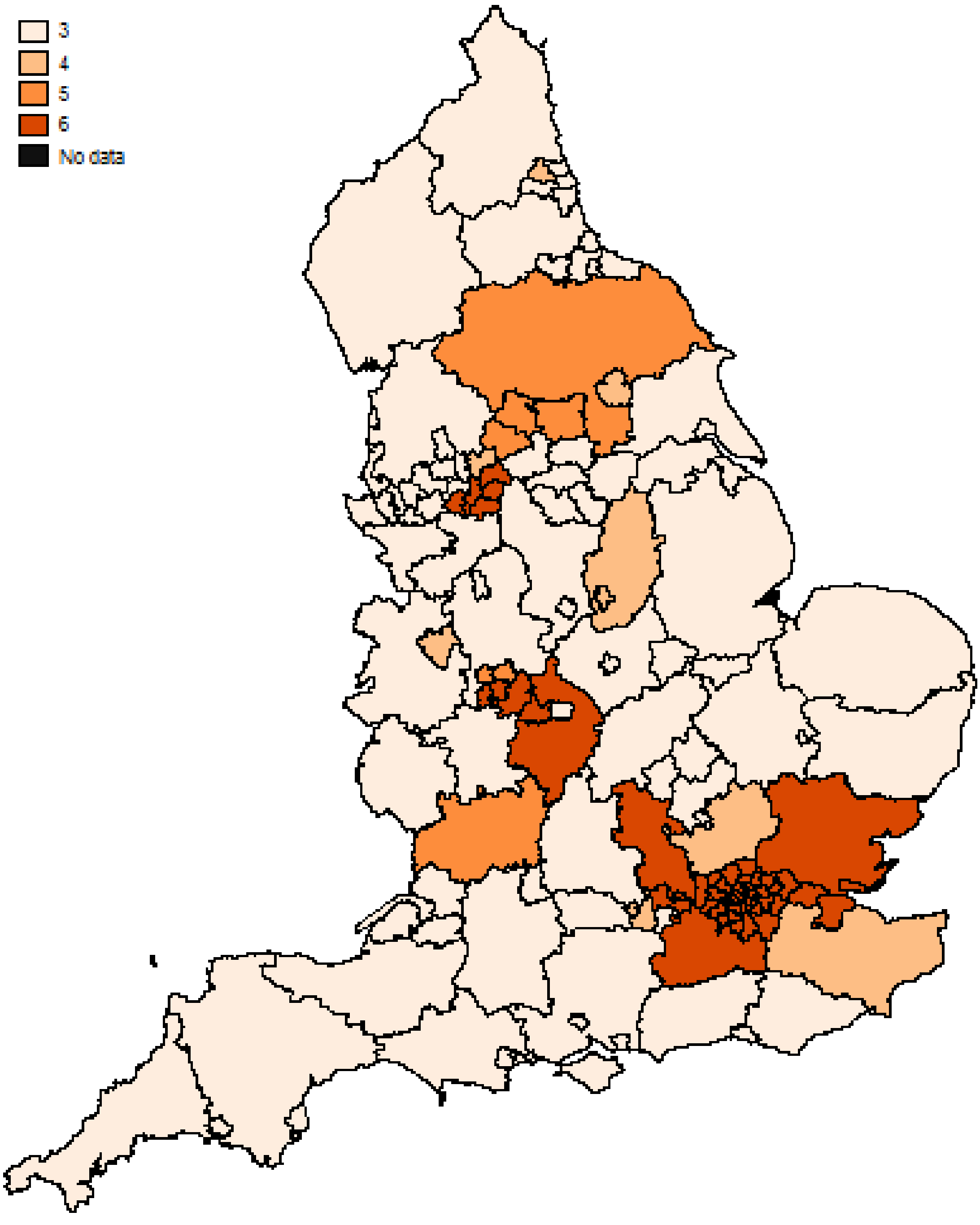
$$\hat{\mu}_g = \underset{\mu_g \in \mathbb{R}^+}{\arg\max} \left\{ \phi(Z_g - \mu_g) + \phi(Z_g + \mu_g) \right\},$$

where $\phi(\cdot)$ is a standard normal density function. Since $\mu_g = |\pi_{g,0} - \pi_{g,1}|/\sigma_g$ then the density-corrected estimate of the Index of Dissimilarity is

$$D_{\text{dc}} = \frac{1}{2}\sum_{g=1}^{G} \hat{\sigma}_g \hat{\mu}_g.$$

For any $Z_g \leq 1$, that is, whenever the observed absolute difference is smaller than the standard deviation of the estimate, the density corrected estimate of unevenness at that site is zero, and as Z_g grows, μ_g approaches Z_g.

Figure B2.1: Maximum number of choices possible



Source: See Table 3.1.
Note: Borders shown are Local Authority (LA) boundaries.

A5.2 Extensions to the model

A5.2.1 Private school outside-option

The current model excludes an outside option for schooling in the form of private education. Intuitively, a private school option decreases the utility from priority at the above average school. Whether the private school option is taken up depends on the relative quality of the private, above average and below average schools, the school priority system in place and the equilibrium rents in the High and Low quality neighbourhoods.

The utility function including a private school option is would be identical to equation 4, but would include additional terms for the utility of attending the private school (θ_P) and the cost of attending the private school (p). Note that we would assume households incur a cost of travel if choose to attend the private school. This is equivalent to assuming that the private school is outside both High and Low quality neighbourhoods. This is consistent with our chosen neighbourhoods in Bristol, but could easily be modified to apply to other cities.

Solving this model would require households to compare the expected utility from each neighbourhood and school across all periods. The rent premium in the High quality neighbourhood under the geographic preference system is likely to decrease, as some households would prefer to pay the private school fees than additional rent to gain access to a high performing school.

A5.2.2 Endogenous school quality

School quality is fixed and exogenous in our baseline model. School quality (depending on the definition) is likely to respond to multiple factors, however, such as school inputs, pupil inputs, teacher quality and management quality. As an extension of the model, we could therefore consider the case the school quality changes in response to the pupil composition, which we could operationalise by increasing households' utility from the Above Average school with the mean income of pupils at the school. This would incorporate changes in school quality which result from changes in pupil demographics in one dimension, and teacher and management quality in response to this.

A5.2.3 Heterogeneous preferences

Our baseline model assumes that all households have the same preferences for school quality. This assumption is supported by some empirical work but contradicted by others (Hastings, Kane, and Staiger, 2009, Burgess et al., 2015, Borghans, Golsteyn, and Zölitz, 2015, Abdulkadiroğlu, Agarwal, and Pathak, 2017, Glazerman and Dotter, 2017, Beuermann et al., 2018, Harris and Larsen, 2019, Ruijs and Oosterbeek, 2019, Ajayi and Sidibe, 2020, Walker and Weldon, 2020). We could extend our baseline model to incorporate increasing utility from attending the Above Average school for higher income households.

