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ABSTRACT

School Choice during a Period of Radical School Reform: Evidence from the Academy Programme*

Education policy worldwide has sought to incentivize school improvement and facilitate pupil-school matching by introducing reforms that promote autonomy and choice. Understanding the way in which families choose schools during these periods of reform is crucial for evaluating the impact of such policies. We study the effects of a recent shock to the English school system – the academy programme – which gave existing state schools greater autonomy, but provided limited information on possible expected benefits. We use administrative data on school applications for three cohorts of students to estimate whether academy conversion changes schools' popularity. We find that families – particularly non-poor, White British ones – rank converted schools higher on average. We investigate the likely mechanisms that could give rise to our findings. The patterns we document suggest that families combine academy conversion with home-school distance and prior information on quality and popularity as a heuristic to inform school choice.

JEL Classification: I21, H75
Keywords: school reform, choice and autonomy, preference formation

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1. Introduction

Giving schools greater freedom from state control is often advocated as a way to improve education standards. A number of countries have embarked on reforms that give state-funded schools greater flexibility and autonomy over budgets, personnel, curriculum and teaching. In the US, policy changes that started in the 1990s led to the creation of Charter schools. Similar reforms have occurred in Europe, with the birth of 'Friskolor' in Sweden in the 1990s and Academies and Free Schools in the UK in recent years.

The notion that reforms that promote autonomy from state control can improve education is predicated on two main mechanisms. First, autonomous schools can differentiate their teaching offer – e.g., by adjusting taught curriculum or extending school days – thus catering for different pedagogical needs. Better matching between pupils' needs and differentiated school provision should raise standards. Second, autonomous schools have more scope for changing management, teaching and recruitment practices in ways that promote student achievement. These channels can, in principle, raise standards when coupled with incentives generated through quasi-market mechanisms in the process of school choice. This in turn creates competitive pressure on other schools to improve or leave the market (i.e., close) – further increasing standards on average.[1]

The effectiveness of these channels – and thus of reforms promoting choice and autonomy – rests on the idea that families actively exercise school choice and are attracted to the advantages that more autonomous arrangements can bring. However, little is known about how families respond to these changes *in practice* given that the benefits and costs of such reforms – as well as parental beliefs and information about these aspects – are largely unknown a priori. Our paper sheds light on this question by examining how preference rankings for schools changed in response to a far-reaching policy shock to the English school system – i.e., the academy programme.

Academies are schools that, despite being state-funded, non-selective and non-fee-charging, fall outside the control of the local government in terms of key strategic decisions and management. Academies were originally introduced in 2002 as a remedial intervention aimed at failing schools, forcing organizational change under the guidance of a private or charitable sponsor willing to invest in the school. However, from 2010, the incoming Conservative/Liberal Democrat Coalition Government dramatically expanded the academy programme and changed its nature by mainly allowing high-performing schools to apply to become academies and acquire autonomy in terms of budgeting, hiring of staff, pay, performance management, curriculum and length of the school day. The shift was relatively unexpected and occurred rapidly: the new

[1] A large literature looks at the effects of autonomy and/or competition on performance. For example, Clark (2009) studies the effects of a previous generation of partly autonomous schools in the UK – Grant Maintained schools – while Abdulkadiroglu et al. (2011) and Bohlmark and Lindahl (2012) discuss the effects of autonomous schools in the US and Sweden, respectively. Among others, Hoxby (2000) and (2004) and Gibbons et al. (2008) provide a discussion of school competition and its effects on educational attainments in the US and UK, respectively. See also Brunello and Rocco (2008) and McMillan (2004) for some counterarguments on the possible pitfalls of 'quasi market' in education.

Government was installed May 2010 and the Academies Act 2010 ushering these changes passed in June 2010. Over 1100 academies – 35% of the secondary school stock – were created in the first 3 years of the programme. Survey evidence (Department for Education, 2013) suggests that most schools converted to gain additional freedom in the use of resources and to free themselves from local government oversight. Despite the pervasiveness of academies, little information or hard evidence was available to families on the likely structural changes associated with academy status and their potential benefits. Therefore, from a choice perspective, the only explicit short-run difference was a change in the school name – i.e., 'rebranding' – coupled with the idea that academy schools would be largely independent of local government control. Our study investigates the effect of this rebranding and offer of autonomy on families' choice of school.

In order to carry out our analysis, we use merged administrative data on preference rankings, student and school characteristics. The data cover 40,000 families, over three years (2009-2011), choosing from 125 secondary schools in and around Birmingham – the second largest metropolitan area in England. Birmingham uses a constrained deferred-acceptance allocation mechanism with six choices in which families' first choice rankings should reflect true preferences (Adulkadiroglu et al., 2015a; Calsamiglia et al., 2010; Pathak and Sonmez, 2013). We treat a family as the decision making unit, since we have no information on who within a family makes the decision or on intra-family decision processes (Giustinelli, 2016). Estimation is based on discrete choice regressions in which we quantify the impact of academy conversion on the probability of listing a school as the top preference amongst a set of potential alternative schools. The data structure allows us to control for school fixed-effects to partial out time-fixed unobservables, and to control for an extensive set of student, school-by-student and time-varying school characteristics. In our preferred specification, we restrict the choice set to schools which ever become academies up to 2015 – and either convert during our observation window or right afterwards – to further account for unobserved heterogeneity between schools that convert and those that do not.

Our work makes three main contributions. First, we show that on average families respond positively to academy conversion. All else equal, schools approved for conversion to academy are up to 14% more likely to be ranked as the most preferred choice relative to a baseline probability of picking a first-choice school at random. Looked at another way, families are willing to travel 2.5% – i.e., nearly 300m – further than average to attend a school that becomes an academy. We present a wide range of tests which demonstrate that our estimates represent a causal response to academy conversion, including: *(i)* balancing tests showing that the timing of conversion is unrelated to school characteristics and that conversion is unrelated to pre-dating trends in school characteristics; *(ii)* robustness to truth-telling behaviour and various specification checks; and *(iii)* an instrumental variables design that exploits institutional constraints in the government approval process that affect the timing of academy conversion.

Our second contribution is to show that this headline result masks substantial heterogeneity by family background. Families with higher income – i.e., those ineligible for Free School Meals (FSM) – rank a school higher by 17% when it converts, while poor families are indifferent. We detect a similar pattern when comparing White British families with other ethnic groups. When we split the sample by both income and ethnicity, we find that it is primarily high-income, White British families who drive our results – being 30%

more likely to list a school as their first choice if it becomes an academy. We provide evidence that these heterogeneous effects are unlikely due to differential accessibility to academy conditional on place of residence, or to differences in information about academy conversion between groups.

Although heterogeneity in the demand for school attributes has been documented in the literature, our work is the first to focus on school autonomy. Furthermore, previous evidence has been mostly cross-sectional or has not exploited the time series dimension – even when a panel component was available. Therefore previous work has not been able to disentangle the effect of specific school characteristics from the effect of other unobserved school attributes. Our analysis addresses this issue by exploiting multiple years of choice and focussing on the impact of a specific policy change – while holding fixed school unobservables.

Our last contribution is to provide evidence on the likely mechanisms that give rise to our findings. There are fundamental challenges to this exercise since we know little about what families believe academies will offer and what school attributes they prefer. Nevertheless, we explore some possible channels.

The first possibility is that higher income, White British families are drawn to high-status peer groups and/or academic quality, and expect academies to offer improvements on these dimensions. To test for these mechanisms, we assume that families have rational expectations about the changes they will experience once enrolled at an academy and control for the *actual* changes in composition and effectiveness following conversion (as in Ferreyra and Kosenok, 2015).[2] Our results show that expected changes in composition and effectiveness cannot explain the strong preference for converter academies among White, higher income families: converters are still ranked higher even when we control for these factors in our regressions.[3]

A better explanation emerges when we study the influence of pre-existing school characteristics in interaction with academy conversion on the changing patterns of demand. We find that it is mainly schools that are oversubscribed and deliver high standards before conversion that become even more popular when they convert to academies. These patterns suggest that families might be using pre-policy indicators of a school's quality and popularity in combination with the academy 'branding' to form heuristic judgements on a school's suitability for their children. A plausible interpretation is that parents take the signals of prior quality and academy conversion as mutually reinforcing – with conversion providing assurance that a good school will continue to deliver good performance following conversion, and prior quality providing assurance that the process of conversion will deliver benefits. We also find that the positive demand effect of academy

[2] In order to estimate changes in effectiveness following conversion, we take two routes. First, we measure effectiveness by considering the attainments of 'legacy' pupils enrolled in academies prior to their conversion (Eyles et al., 2017b; Abdulkadiroglu et al., 2016). Second, we consider the attainments of students who choose to take their exams at a converted school. The first estimate should represent the effect of academisation on the attainments of a randomly picked student, while the second potentially contains a 'match-specific gain' component. We return to this issue in Section 7.1.

[3] Unlike recent research on subjective expectations and school choice (e.g., Giustinelli, 2016; Giustinelli and Manski, 2016; and Wiswall and Zafar, 2014), we do not have survey data on parental beliefs and preferences for specific school attributes. We return to this issue and discuss its implications for our results in Section 7.1.

conversion among better-off, White British families is much more pronounced for schools located close to home. This pattern could also be consistent with a 'heuristic-based choice' interpretation of our findings and emerge if living close to a converting school simply makes families aware of academies and therefore more likely to choose one – irrespective of the expected benefits. We consider but rule out other interpretations of our results that are not consistent with the overall evidence.

Irrespective of the exact underlying mechanisms, our results imply that reforms that promote autonomy and choice could lead to segregation of pupils of different background in different schools and concentrate any benefits of more autonomous school arrangements on students who are already better off. This is consistent with Eyles et al. (2017a) who document significant changes in schools' intake following conversion using the national sample.

Our paper contributes to several strands of research. First, it adds to a sizeable literature on parental preferences for school attributes and on differences in choices according to family background. Our work is closely related to research on heterogeneity in preferences for Charter schools. Walters (2014) uses a structural model to study charter school choice in Boston (MA) and shows that poor, low attainment children who stand to benefit most from going to a charter school are less likely to choose one. Similarly, Butler et al. (2013) provide evidence that richer students are more likely to choose charter schools. More generally, Hastings et al. (2005) document that better-off parents are more likely to choose high test score schools and willing to travel more to secure their pupils' attendance to a better school than worse-off families. These patterns are echoed by Burgess et al. (2015) on school choice in England, Calsamiglia et al. (2015) on Barcelona. Similar heterogeneity in choices is observed at higher education level too (Hoxby and Avery 2013; Arcidiacono et al. 2016). Our findings are broadly consistent with this evidence.

Second, our work indirectly contributes to the literature on the role of information in influencing school choices (Figlio and Lucas, 2004; Hastings et al., 2007; Hastings and Weinstein, 2008; Gomez et al., 2012; Mizala and Urquiola, 2013; Hussain, 2016; and Imberman and Lovenheim, 2016). We focus on a diametrically opposed case: the implementation of a vast scale policy reform with little information provided to guide parental decision making. Recent work by Lovenheim and Walsh (2017) studies the related question of how choice affects information gathering.

Third, our findings contribute to the evidence from behavioural economics showing that heuristic-based choices are common in individuals' day-to-day real-life decision making (see Tversky and Kahneman, 1974 for the original insights; Della Vigna, 2009 for a comprehensive review of the field; and Lavecchia et al., 2014 for a survey of behavioural economics of education). To the best of our knowledge, our paper is the first to document possible heuristic-based problem solving behaviour in relation to school choice in the context of a large policy change.

Finally, our work is related to a growing literature that exploits features of the school allocation mechanism to infer parental 'true' preferences for schools and identify their demand for school attributes. See among others Abdulkadiroglu et al. (2017b), Abdulkadiroglu et al. (2015a), Agarwal and Somaini (2016), Akyol and Krishna (2017), Fack et al. (2015), and Kapor et al. (2017). In our context, the main feature of the

matching mechanism we rely on is that the first choice on families' applications should be truthful. However, we explore some extensions in directions suggested by this literature in Section 5.4.

The rest of the paper is organized as follows. In Section 2, we discuss the institutional context while in Section 3 we describe the data that we use. Section 4 presents our empirical model. Section 5 discusses our results, while Section 6 investigates heterogeneity and Section 7 studies some potential mechanisms underlying our findings. Finally, Section 8 presents some concluding discussion.

2. Institutional context

2.1. Main features of the English school system

Compulsory education in England is organised into five stages referred to as Key Stages (KS). In the primary phase, pupils usually enter school at age 4-5 in the Foundation Stage (grade 0) and then move on to KS1, spanning ages 5-6 and 6-7 (grades 1 and 2). At age 7-8, pupils progress to KS2, and at age 10-11 they complete the primary phase (grade 6) and move on to secondary school (grade 7) where they progress through KS3 to age 13-14 (grade 9), and KS4, up to age 15-16, which marks the end of compulsory education (grade 11). Throughout their education, pupils are assessed on the basis of standardized national tests. At KS2, students take tests in English, Mathematics and Science, which are externally assessed. At KS4, pupils sit academic (GCSEs) and/or vocational (NVQ) tests in a range of subjects, although English, Mathematics and Science are compulsory for every student at this stage. These tests are externally assessed.

School average attainments at these Key Stages and measures of school average value-added are published alongside other school characteristics (such as size and composition) in performance tables. These are highly salient in the media and policy debate, and routinely used by parents to inform their school choices.

Additional information on quality is disseminated through the publication of school ratings provided by the school inspectorate, OFSTED. OFSTED visits schools every three to five years and inspections result in publicly available reports rating schools from 'Outstanding' to 'Inadequate' on overall quality as well as on specific aspects such as teaching, management and pupil behaviour.

2.2. School choice, admission and allocation

Admission to state schools at both the primary and secondary phase is based on principles of free choice, although in practice freedom to choose is constrained by the fact that popular schools become over-subscribed. When this occurs, various criteria are used to prioritise students, usually favouring those who live nearby, those with special educational needs or in care of the local authority (LA), and those with siblings in the school. Certain types of schools can prioritise students according to other criteria – e.g., religion (faith-schools) or specific aptitudes (music and other specialist schools). Finally, a small proportion of state secondary schools select on prior achievement or admission tests (Grammar schools).

In our analysis, we consider preferences expressed by pupils living in Birmingham for schools in the Birmingham LA as well as seven proximate LAs – namely Dudley, Sandwell, Solihull, Staffordshire, Walsall, Warwickshire and Worcestershire. Birmingham is the second largest English city with approximately 1.1

million inhabitants. Its population is very ethnically diverse: less than 55% of its inhabitants are of White British origin (compared to approximately 80% in England overall), with the second biggest ethnic group composed of Asians (in particular Indian and Pakistani).

Birmingham adopts a centralised clearing house for secondary school applications, collecting parental preferences for all state schools – inside and outside the LA. This means we observe preferences expressed for all state secondary schools – even if these are located in other school jurisdictions. Although we do not observe preferences for private schools – the only 'outside option' in our context – aggregate statistics show that for the academic year 2010/2011 less than 5% of secondary school pupils are privately educated in Birmingham. Neglecting this sector therefore has a limited impact on our analysis.

The LA contacts parents in late Spring of the academic year before students are expected to enrol in secondary education (e.g., April/May 2009 for secondary school admissions to the academic year 2010/2011) and provides detailed information booklets – similar to the 'school directories' discussed in Abdulkadiroglu et al. (2015a) – containing information on types, characteristics, admissions criteria for schools in the LA as well as links to information about schools in other LAs. The booklets also explain the timeline of the admissions procedures and stress the fact that schools will hold open events during September of the current year (e.g., September 2009 for admissions to the academic year 2010/2011) and parents are expected to apply to their preferred schools by the beginning of October – even though applications close at the end of the month. After this, parents receive school offers the following March (e.g., March 2010 if they applied in October 2009), with the aim of starting secondary school in September of the same year (e.g., September 2010 for the academic 2010/2011).

Families can apply to up to six secondary schools. In order to allocate pupils to their preferred schools, the LA uses a constrained student-optimal stable mechanism (also known as Deferred Acceptance algorithm). Pathak and Sonmez (2013) have studied the details of the English admissions system and have shown that this matching algorithm is less open to manipulation than alternative arrangements and likely to elicit an honest ordering (see also the recent survey by Cantillon, 2017). The information provided in the admission booklets also encourages a truthful ranking: "Rank your six preferences in the order of schools you most prefer. (We will try to offer schools in the order you rank them, so put your first choice first even if there is only a slim chance you will be offered that school [...])".