A5.3 Mechanisms

This section gives more detail to the discussion in section 6.3.2 about the underlying responses to a school choice admissions priorities reform. To explore the underlying mechanisms of the model, Figure B5.1 shows the equilibrium price in H under the reform as the cost of transport and costs of moving varies, under the scenario when there is no correlation between income and ability (and so parents can't predict admission to school).[105] This shows that equilibrium

[105] As noted, the discussion in the previous sub-section also holds: the equilibrium price change is a function of the number of parents in the population.

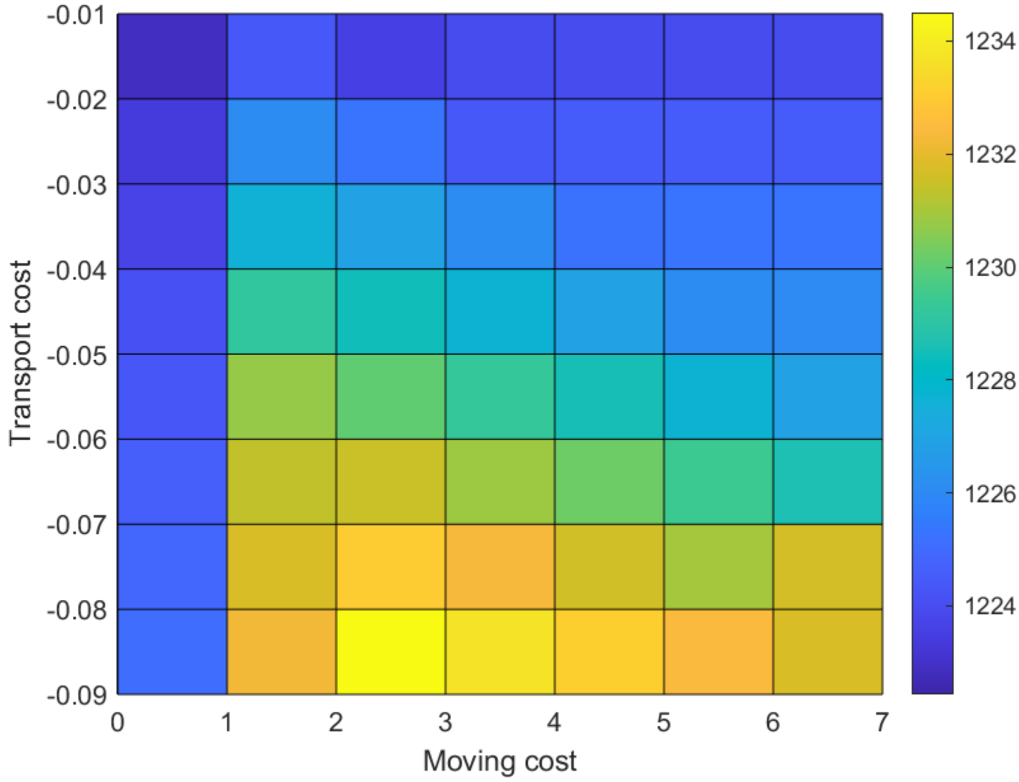


Figure B5.1: Equilibrium outcomes as transport costs and moving costs vary. 'Moving cost' is the cost a household incurs to move neighbourhoods across periods (in absolute values: 0 is low and 7 is high moving cost). 'Transport cost' is the cost a household incurs to travel across neighbourhoods to school.

rents in H remain highest under this reform when transport costs and moving costs are high. When moving costs are low, it is more likely that households re-optimise location in response to the reforms. When transport costs are low, it is more likely that households relocate to the neighbourhood where their child(ren) is assigned. Discussion of external validity of studies of this kind must therefore consider the moving costs and transport costs in the study setting, in addition to the population composition, as there will be different price across contexts. Note that when households *can* predict admission to school (perhaps when there is a perfect correlation between income and ability) this would remove uncertainty for households with young children and induce more sorting into neighbourhood by income (to reduce travel costs and moving costs across the lifecycle).[106]

[106]A potential explanation from outside our model is that elementary and middle school quality is correlated with high school quality. The observed remaining premium may therefore be related to earlier stages of education. Our model also treats school quality as exogenous, but if school quality is endogenous to the peer group, then the reform might also affect observed or expected school quality, and therefore school choices. The existing of private school options is also important to consider, as this may provide an 'exit' option for higher income households.

A5.4 Data Appendix

This appendix provides detail on all the sources of data used for the stylised facts and calibration of the model.

A5.4.1 School Quality

Publicly available information on school performance measures is downloaded from the 'Find and compare schools in England' website. We use the most commonly used performance metrics for schools used in this time period: the percentage of pupils that achieve at least 5 GCSE grades at A* to C (including English and Maths) and the percentage of pupils that achieve the English Baccalaureate (5+ A*-C grades in English, mathematics, two sciences, a foreign language and history or geography). The most recent Ofsted grade is taken from the Ofsted website. The overall measure of school performance given by Ofsted can be Outstanding, Good, Satisfactory (now Requires Improvement) or Inadequate.

A5.4.2 School Choices

School choices are from the earliest administrative data on school choices available in England (for entry in the 2014/15 academic school year). The parental choice data contain for each pupil: the ID of each nominated school (in Bristol the first, second and third choice), the ID of the school that the child was offered, which may differ from the school that the pupil was finally enrolled in. We use the earliest recorded postcode of the household to link households to Lower Level Super Output Areas and hence catchment areas/school zones. Access to these data was provided by the Department for Education, through the National Pupil Database application process.

A5.4.3 Census 2011

Publicly available information from the 2011 Census at the Lower Level Super Output Area (LSOA) from nomis (official labour market statistics). We use information on household life-stage (dataset LC1115EW), which contains the number of families by banded age group of the family reference person and age of the youngest dependent child.[107] The age bands are 16-24, 25-49, 50-64, 65 and over. We use this dataset to calculate the percentage of households with a dependent child (of any age) and percentage of families with a dependent child (or children) where the youngest is of secondary school age (10-18).

Taking person level data (rather than household or family level data) of the count of people of each age (dataset QS103EW) we also count the number of children with ages consistent with pre-school (0-4), primary school (5-10) and secondary school (11-16). We take the number of children aged 10 to be the number of children potentially applying to a state-school from the LSOA. We combine this with information on the actual number of applicants to state-schools from the LSOA from the national school choice data described above to calculate the proportion of pupils that apply to a state school. There is likely to be some measurement error in this variable, as the Census data are from 2011 while the school choice data are from 2013.[108] In this time the population may have changed.

We derive the number of households in each period of our model from information on the age group of the household reference person (dataset LC1102EW). The given age bands are: age

[107] Note that family and household reference person can be different if multiple families live within the same household. This applies to only 2.7% of households with dependent children on average across Bristol.

[108] School choices are submitted in October the year before the academic year of entry. Pupils starting school in September 2014 (academic year 2014/2015 therefore submitted their choices in October 2013).

24 and under, 25-34, 35-49, 50-64, 65 and over. To calculate the number of households in each life-stage defined by our model, we assume an even distribution of households across ages within age groups. For example, the first time period $t = 0$ is when households are between 25 and 40. In this case the total number in this group is all households from the second age group (25-34) and 5/15 households from the third age group (35-49). The percentage of households in each life-stage is calculated using the total number of households where the reference person is above 25 as the denominator. The total stock of households in each neighbourhood is defined from these data, using the total number of households where the household reference person is above 24.

We present the percentage of properties with at least 3 bedrooms (dataset QS411EW) and at least 6 rooms (dataset QS407EW) for stylised facts but not the residual property prices or calibration. This information is also taken from nomis at the LSOA level.

Finally, we use information on the National Statistics Socio-Economic Classification (NS-SEC) (dataset LC6101EW) available from nomis at the LSOA level. This gives us the percentage of households where the household reference person is in each broad NS-SEC category by broad age category (16-34, 35-49, 50-64, 65 and over). From this information, we compute average earnings at the LSOA level using information on median earnings per NS-SEC category and age group available in the Labour Force Survey, which is described below.[109]

A5.4.4 Calculating the proportion of households in each family type and life-stage

The proportion of households of each life-stage and family type is estimated from the 2011 Census data. This is used for subsequent calculations for the number of applicants and demand for housing across neighbourhoods and so on. The process is as follows:

- Find the proportion of households currently with a dependent child, where the family reference person is above 24 and below 50.[110]

- Assign this total proportion of households across life-stages according to the proportion of each life-stage observed in the Census. For example, if the proportion of families currently with a dependent child (where the family reference person is between 25 and 49) is 63% and the proportion of households in life-stage 0 of all those in life-stage 0, 1, 2 is 59%, the proportion of households currently with a dependent child is 0.37.

- Separate the proportion in each life-stage by family type using the national proportion of households with completed fertility of one. [111] If 0.22 of households with completed fertility more than 0 have completed fertility of 1, then the final calculation for the proportion of households in life-stage 0 with completed fertility of one is 0.37 (from above) multiplied by 0.22, equal to 0.08. Similarly, the calculation for those with completed fertility more than one is 0.37*(1-0.22) = 0.29.

- Repeat this calculation for all life-stages for family type 1 and 2.

- Sum all completed cells for life-stages 0 to 4 for family type 1 and 2.

- Assign the remaining percentage of household types to family type 0.

[109] There are three age/NS-SEC categories in the LFS for which median earnings are not available. These three categories are excluded from the denominator in the final calculation of average earnings.

[110] These ages broadly correspond to the ages we specify for the limits of life-stage 0 - pre-school choice phase - and life-stage 2 - secondary school phase.

[111] This statistic is from the Office for National Statistics, for women born in 1972 and completed fertility at age 45. 22% of women in this cohort that have at least one child have completed fertility of one. Source: 'Childbearing for women born in different years'.

- As above, separate family type 0 into life-stages by the observed proportion of each family type in Census 2011 data.
- Input the matrix of population proportions across 3 family types and 4 life-stages.

This matrix is then used to define the number of applicants across and within our catchment areas/school zones of interest. The number of applicants is the sum of the number of households in life-stage 2 (school choice phase) in family type 1 or 2, divided by two as our model assumes two cohorts of pupils always in this phase. The number of *uncertain* applicants is the sum of the number of households in life-stage 2 in family type 1, divided by 2, and family type 2, divided by 4. This is because half of those applying with a sibling have an older sibling already at the school and so the result of the application is not uncertain. This yields a sensible number of applicants from our two chosen catchment areas: 278 compared to 277 in the school choice data described above.

A5.4.5 Labour Force Survey

The Labour Force Survey (LFS) collects detailed information on employment and earnings from approximately 39,000 households (or approximately 95,000 individuals) every quarter. We use data downloaded from the UK Data Archive (End User rather than Secure Access version) for the quarter from July to September 2011. From these data we calculate total net weekly pay per individual, and calculate the median net weekly pay per broad NS-SEC group and age group (to match the age groups in the 2011 Census). The median value for each NS-SEC/age group was then applied to each NS-SEC group specific household from the 2011 Census, then averaged to create a measure of imputed average earnings.

Gross earnings over the lifecycle: Figure 2.2: https://www.ifs.org.uk/comms/r92.pdf

A5.4.6 Low income score

Low income score is the 2010 Index of Multiple Deprivation (Income Domain), which classifies small areas according to the proportion of the population in an area experiencing deprivation according to low income. This measure captures variation only at the bottom of the income distribution, so is useful for stylised facts but not for calibration of the model which requires information on income types across the distribution.

A5.4.7 Property Prices

Property prices are taken from the Price Paid Data from HM Land Registry, which covers all property sales in England and Wales that are sold for full market value and are lodged with HM Land Registry for registration. We download the single file (including prices paid between 1 January 1995 to the most current monthly data) and extract all prices paid in 2011. The dataset includes information on the transaction price, type (Detached, Semi-detached, Terrace or Flat/Maisonette), whether the property is newly built and whether the property is freehold or leasehold. Full address information is provided, including the Primary Addressable Object Name (PAON), Secondary Addressable Object Name (SAON) and postcode. The data was merged to Lower Level Super Output Area (LSOA) by postcode using the National Statistics Postcode Lookup for August 2011. 99.27% of postcodes match between the price paid data and this postcode directory.

1.76% of addresses have multiple sales recorded in 2011. For these properties we take the average price. Median and mean prices at the LSOA level are calculated by collapsing the postcode to LSOA. These prices do not take into account the floor size and number of rooms in the property. For this information we use data from the Energy Performance Certificate database, which is described next.

A5.4.8 Energy Performance of Buildings Data: England and Wales

The Energy Performance of Buildings Data: England and Wales (EPC) is a database of all Energy Performance Certificates which is provided under Open Government Licence v3.0. Relevant for our purposes, the data contains information on the number of rooms, total floor size, single glazing and presence of an open fireplace for each property. Comparable address information to the prices paid data (described above) is also provided, although some cleaning is required to make it as similar as possible. For most properties, 'address1' in the EPC corresponds to PAON in the prices paid data, and 'address2' corresponds to SAON. This is not the case for flats (or apartments or units), however. The cleaning process is therefore:

- Replacing the SAON with information in the field 'address1' if the property is a flat (for example 'flat 9'.
- Removing information from the PAON now contained in SAON if the property is a flat.
- For properties with a house number, retain the house number only.

87.65% of properties in Bristol merge perfectly using PAON, SAON and postcode, and so have a complete record of price paid and property characteristics.[112] These properties are used to estimate the price residual for properties across LSOAs, which is explained in further detail below.