However, the limit to the length of the list of choices implies rankings may not reflect families' unconstrained preferences because they are aware of the way schools' rank students. Families may therefore skip schools that appear not to be feasible (e.g., because they are too far away to stand a chance of admission) and include less preferred but feasible choices in the top six as a backup option (Calsamiglia et al., 2010; Fack et al, 2015). Nevertheless, the constraints on the choice list should not affect which school a family lists first – hence we focus on first choices in our main estimates. Moreover, 71% of the families in our sample rank less than six schools, suggesting that lack of 'truth telling' should be relatively marginal in our data. Still, when we look at rank ordered choices, we consider the implications of the limited number of choices for truth telling and conduct various robustness tests (see Section 5.4).

Importantly, the allocation mechanism did not change with the roll out of the academies programme. Furthermore, the criteria used to rank applicants to academies follow similar principles to other schools and remained broadly unaffected by conversion. This environment of stable priorities makes it more likely that changes in preference rankings reflect families' demand for academies (holding fixed the supply side).

2.3. Academies: institutional characteristics

Secondary schools can take one of the following organizational structures: community schools, voluntary controlled schools, foundation schools, voluntary aided schools and academy. Community and voluntary controlled (VC) schools are mainly organized and managed through the LA, which employs the staff, owns the buildings and organises admissions. Their governing bodies include members of staff, representatives of the LA, parents, community representatives and, in the case of VC schools, members of the foundation (usually religious) supporting the school. Voluntary-aided (VA) and foundation schools enjoy more autonomy from the control of the LA, although the LA still plays a significant role on the governing body and has powers of oversight. In all these cases, funding comes from the LA using money provided by central government through general taxation.

Academies enjoy far more autonomy than any of these school types, despite remaining non-fee-charging, state-funded schools. They broadly fall outside the control of the LA in terms of strategic decisions and day-to-day management – which is administered by the head-teacher and a self-appointed board of governors with limited representation from the LA. This body has responsibility (shared with the head-teacher) for hiring staff, negotiating pay and working conditions, managing the school budget, and deciding on matters such as career development, performance assessment and management. Furthermore, academies enjoy more autonomy in terms of the taught curriculum (except for English, Maths, Science and IT), as well as of the structure and length of the school day.

According to the Department for Education (DfE, 2013) 'Survey of Academy Freedoms', secondary schools reported having implemented the following changes since becoming converter academies: *(i)* 65% to 75% changed their taught curriculum by focussing on core subjects/topics; *(ii)* 60% to 70% introduced new systems to monitor pupil and teacher progress/performance more formally, regularly and/or thoroughly; and *(iii)* around 85% linked more explicitly teachers' pay and promotions to pupils' performance. On the other hand, very few secondary schools reported that they were able to substantially alter the length of the school day – although they wanted to implement this change – most likely because of teachers' resistance. Approximately 60% of the schools' head-teachers believe that standards improved as a result of these changes – with leadership, curriculum and teachers' management being the driving forces.[4]

[4] Note that these are self-reported retrospective assessments of changes implemented since conversion. At present little 'hard' evidence has been collected using administrative data to document whether these changes actually occurred and had an effect on attainments. We take a first stab at this issue later in the paper when we study the mechanisms giving rise to our findings.

Academies funding is linked to the number of students on roll – i.e., 'money follows pupils'. However, unlike other schools, they receive funding directly from the Government – not from the LA. Academies also become responsible for their own 'back-office' administrative functions and maintenance of the premises vis-à-vis the release of resources previously held by the LA for the centralised provision of these services. Lastly, legally academies cannot run budgetary deficits and the DfE can close academies after two years of financial shortcomings. Sibieta (2016) argues that these changes have made academies more financially accountable and likely to engage in strategies aimed at sustaining their pupil roll (e.g., marketing). As already noted, academies cannot significantly change the criteria used to prioritise students following conversion, and abide to the same admission principles that apply to other schools.

2.4. Academies: the process of conversion and the incentives to convert

Academies were introduced by the Labour Government starting from September 2002 as a targeted remedial intervention aimed at addressing underperformance by imposing organizational restructuring and by allowing a Government-approved sponsor – usually a charity or a business group – to 'take over' the school to drive through educational improvements. Given this feature, this type of academy has commonly been referred to as a 'sponsored' academy. The programme however dramatically changed in May 2010 with the appointment of the new Conservative/Liberal Democrat Coalition Government. The aim of the Academies Act 2010 – swiftly passed in June 2010 – was to allow as many schools as possible to convert to academies and drive transformational changes to the organization of the English state school sector.

To grasp the rapid expansion of the programme, consider that at present there are more than 1800 secondary academies out of around 3200 secondary schools. Of these, around 500 are sponsored academies – with almost 300 created during the Labour Government. More than 1300 academies were instead created between 2010 and 2015 through the converter route – with approximately 85% of this expansion taking place in the first three years. These represent 40% of all secondary schools and 80% of the increase in the academy sector in the past five years. In our analysis, we focus on these converter academies to identify the impact of autonomy on parental preferences.[5]

Under the new system, the process of conversion is initiated by the schools themselves (rather than triggered by the government) and the force behind the decision to apply for conversion is usually the head-teacher and/or governing body. Applications for conversion are processed by the DfE, which provides the following guidelines for applicants: *(i)* discuss the possibility of converting to academy with parents of enrolled pupils, members of staff and the interested community at large; *(ii)* register with the DfE the intention to apply and send information about school attainment, pupil progress and school finances for the past three

[5] Conversely, we do not investigate the impact of conversion through the sponsored route because, during sponsored academisation, management, pedagogical methods and teaching workforce undergo substantial restructuring. Further, sponsored academies generally reopen in new or completely refurbished buildings. In short, sponsored status identifies a host of changes – and not just the impact of autonomy.

years; and *(iii)* provide the most recent school report prepared by OFSTED. After receiving this information, the DfE considers the application and initiates discussions about funding arrangements, teaching matters and transfer of assets (such as the school building) or liabilities from the LA to the school. This process can take two to five months, with the exact timing dependent on specific aspects of the proposed conversion as well as the volume of applications in the system.

A few important aspects are worth noting. First, OFSTED inspection ratings are important determinants of approval for conversion. Initially, conversion was mainly reserved for 'outstanding' schools which were fast-tracked for approval. Subsequently 'good' schools with 'outstanding features' (e.g., in teaching or management) were also allowed to apply through the standard procedure – if their average attainments and value-added were above average (further changes were enacted after our observation window). The aim of this change in eligibility criteria was to expand the academy programme *without compromising on the quality* of the schools allowed to convert. Nonetheless, throughout the period, schools with other OFSTED ratings (e.g., 'satisfactory') could apply for conversion – but their application process included further requirements, such as confederating (through the creation of a multi-school trust) with a school of higher quality and detailing how cooperation would improve standards in conjunction with academisation. In our data, 57.5% of the 'outstanding schools' convert, while approximately 35% and 11% of schools with 'good' and other OFSTED ratings become academies, respectively.

Second, approval by Department takes place in two steps. First, 'Lead Teams' are assigned to evaluate academies' proposals for conversion. Teams work independently and are assigned to applications coming from broad geographical areas. Teams are given targets for percentages of applications processed within a given time (irrespective of the amount of applications received), and the best performing teams are flagged as 'best-practice' examples within the Academies Unit at the DfE. Objective criteria for approval make it highly unlikely that faster turnover time is associated to less scrutiny in the approval decision. The second step in the approval process involves an 'Academy Board' where senior civil servants, policy makers and education experts (external to the Department) meet to discuss and finally approve the cases put forward by Lead Teams. Members of the Academy Board are the real 'gate keepers' of the approval process. The frequency of these meetings is not fixed and varies depending on the amount of applications in the pipeline. At times of high demand, Academy Boards meet more than once per week, and the number of applications assessed at each gathering is not capped. Conversely, at times at low demand, board meetings can be delayed. Although this is not institutionalised, the reason why delays happen is due to the fact that it becomes more difficult to coordinate the tightly scheduled diaries of the involved senior figures when a sense of urgency is lacking. All in all, this means that the system processes applications faster when larger numbers are in the pipeline.

Last, it is important to discuss schools' incentives to apply for conversion. To begin with, conversion frees up funds previously kept by the LA to provide back-office activities – such as accounting, procurement and site-maintenance. Although following conversion these operations have to be managed by the school, the funds released by the LA could be used more flexibly and efficiently by schools – possibly creating savings to be reinvested in teaching-related activities and to improve education standards. Indeed, according to the DfE (2013) academies' survey, the two mostly cited main reasons for converting are 'to gain greater freedom to

use funding as seen fit' and 'to obtain more funding for front-line education' (with the third reason being 'to improve standards'). It is also likely that managerial independence and reduced bureaucratic control acted as a significant incentive – as highly-performing schools with proactive management have historically resented LA control over school activities and interference with personnel practices. Consistently, the fourth most cited reason for conversion is 'to become independent of the LA'. On the other hand, the possibility of expanding pupil roll does not seem to act as a strong incentive for conversion. While head-teachers'/senior teachers' reputation might partly depend on school size, expansion at these generally over-subscribed schools is not easy because of institutional constraints – mainly availability of suitable sites which are tightly controlled by the LA. In fact, the 2013 survey shows that less than 30% of the academies increased pupil roll in the first few years after conversion (and only 10% changed the geography of intake).

2.5. Academies: what parents knew

When families make their applications for admission, the basic information provided regarding academies is that they are state-funded directly from central government on a comparable basis to other schools, managed directly by the governing body and the head teacher, and that the LA coordinates admissions based on oversubscription criteria set by the governing body (Birmingham, 2012; p.8).

However, as part of their application process would-be academies have to consult with the 'interested community' – and parents might gather more information through this process. Available survey evidence suggests this is unlikely to be the case: parents show very little understanding of the academy model. An early survey by HCSS Education (2015) – a consultancy firm – on a sample of 1,000 households shows that 58% of parents say they do not understand what academies do, and 32% think there is not enough information on the benefits of academies. These findings are echoed by the National Foundation for Educational Research (NFER, 2015) report on school choice. This shows that 71% percent of parents do not know much or nothing at all about academy schools – though this figure is lower for high-income individuals (68%) than for low or middle earners (73%). Furthermore, the survey shows that parents on average can only correctly answer four out of nine questions about academies' functioning – though once again there is some heterogeneity by income (5 questions for better off households against 3.5 for low/middle income ones).[6]

[6] The Government substantially advertised the potential benefits of academy conversion – see for example Michael Gove's speech in January 2012. Most of the Government assessment was based on extrapolation from the evidence on the Labour academy programme – although the evidence in Eyles et al. (2017a) argues against such projections – and from international evidence from substantially different settings (e.g., Swedish 'Friskolor' and US Charter schools). It is hard to know how such campaigning affected parents' perceptions of any benefits, though the evidence cited above suggests it did not clarify their understanding of the academy model.

3. Data construction

3.1. Applications and school choice set

Data on parental preferences for schools comes from administrative records from the Birmingham LA. These contain information about the preferences expressed by parents residing in Birmingham and applying for a place in a secondary school for their children for the academic years beginning in September 2010, 2011 and 2012. The data contain the ordered list of preferences expressed by parents; whether any special criteria for admission (i.e., having siblings in the school; being looked after by the LA; having special educational needs) applies for a given pupil; the postcode of residence at the time of the application; the pupil's gender; and a unique pupil identification number that allows us to match students with other data sources. The data also contains details about the school(s) where the pupil was offered a place and the school finally attended.

We observe applications for a total of 40,924 pupils over the three cohorts (13,836 for admission in 2010, 13,536 for 2011, and 13,552 for 2012). As discussed, the Birmingham LA acts as a centralized hub coordinating parents' applications for schools within the LA as well as schools in other LAs. In our data we initially observe preference for nearly 300 different schools – i.e., schools that received at least one preference in one of the three years – of which approximately one third is located in Birmingham and two thirds in other 42 LAs. Of the schools outside Birmingham, however, around 130 receive just one preference in one given year and are clearly not part of a consistently defined and stable choice set. Similarly, other schools receive more preferences – but only in one of the three years and none in the other two. In order to avoid including 'outliers' within the set of schools commonly chosen by pupils residing in Birmingham, we define our choice set to include only: (a) all Birmingham schools; (b) LAs other than Birmingham that receive at least 30 preferences from pupils residing in Birmingham in each of the three years of data; and (c) schools within these LAs that received preferences from Birmingham pupils in each of the three years of data. The LAs selected for our analysis are all adjacent to Birmingham, and include: Dudley; Sandwell; Solihull; Staffordshire; Walsall; Warwickshire; and Worcestershire. Our final data contains 125 schools, 75 of which located in Birmingham. We exclude female-only and male-only schools for males and females, respectively. As a result, the choice set counts 110 schools for boys and 115 schools for girls.

3.2. Background data on pupils' characteristics and attainments

To obtain information on pupils' background and attainment in National Curriculum tests, we merge our data on school preferences to the National Pupil Database (NPD) and the Pupil Level Annual School Census (PLASC). These are administrative datasets covering the student population in England's state schools. The NPD/PLASC data provides information on pupils' KS2 test score records in English, Mathematics and Science at the end of primary school (age 11/grade 6); pupils' eligibility for free school meals (FSM; a commonly used proxy for poverty); special educational needs (SEN) status; and information on ethnicity (we use White British or not). The data also provide postcode of residence for each pupil in every year from grade 2 (at KS1/age 7, when pupils are in the middle of primary education) to grade 7 (when pupils have just started secondary schooling). In the UK, postcodes typically correspond to 15-17 contiguous housing units on one

side of a street. This detail allows us to assign students to residential neighbourhoods with very high precision and compute home-to-school straight line distance. We can also track individuals who change address, which we use in a robustness check to address potential issues in relation residential moves in response to the academy conversion.

The preference and NPD/PLASC data are linked using a unique pupil identifier which is available for 39,318 pupils out of the original 40,924. The missing pupils are either enrolled in private schools (not in the NPD) or lost because of tracking problems with the Birmingham's LA – possibly because they moved to another LA or another country before the start of secondary school. From this sample, we further drop around: *(i)* 670 pupils who submitted their preferences after the deadline, as we cannot reconstruct the exact information about schools' academy status available to these students at the time of listing their choices; *(ii)* 120 pupils who did not express any preference; or expressed the same preference ranking for different schools, thus preventing us from correctly sorting their choices; or only applied to schools outside of the choice set we consider (described above). Approximately 1400 pupils have missing values in some of these variables – mainly due to non-reported ethnicity and missing Key Stage data for students that have been privately educated for their primary education (few cases) or have recently moved to the country from abroad (the majority of instances). After dropping these observations, our final sample consists of 37,140 pupils (12,605 starting secondary school in September 2010; 12,388 in 2011; and 12,147 in 2012) or approximately 91% of the original set of pupils.

3.3. School level data

We collect a wealth of information on each of the 125 schools in our data, merging data from several administrative sources. To start with, we use a school register (Edubase) to gather information about each school's exact location (the postcode), type (our data include community, foundation, voluntary aided schools, and academies; there are no voluntary controlled schools), gender composition (mixed or male-/female-only), and admission procedure (selective/Grammar or comprehensive).

The dataset also contains information on the academy opening date. While the vast majority of sponsored academies open in September at the beginning of the academic year, the open date for converter academies is spread out across the months. In our sample, approximately 60% of the converters open between July and September, with the other opening dates clustered in October, January, April and July – i.e., during the academic year. Although opening marks the conclusion of legal aspects of academisation (e.g., land transfers from the LA to the school) and any official inaugural event, it is not necessarily the most relevant milestone when considering school choice. Before opening, families will be aware that a school is approved to convert from information provided by LAs during the application process, for instance via application booklets, or at open days held by schools. Therefore, in our analysis, we define schools as academies if they are approved for conversion in time for families to identify them as academies before the deadline for application for school admission – i.e., the end of October in the year prior to starting secondary school. Information on each converter school's stage in the process of academy conversion was provided by DfE (up till March 2015, the time when the data was assembled). This information identifies: *(i)* the time when a school applies for

conversion to academy; *(ii)* the time when the application is approved (on average 2.3 months after application, with a standard deviation of 3.7); and *(iii)* the time when the school opens as an academy (on average 4.5 months after approval, with a standard deviation of 3.4).