A5.4.9 Residual Property Prices

The 87.65% of properties with a perfect match between the price paid and EPC data are used to run a linear regression where price is the dependent variable and the independent variables are total floor area, an indicator for the presence of an open fireplace, total floor area interacted with whether the property is a flat/maisonette, and total floor area interacted with the number of habitable rooms.[113] The residual price is therefore the property price accounting for floor size, number of habitable rooms, type and a proxy for period feature of the property. The residual price does not account for other features that may affect sale prices and vary across areas, for example the total property area (including garden), off-street parking, and the internal decoration. It does, however, capture important determinants of sale prices. We therefore interpret the residual price as reflecting demand for neighbourhood and local school attributes in addition to some unobserved characteristics of the property.

[112]Properties without a perfect match are then matched by type (flat/house), PAON and postcode (4.23%), type, SAON and postcode (0.17%), type and postcode (6.75%), and finally type and postcode sector (1.19%).
[113]We also exclude 11 properties where a mean price had been imputed as there were multiple sales in 2011.

Table A6.1: Final sample selection for the ONS Longitudinal Study

Census	Cohort 1	Cohort 2	Cohort 3
No restrictions	358,309	334,130	351,125
Age consistent across Censuses	358,165	334,029	351,020
Age correct at Key Census	340,561	332,257	350,147
Observe social class at Key Census	328,806	319,988	338,538
Observe 'ever parent'	328,630	318,893	336,389
Observe area characteristics (1991, 2001 and 2011)	306,765	298,419	314,379
Observe move (1991, 2001 and 2011)	248,952	251,542	281,190
Observations (1991, 2001 and 2011 only)	149,389	150,175	166,473

Source: ONS Longitudinal Study. Note: This table shows the process from unrestricted to final sample. The number of observations is at the individual by Census level. 'Age consistent across Censuses' refers to age correctly increasing by 10 years across Censuses, or it is possible to infer the correct age sequence across Censuses. The 'Key Census' is the closest Census to when the LS sample member is aged 40. For example, if an individual was 34 in 1991 and 44 in 2001, then the variables from the 2001 census would be used to define their 'key age' variables. 'Age correct at Key Census' means that the Key Census occurs at the expected age. Where the Key Census is too early or too late, it means that LS sample members are missing from the Census closest to when they are 40. 'Observe social class at Key Census' means that social class is observed for the LS sample member at the Key Census. 'Observe 'ever parent'' means that whether the LS sample member ever becomes a parent is observed. 'Observe area characteristics (1991, 2001 and 2011)' means that it is possible to match area-level data on local school quality, local prices, and the admissions system to the LS sample member's home postcode in each of the relevant Census years. 'Observe move (1991, 2001 and 2011)' means that whether the LS sample member moved between Censuses is observed (non-missing) in each of the relevant Census years. The final row, 'Observations (1991, 2001 and 2011 only)' shows the number of observations in the relevant Census years only.

Table A6.2: Characteristics of LS sample members that are resident in or move between Local Authorities with 'selective' or 'grammar' school admissions, conditional on moving at least twice

Cohort 1 Variable	Always geographic Mean (SD)	Always non-geographic Mean (SD)	Switch to non-geographic Mean (SD)	Switch to geographic Mean (SD)	Switch to and from Mean (SD)
Not born in UK	0.02 (0.13)	0.02 (0.13)	0.02 (0.12)	0.01 (0.10)	0.01 (0.09)
Both born in UK	0.78 (0.42)	0.78 (0.42)	0.78 (0.42)	0.79 (0.41)	0.72 (0.45)
Professional	0.02 (0.14)	0.01 (0.11)	0.03 (0.17)	0.03 (0.17)	0.02 (0.14)
Intermediate	0.23 (0.42)	0.19 (0.39)	0.31 (0.46)	0.31 (0.46)	0.38 (0.48)
Skilled Non-Manual	0.22 (0.41)	0.22 (0.42)	0.23 (0.42)	0.25 (0.43)	0.21 (0.41)
Skilled Manual	0.23 (0.42)	0.26 (0.44)	0.19 (0.39)	0.19 (0.39)	0.21 (0.41)
Partly Skilled	0.23 (0.42)	0.23 (0.42)	0.18 (0.38)	0.17 (0.38)	0.14 (0.35)
Unskilled	0.07 (0.25)	0.08 (0.27)	0.04 (0.20)	0.04 (0.19)	0.03 (0.16)
Degree	0.41 (0.49)	0.34 (0.47)	0.50 (0.50)	0.51 (0.50)	0.53 (0.50)
One in work	0.29 (0.45)	0.31 (0.46)	0.30 (0.46)	0.29 (0.45)	0.29 (0.46)
Both in work	0.65 (0.48)	0.64 (0.48)	0.65 (0.48)	0.67 (0.47)	0.66 (0.48)
Partner	0.86 (0.35)	0.85 (0.35)	0.88 (0.33)	0.88 (0.33)	0.82 (0.39)
'Ever parent'	0.80 (0.40)	0.82 (0.38)	0.77 (0.42)	0.73 (0.44)	0.71 (0.45)
Cohort 2					
Not born in UK	0.06 (0.24)	0.04 (0.19)	0.03 (0.17)	0.03 (0.16)	0.02 (0.13)
Both born in UK	0.78 (0.42)	0.80 (0.40)	0.77 (0.42)	0.77 (0.42)	0.71 (0.45)
Professional	0.02 (0.13)	0.01 (0.12)	0.02 (0.15)	0.03 (0.16)	0.04 (0.20)
Intermediate	0.23 (0.42)	0.20 (0.40)	0.29 (0.45)	0.32 (0.47)	0.30 (0.46)
Skilled Non-Manual	0.22 (0.41)	0.25 (0.43)	0.25 (0.44)	0.26 (0.44)	0.31 (0.46)
Skilled Manual	0.23 (0.42)	0.23 (0.42)	0.19 (0.39)	0.16 (0.37)	0.14 (0.34)
Partly Skilled	0.22 (0.42)	0.22 (0.41)	0.18 (0.39)	0.17 (0.37)	0.15 (0.36)
Unskilled	0.08 (0.26)	0.07 (0.26)	0.05 (0.21)	0.05 (0.22)	0.04 (0.20)
Degree	0.37 (0.48)	0.33 (0.47)	0.43 (0.49)	0.45 (0.50)	0.44 (0.50)
One in work	0.30 (0.46)	0.29 (0.45)	0.34 (0.47)	0.32 (0.47)	0.33 (0.47)
Both in work	0.64 (0.48)	0.66 (0.47)	0.60 (0.49)	0.64 (0.48)	0.63 (0.48)
Partner	0.89 (0.31)	0.90 (0.30)	0.88 (0.33)	0.89 (0.32)	0.84 (0.36)
'Ever parent'	0.86 (0.35)	0.88 (0.32)	0.82 (0.38)	0.80 (0.40)	0.79 (0.41)
Cohort 3					
Not born in UK	0.05 (0.23)	0.05 (0.21)	0.03 (0.17)	0.03 (0.18)	0.02 (0.14)
Both born in UK	0.80 (0.40)	0.81 (0.39)	0.81 (0.39)	0.81 (0.39)	0.85 (0.36)
Professional	0.03 (0.17)	0.03 (0.16)	0.03 (0.16)	0.03 (0.16)	0.03 (0.16)
Intermediate	0.24 (0.43)	0.23 (0.42)	0.26 (0.44)	0.29 (0.45)	0.28 (0.45)
Skilled Non-Manual	0.21 (0.41)	0.21 (0.41)	0.23 (0.42)	0.24 (0.43)	0.19 (0.39)
Skilled Manual	0.23 (0.42)	0.24 (0.42)	0.22 (0.41)	0.20 (0.40)	0.24 (0.43)
Partly Skilled	0.20 (0.40)	0.20 (0.40)	0.18 (0.38)	0.17 (0.37)	0.13 (0.33)
Unskilled	0.07 (0.26)	0.07 (0.25)	0.07 (0.25)	0.05 (0.21)	0.09 (0.29)
Degree	0.28 (0.45)	0.26 (0.44)	0.28 (0.45)	0.33 (0.47)	0.25 (0.43)
One in work	0.36 (0.48)	0.37 (0.48)	0.33 (0.47)	0.38 (0.49)	0.32 (0.47)
Both in work	0.61 (0.49)	0.60 (0.49)	0.64 (0.48)	0.59 (0.49)	0.65 (0.48)
Partner	0.91 (0.29)	0.91 (0.29)	0.92 (0.27)	0.88 (0.32)	0.91 (0.29)
'Ever parent'	0.88 (0.32)	0.90 (0.30)	0.88 (0.33)	0.84 (0.36)	0.91 (0.29)

Source: ONS Longitudinal Study and School Performance Tables (Department for Education). Note: The final common sample is applied. 'Selective' and 'grammar' are used interchangably. Local Authorities are defined as 'selective' if at least 25% of schools are classified as 'selective' or 'modern' in the School Performance Tables around the Census years 1991, 2001 and 2011. The definition is refined by consistent across Census years and accounting for changes in Local Authority boundaries between the 1991 and 2001 Censuses.

Table A6.3: Difference-in-differences estimation for the effect of geographical admissions priorities for secondary schools in England on the probability of moving, by cohort and age band

	Cohort 1	Cohort 2	Cohort 3
0-20	-0.009		
	(0.044)		
21-25	0.009		
	(0.025)		
26-30	0.194***	0.143**	
	(0.026)	(0.049)	
31-35	0.193***	0.193***	
	(0.025)	(0.029)	
36-40	0.122***	0.150***	0.003
	(0.026)	(0.030)	(0.052)
46-50	-0.090**	-0.091**	-0.128***
	(0.028)	(0.031)	(0.032)
51-55		-0.199***	-0.173***
		(0.031)	(0.031)
56-60		-0.287***	-0.312***
		(0.035)	(0.033)
61-65			-0.329***
			(0.032)
66-70			-0.362***
			(0.038)
'Ever parent'	0.005	0.033	-0.015
	(0.021)	(0.023)	(0.023)
Geographic LA	-0.024	0.006	-0.043+
	(0.020)	(0.022)	(0.023)
Geographic LA # 'Ever parent'	-0.016	-0.055*	-0.017
	(0.022)	(0.024)	(0.024)
0-20 # 'Ever parent'	0.042		
	(0.052)		
21-25 # 'Ever parent'	0.171***		
	(0.028)		
26-30 # 'Ever parent'	0.122***	0.197***	
	(0.029)	(0.054)	
31-35 # 'Ever parent'	0.106***	0.090**	
	(0.028)	(0.032)	
36-40 # 'Ever parent'	0.049+	-0.003	0.063
	(0.029)	(0.033)	(0.056)
46-50 # 'Ever parent'	-0.062*	-0.030	0.027
	(0.032)	(0.034)	(0.034)
51-55 # 'Ever parent'		-0.016	0.011
		(0.034)	(0.033)
56-60 # 'Ever parent'		0.012	0.046
		(0.038)	(0.035)
61-65 # 'Ever parent'			0.045
			(0.034)
66-70 # 'Ever parent'			0.034
			(0.040)

Continued on next page

Table A6.3 – continued from previous page

	Cohort 1	Cohort 2	Cohort 3
0-20 # Geographic LA	-0.046		
	(0.047)		
21-25 # Geographic LA	0.017		
	(0.027)		
26-30 # Geographic LA	-0.042	0.049	
	(0.027)	(0.051)	
31-35 # Geographic LA	0.025	-0.009	
	(0.027)	(0.030)	
36-40 # Geographic LA	-0.002	-0.019	0.074
	(0.027)	(0.032)	(0.056)
46-50 # Geographic LA	-0.006	-0.021	0.016
	(0.030)	(0.033)	(0.034)
51-55 # Geographic LA		-0.012	-0.060+
		(0.033)	(0.033)
56-60 # Geographic LA		0.033	0.030
		(0.037)	(0.035)
61-65 # Geographic LA			0.009
			(0.034)
66-70 # Geographic LA			0.028
			(0.040)
0-20 # Geographic LA # 'Ever parent'	0.048		
	(0.055)		
21-25 # Geographic LA # 'Ever parent'	-0.010		
	(0.030)		
26-30 # Geographic LA # 'Ever parent'	0.050	-0.043	
	(0.031)	(0.057)	
31-35 # Geographic LA # 'Ever parent'	0.002	0.012	
	(0.030)	(0.034)	
36-40 # Geographic LA # 'Ever parent'	0.004	0.020	-0.064
	(0.031)	(0.035)	(0.060)
46-50 # Geographic LA # 'Ever parent'	0.053	0.036	-0.017
	(0.034)	(0.036)	(0.036)
51-55 # Geographic LA # 'Ever parent'		0.040	0.016
		(0.036)	(0.035)
56-60 # Geographic LA # 'Ever parent'		0.004	0.001
		(0.040)	(0.037)
61-65 # Geographic LA # 'Ever parent'			-0.005
			(0.037)
66-70 # Geographic LA # 'Ever parent'			0.005
			(0.043)
N	149,389	150,175	166,473
R^2	0.096	0.143	0.058

Source: ONS Longitudinal Study linked to area characteristics from the School Performance Tables (Department for Education). Note: + p<0.10, * p<0.05, ** p<0.01, *** p<0.001. The final common sample is applied. # refers to the interaction between variables. The table shows coefficients from an ordinary least squares regression, with standard errors in parentheses.