Measures of school composition are obtained by aggregating the pupil data from the NPD/PLASC at the school-by-year level and using all pupils in the secondary school from grade 7/age 12 to grade 11/age 16. Demographic variables include the proportion of girls, share of pupils registered for FSM, share of pupils with special educational needs, and share of White British pupils. Achievement-related variables are mean KS2 scores (averaged across English, Mathematics and Science) of pupils enrolled in a given secondary school in 7th grade (the first year of secondary education), mean scores at the KS4/GCSE (again averaged across English, Mathematics and Science) and KS2-to-KS4 (primary-to-secondary) value-added. Mean KS2 scores proxy intake quality and KS4/GCSE scores are the headline academic quality indicator advertised in school league tables; value-added instead provides an indicator of the school's educational effectiveness.

Other school-level indicators – such as total roll, total number of teachers, the pupil-to-teacher ratio and the number of support teachers that assist SEN students – are obtained from the School Level Annual School Census. Lastly, as discussed in Section 2, inspection results are an important determinant of schools' eligibility for conversion, so we collect data on the dates and outcomes of OFSTED inspections.[7]

In terms of timing, we line up the information on school composition, performance and inspections with our data on applications in a way that reflects the information that was available at the time families made their choices. Specifically, for pupils starting secondary school in September 20XX and expressing their preference by October of 20XX-1, we match school level data that refer to the academic year 20XX-3/20XX-2. OFSTED inspections are not carried out every year, so we match the most recent OFSTED data prior to the time-window during which parents choose their schools.

4. Empirical approach

The aim of our analysis is to estimate the average causal effect of a switch to autonomous institutional arrangements on a school's ranking in the list of preferences that families submit when choosing schools. To do this, we compare preference rankings for academies before/after conversion with those of comparable non-

[7] We also gather yearly Consistent Financial Reporting (CFR) information about schools' expenditure and sources of income. This information is missing for some academies, which do not have the same financial reporting obligations as other state-maintained schools. We therefore only use this information in some extensions and checks. Similarly, we use the School Workforce Census to collect yearly data about schools' workforce characteristics, including: number of teachers; percentages of permanent teachers, female teachers, part-time teachers and teachers with post-degree qualifications; average age of teachers; teacher turnover; and a dummy for head-teacher change. Because of problems with the original data collection, information on teachers is not consistently available for all schools in all years. Therefore, we use this data only in some extensions.

academy schools while holding constant other school, pupil and neighbourhood level characteristics that affect school demand and correlate with the likelihood of converting to academy.

Our baseline specification is as follows:

$$Pref_{ist} = \alpha_i + \sigma_s + \theta_t + \beta Academy_{st} + \delta dist_{ist} + X_{it}^{'} \Gamma + Z_{st}{}' \Delta + \varepsilon_{ist} \quad (1)$$

Where $Pref_{ist}$ measures the preference ranking that the family of pupil *i* places on school *s* at time *t*; α_i, σ_s and θ_t capture respectively pupil, school and time specific unobservables; $Academy_{st}$ is a dummy identifying whether school *s* operates as an academy by time *t* when pupils/parents in our three cohorts express their preferences; $dist_{ist}$ measures logarithm of the (straight-line) distance between the residence of pupil *i* at time *t* and school *s*; $X_{it}^{'}$ is a set of pupils characteristics measured at the time *t* – when pupils/parents choose their school; and Z_{st} is a time-varying set of school characteristics that would have been observed by parents at time *t*. Finally, ε_{ist} is an error term assumed to be uncorrelated with the other regressors in Equation (1). In our analysis, we allow unobserved shocks to be correlated across students applying to the same school across cohorts and thus cluster standard errors at the school level. We also experimented with two-way clustering – at the pupil and school level – and found virtually identical results.

The main variables of interest are $Pref_{ist}$ and $Academy_{st}$. For most of our analysis, $Pref_{ist}$ is a dummy variable equal to one if the school was rated as the most preferred school and zero for all other schools. The second variable, $Academy_{st}$, indicates whether school *s* at time *t* is operating as an academy. As already discussed, we mainly focus on whether the school was approved for conversion to academy at the time when parents expressed their preferences.[8]

In our main specifications, we estimate the Equation (1) by OLS – i.e., using a linear probability model – and using all pupils and all the alternatives in their choice set (discussed in Section 3.1). However, in some extensions we use a conditional-logit specification on the binary outcome variable discussed above. We also consider an alternative ordered dependent variable measuring the (inverse) rank of the schools listed on the form (and coded to 'missing' for all unranked schools). When using this alternative outcome, we estimate non-linear rank-ordered logit specifications. When fitting these models, we further experiment with restricting pupils' choice set to only consider 'feasible schools' for which the family's child is likely to be eligible – as in Fack et al. (2015). This restriction removes from the choice set any infeasible schools which a family might not have listed – but which they in fact prefer – to allay concerns about the 'truthfulness' of rankings when the number of options is limited. Finally, we present a number of other checks on whether institutional features of the admission process might confound our results, in particular the possibility that families move closer to schools which prioritise applicants by distance, and that school admission rules prioritise students with special needs or with siblings already at the school.

[8] In our sample four schools become sponsored academies. While we control for this switch in our analysis, we do not consider the effect of sponsored academisation as it does not solely capture the impact of autonomy (see Section 2).

The data set up allows us to include in our specification both school and pupil fixed-effects. School fixed-effects partial out the impact of time-fixed unobservable school characteristics that make them more likely to become an academy and at the same time affect parental preferences. These could include persistent dimensions of 'quality' – such as the managerial talent of the head-teacher or the dynamism of the school governing body. We also control for an extensive set of time varying school characteristics, including: school average KS4 attainments; the incidence of pupils eligible for FSM; the shares of female and of White British students; the pupil-teacher ratio; and a dummy indicating whether the school was rated 'outstanding' at the school OFSTED inspection. Pupil fixed-effects take into account unobservable characteristics of both pupils and their place of residence (α_i in our model) which affect their choice sets. Given the data set up, in our baseline specification pupil fixed-effects effectively only capture year effects (θ_t) and net out differences in the choice sets for the two genders (because of single-sex schools; see Section 2.1) – so they could be replaced by cohort and gender dummies. However, we include pupil fixed-effects in all specifications because in a number of extensions we either change our dependent variable (e.g., we consider only ranked preferences for listed schools) or restrict the sample of schools available to pupils (e.g., only 'feasible' schools) in ways that generate pupil-level variation in choice sets.

The identifying assumption underlying our analysis is that the timing of conversion to academy is 'as good as random' and unrelated to school unobserved shocks that might affect parental preferences and the school propensity to become an academy. In order to deal with potential residual confounders, we make the baseline specification in Equation (1) progressively more demanding.

First, we restrict our analysis to consider a *converters* sample that contains only schools that convert to academy *within* our data period – the treated group – and schools that will convert after our observation period – i.e., a control group formed of future converters (up to March 2015, when the data collection took place). By dropping institutions that never convert, we exclude schools that may differ from those that convert along unobservable dimensions which may affect families' school choices. Furthermore, in this way, we only exploit the timing of conversion – within the set of schools that convert at some point – to identify the impact of autonomy on parental choice. We provide evidence that – conditional on school fixed-effects – time-varying characteristics of schools and the neighbourhoods from which they attract pupils are uncorrelated with the likelihood that a school is approved for conversion in a given year. We also show that there is no difference in pre-conversion trends in these characteristics between 'current' and 'future' converters.

Second, we include in our specifications interactions between cohort dummies and: *(i)* a detailed set of school characteristics; or *(ii)* a school-specific 'academy propensity', obtained as the prediction of a linear probability model of school approval for academisation on school characteristics. These controls effectively account for changes over time in parental preferences for school attributes that might be correlated with academy conversion, allowing us to isolate the impact of autonomy on preferences.

Lastly, we use an instrumental variable (IV) strategy that predicts the timing of academisation using institutional details of the process of academy conversion. We follow this route since endogeneity could still be an issue – even conditional on school fixed effects and our rich set of controls. This is for two reasons. First, there might be time-varying unobservable factors that drive schools' decision to apply for conversion in a

given year (e.g., a change in head-teacher). Second – conditional on applying – there might be unobservables that could drive swift approval (e.g., a high-quality application prepared by a talented school managerial team). Both factors might correlate with parental preferences for schools. To by-pass the first issue, we create an instrument that exploits a change in the criteria demarking eligibility for conversion occurring between 2010 and 2011 (see Section 2.4 for more details). Initially, only schools rated 'outstanding' could apply for conversion through the standard route, whereas subsequently 'good' schools with 'outstanding features' could also apply – if their end-of-secondary attainments and primary-to-secondary value-added were above average. In practice, there is little difference between these two categories in terms of observable characteristics and quality. So the change in rule is in effect an arbitrary random shock which determines which schools can convert in which year (i.e., the second or the third year in our data) – but is otherwise unlikely to be correlated with school attributes that affect family school choices.[9] We instead by-pass the second potential source of endogeneity by constructing a second instrument that predicts whether – conditional on having applied at a given point in time – the school's application is likely to be accepted in time to affect parental preferences in the current application round (i.e., by October of a given year) or after the application deadline. This instrument is the number of applications for conversion received by the DfE from LAs other than those in our sample in the same year and same month in which one of our schools applies for conversion.[10] As discussed in Section 2.4, the volume of applications affects the rate at which conversion is approved by the DfE, so the instrument is relevant for the probability of a school converting during our sample period. However, the number of contemporary applications from other parts of the country is unlikely to be correlated with attributes affecting school choices amongst Birmingham residents. Indeed, we find no correlation between the characteristics of the schools applying to convert in our sample and the number of applications received by the DfE from other LAs at the same time.

5. Results

5.1. Descriptive facts

Table 1 presents descriptive statistics for our sample. Panel A tabulates information on pupils' background. KS2 attainments averaged across English, Mathematics and Science have a mean of approximately 27.8 points on a scale ranging from 15 to 39. This is in line with the national average and corresponds to the expected

[9] More precisely, we construct this instrument by first identifying whether schools satisfy the early or the revised eligibility criteria based on time-fixed schools' characteristics measured at the beginning of our observations window and prior to the 2010 Academy Act. We then interact this variable with the time of the change in eligibility criteria to give the instrument variation along the time dimension.

[10] We obtain variation over time in the instrument by interacting the number of concurring applications with a dummy that is equal to one if the school has applied by a given year, and to zero if the school has not applied yet. We do so as this instrument is meant to isolate variation in the likelihood of obtaining approval in time *within* an application year.

level of attainment for pupils at this age. The statistics also reveal that pupils in our sample are much more likely to be on FSM (33% against a national average of approximately 16%) and less likely to be White British (40% against a national average of nearly 80%). This reflects the overall ethnic make of Birmingham, which is inhabited by a diverse and relatively deprived population. Further, the data show that on average parents in our sample expressed preferences for 3.8 schools (out of the 6 they are allowed to rank on the application form), and that only 3% of them only chose schools outside the LA.

As far as school-level information is concerned, the first two columns of Panel B of Table 1 report descriptive statistics for the full sample of 125 schools in our data (over three years), while the next two columns repeat the analysis on the sub-sample of converter schools only.[11] On average, approximately 12% of the schools are approved as converter academies (roughly a third within the converter-only sample). However, this figure masks a very dynamic evolution of the sector. As Appendix Table 1 shows, while there were no converter academies in 2009, three were approved for conversion by October 2010 and forty-one by October 2011 – representing approximately 33% of the secondary schools in our choice-set. Note that 32 of the 41 schools approved for conversion are also open as academies by October 2011 (i.e., 78%). Of the remaining nine, two open by December while the others open within the academic year – mainly in April and May.

Appendix Table 1 further shows that converter academies quickly started attracting growing parental demand: although they represented approximately 33% of the secondary sector by October 2011, they attracted almost 41% of first preferences. Interestingly, heterogeneous patterns already emerge from simple descriptive statistics: better-off parents are substantially more likely to rate academies as their preferred choice than parents of pupils eligible for FSM. Similarly, White British parents are more likely to apply for a seat at an academy than non-White British ones – although the heterogeneity along this margin is less marked.

Panel B, Column 1 of Table 1 shows that around 40% of all schools in our sample are located outside the Birmingham LA and that 10% admit pupils on the basis of academic ability (i.e., they are selective Grammar schools). The KS2 average attainments of their intake (i.e., the end-of-primary achievement of pupils starting secondary school) and average KS4 (end-of-secondary school) attainment are close to the national average. The mean proportions FSM-eligible and White British pupils are 24% and 55% respectively. This is less than in the Birmingham pupil data in Panel A because two-fifths of the schools are located in more affluent LAs surrounding Birmingham. The average pupil-to-teacher ratio is 15 and the share of schools rated outstanding by OFSTED is 29%. Converter schools are broadly similar to other schools in our sample (Columns 3-4) in terms of demographics. Given the criteria for conversion, it is unsurprising that more of them are rated outstanding. They also exhibit slightly higher intake ability (KS2) and final achievement (KS4) and more of them are selective, again features that are most likely due to the conversion criteria.

The last panel of Table 1 displays two choice-level variables. These refer to the dataset in which the parents of the 37,140 pupils retained in our sample are paired with each of the 125 schools retained in our analysis (see Section 3.1 for a discussion) with the exception of: *(i)* 15 female-only school excluded from the

[11] We collected information on some variables solely for converter schools, as we use them only in this sub-sample.

choice set of male pupils; and *(ii)* 10 male-only schools excluded from the choice set of female students. This gives rise to a total of 4,176,755 observations. The proportion of first choices in this student-by-school data is 0.0088. This corresponds to an estimate of the probability that a school gets picked as first choice at random by a pupil of the eligible gender. Furthermore, the average home-to-school distance is approximately 11.2km with a standard deviation of 6.7km and a median of 10km. However, the median distance among the schools listed by the parents on their application forms is much shorter, at 2.5km, while the median distance from the most preferred school is even shorter, at 1.8km. Parents of FSM eligible pupils tend to list schools that are closer to their home than parents of non-FSM eligible pupils, although this difference is not pronounced: the median distance for all listed school is respectively 2.25km and 2.54km. A similar pattern emerges for White British and non-White British families (the median distances for listed schools being 2.53km and 2.38km).[12]

5.2. Regression results

We present our first set of results in Table 2. Across all columns, the outcome is a dummy variable identifying whether the school was listed as the most preferred choice by parents. The coefficients (and standard errors) on schools' academy status have been multiplied by 100. The implied academy effects are semi-elasticities and have been obtained by dividing the coefficients by the probability that a school is top-ranked by parents. Standard errors are clustered at the school level. Finally, we report the coefficient on the (log of) home-to-school distance (not multiplied by 100) and the marginal willingness to travel (MWT) obtained by dividing the academy coefficient by the distance estimate.

Columns (1) to (3) consider all schools in the sample and control for: school fixed-effects, year dummies and pupil gender (Column 1); school and pupil fixed-effects (Column 2); and school and pupil fixed-effects alongside time-varying school controls (detailed in the note to the table and described in Table 1). Our results show that, following approval for conversion to academy, an average school is approximately 8% more likely to be listed by parents as their top preference. The remaining columns of the table assess the robustness of this finding.

In Column (4) we focus on the converters sample that includes current and would-be academies – but excludes schools that do not become academies at any point in time up to March 2015 (when the data was collected). More precisely, we identify the impact of autonomy on parental demand by comparing preferences for schools approved for conversion to academies at the time when parents choose schools (i.e., before October of year t) to preferences for schools that will be approved to become academies in future academic years – but excluding the immediately adjacent academic year (i.e., from November of year t to September of year t+1) to overcome possible anticipation effects and spill-overs between current and future converters. When we do this, we find a slightly larger and still significant effect of academy conversion, at 9.3%.

[12] The number of schools listed is also not very different across subgroups. FSM pupils list 3.6 schools, while non-FSM rank 3.9 schools. The average number of schools listed is 3.3 for White British and 4.1 for non-White families.

Column (5) and (6) deal with the possibility that our results are driven by changes in parental preferences for school attributes that are correlated with academy conversion. Column (5) includes interactions between cohort dummies and the following characteristics (averaged over the three years): KS4 attainment; share of FSM eligible pupils; share of female pupils; share of White British pupils; pupil-teacher ratio; and OFSTED rating.[13] Column (6) instead includes interactions between cohort dummies and a school-specific 'academy propensity'. This is obtained as the prediction from a linear probability model that estimates the likelihood that a school is an academy as a function of the school characteristics listed above. Irrespective of the approach we use, we find that our previous results are confirmed – in fact, our estimates become more sizeable, at 12%-13%, and more precisely estimated.