Table A6.4: Difference-in-differences estimation for the effect of geographical admissions priorities for secondary schools in England on local school quality, by cohort and age band

	Cohort 1	Cohort 2	Cohort 3
0-20	0.177		
	(0.278)		
21-25	-0.024		
	(0.156)		
26-30	0.189	0.040	
	(0.161)	(0.295)	
31-35	-0.038	-0.064	
	(0.156)	(0.175)	
36-40	0.008	0.135	-0.230
	(0.162)	(0.185)	(0.314)
46-50	0.074	0.237	-0.225
	(0.178)	(0.189)	(0.191)
51-55		0.257	-0.120
		(0.186)	(0.185)
56-60		0.135	-0.061
		(0.212)	(0.197)
61-65			0.072
			(0.192)
66-70			0.062
			(0.230)
'Ever parent'	-0.077	0.119	-0.199
	(0.130)	(0.139)	(0.138)
Geographic LA	0.250*	0.357**	0.253+
	(0.123)	(0.134)	(0.137)
Geographic LA # 'Ever parent'	0.036	-0.154	0.157
	(0.139)	(0.147)	(0.147)
0-20 # 'Ever parent'	-0.088		
	(0.325)		
21-25 # 'Ever parent'	-0.126		
	(0.178)		
26-30 # 'Ever parent'	-0.385*	-0.051	
	(0.182)	(0.326)	
31-35 # 'Ever parent'	-0.073	-0.105	
	(0.178)	(0.192)	
36-40 # 'Ever parent'	-0.020	-0.231	0.088
	(0.183)	(0.201)	(0.339)
46-50 # 'Ever parent'	-0.015	-0.245	0.146
	(0.199)	(0.206)	(0.204)
51-55 # 'Ever parent'		-0.336+	0.230
		(0.204)	(0.198)
56-60 # 'Ever parent'		-0.132	0.044
		(0.230)	(0.210)
61-65 # 'Ever parent'			0.055
			(0.206)
66-70 # 'Ever parent'			0.108
			(0.243)

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Table A6.4 – continued from previous page

	Cohort 1	Cohort 2	Cohort 3
0-20 # Geographic LA	-0.415		
	(0.294)		
21-25 # Geographic LA	-0.154		
	(0.167)		
26-30 # Geographic LA	-0.396*	-0.200	
	(0.171)	(0.312)	
31-35 # Geographic LA	-0.028	-0.040	
	(0.167)	(0.185)	
36-40 # Geographic LA	-0.104	-0.158	0.287
	(0.173)	(0.196)	(0.336)
46-50 # Geographic LA	-0.145	-0.183	0.208
	(0.189)	(0.201)	(0.203)
51-55 # Geographic LA		-0.195	0.116
		(0.198)	(0.198)
56-60 # Geographic LA		-0.033	0.036
		(0.224)	(0.210)
61-65 # Geographic LA			-0.139
			(0.206)
66-70 # Geographic LA			-0.063
			(0.243)
0-20 # Geog. # 'Ever parent'	-0.042		
	(0.344)		
21-25 # Geog. # 'Ever parent'	-0.037		
	(0.190)		
26-30 # Geog. # 'Ever parent'	0.317	-0.085	
	(0.194)	(0.345)	
31-35 # Geog. # 'Ever parent'	-0.021	0.059	
	(0.190)	(0.204)	
36-40 # Geog. # 'Ever parent'	0.104	0.228	-0.136
	(0.195)	(0.214)	(0.363)
46-50 # Geog. # 'Ever parent'	0.136	0.265	-0.135
	(0.212)	(0.219)	(0.217)
51-55 # Geog. # 'Ever parent'		0.302	-0.192
		(0.216)	(0.212)
56-60 # Geog. # 'Ever parent'		0.077	-0.043
		(0.243)	(0.223)
61-65 # Geog. # 'Ever parent'			0.005
			(0.220)
66-70 # Geog. # 'Ever parent'			-0.097
			(0.258)
N	149,389	150,175	166,473
R^2	0.003	0.002	0.002

Source: ONS Longitudinal Study linked to area characteristics from the School Performance Tables (Department for Education). Note: + p<0.10, * p<0.05, ** p<0.01, *** p<0.001. The final common sample is applied. # refers to the interaction between variables. The table shows coefficients from an ordinary least squares regression, with standard errors in parentheses.

Table A6.5: Difference-in-differences estimation for the effect of geographical admissions priorities for secondary schools in England on the probability of moving, by cohort and age band. Highest social class only (Professional and Intermediate)

	Cohort 1	Cohort 2	Cohort 3
0-20	-0.006		
	(0.072)		
21-25	0.078+		
	(0.043)		
26-30	0.238***	0.208*	
	(0.043)	(0.082)	
31-35	0.266***	0.222***	
	(0.043)	(0.049)	
36-40	0.133**	0.151**	0.027
	(0.043)	(0.051)	(0.091)
46-50	-0.055	-0.027	-0.216***
	(0.047)	(0.053)	(0.061)
51-55		-0.203***	-0.201***
		(0.052)	(0.056)
56-60		-0.353***	-0.394***
		(0.059)	(0.064)
61-65			-0.394***
			(0.057)
66-70			-0.497***
			(0.075)
'Ever parent'	0.061	0.067+	-0.038
	(0.038)	(0.041)	(0.043)
Geographic LA	0.001	0.001	-0.075+
	(0.034)	(0.038)	(0.042)
Geographic LA # 'Ever parent'	-0.042	-0.036	0.047
	(0.040)	(0.043)	(0.046)
0-20 # 'Ever parent'	-0.014		
	(0.089)		
21-25 # 'Ever parent'	0.055		
	(0.051)		
26-30 # 'Ever parent'	0.009	0.088	
	(0.051)	(0.095)	
31-35 # 'Ever parent'	-0.027	0.020	
	(0.051)	(0.056)	
36-40 # 'Ever parent'	0.061	-0.021	0.065
	(0.051)	(0.058)	(0.101)
46-50 # 'Ever parent'	-0.172**	-0.161**	0.098
	(0.055)	(0.060)	(0.066)
51-55 # 'Ever parent'		-0.053	0.040
		(0.059)	(0.061)
56-60 # 'Ever parent'		-0.044	0.062
		(0.067)	(0.068)
61-65 # 'Ever parent'			0.065
			(0.063)
66-70 # 'Ever parent'			0.097
			(0.080)

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Table A6.5 – continued from previous page

	Cohort 1	Cohort 2	Cohort 3
0-20 # Geographic LA	-0.097		
	(0.077)		
21-25 # Geographic LA	-0.040		
	(0.046)		
26-30 # Geographic LA	-0.059	0.008	
	(0.046)	(0.086)	
31-35 # Geographic LA	-0.031	-0.006	
	(0.046)	(0.052)	
36-40 # Geographic LA	-0.011	0.009	0.048
	(0.046)	(0.054)	(0.098)
46-50 # Geographic LA	-0.056	-0.086	0.085
	(0.050)	(0.056)	(0.065)
51-55 # Geographic LA		-0.039	-0.036
		(0.055)	(0.060)
56-60 # Geographic LA		0.063	0.054
		(0.063)	(0.068)
61-65 # Geographic LA			0.026
			(0.062)
66-70 # Geographic LA			0.095
			(0.079)
0-20 # Geog. # 'Ever parent'	0.106		
	(0.095)		
21-25 # Geog. # 'Ever parent'	0.033		
	(0.055)		
26-30 # Geog. # 'Ever parent'	0.098+	-0.013	
	(0.054)	(0.100)	
31-35 # Geog. # 'Ever parent'	0.068	0.024	
	(0.055)	(0.060)	
36-40 # Geog. # 'Ever parent'	0.002	0.024	-0.055
	(0.055)	(0.062)	(0.109)
46-50 # Geog. # 'Ever parent'	0.152**	0.124*	-0.091
	(0.059)	(0.063)	(0.070)
51-55 # Geog. # 'Ever parent'		0.030	-0.060
		(0.063)	(0.066)
56-60 # Geog. # 'Ever parent'		-0.008	-0.021
		(0.070)	(0.073)
61-65 # Geog. # 'Ever parent'			-0.069
			(0.068)
66-70 # Geog. # 'Ever parent'			-0.081
			(0.084)
N	35,918	33,747	38,460
R^2	0.099	0.190	0.089

Source: ONS Longitudinal Study linked to area characteristics from the School Performance Tables (Department for Education). Note: + p<0.10, * p<0.05, ** p<0.01, *** p<0.001. The final common sample is applied. # refers to the interaction between variables. The table shows coefficients from an ordinary least squares regression, with standard errors in parentheses.

Table A6.6: Difference-in-differences estimation for the effect of geographical admissions priorities for secondary schools in England on on local school quality, by cohort and age band. Highest social class only (Professional and Intermediate)

	Cohort 1	Cohort 2	Cohort 3
0-20	0.812+		
	(0.480)		
21-25	-0.005		
	(0.286)		
26-30	0.244	0.368	
	(0.284)	(0.516)	
31-35	0.069	-0.043	
	(0.286)	(0.309)	
36-40	-0.137	0.401	0.343
	(0.287)	(0.324)	(0.534)
46-50	0.223	0.428	-0.204
	(0.311)	(0.333)	(0.358)
51-55		0.614+	-0.466
		(0.327)	(0.329)
56-60		0.492	-0.044
		(0.375)	(0.374)
61-65			0.050
			(0.338)
66-70			-0.711
			(0.441)
'Ever parent'	0.320	0.521*	-0.116
	(0.251)	(0.258)	(0.251)
Geographic LA	0.381+	0.666**	0.325
	(0.226)	(0.239)	(0.245)
Geographic LA # 'Ever parent'	-0.207	-0.361	0.264
	(0.267)	(0.275)	(0.269)
0-20 # 'Ever parent'	-1.297*		
	(0.592)		
21-25 # 'Ever parent'	-0.480		
	(0.341)		
26-30 # 'Ever parent'	-0.639+	-0.423	
	(0.338)	(0.599)	
31-35 # 'Ever parent'	-0.443	-0.084	
	(0.341)	(0.357)	
36-40 # 'Ever parent'	-0.097	-0.696+	-0.286
	(0.340)	(0.368)	(0.593)
46-50 # 'Ever parent'	-0.308	-0.537	0.003
	(0.367)	(0.377)	(0.386)
51-55 # 'Ever parent'		-0.656+	0.517
		(0.375)	(0.360)
56-60 # 'Ever parent'		-0.677	-0.034
		(0.420)	(0.401)
61-65 # 'Ever parent'			-0.079
			(0.370)
66-70 # 'Ever parent'			0.820+
			(0.469)

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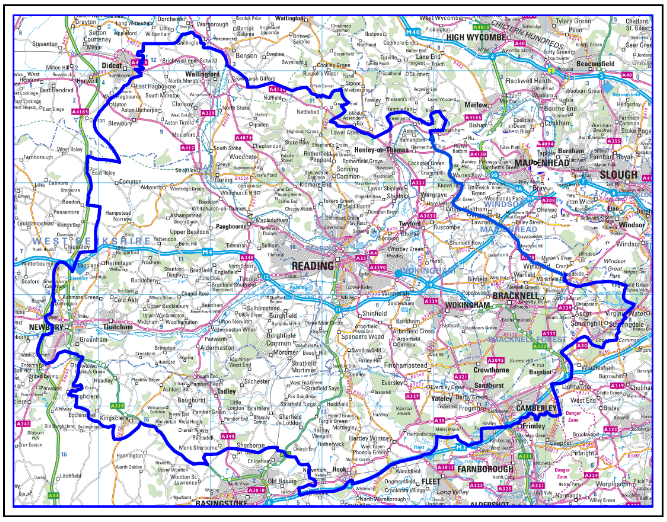
Table A6.6 – continued from previous page

	Cohort 1	Cohort 2	Cohort 3
0-20 # Geographic LA	-1.242*		
	(0.509)		
21-25 # Geographic LA	-0.279		
	(0.304)		
26-30 # Geographic LA	-0.476	-0.727	
	(0.303)	(0.547)	
31-35 # Geographic LA	-0.143	-0.078	
	(0.304)	(0.329)	
36-40 # Geographic LA	-0.076	-0.402	-0.226
	(0.305)	(0.343)	(0.576)
46-50 # Geographic LA	-0.371	-0.378	0.387
	(0.331)	(0.353)	(0.381)
51-55 # Geographic LA		-0.575+	0.452
		(0.348)	(0.353)
56-60 # Geographic LA		-0.359	0.159
		(0.396)	(0.397)
61-65 # Geographic LA			-0.242
			(0.363)
66-70 # Geographic LA			0.768+
			(0.466)
0-20 # Geog. # 'Ever parent'	1.401*		
	(0.628)		
21-25 # Geog. # 'Ever parent'	0.326		
	(0.363)		
26-30 # Geog. # 'Ever parent'	0.588	0.442	
	(0.359)	(0.635)	
31-35 # Geog. # 'Ever parent'	0.372	0.005	
	(0.363)	(0.379)	
36-40 # Geog. # 'Ever parent'	0.361	0.683+	0.232
	(0.362)	(0.390)	(0.638)
46-50 # Geog. # 'Ever parent'	0.568	0.548	-0.132
	(0.390)	(0.400)	(0.411)
51-55 # Geog. # 'Ever parent'		0.595	-0.552
		(0.399)	(0.387)
56-60 # Geog. # 'Ever parent'		0.527	-0.118
		(0.445)	(0.427)
61-65 # Geog. # 'Ever parent'			0.243
			(0.398)
66-70 # Geog. # 'Ever parent'			-0.860+
			(0.496)
N	35,918	33,747	38,460
R^2	0.005	0.004	0.006