Finally, in Column (7), we use the instrumental variable strategy described in Section 4 (but drop the additional controls added in Columns 5 and 6). The two instruments generate a strong first-stage: the F-test on their joint significance is 41.04. This stems from a first-stage coefficient of 0.235 (with a standard error of 0.090, significant at the 5% level) on the instrument based on the changes in eligibility criteria; and a coefficient of 0.0038 (with a standard error of 0.0004, significant at less than the 1% level) on the instrument based on the number of concurrent applications. Figure 1 shows the variation in the second instrument in the months between June 2010 – right after the Academies Act was passed – and December 2014 – when the last of the academies in our data applies for conversion. The left panel depicts this information for all months and including applications coming from our eight LAs. The right hand side panel instead presents the variation we actually use – i.e., for the months in which our schools apply and considering only applications coming from other parts of the country. The average number of concurrent applications is approximately 118 with a standard deviation of 54. Although there is a sizeable peak in the central part of 2011 and some obvious seasonality (fewer applications in the summer months), the plots display substantial variation. As for the positive sign of the first-stage coefficient on this instrument, this is consistent with the institutional details discussed in Section 2.4 which make it more likely that applications are processed faster when there are more requests in the pipeline. The second stage results confirm our previous conclusions: we find that schools approved for academy conversion are 14% more likely to be ranked as the most preferred choice by parents. It is interesting to note that this implied effect is remarkably similar to the one obtained in Columns (5) and (6). This is likely due to the fact that both approaches deal with time-varying unobservables that correlate with academisation and affect preference rankings. Given their similarity, in the rest of our analysis we use the specification adopted in Column (6) which we regard as our favourite.[14]

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[13] We experimented with the inclusion of interactions between cohort dummies and time-varying school characteristics (rather than time-averaged). This approach yields almost identical findings.

[14] We also tried to use the two instruments separately. When we only use the number of concurrent applications, we find a strong first stage (t-stat on the instrument: 8.80) and a similarly sized second stage effect (0.119; standard error: 0.078). When instead we only use the eligibility instrument, we find a bigger but very imprecisely estimated effect. This reflects

How sizeable is this effect? To benchmark our results, note that a 1% increase in the home-to-school distance (i.e., approximately 110m) reduces the likelihood of a school being top-ranked by approximately 5%. Our findings therefore suggest that the impact of academisation is equivalent to a 2.5% decrease in the home-to-school distance – or 'marginal willingness to travel' (MWT) – assuming constant elasticity with respect to distance. This corresponds to approximately a 280 metre reduction in the home-to-school travel distance. We return to this issue below where we investigate how the impact of academisation varies with school distance from home.

5.3. Tests for the randomness of the timing of academy conversion

One of the assumptions underlying the approach used in Table 2, Columns (1)-(6) is that the timing of academisation within the set of current and future converters is as good as random and uncorrelated with other changes occurring simultaneously or pre-existing trends. We provide support for this assumption in Table 3.

In the top part of Panel A, we regress time-varying school characteristics on a dummy capturing whether the school is approved for conversion at that point in time. In the bottom part of Panel A, we perform a similar analysis, but focus on the characteristics of neighbourhoods around the schools. These are defined as the set of postcodes that falls within the 75th percentiles of the home-to-school distance measured for all pupils attending the schools in the years 2007 to 2009, prior to our observation window – i.e., postcodes in the de facto catchment areas of these schools (see Gibbons et al., 2008). While these areas are identified using attendance patterns prior to the years in our analysis, the characteristics of these postcodes are: *(i)* time-varying and measured at the same time as the school variables; and *(ii)* calculated using pupils of all ages between 5 and 16 (except for KS2, only available for pupils aged 11; and KS4, only available for pupils aged 16). Regressions are run at the school level with standard errors clustered by school. Column (1) only includes year dummies, while Column (2) adds school fixed-effects. Results in Column (1) suggest that academies have better intakes (KS2), higher attainments (KS4), fewer FSM eligible pupils and are more likely to be rated 'outstanding', which is to be expected given the criteria for conversion. Similar patterns are observed in the neighbourhoods surrounding these schools. However, all these associations become insignificant, substantially smaller or of the opposite sign once school fixed-effects are included. This implies that the timing of conversion is uncorrelated with time-varying school and neighbourhood characteristics and support the assumption that the moment in which current and future academies receive approval for conversion is as good as random. In Appendix Table 2, we present a similar analysis for the instrumental variables used in Column 7, Table 2. The results show that the two instruments are uncorrelated with changes in school and neighbourhood characteristics and therefore plausibly uncorrelated with unobservable confounders.

The validity of our research design also hinges on the assumption of counterfactual parallel trends in demand for early and late converters. Since we do not have data on preferences for schools before 2010, we

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the fact that this second instrument alone does not generate enough time variation in the timing of approval and is too weak to yield a reliable estimate of the impact of conversion on preferences.

cannot directly test the assumption of parallel pre-trends in demand. However, we provide supportive evidence by comparing trends in observable characteristics that might correlate with demand for these two groups of schools in the years before our observation period. Our results are presented in Table 3, Panel B. The only significant coefficients suggest a slight decline in intake ability prior to conversion (KS2), and a marginal increase in neighbourhood disadvantage (FSM) – neither of which is likely to explain the increase in demand shown in Table 2. We also find that, for the academic year just before our observation period, early and later converters are similar with respect to two broad measures of school popularity: the ratio of the number of enrolled pupils to total school capacity and an indicator for whether the school was oversubscribed (gathered from the LAs admission booklets). This brings further support in favour of the ex-ante comparability of these two groups of schools.

5.4. Robustness tests: methods used in related literature

Most of the literature on school choice has used non-linear models estimated via maximum-likelihood – in particular, conditional logit and rank ordered logit specifications. We test the robustness of our findings to these alternative methods. Our results are presented in Table 4. To facilitate a comparison with the findings presented in Table 2, we discuss magnitudes in terms of MWT.[15]

To begin with, in Column (1) we present estimates of a conditional logit model that includes alternative specific constants (school dummies). The dependent variable still identifies whether the school was top-ranked or not. The results show a positive and significant impact of academy conversion on preferences. However, the implied effect corresponds to a larger implied MWT – at approximately 13% or 1.46km.

In Column (2), we exploit the full ordering of the preferences expressed by parents and estimate a rank-ordered logit model with school dummies. Once again, we find a positive and significant effect of academy conversion – corresponding to a MWT of approximately 4.5% or around 500m.

When looking at rank ordered preferences, there is a concern that the listed choices and their order may not represent true preferences when the allocation mechanism is a constrained version of the DA algorithm (see discussion above as well as Calsamiglia et al., 2010 and Fack et al., 2015). To check for the robustness of our estimates to lack of truth telling, we use a similar approach to Fack et al. (2015), Akyol and Krishna (2017) and Abdulkadiroglu et al. (2015a), and restrict the sample to the set of 'feasible schools'. This is a set of schools at which families qualify (*ex-post*) on the basis of school-specific admission criteria and for which families have no incentives to rank preferences out-of-preferred order. To operationalize this approach, we consider schools to be feasible if their *de facto* catchment area (defined above in Section 5.3) encompasses a

[15] Marginal effects from conditional logit models are not suitable for dichotomous variables like the academy status. Furthermore, rank-order logit models estimate the effect of conversion on the preference ranking – so any implied effect derived from such specifications would be difficult to compare with our OLS models that focus on first choice.

student's home address.[16] This approach is a simplification of Fack et al. (2015) and exploits the fact that the vast majority of oversubscribed schools in our sample broke ties within other priority groups (e.g., special educational needs or children with siblings at the school) by distance. The implication is that the *de facto* catchment areas based on distance identify quite well schools at which students would *ex post* qualify. However, to assess the validity of this approximation, we also perform our analysis dropping students who have priority based on the 'looked after', 'special education needs' and 'siblings' rules.

The 'feasible school' restriction reduces the number of schools in the choice set to an average of 8.6 per students. Furthermore, the average/median distance between home and school shrinks to 2.6/3.3 km (from around 11/10km). The point estimate in Column (3) reveals a positive and significant impact on conversion on family rankings – with larger implied MWT of 21%.[17] Note however that given the average distance in this sample, this still corresponds to approximately 550 metres.[18] Finally, in Column (4), we further drop pupils who have the specific priorities for school admissions discussed above. This restriction does not significantly affect our results: we still find an implied MWT of approximately 18%.

We carried out further checks on the possible effects of truth telling on our findings – similar to those reported in Abdulkadiroglu et al. (2015a). First, we note that more than 70% of the families rank less than six schools suggesting that 'truth telling' is likely to be dominant for the vast majority of the sample. Second, we find that the percentage of pupils attending the school they were offered is 97% – which is reassuring: this number would be significantly lower if pupils were offered schools they had only strategically listed as highly preferred and so tried to change school after admission.[19] Next, we excluded from our estimation of the rank-ordered logit models of Columns (2) to (4) pupils that express six preferences. Although this affects the precision of our estimates (we lose 29% of the sample), our main results are confirmed. All in all, we take these findings as evidence that our main results are robust to issues with truth telling – and if anything underestimate the impact of academisation on preferences.

5.5. Additional robustness checks

We carried out a number of other checks which are available in the on-line appendix to this paper. Among others, these include: *(i)* restricting the choice set for each family to schools within 2.5km – i.e., the median distance for schools listed by parents on the application form; *(ii)* dropping recent residential movers to check

[16] The exact size of the catchment areas does not vary much across years, so it does not matter empirically whether we create these by averaging over three years or by using specific years – including those we analyse.

[17] If we use a specification that restricts the coefficient on distance (in kilometres; not logs as in our main specification) to -1 as Fack et al (2015) and Abdulkadiroglu et al. (2015a), we find an implied MWT of 19%.

[18] For comparison, OLS estimation of the model in Column (6) of Table 2 on the 'feasible school' set yields a MWT of almost 29%.

[19] In more detail, the percentages of pupils offered their 1st, 2nd and 3rd choice are approximately 71%, 11.5% and 6% with 98.6%, 97.5% and 97.1% of students attending their 1st, 2nd and 3rd choice (conditional on this being offered).

for sensitivity to residential sorting on preferred schools; and *(iii)* expanding the set of school-level control variables. None of these modifications make any substantive difference to the results.

6. Who chooses academies?

6.1. Heterogeneity by income, ethnicity and home-to-school distance

Table 5 presents results on the heterogeneity of parental demand for school autonomy focussing on income and ethnicity – measured by FSM/non-FSM status and White British/other ethnic group respectively. The table present the effect of academy conversion on the probability of ranking a school first estimated separately for different sub-samples of pupils. These specifications allow the effect of observed time-varying school characteristics (the control variables) and unobserved time-fixed school characteristics (the school fixed effects) to vary by pupil type.

Columns (1a) and (1b) suggest that a converter academy is 17% more likely than an average school to be ranked first by families of non-FSM children. This effect corresponds to a MWT of 3.2% or approximately 360 metres. Conversely, these schools are only 6% more likely to be ranked first by poor households eligible for FSM, and this effect is not statistically significant (MWT: 1.1%). Although the difference in the point estimates across the two groups in not statistically significant (p-value: 0.443), it is sizeable and economically meaningful. Interestingly, given the usual finding that poor families are more likely than non-poor to choose local schools (see Burgess et al., 2015), the impact of home-to-school distance on our metropolitan study area is similar for non-FSM and FSM pupils.

Columns (2a) and (2b) look at differences by ethnic group. The evidence reveals a marked heterogeneity. White British families are almost 26% more likely to list a school as their first choice after conversion to academy (MWT: 5.2% or nearly 600 metres); on the other hand, we detect no significant effect among other ethnicities. Given the strong correlation between income and ethnicity and in order to understand which of these two attributes drives the heterogeneous patterns of demand, we further distinguish between FSM and non-FSM eligible pupils within the two ethnic groups. Our results – reported in Columns (3a)-(3d) – show that the positive effects previously documented separately for non-FSM eligible pupils and for White British families are in fact caused by the stark preference for academies expressed by the subgroup of non-FSM eligible, White British pupils. A school is 31% more likely to be listed by this group as a first preference when it converts to academy. This effect corresponds to a MWT of 6.1% or around 680 metres. On the other hand, the effects for the other three sub-groups, while still positive, are much smaller in magnitude (ranging between 12.6% and 5.4%) and not statistically significant. The differences across these groups are large in magnitude and statistically significant: a test for the equality of the coefficient for the non-FSM, White British group and the coefficient for the FSM, White British group rejects the null with a p-value of 0.069. Similarly, the coefficients for non-FSM, White British group and FSM, other ethnicity group are statistically different with a p-value of 0.100. Lastly, the p-value for the equality test on the remaining pair (non-FSM, White British versus non-FSM, other ethnicity pupils) is 0.147. While not significant at conventional level, the difference in the implied effect on preferences is still sizeable.

Our results on preferences can also shed some light on the determinants of segregation between schools of non-FSM eligible and White pupils vis-à-vis the others, when compared to the corresponding patterns of enrolment. We investigated these issues by re-estimating our regressions with indicators capturing whether a student has received an offer or whether the student is enrolled in a given school as the dependent variable, rather than whether the family ranks the school first in their choice list. These regressions indicate that the groups who are more likely to list an academy first – particularly non-FSM, White British pupils – are also significantly more likely to receive an offer and be enrolled in one subsequently (results are available in the On-line Appendix Table 2).[20] These patterns are consistent with the evidence presented in Table 5 and suggest that school and LA admission criteria do not offset the heterogeneity in preferences in determining school attendance and school composition. Interestingly, these findings also suggests that heterogeneous parental preferences – rather than 'covert' selection by schools – explain the patterns of segregation documented by other studies in relation to academies' intake following conversion (see for example Eyles et al., 2017).

Given the importance of distance in relation to both school admission and parental preference, we next investigate how our results vary by home-to-school distance and by FSM eligibility and ethnic background. Our results are presented graphically in Figure 1 which displays the impact of academy conversion on parental demand at varying levels of the home-to-school distance (with confidence intervals). The plotted effects come from specifications that include the usual academy conversion indicator and the control for home-to-school distance (in logs) – but now add an interaction between the two. The panels display the linear combination of the effect of conversion – representing the impact of academisation at zero log-distance (1km) – with the estimated interaction term effect at varying distances. Therefore they represent the increase in the probability that a school at a given distance is ranked as the top preference following conversion relative to a comparable school at a similar distance that does not convert. In the top panel, we consider FSM eligibility (Panel A); the central panel focuses on pupils' ethnic background (Panel B); and the bottom panel considers non-FSM eligible, White British pupils and students who are either FSM eligible or non-White British.[21]

Panel A shows a very strong effect of conversion on the chances that a *close-by* school is rated as the top preference by non-FSM eligible pupils as well as a strong spatial decay in the demand for academies as these are located further away from home. At the median distance for schools listed by parents on their application form (2.5km), a school that converts to academy is approximately 190% more likely to be listed as top preference. This impact further climbs to factor of more than two and a half for schools 1.5km away from home – i.e., the median distance for top ranked schools. The steep spatial preference decay implies that the 'extra demand' effect of academisation dissipates at around 7km. On the other hand, we find no evidence that families of FSM eligible students rank converter schools higher – even when these are very close-by.

[20] These results do not condition on schools being the first choice. If we focus on first choice only, we find consistent evidence: pupils with more advantageous family background are more likely to be offered a place or attend an academy.

[21] We also estimated the impact of academy conversion on parental preferences at various home-to-school distances pooling all pupils. Results are presented for comparison in the On-Line Appendix Figure 1.

Similar but even bigger effects are displayed in Panels B and C where we focus on pupils' ethnic background and the interplay of ethnicity and FSM eligibility. In particular, for the group of non-FSM eligible, White British pupils, a school that becomes academy and is 2.5km away from home is three times as likely to be listed as the family's top choice as a comparable school that does not convert. On the other hand, the impact for a pupil who is either eligible for FSM or non-White British is close to zero and flat across the distance span considered.

An important implication of these results on heterogeneity in preferences is that they imply a high degree of sorting of students of different incomes and ethnicities in response to changes in school policy. Previous research (e.g., Burgess et al., 2015 for England) provides evidence of sorting into schools due to differences in preferences for school quality and school composition between socioeconomic groups. However, to the best of our knowledge, the vast majority of previous studies have only had a single cross section of choice data, making it difficult to determine whether this sorting represents a causal link between school quality and demand, or something unobserved and persistent about the geographical area or the school. The results above are the first to show that families respond quickly to changes in institutional characteristics, with significant heterogeneity in demand across different groups, and the potential for sorting and segregation.

We also investigated differences by gender but found nothing of interest. Further, we studied whether there is heterogeneity by student prior attainments by considering pupils with KS2 test scores above/below the median of the national distribution. We found that schools are 21% more likely to get ranked first when they convert by families of low-KS2 families, but only 7% more likely to be ranked first by high-KS2 families. However, this pattern was sensitive to the exact specification we used and disappeared once we considered heterogeneity along both the prior attainment and the distance dimensions. We therefore do not report these results or discuss them any further.

6.2. Heterogeneous patterns: what explanations?

In this section, we investigate some possible explanations for the starkly heterogeneous demand patterns discussed above. To start with, we consider whether heterogeneity in preferences is due to differential academy availability – conditional on place of residence – for individuals with different background characteristics. To do so, we take two routes: *(i)* we focus on the sub-set of pupils who live within the de facto catchment area of at least an academy (approximately 98% of the sample); and *(ii)* we control for interactions between school fixed effects and home-to-school distance in our specifications. This approach accounts for the possibility that schools with certain characteristics (possibly unobservables) that correlate with academisation and attract applications might lead to increased sorting of families across space. This in turn compresses their de facto catchment area – i.e., their availability – affecting the impact of distance on parental preferences estimated pooling all schools. Our results are tabulated in Panels A and B of Table 6 and suggest that the heterogeneity along the dimensions of income and ethnic background is robust to considerations about accessibility. We also studied whether our results change if we only consider 'feasible schools' – instead of focussing on pupils falling in the catchment area of at least one academy. This alternative restriction did not affect our findings.

Next, we consider the possibility that parents of different background have access to different amounts of information – including school conversion to academy – and so the pattern discussed above are explained by an information story. While plausible, this explanation is hard to square with the evidence on the interplay between academisation and distance: even for schools as close as 1km (i.e., the 1st percentile of the distance distribution) and for which information should be easy to gather, parents of FSM eligible pupils and/or non-White British pupils express no additional demand following conversion. To further dispel this channel, we run regressions that only consider schools in the Birmingham LA. As discussed, the LA sends booklets to parents containing precise information for schools in the LA. Although these booklets contain information for schools in other LAs – for example links to the webpages of the LAs – the level of detail provided is much less exhaustive. We therefore consider whether our results differ if we only analyse the subset of schools for which easily accessible information was provided to all parents in a uniform way. Our results are presented in Panel C and suggest that this explanation cannot account for our findings.

Another possibility is that parents of poorer background and ethnic origins simply 'do not care about schools' – and so do not care about academy conversion either. To investigate this issue, we follow the approach used in Burgess et al. (2005) and estimate school choice models with postcode of residence-by-cohort fixed effects instead of pupil and school fixed effects – enabling us to recover estimates of preferences for schools with different characteristics. Our results (not tabulated) show that, although there are differences between the various groups we consider, families are more likely to rank schools first if they have higher KS4 attainments, a larger share of White British pupils and a lower incidence of FSM eligible students – irrespective of their own background. If we use our standard specification which includes school fixed effects, we come to similar conclusions although our estimates are much less precise because of the limited amount of within-school variation in these variables over three years. In light of these patterns, it is natural to assume that parents of different backgrounds would have also taken into account academy conversion – given its potentially far-reaching implications – when forming their preferences about schools.

Finally, it is conceivable that non-FSM, White British families are more likely to support the Conservative party – as well as the autonomy/market-oriented values it embraces and were partly embodied in the academy programme – than poor non-White British families,. It is thus possible that school choices reflect political preferences rather than academic ones. Indeed these political orientation patterns can be seen in data from the British Household Panel Survey (BHPS). The BHPS is a longitudinal survey that follows a representative sample of families in Britain since the early 1990s and gathers information on individual background, income and labour market status and, in some of the waves, data on political orientation and views about society and the economy. In order to match the likely demographic characteristics of the households in our sample, we retain data on heads of household aged between 25 and 55 in families with children (the number of observations ranges between approximately 7,500 and 12,000 depending on the outcome considered). When analysing these data, we find that better-off (higher income) and non-ethnic minority head of households were significantly: *(i)* more likely to vote for the Conservative party; *(ii)* more inclined to believe that private enterprise is the best way to solve socio-economic problems; and *(iii)* less likely to think that public services should be owned and delivered by the state. This was true in unconditional

correlations as well as in regressions that control for age, gender, marital status, number of children and educational attainments. We discuss the implications of these patterns in Section 7.3 where we study some possible channels that could give rise to our findings

7. Exploring the mechanisms

In this section, we explore some mechanisms that could explain the findings documented so far. We only tabulate results for all pupils and for the groups of families (non-FSM eligible and White British) where we found significant demand responses following academy conversion. Results for the complementary groups are not tabulated – as we still find no evidence of any significant effect.

7.1. Expectations about changes in school composition and effectiveness

We start by investigating the most likely explanation: families choose an academy because they expect the characteristics and performance of the school to improve – and they are choosing based on these expected changes. Uncovering what families value about schools and separating their preferences from their beliefs and expectations is an exercise fraught with identification problems (Giustinelli, 2016; Giustinelli and Manski, 2016; and Wiswall and Zafar, 2014). Here we simply focus on two aspects of schooling – peer group quality and academic effectiveness (value-added) given that these factors are generally acknowledged as objects of preference – both anecdotally and according to empirical evidence (e.g., Gibbons et al. 2014; Burgess et al. 2015; Abdulkadiroglu et al. 2017b). Of course, given that academies were a new phenomenon in our study period, families had limited information on which to base their expectations *ex-ante*. We therefore assume that families have perfect foresight and that their expectations are correct, and so estimate the impact that controlling for *actual* changes in school characteristics *ex-post* has on the patterns of demand for academies observed in Tables 3-5 (our approach follows Ferreyra and Kosenok, 2015).[22]

The results of this analysis are presented in Table 7, in which the coefficient for the effect of academy conversion is presented first for all pupils (Column 1), and then by the FSM and ethnic group (Columns 2-4). The three panels show how this coefficient changes as we control for school post-conversion characteristics – i.e., our proxies for expectations about school changes following academisation – which might plausibly explain the decision to list an academy first.

Panel A introduces controls for school composition – i.e., the proportions of FSM, White British and female students, and the mean KS2 prior achievement in the entry cohort – measured in the year when students enrol in secondary school (one year after expressing preferences). As discussed above, the

[22] Under the assumption these variables capture parents' expectations and that these are formed before parents express their choices, controlling for these measures is not harmful to our estimates of the impact of conversion on demand. However, these variables could be 'bad controls' – in the sense of Angrist and Pischke, 2009 – if these assumptions do not hold. Even then, it is instructive to study whether the coefficient on academy conversion is attenuated once we control for specific school characteristics that could explain the academy effect on choices.

heterogeneity in demand that we document in Table 5 implies that academies are more likely to make offers and enrol well-off students – leading to a more advantaged and stratified intake. Nevertheless, adding these controls does not substantially affect our main findings.

Panels B and C investigate whether expectations about changes in school effectiveness following academy conversion can explain the demand for academies. In order to estimate academies' impact on pupil value added, we follow two approaches. First, we take the route used in Eyles et al. (2017b) and compare the KS4 attainments of pupils that already enrolled for secondary education (grade 7) in converter academies prior to their conversion to the attainments of pupils enrolled in schools that convert to academies after our observation window. By focussing on students enrolled in academies prior to their actual conversion, we by-pass the endogeneity of school choice/mobility. The use of 'legacy' students is similar to the 'grandfathering' method used by Abdulkadiroglu et al. (2016) for US charter takeovers. This approach should yield an unbiased causal estimate of the impact of academy conversion on a randomly picked student. Note that since different schools had different eligibility criteria (and possibly incentives; see Section 2.4) to convert depending on their inspectorate rating, we estimate our models separately for schools rated 'outstanding', 'good' and 'satisfactory/inadequate' at the latest inspection prior to 2010. Our results show that schools rated 'outstanding' prior to conversion have a positive and significant effect on students' KS2-to-KS4 value added following academisation. However, we find no evidence that schools rated 'good' or 'satisfactory/inadequate' affect their students' outcomes after conversion (neither positively nor negatively). Second, we compare the attainments of students who endogenously choose to take their KS4 exams at a school that has already converted to academy to those of students who choose to sit their tests at a future academy (again stratified by OFSTED ratings). Although these estimates are potentially biased by sorting, they are likely to contain a 'match-specific gain' component which might be relevant for parental preferences. In practice, these specifications yield similar estimates to those obtained using legacy enrolment.[23]

The most striking finding from Table 7 is that adding in these controls makes very little difference to the estimated impact of academy conversion on preference rankings. This is true both for the full sample (Column 1) and for the various income and ethnic groups (Columns 2-4).

Given the evidence that academies attract more advantaged peer groups and that (at least some) converters increase their effectiveness, the results in Table 7 suggest that either: *(i)* families do not rank schools on the basis of these attributes; or *(ii)* their expectations and beliefs about the effect that academies will have on these attributes ex-ante do not coincide with the reality ex-post. Assuming the earlier literature is correct – i.e., families are indeed attracted to schools with good peer groups and high value-added – our results in Table 7 suggest that families have too little information to form rational expectations about the changes along these dimensions that will stem from academy conversion. This seems plausible given the sudden and radical nature of the policy shock and the lack of information about costs and benefits of academisation. However, given the caveats above, our evidence should be taken as suggestive.

[23] More details about our methods and findings are provided in On-Line Appendix 2.

In some additional checks (not tabulated), we introduce other controls for school inputs and policies that could have changed following conversion, including: *(i)* school expenditure; *(ii)* number and types of KS4/GCSE subjects offered; *(iii)* characteristics of the school workforce as captured by number of teachers, teacher average age, teacher turnover, a dummy for a head-teacher change and proportions of teachers who are permanent, female, part-time and with post-degree qualifications. These characteristics were chosen to capture other key dimensions on which the autonomy granted to academies status might influence the organisation of the school and parental preferences (see Section 2.3).[24] None of these modifications affects our key findings.

7.2. Academisation and pre-conversion school quality and popularity

In this section, we study whether parental preferences for academies vary depending on school characteristics that pre-date conversion. Our results are presented in Table 8. This has a structure similar to Table 7, but introduces interactions between academy converter status and three pre-existing school characteristics. These are selected to represent school attributes observable by parents – mostly through school league tables or admissions booklets – which might signal quality or popularity. In conjunction with academy 'branding', these features might be salient in conveying a sense of school excellence which influences families' choices. Note that we study heterogeneity in the response of preferences to academy conversion by fixed pre-existing characteristics – in contrast to Table 7 which included controls for future time-varying school characteristics.

Panel A interacts converter academy status with an indicator that the school had an above-median proportion of students scoring A*-C in their KS4/GCSE qualifications over the 2007-2009 period. This is the headline performance figure reported in the publicly available league tables on school quality. Panel B looks at the interaction between school popularity and academy status – assuming that popularity suggests quality through revealed preference. Specifically, we use an indicator identifying whether over the period 2007-2009 the school had more pupils enrolled than its official capacity. Gibbons and Machin (2006) show that preferences for this indicator of quality are revealed in house prices.[25] Finally, Panel C introduces an interaction between academy status and an indicator that the latest school OFSTED inspection rating prior to 2010 was 'outstanding' (see Section 2.3).

The initial picture from Table 8 for the whole population of pupils (Column 1) is that there is a moderate difference between high-quality/popular schools and lower-quality/less popular schools in the effect of conversion on family choices. High KS4-GCSE/over-capacity schools tend to attract more demand on conversion than do low KS4-GCSE/less popular schools, although the differences between the coefficients are

[24] Empirically, we find little evidence that academies introduce significant changes in these dimensions. Regarding the taught curriculum, academies offer 1.25 more subjects (out of an average of 25 subjects) although this estimate is not significant. Similarly, we find no evidence of school expenditure changes and limited evidence of meaningful differences in schools' workforces following conversion. The only significant estimate concerns the change in head-teacher, which shows an increase after academisation.

[25] Note that 81%/43% of current converters and 44%/33% future converters are high quality/overcapacity, respectively.

generally quite small and not statistically significant (p-values >0.2). Similarly, there is basically no heterogeneity along the OFSTED inspection rating dimension – with the point estimate being slightly larger for 'outstanding' school but only significant for non-outstanding ones.

The heterogeneity between high and low prior KS4/GCSE and popular/not-popular converters becomes more marked amongst non-FSM (wealthier) families in Column (2), White-British families (Column 3) and especially so amongst better-off families who are both White British and non-FSM (Column 4). For the latter group, prior-KS4/GCSE attainments and popularity is a crucial factor conditioning the demand for converter academies. The coefficient on the interaction between above-median percentage of A*-C KS4/GCSE and the converter status in Panel A, Column (4) is very large and statistically different from that for low GCSE converter schools (p-value: 0.0143). Similarly, over-capacity schools attract a much larger increase in demand following conversion than non-overcapacity schools: the impact of academy conversion for popular schools is three times larger than the effect for schools that are in lesser demand – and this difference is statistically significant (p-value: 0.0125). Similar findings emerge when focussing on non-FSM pupils (Column 2) and White British pupils (Column 3) only – with the difference in the effects for popular/non-popular academies being sizeable (by a factor of two to three) and significant (at better than the 10% level). Conversely, the patterns are less clear when we consider inspectorate ratings: while the effects are only significant for non-outstanding academies, the point estimates for the outstanding group are always larger.

We experimented with other proxies for quality pre-dating conversion (results not tabulated). To being with, we considered whether there is heterogeneity in the response to academy conversion when we consider quality as measured by the school 'contextual value added' (CVA) averaged between 2007 and 2009. CVA is the proxy for school effectiveness (given intake) published in school league tables; it is basically the residual from a regression of KS4 test scores on KS2 test scores and student characteristics. Our findings confirm the patterns discussed above for school KS4/GCSE: non-FSM and White British families express a significantly higher demand for academies with above-median pre-conversion CVA. Next, we refer to the admission booklet sent by the LA to families and derive an indicator of whether the school had more applicants than places in 2009, just before our study period (i.e., whether the school was over-subscribed). Again, we find similar patterns: schools that are oversubscribed prior to conversion experience significantly larger changes in demand following conversion than schools that are not oversubscribed.

Overall, the evidence suggests that families treat academy conversion in conjunction with pre-existing quality measures as markers of excellence – and possibly use this information in a 'heuristic way' to assess a school's suitability for their children. For example, parents might take the high conversion rate among *ex-ante* good schools – i.e., those that are popular and well-performing – as evidence that good converting schools will be able to deliver high standards *ex-post*. The only exception emerges when we measure quality using the 'outstanding' inspectorate rating – even though the results discussed in Section 7.1 suggest that this group of schools experiences the largest improvements in effectiveness following conversion. We discuss possible rationalizations of our findings in the next section.

7.3. Interpretation and discussion

Our results suggest that families use a heuristic-based approach to decision making when choosing schools in a fast changing policy environment. In a context – like ours – where little information is available about the likely effects of a radical policy change, the use of simple rules of thumb from available school indicators seems plausible. While the standard economic model of human decision making under uncertainty assumes that individual are 'unbounded rational', starting from the 1950s, economists began suggesting that 'bounded rationality' might be a more plausible description of human behaviour (see Simon, 1955). In particular, in situations in which decision under uncertainty involves gathering and systematizing vast amounts of information – and using 'costly' brainpower to process it – it might be rational to use short-cut and rules-of-thumb to problem-solving and economise on human limited and exhaustible computational capacity. Paraphrasing Kahneman and Tversky in their various works on this topic, heuristics are simply judgmental shortcuts that give individuals the right answer most of the time and quickly – at the cost of sometimes sending them in the wrong direction. We use the wording 'heuristic-based problem solving' in this sense.

In fact, the patterns we document are reminiscent of two of the heuristics discussed in the 1974 Tversky and Kahneman study: the 'representativeness' and the 'availability' heuristics. According to the first, parents equate a high probability of conversion given pre-conversion high quality with a high probability of future high quality given conversion. This logic reasoning is partly sound by design: the government mainly allowed high-performing schools to convert and conversion was costly from the school point of view – so parents might have rationally interpreted conversion as a credible signal of quality. Our results further suggest that families only found this signal credible when combined with an observable track record of high attainments scores (i.e., KS4/GCSE scores) and popularity. While convenient, this heuristic can lead to fallacious conclusions as it does not consider the possibility that high pre-conversion quality might not lead to future high quality – that is, it neglects mean-reversion. Similarly, the results we presented in Section 6.1 on the interaction between home-to-school distance and parental preferences for converters could be interpreted as suggesting that parents inform their decision-making using an 'availability'-like heuristic: parents choose academies close-by because proximity simply makes them more salient and retrievable when calculating costs and benefits of choices under uncertainty – irrespective of the actual gains they may confer to pupils. In fact, it is possible that both mechanisms played a role: continuous discussions about academies in the media – alongside the government relentless efforts to associate the idea of academy conversion with high education standard – might have influenced parental choices.