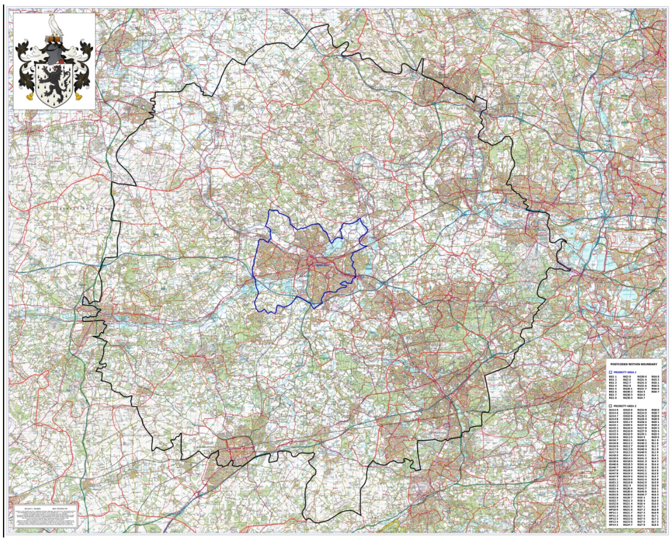
Source: ONS Longitudinal Study linked to area characteristics from the School Performance Tables (Department for Education). Note: + p<0.10, * p<0.05, ** p<0.01, *** p<0.001. The final common sample is applied. # refers to the interaction between variables. The table shows coefficients from an ordinary least squares regression, with standard errors in parentheses.

A5.5 Appendix Figures

Figure B6.1: Example catchment areas: selective schools in Reading



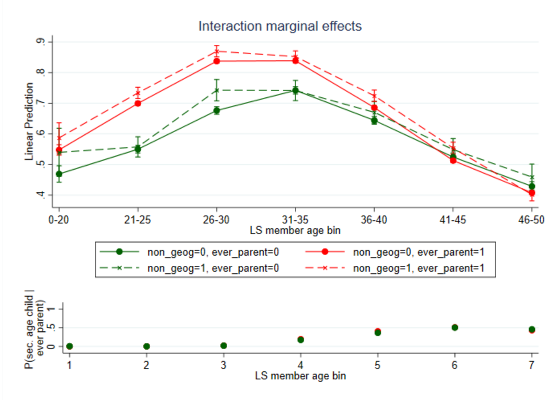
(a) Reading School



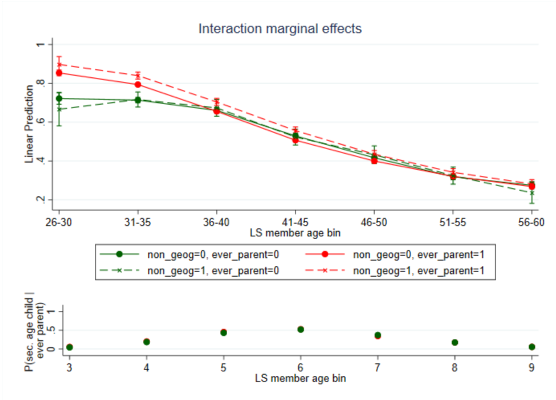
(b) Kendrick School

Source: School websites.

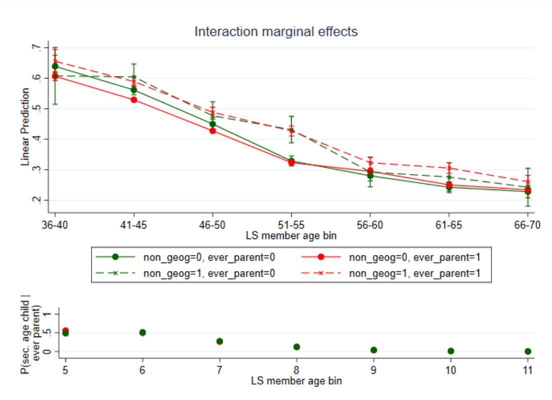
Figure B6.2: Marginal effects from difference-in-differences model for the extensive margin



(a) Cohort 1



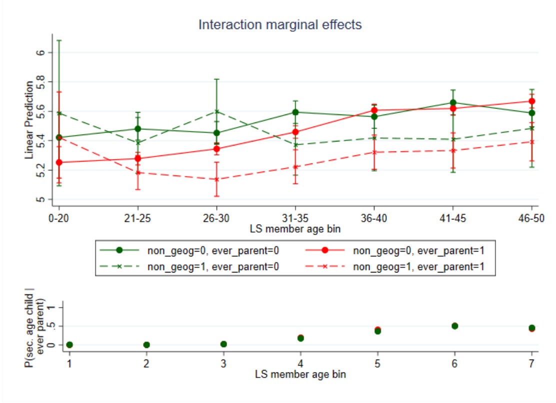
(b) Cohort 2



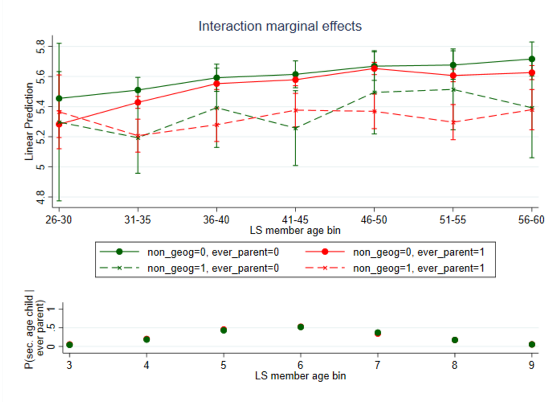
(c) Cohort 3

Source: ONS Longitudinal Study linked to area characteristics from the School Performance Tables (Department for Education). Note: The final common sample is applied.

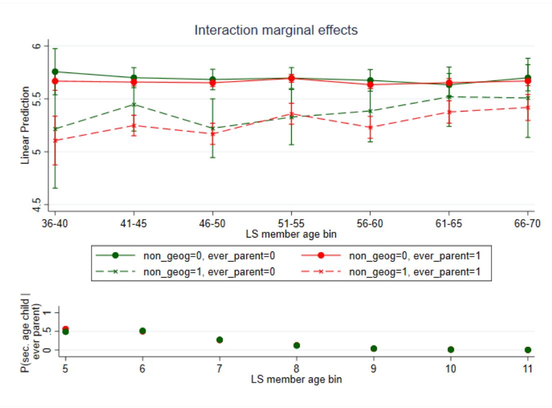
Figure B6.3: Marginal effects from difference-in-differences model for the intensive margin



(a) Cohort 1



(b) Cohort 2



(c) Cohort 3

Source: ONS Longitudinal Study linked to area characteristics from the School Performance Tables (Department for Education). Note: The final common sample is applied.