Can we say anything about whether this possibly heuristic-based decision making leads families to behave rationally or not? Providing an answer to this question is virtually impossible since we have no information about beliefs or survey data eliciting the aspects of academy conversion that families report to value (see the discussion in Giustinelli and Manski, 2016). However, we can provide some (limited) evidence on a related question – i.e., on whether families make decisions that socio-economic researchers (and policy makers) would identify as conducive to better educational outcomes. One metric to gauge this is in terms of higher effectiveness and better composition *ex-post* – i.e., following academisation. Our evidence offers little

support for the idea that high KS4/GCSEs, popular and close-by schools – i.e., those that attract most first preferences following conversion – are those that experience the biggest gains. The correlation between post-conversion changes in effectiveness and pre-conversion KS4/GCSE attainments and popularity is positive, but small – at 0.078 and 0.096, respectively. Furthermore, the association between changes in effectiveness and home-to-school distance is small and positive – at 0.029 – suggesting, if anything, that academies further away from pupils' homes provide higher value-added following conversion. Conversely, 'outstanding' inspectorate ratings prior to conversion is associated with higher effectiveness following conversion (as discussed above) – although it does not significantly interact with academy conversion in shaping parental preferences. We find similar patterns when we look at variation in composition following academisation: changes in the quality of a converter's intake do not bear a strong association with its pre-conversion characteristics or its distance from a pupil's home. All in all, these findings suggest that families' updating in preferences is not associated with clear improvements in the expected quality of the top ranked schools.

To conclude, we note that the results described above could be consistent with other interpretations. First, proximity might make it easier to gather 'soft' information about what academy conversion actually means in terms of organisational change and impacts on achievement. So parents might be choosing on the basis of information that we cannot observe. While plausible, this explanation does not account for the significant interaction effect between academy status and pre-conversion measures of quality and popularity. Further, our results could be explained by a 'self-fulfilling coordination' story: middle class 'pushy' parents want to segregate into schools with similar peers. Academy conversion creates a 'focal point' that allows these families to coordinate and self-sort into schools that have their desired 'high profile' intake – irrespective of whether anything else really changes. This interpretation is also not fully consistent with our evidence: the results discussed in Section 7.1 show that controlling for future expected composition cannot explain our results – casting doubts on the validity of this explanation. Lastly, it could be that non-poor, White British households are more inclined to choose academies because of their affinity with the Conservative party that championed them and the autonomy and market-oriented values they embody. The evidence gathered using the BHPS and discussed in Section 6.2 suggests that this could explain the stark differences between non-FSM eligible, White households and poor families of ethnic origins. However, this explanation in isolation cannot provide a rationale for our evidence that is mainly proximate, popular and previously high-performing academies that attract extra demand.

8. Concluding remarks

In this paper, we have exploited pupil level information about school preferences coupled with the rapid expansion of the academy sector in the England to uncover how families respond to policy changes that inject significant amount of school autonomy within the state sector while providing little guidance about possible expected benefits that these alternative arrangements might confer to students. Stated differently, we have studied how families value the offer of freedom and the 'branding' of schools as autonomous – irrespective of the actual changes implemented under this label.

Our results show that – on average – families have a significant preference for schools that opt-out of the control of the local authority. However, this average result masks some substantial heterogeneity along dimensions of family background. In particular, better-off families respond strongly to conversion to autonomous school arrangements and are significantly more likely to list converter academies as their most preferred school. Conversely, worse-off families with pupils eligible for FSM and/or non-White British students either show no significant interest in schools converting to academy status.

How sizeable are these findings? In order to quantify them, we use the implied average MWT in our sample – at 2.5% or just below 300 metres – and the MWT estimated for the most responsive group of non-FSM White-British families – at 6% or roughly 700 metres. Using an average walking speed of 12 minutes per kilometre and considering a two-way journey (from home to school and back), this means between 0.12 hours and 0.28 hours per day. Further using standard estimates of the monetary value of a non-working hour used in transport project appraisal in the UK (£5, Department of Transport 2014) and assuming three school terms of 12 weeks each per year, this means between £110 and £250 per year. This corresponds to 0.5%-1% of the median full-time Birmingham resident annual earning (at around £22,000 in 2010) or 1.8%-4.2% of the average annual secondary school expenditure per pupil (at around £6000 in 2010). While not very large, these sums are non-negligible.

We have investigated several possible mechanisms that could give rise to our findings. To start with, we assumed that parents and their children are rational and forward looking, and can correctly anticipate the changes schools will experience following academy conversion. We therefore studied whether expectations of shifts in the student composition of academies and their effectiveness at raising end-of-secondary education attainments can account for our findings. All in all, we find that this is not the case.

We then investigated whether families' increased preference ranking for academy converters depends on indicators of prior performance, quality and popularity. Our evidence in this respect suggests that high performing and popular schools are those gaining most of the extra demand following conversion – especially among parents of non-FSM, White British pupils. A potential explanation is that families use the event of academy 'branding' in conjunction with prior information on high quality and popularity to form heuristic-based expectations about schools' suitability for their children. This might be because families take the observed high probability of conversion among *ex-ante* high quality schools to suggest that good converting schools will deliver high quality *ex-post*, and choose accordingly. We also found evidence that converter schools close to pupils' homes are those experiencing the biggest increases in demand. Most likely this is because parents are just more aware of the conversion happening – a kind of 'availability' heuristic.

Given the context, these interpretations are not entirely implausible: the education system was hit by a sudden 'shock' and experienced a very rapid increase in the number of academies – even though little was known about their functioning and impacts. Therefore, families had little valuable information to form forward-looking expectations on the effects of academy conversion – and instead relied on rule-of-thumbs to guide their decisions under uncertainty. In fact, these explanations are consistent with the growing evidence from behavioural economics showing that 'coarse thinking' is common in individuals' day-to-day real-life

decision making and in particular in education choices (see Mullainathan et al., 2008; Della Vigna, 2009 for a review of the field and Lavecchia et al., 2014 for a survey of behavioural economics of education).

Besides advancing our understanding of parental preferences and choice behaviour in the context of significant school reforms, our findings carry clear and important policy implications. To being with, there is evidence – from the UK, Sweden and the US – showing that more autonomous schools tend to have more stratified pupil intakes relative to other comparable schools. These findings have often been taken as suggesting that these institutions operate selective admission practices – either openly or 'by the back door' when the code of practice regulating school admissions does not allow selection (as in the English case). Our results suggest that irrespective of school admission practices there is a more fundamental problem at the heart of this evidence: parental preferences for autonomous schooling are heterogeneous along dimensions of family background. 'Demand-side' policies aimed at raising awareness of any benefits of autonomous school and – more generally – awareness of the value of a good education among worse-off parents are more likely to have significant effects in terms of counterbalancing schools' tendency to become stratified along the dimensions of family background.

More fundamentally, our findings of heterogeneous patterns of response across demographic groups carry a note of warning for policies centred on parental choice, autonomy and school competition. Although at present there is only limited and mixed evidence to suggest that converter academies are effective at improving students' end-of-secondary school achievements (see Eyles et al., 2017b), the autonomous arrangements of these schools might still impact students' non-cognitive abilities as well as their longer-run attainments and labour market outcomes (see Angrist et al., 2016 and Dobbie and Fryer, 2015 for some evidence on US charter schools). Furthermore, the skewed composition of these schools might concentrate beneficial peer effects among better-off pupils. Once more, evidence on the impact of schoolmates' characteristics on short-term achievement in England is mixed (see Lavy et al., 2012 and Gibbons and Telhaj, 2016). However, recent work on the long-term impact of peers shows that school composition is an important determinant of young adults' wellbeing and success (see Bertoni et al., 2017; Black et al., 2013; Bifulco et al., 2011; and Chetty et al., 2011). Lastly, academies' autonomous arrangements might make them more responsive to competition-like incentives in education by raising students' achievement. In the English context – with its growing emphasis on quasi-market reforms in education and a share of autonomous secondary schools that is set to increase in the near future – these features might make these schools better positioned at thriving in the education arena. At present no evidence has been gathered on these issues. Understanding the systemic 'general equilibrium' effects of academisation is clearly a crucial area of future investigation.

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Tables and Figures

Table 1: Key descriptive statistics – Full sample and converters sample

	Full Sample		Converters sample	
	Mean	Std. Dev.	Mean	Std. Dev.
Panel A: Pupil level information				
KS2 attainment	27.79	4.121	--	--
Pupil is FSM eligible (FSM)	0.328	0.470	--	--
Pupil is female	0.491	0.500	--	--
Pupil is White British	0.401	0.490	--	--
Number of preferences expressed	3.773	1.800	--	--
Pupil has only preferences outside LA of residence	0.034	0.180	--	--
Panel B: School level information				
School approved as a converter academy	0.117	0.322	0.336	0.474
School is outside LA of residence	0.400	0.490	0.420	0.495
School is selective	0.096	0.295	0.168	0.375
KS2 average attainment (lagged)	27.53	2.097	28.188	2.383
KS4 average attainment (lagged)	38.00	6.288	40.582	6.848
Share of FSM pupils (lagged)	0.244	0.188	0.190	0.186
Share of female pupils (lagged)	0.497	0.224	0.512	0.229
Share of White British pupils (lagged)	0.546	0.322	0.555	0.329
Pupil/teacher ratio (lagged)	15.16	1.671	12.44	1.628
School rated 'Outstanding' by OFSTED (most recent)	0.291	0.455	0.389	0.489
School was overcapacity in 2009	--	--	0.500	0.504
School was oversubscribed in 2009	--	--	0.850	0.360
School rated 'outstanding' by OFSTED, last inspection up to 2009	--	--	0.367	0.486
Panel C: Choice level information				
Probability school is highest preference	0.0088	0.0939	0.0097	0.0983
Pupil-school straight-line distance (in km)	11.159	6.655	11.885	7.045

Note: Number of observations in the full sample: 37,140 pupils; 125 schools over three years (i.e. 375 school-by-year observations); 4,176,755 pupil-school possible choice combinations. Converters sample considers only schools that are already academies at the time when pupils choose (i.e. prior to October of year *t*) and schools that will become academies in the future (but excluding the immediately adjacent year, i.e. the one starting from November of year *t* and finishing in October of year *t+1*). Number of observations in the converters sample: 37,140 pupils; 60 schools; 1,448,688 pupil-school possible choice combinations. In both the full and the converters sample, schools in the choice set exclude 15 female-only and 10 male-only schools for male and female respectively. LA of residence is Birmingham. Other LAs include Dudley, Sandwell, Solihull, Staffordshire, Walsall, Warwickshire and Worcestershire. KS2 (age 11/grade 6) attainment refers to level attained on average in English, Maths and Science (average point scores). KS4 (age 16/grade 11) attainment refers to level attained on average in English, Maths and Science (average point scores). KS2 at the school level refers to primary school test scores (taken in grade 6) of pupils starting secondary school (in grade 7). Lagged school characteristics refer to the academic year prior to the one in which pupils express their preference (e.g. for preferences expressed between May 2009 and October 2009, school characteristics refer to the academic year 2007/2008). OFSTED is the English school inspectorate. OFSTED inspections are not carried out every year. The rating refers to the most recent inspection available at the time when parents were making their school choice. Overcapacity in 2009 identifies schools with a ratio of total pupils to total capacity higher than one. Information on oversubscription in 2009 is obtained from local authority booklets. Information on these two variables and for 2009 OFSTED rating only collected for converter schools.

Table 2: The impact of conversion to academy on the demand for schools – Pupils' highest preference

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Full Sample	Full Sample	Full Sample	Converters Sample	Converters Sample	Converters Sample	Converters Sample
	OLS	OLS	OLS	OLS	OLS	OLS	IV
Converter Academy	0.070 (0.031)**	0.071 (0.031)**	0.068 (0.029)**	0.091 (0.055)*	0.118 (0.057)**	0.132 (0.061)**	0.136 (0.069)*
Log of pupil-school Distance	-0.045 (0.002)***	-0.048 (0.002)***	-0.048 (0.003)***	-0.053 (0.004)***	-0.053 (0.004)***	-0.053 (0.004)***	-0.053 (0.004)***
Implied highest preference effect	*7.95%*	*8.07%*	*7.72%*	*9.32%*	*12.09%*	*13.52%*	*13.93%*
Marginal willingness to travel	*1.55%*	*1.48%*	*1.42%*	*1.72%*	*2.23%*	*2.49%*	*2.57%*
F-Test first stage	--	--	--	--	--	--	*41.04*
School effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year and gender effects	Yes	No	No	No	No	No	No
Pupil effects	No	Yes	Yes	Yes	Yes	Yes	Yes
School time-varying controls	No	No	Yes	Yes	Yes	Yes	Yes
School time-averaged controls × Year effects	No	No	No	No	Yes	No	No
School 'academy propensity' × Year effects	No	No	No	No	No	Yes	No

Note: The table reports coefficients and standard errors in parenthesis (clustered at the school level) on a dummy for academy conversion (multiplied by 100) and on the log of home-to-school distance. All regressions control for a variable identifying schools approved as 'sponsored' academies. The dependent variable is a binary outcome denoting the students' highest preference. Number of observations in Columns (1) to (3): 4,176,755 (125 schools and 37,140 pupils). Converters sample considers only schools that are already academies at the time when pupils choose (i.e. prior to October of year *t*) and schools that will become academies in the future (but excluding the immediately adjacent year, i.e. the one starting from November of year *t* and finishing in October of year *t+1*). Number of observations in Columns (4) to (7): 1,448,688 (60 schools and 37,140 pupils). Implied academy effect obtained by rescaling the coefficient by the probability of a school being the highest preference (0.0088 in full sample and 0.0097 in converters sample). School-level, time-varying controls include: KS4 attainment; share of FSM eligible pupils; share of female pupils; share of White British pupils; pupil-teacher ratio; and school rated 'Outstanding' by OFSTED. School time-averaged controls are averages of the latter over the period 2009-2011. School 'academy propensity' refers to the predicted probability of a school becoming an academy over the period under analysis (2009-2011) estimated using a linear probability model and including the school controls described above. Column (7) instruments the binary indicator capturing whether the school is an approved converter academy at that time. The two instruments are as follows. 1- Whether the school meets the time varying eligibility criteria to apply for conversion (see body text for a discussion); 2-Total number of applications received by the DfE from LAs other than the ones used in the analysis in the month and year in which the academy has submitted its application for conversion (summary statistics of instrument: mean=117.6; std.dev.=54.4). First stage coefficients (standard errors) as follows. School meets eligibility criteria: 0.235 (0.090)**. Total number of applications: 0.0038 (0.0004)***.***: significant at the 1% level; **: significant at the 5% level; *: significant at the 10% level.

Table 3: Timing of academy approval and selected school and neighbourhood characteristics – Converters sample, balancing evidence

	(1)	(2)
	Cross-sectional	Within-school
Panel A: Time-varying shocks		
School:		
KS2 average attainment	1.668 (0.537)***	-0.061 (0.221)
KS4 average attainment	5.331 (1.643)***	-1.409 (0.911)
Share FSM pupils	-0.131 (0.056)**	0.014 (0.009)
Share of White British pupils	0.116 (0.117)	-0.054 (0.038)
Pupil/teacher ratio	0.735 (0.614)	0.420 (0.692)
School rated 'Outstanding'	0.416 (0.144)***	-0.052 (0.174)
Neighbourhood:		
KS2 average attainment	0.796 (0.310)**	0.060 (0.181)
KS4 average attainment	2.868 (1.086)**	-0.030 (0.584)
Share FSM pupils	-0.081 (0.038)**	0.001 (0.005)
Share of White British pupils	0.084 (0.094)	-0.001 (0.007)
Neighbourhood size (pupils)	-1.795 (1.166)	0.068 (0.099)
Neighbourhood turnover	-0.001 (0.003)	0.001 (0.005)
Panel B: Initial conditions and pre-trends 2007-2009		
School:		
Pupil-to-capacity ratio, 2009	0.028 (0.024)	--
Oversubscribed, 2009	0.166 (0.113)	--
KS2 average attainment, change 2007 to 2009	-0.352 (0.157)**	--
KS4 average attainment, change 2007 to 2009	-0.010 (0.603)	--
Share FSM pupils, change 2007 to 2009	0.000 (0.008)	--
Share of White British pupils, change 2007 to 2009	-0.014 (0.022)	--
Pupil/teacher ratio, change 2007 to 2009	0.145 (0.361)	--
Neighbourhood:		
KS2 average attainment, change 2007 to 2009	-0.113 (0.085)	--
KS4 average attainment, change 2007 to 2009	-0.335 (0.244)	--
Share FSM pupils, change 2007 to 2009	0.007 (0.004)*	--
Share of White British pupils, change 2007 to 2009	-0.010 (0.008)	--
Neighbourhood size (pupils) , change 2007 to 2009	-0.098 (0.069)	--
Neighbourhood turnover, change 2007 to 2009	-0.002 (0.003)	--

Note: The table reports coefficients and standard errors (clustered at the school level) of each of the school/neighbourhood characteristics listed in the first column on a dummy variable indicating whether a school has been approved as a converter academy. Each cell corresponds to a different regression. Neighbourhoods are time fixed and defined as postcodes that fall within the 75th percentiles of the school-specific home-to-school straight line distance for secondary school pupils (year 7 to year 11) attending the secondary school in the three years prior to our observation window (2007 to 2009). Neighbourhood characteristics are calculated using all pupils in primary and secondary education (reception to year 13) residing in these areas. Turnover measures the percentage of pupils in the neighbourhood who changes the postcode of residence across two adjacent years. Regressions at the school level and only include schools belonging to the converters sample (60 schools). Correlated time-varying shock regressions consider time-varying school and neighbourhood attributes measured in the year prior to the current observation. Initial condition and pre-trends use time fixed attributes either measured in 2009 or as the difference between 2009 and 2007. Sample includes observations for schools approved for conversion during our observation window and at the time when parents choose (i.e., before October of year t) and schools that approved for conversion after our sampling period – but excludes observations in immediately adjacent academic years (i.e., from November of year t to September of year t+1). Number of year-by-school observations: 131. ***: significant at the 1% level; **: significant at the 5% level; *: significant at the 10% level.

Table 4: The impact of conversion to academy on the demand for schools – Specification checks and 'truth telling'

	(1)	(2)	(3)	(4)
	Top pref. Conditional Logit	Rank Ordered Logit (ROLOGIT)	ROLOGIT Feasible Schools	ROLOGIT Feasible Schools No priority rules
Converter Academy	0.295 (0.062)***	0.091 (0.030)***	0.139 (0.052)***	0.120 (0.030)***
Log of pupil-school distance	-2.224 (0.015)***	-2.040 (0.007)***	-0.661 (0.016)***	-0.672 (0.018)***
Marginal willingness to travel	*13.2%*	*4.46%*	*21.0%*	*17.9%*

Note: The table reports coefficients and standard errors in parenthesis on a dummy for academy conversion and on the log of home-to-school distance. All regressions control for a variable identifying whether schools become 'sponsored' academies and for: *(i)* pupil and school effects; *(ii)* school time-varying controls, and *(iii)* school 'academy propensity' × year effects. The dependent variable in Columns (1) a binary outcome denoting the students' highest preference. The dependent variable in Columns (2) to (4) is the (inverse) rank of the students' school preferences (top preference coded as six; non-listed school coded as zeros). Column (3) and (4) only consider 'feasible schools' as in Fack et al. (2017). These are defined as schools with de-facto catchment areas encompassing students' postcode of residence. De-facto catchment areas include postcodes that fall within the 75th percentiles of the home-to-school distance measured for all pupils attending the schools in the years 2007 to 2009 (prior to our observation window). Median (average) home-to-school distance in the 'feasible school' set: 2.62km (3.31km). Median (average) number of 'feasible schools' per pupil: 8 (8.63). Column (4) further drops pupils that are potentially admitted to schools on the basis of one of the following priority rules: special education needs (SEN); in need or care of the local authority; siblings at the school. Number of observations as follows. Column (1): 631,299 in 60 schools (individuals with no variation in the dependent variable are dropped). Column (2): 1,488,688 in 60 schools. Column (3): 95,521 in 46 schools. Column (4): 66,108 in 46 schools. ***: significant at the 1% level; **: significant at the 5% level; *: significant at the 10% level.

Table 5: The impact of conversion to academy on the demand for schools–
Heterogeneity by free school meal eligibility (FSM) and ethnicity (White British)

	(1a)	(1b)	(2a)	(2b)	(3a)	(3b)	(3c)	(3d)
	Pupil is non-FSM	Pupil is FSM	Pupil is White British	Pupil is other ethnicity	Pupil is White British & Non-FSM	Pupil is White British & FSM	Pupil is other ethnicity & Non-FSM	Pupil is other ethnicity & FSM
Converter Academy	0.169 (0.064)**	0.059 (0.078)	0.251 (0.082)***	0.096 (0.088)	0.306 (0.094)***	0.126 (0.083)	0.121 (0.089)	0.054 (0.111)
Log of pupil-school distance	-0.053 (0.005)***	-0.052 (0.004)***	-0.048 (0.006)***	-0.058 (0.005)***	-0.050 (0.007)***	-0.039 (0.006)***	-0.056 (0.005)***	-0.061 (0.006)***
Implied highest preference effect	*17.3%*	*6.0%*	*25.7%*	*9.8%*	*31.3%*	*12.9%*	*12.4%*	*5.5%*
Marginal willingness to travel	*3.19%*	*1.13%*	*5.22%*	*1.65%*	*6.12%*	*3.23%*	*2.16%*	*0.88%*

Note: The table reports coefficients and standard errors in parenthesis (clustered at the school level) on a dummy for academy conversion (multiplied by 100), on the log of home-to-school distance and on the interaction between the two variables. All regressions control for a variable identifying whether schools become 'sponsored' academies and for: *(i)* pupil and school effects; *(ii)* school time-varying controls, and *(iii)* school 'academy propensity' × year effects. The dependent variable is a binary outcome denoting the students' highest preference. Regressions use schools in the converters sample only (i.e. schools that are already academies at the time when pupils choose and schools that will become academies in the future). All results come from split-sample regressions run separately for the groups of pupils reported in the column headings. Number of observations as follows. Columns (1a) and (1b): 974,218 and 474,470, respectively (in 60 schools). Columns (2a) and 21b): 584,019 and 864,669, respectively (in 60 schools). Columns (3a), (3b), (3c) and (3d): 426,669, 157,350, 547,549, and 317,120 respectively (in 60 schools). Implied academy effect obtained by rescaling the coefficient by the probability of a school being the highest preference. ***: significant at the 1% level; **: significant at the 5% level; *: significant at the 10% level.

Table 6: Accessibility, information and heterogeneous academies effects

	(1a)	(1b)	(2a)	(2b)	(3a)	(3b)
	Pupil is non-FSM	Pupil is FSM	Pupil is White British	Pupil is other ethnicity	Pupil is White British & Non-FSM	Pupil is Non-White British Or FSM
Panel A: Only pupils in the catchment area of at least one academy						
Converter	0.172	0.068	0.260	0.102	0.316	0.097
Academy	(0.066)**	(0.077)	(0.086)***	(0.087)	(0.100)***	(0.075)
Panel B: Controlling for school × distance effects						
Converter	0.159	0.038	0.212	0.090	0.261	0.092
Academy	(0.064)**	(0.071)	(0.081)**	(0.090)	(0.093)***	(0.075)
Panel C: Only Birmingham schools						
Converter	0.245	0.121	0.379	0.166	0.419	0.171
Academy	(0.105)**	(0.127)	(0.138)***	(0.141)	(0.163)**	(0.120)

Note: The table reports coefficients and standard errors in parenthesis (clustered at the school level) on a dummy for academy conversion (multiplied by 100). All regressions control for a variable identifying whether schools become 'sponsored' academies and for: *(i)* pupil and school effects; *(ii)* school time-varying controls, and *(iii)* school 'academy propensity' × year effects. The dependent variable is a binary outcome denoting the students' highest preference. Regressions use schools in the converters sample only (i.e. schools that are already academies at the time when pupils choose and schools that will become academies in the future). Columns (1a) and (1b) consider FSM eligible/non-eligible pupils. Columns (2a) and (2b) consider White British/Non-White British pupils. Columns (3a) and (3b) consider White British, non-FSM eligible pupils and those in the remaining groups. Panel A only considers pupils that fall in the *de facto* catchment area of at least one academy. De-facto catchment areas include postcodes that fall within the 75th percentiles of the home-to-school distance measured for all pupils attending the schools in the years 2007 to 2009 (prior to our observation window). Sample includes approximately 98% of the students. Panel B controls school-by-distance effects. Panel C only includes schools in Birmingham. For these schools all families were sent the same school directories (booklets) including information about academy conversions. Mean dependent variable for this subset: 0.0140. Number of observations as follows. Panel A - Columns (1a) and (1b): 949,294 and 465,250 (in 60 schools). Columns (2a) and (2b): 558,889 and 855,655 (in 60 schools). Columns (3a) and (3b): 407,076 and 1,022,019 (in 60 schools). Panel B - Columns (1a) and (1b): 974,218 and 474,470 (in 60 schools). Columns (2a) and (2b): 584,019 and 864,669 (in 60 schools). Columns (3a) and (3b): 426,669 and 1,007,468 (in 60 schools). Panel C- Columns (1a) and (1b): 541,221 and 263,708 (in 34 schools). Columns (2a) and (2b): 324,498 and 480,431 (in 34 schools). Columns (3a) and (3b): 237,084 and 567,845 (in 34 schools). ***: significant at the 1% level; **: significant at the 5% level; *: significant at the 10% level.

Table 7: The mechanisms behind the impact of conversion to academy – School expected composition and effectiveness

	(1)	(2)	(3)	(4)
	All pupils	Pupil is non-FSM	Pupil is White British	Pupil is White British & non-FSM
Panel A: Controlling for expected composition (intake) changes				
Converter	0.120	0.149	0.223	0.264
Academy	(0.059)**	(0.057)**	(0.074)***	(0.089)***
Panel B: Controlling for expected changes in effectiveness (KS2 to KS4 value-added – 'legacy' enrolment)				
Converter	0.131	0.169	0.249	0.301
Academy	(0.061)**	(0.064)***	(0.083)***	(0.097)***
Panel C: Controlling for expected changes in effectiveness (KS2 to KS4 value-added – endogenous KS4 enrolment)				
Converter	0.130	0.168	0.247	0.299
Academy	(0.061)**	(0.064)***	(0.084)***	(0.099)***

Note: The table reports coefficients and standard errors in parenthesis (clustered at the school level) on a dummy for academy conversion (multiplied by 100). All regressions control for a variable identifying whether schools become 'sponsored' academies and for: *(i)* pupil and school effects; *(ii)* school time-varying controls, and *(iii)* school 'academy propensity' × year effects. The dependent variable is a binary outcome denoting the students' highest preference. Regressions use schools in the converters sample only (i.e. schools that are already academies at the time when pupils choose and schools that will become academies in the future). Column (1) considers all pupils. Column (2) considers non-FSM eligible pupils only. Column (3) considers White British, pupils only. Column (4) considers White British, non-FSM eligible pupils only. Panel A controls for expected changes in intake composition by including among the controls the percentage of FSM eligible pupils, the percentage of White British pupils, the percentage of female students and the average KS2 (age 11) attainments of pupils at the beginning of secondary school in year 7 at the time when pupils will enrol (i.e. one year after the period under analysis when they express their preferences). Panel B controls for a proxy for the expected changes in school effectiveness following academy conversion. This refers to the school-specific extra KS4 attainments generated by the school following conversion. School effectiveness estimated comparing performance of actual converters to the performance of future converters using only 'legacy pupils' enrolled in schools prior to conversion. Panel C controls for an alternative proxy for the expected changes in school effectiveness following academy conversion which includes potential 'match specific' gains. This is estimated using pupils enrolled in the school at KS4 instead of 'legacy' pupils who were enrolled in the school at year 7. See Online Appendix B and Online Appendix Figures 2 and 3 for more details. Number of observations as follows. Column (1): 1,448,688 in 60 schools. Column (2): 974,218 in 60 schools. Column (3): 584,019 in 60 schools. Column (4): 426,669 in 60 schools. ***: significant at the 1% level; **: significant at the 5% level; *: significant at the 10% level.

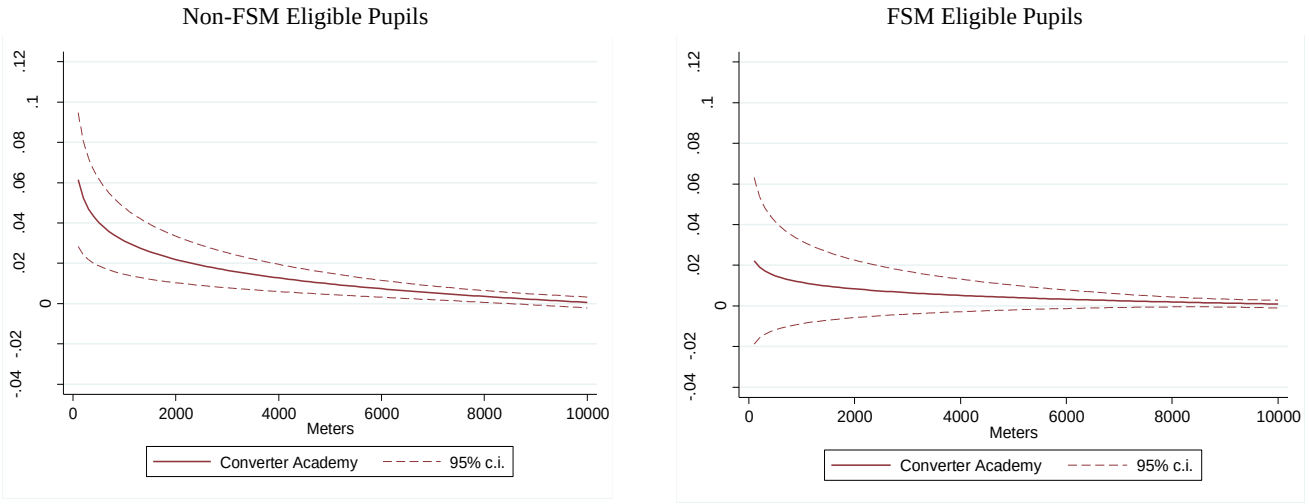
Table 8: The mechanisms behind the impact of conversion to academy – Pre-conversion school quality and popularity

	(1)	(2)	(3)	(4)
	All pupils	Pupil is non-FSM	Pupil is White British	Pupil is White British & non-FSM
Panel A: % 5 A-C KS4/GCSEs (average 2007-2009)*				
Converter Academy × School has above-median % 5 A*-C KS4/GCSEs	0.136 (0.067)**	0.187 (0.072)**	0.335 (0.098)***	0.420 (0.114)***
Converter Academy × School has below-median % 5 A*-C KS4/GCSEs	0.124 (0.073)*	0.131 (0.077)*	0.073 (0.089)	0.065 (0.103)
P-value - significance of difference	*0.8588*	*0.4901*	*0.0243*	*0.0143*
Panel B: School overcapacity (2007-2009)				
Converter Academy × School is overcapacity	0.197 (0.086)**	0.282 (0.089)***	0.487 (0.137)***	0.620 (0.159)***
Converter Academy × School is not overcapacity	0.106 (0.063)*	0.124 (0.069)*	0.158 (0.082)*	0.181 (0.100)*
P-value - significance of difference	*0.2146*	*0.0579*	*0.0188*	*0.0125*
Panel C: OFSTED rating (latest available up to 2009)				
Converter Academy × School is OFSTED 'Outstanding'	0.133 (0.116)	0.190 (0.126)	0.279 (0.192)	0.337 (0.218)
Converter Academy × School is not OFSTED 'Outstanding'	0.132 (0.061)**	0.165 (0.066)**	0.246 (0.080)***	0.300 (0.097)***
P-value - significance of difference	*0.990*	*0.838*	*0.860*	*0.868*

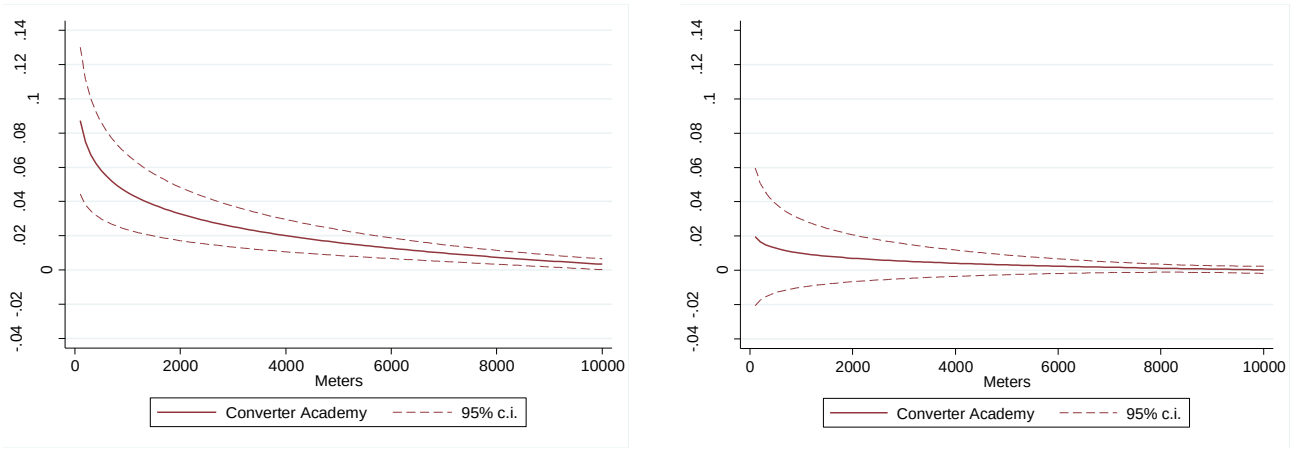
Note: The table reports coefficients and standard errors in parenthesis (clustered at the school level) on a dummy for academy conversion and its interaction with a proxy for 'salience' (multiplied by 100). All regressions control for a variable identifying whether schools become 'sponsored' academies and for: *(i)* pupil and school effects; *(ii)* school time-varying controls, and *(iii)* school 'academy propensity' × year effects. The dependent variable is a binary outcome denoting the students' highest preference. Regressions use schools in the converters sample only (i.e. schools that are already academies at the time when pupils choose and schools that will become academies in the future). Column (1) considers all pupils. Column (2) considers non-FSM eligible pupils only. Column (3) considers White British pupils only. Column (4) considers White British, non-FSM eligible pupils only. The various panels create converter academy interactions with 'salience' proxies using the variables described in the panel headings. % of 5 A*-C GCSEs obtained from school performance tables; median value: 0.44. Number of observations as follows. Column (1): 1,448,688 (in 60 schools). Column (2) 974,218 (in 60 schools). Column (3): 584,019 (in 60 schools). Column (4): 426,669 (in 60 schools). ***: significant at the 1% level; **: significant at the 5% level; *: significant at the 10% level.

Figure 1: Variation in the effect of of academy conversion over distance

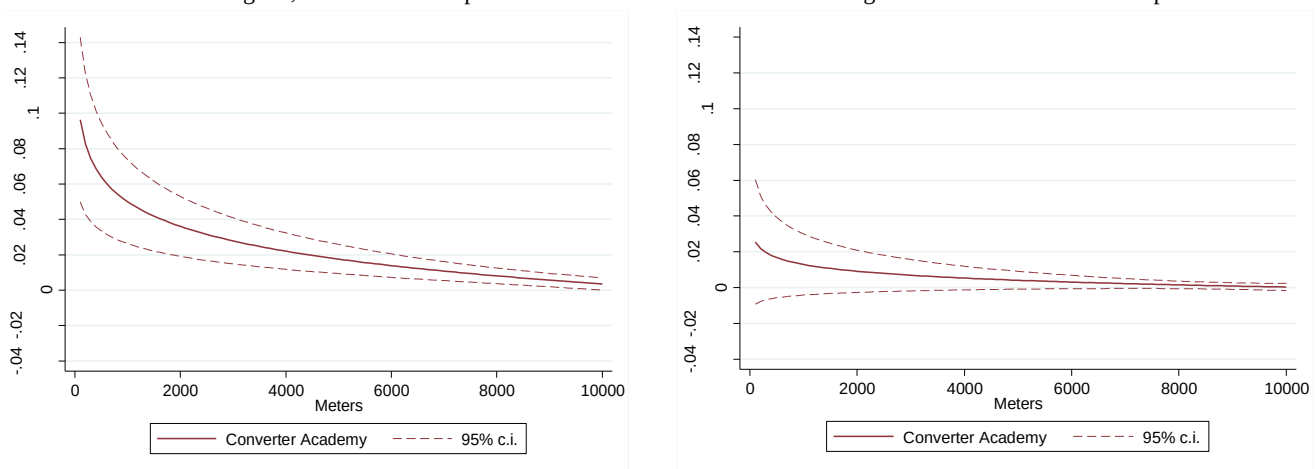
Panel A: by pupil FSM eligibility



Panel B: by pupil ethnic background



Panel C: by pupil ethnic background and FSM eligibility



Note: The figures show the effect of converter academies at varying pupil-to-school distances for pupils with the displayed background characteristics. Figures obtained from specification that: use the converters sample; including all controls and school 'academy propensity'× year effects; add an interaction between school academy conversion and the log of pupil-to-home straight line distance; and consider only the displayed sub-set of pupils. Estimation sample only includes non-movers. 95% confidence intervals come from standard errors clustered at the school level.

Appendix Tables and Figures

Appendix Table 1: Additional descriptive statistics

	School types and preferences by October 2009 (for aayy 2010/2009)	School types and preferences by October 2010 (for aayy 2011/2012)	School types and preferences by October 2011 (for aayy 2012/2013)
Panel A: Prevalence of academies			
Converter academies	0	3 (2.4%)	41 (32.8%)
Other schools	125	122 (97.6%)	84 (67.2%)
Panel B: Pupil preferences: % of students choosing first			
All pupils:			
Converter academies	0	1.68%	40.84%
Other	100%	98.32%	59.16%
Non-FSM eligible students:			
Converter academy	0	1.55%	46.72%
Other	100%	98.45%	53.65%
FSM eligible students:			
Converter academy	0	1.94%	29.01%
Other	100%	98.09%	71.18%
White British students:			
Converter academy	0	1.62%	42.78%
Other	100%	98.38%	57.22 %
Non-White British students:			
Converter academy	0	1.72%	39.65%
Other	100%	98.28%	60.35%

Note: see notes to Table 1a. "aayy" refers to academic year.

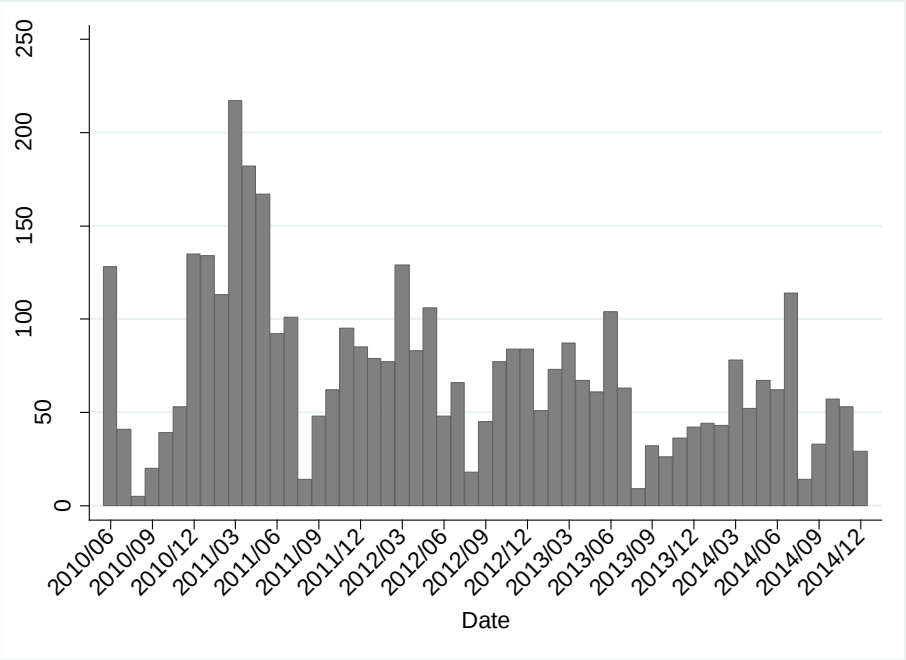
Appendix Table 2: Balancing of instruments – Converters sample

	(1)	(2)
Panel A: Instrument is changes in eligibility criteria		
KS2 average attainment – school	1.495 (0.560)***	-0.077 (0.199)
Share FSM pupils – school	-0.071 (0.049)	-0.002 (0.009)
Share White British pupils – school	0.063(0.089)	0.024 (0.034)
Pupil/teacher ratio – school	-0.108 (0.503)	0.079(0.434)
KS2 average attainment – neighbourhood	0.554 (0.339)	-0.051 (0.129)
Share FSM pupils – neighbourhood	-0.028 (0.039)	-0.004 (0.004)
Share White British pupils – neighbourhood	-0.001 (0.075)	0.001 (0.005)
Neighbourhood size (pupils)	-0.819 (0.833)	0.113 (0.086)
Neighbourhood turnover	0.002 (0.003)	-0.002 (0.004)
Panel B: Instrument is total number of applications		
KS2 average attainment – school	-0.003 (0.007)	--
Share FSM pupils – school	-0.000 (0.001)	--
Share White British pupils – school	0.001 (0.001)	
Pupil/teacher ratio – school	-0.000 (0.005)	--
KS2 average attainment – neighbourhood	0.005 (0.003)	--
Share FSM pupils – neighbourhood	-0.001 (0.001)	--
Share White British pupils – neighbourhood	0.001 (0.001)	
Neighbourhood size (pupils)	-0.011 (0.010)	--
Neighbourhood turnover	-0.000 (0.001)	--
Year effects	Yes	Yes
School effects	No	Yes

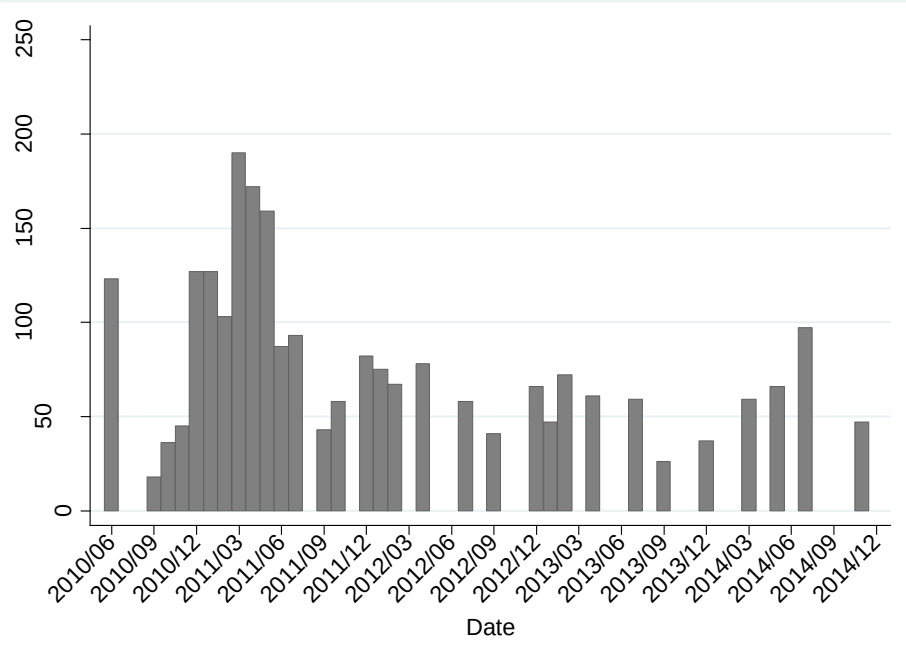
Note: The table reports coefficients and standard errors (clustered at the school level) of one of the school/neighbourhood characteristics listed in the first column on a dummy variable indicating whether a school has been approved as a converter academy. Each cell corresponds to a different regression. Neighbourhoods are time fixed and defined as postcodes that fall within the 75th percentiles of the school-specific home-to-school straight line distance for secondary school pupils (year 7 to year 11) attending the secondary school in the three years prior to our observation window (2007 to 2009). Neighbourhood characteristics are time varying and calculated using all pupils in primary and secondary education (reception to year 13) residing in these neighbourhoods in the year prior to the current observation. Turnover measures the percentage of pupils in the neighbourhood who changes the postcode of residence across two adjacent years. Regressions at the school level. Number of year-by-school observations: 131. ***: significant at the 1% level; **: significant at the 5% level; *: significant at the 10% level.

Appendix Figure 1: Number of application for converter academies received by the Department for Education

Panel A: All schools in England; all months



Panel B: Schools outside the LAs under anlysis; selected months



Note: The figure presents number of applications for converter academies received by the Department for Education by month between 2010/06 and 2014/12. The left-hand side figure uses data for all schools in the whole of England. The right-hand side figure uses number of applications for converter academies received by the Department for Education for dates in which converter academies in our sample have applied to the Department for Education (DfE). Numbers only include applications from outside the LAs considered in our analysis. This is the instrument used in Column (7), Table 2. Figures based on the authors' manipulation of information collected by the Department for Education (DfE).

On-Line Appendix A: Robustness checks

In this section, we discuss a number of checks and extensions on our main results. These are presented in On-Line Appendix Table 1. To start with, in Column (1) we restrict pupils' choice set to only include schools within 2.5km from a pupil's residence (a number that varies by pupil; approximately 5.9 each, with a standard deviation of 1.9). In this case there is a much bigger semi-elasticity – approximately 33% – implying that the relative increase in a converting school's probability of being chosen first within a local choice set is substantially larger than the corresponding increase compared to the overall LA choice set. We discuss these issues in Section 6.1, where we study the interaction between home-school distance and academisation.

We next check that our results are not affected by families moving in the years preceding secondary school in order to gain admission to over-subscribed academies which rank admissions by home-to-school distance. To control for this possibility, we drop from our analysis all students that move their residential address between grade 3 (right after KS1, and at the beginning of the second primary school stage) and grade 7 (right after the beginning of secondary school). As shown in Column (2), dropping these potentially strategic movers does not affect our conclusions.

The next two columns of On-Line Appendix Table 1 investigate whether other institutional features of the English admission system affect our estimates. In Column (3) we drop pupils with special education needs (SEN), as these are prioritised for admissions to their preferred schools. This exclusion does not affect our findings. In Column (4), we focus on rules that prioritise children with siblings at the school for admission and consider the possibility that parents of these pupils might act as a 'driving force' (e.g., by leading consultations) for a school's decision to convert to academy. This dynamic would generate a sort of 'reverse causal' link between parental preferences and conversion, confounding the interpretation of our results. To deal with this possibility, we exclude from our analysis all pupils who are admitted to schools on the basis of the sibling criterion. When we apply this restriction, we still find a positive effect of conversion on parental preference, with an implied effect of approximately 14%.

Finally, Column (5) adds more school controls to the specification, namely: the share of SEN pupils; the ratio of pupils to SEN-support teachers; and the percentage of school sessions missed because of absences. This extension does not affect our results. We further experimented with replacing the control for average end-of-secondary (KS4) school attainments with a measure of average primary-to-secondary (KS2-to-KS4) value-added. This also did not substantially affect our results.

On-Line Appendix B: Estimating Academies' Effect on Pupil Value-Added

In this section, we discuss two complementary approaches we use to estimate the impact of academy conversion on pupil KS2-to-KS4 value added.

First, we follow the method used in Eyles et al. (2017) and compare the KS4 attainments of pupils that already enrolled for secondary education (grade 7) in converter academies prior to their conversion – i.e., 'legacy enrolled' students – to the attainments of pupils enrolled in schools that will become academies after our observation window – i.e., students in 'future converter' control schools. This approach is essentially a

difference-in-difference (D-i-D) method that exploits differences in grade 11 (age 16) attainments among the following three groups of students: *(i)* pupils that start secondary education (grade 7) in schools that convert to academies within our observation window, but are not exposed to academy teaching – i.e., they complete secondary education before the school converts; *(ii)* pupils that start secondary school in converter academies prior to conversion and are potentially exposed to one, two, three or four years of academy education; and *(iii)* children that start secondary education in schools that will convert to academy after the end of our observation window and so are not exposed to any academy year.

We fix attendance to the school in which students are enrolled grade 7 and prior to school conversion to by-pass the endogeneity of choice/mobility in relation to academisation. By concentrating on where pupils start secondary education, we measure an 'intention-to-treat' (ITT) exposure to academy education. However, we can also follow pupils through their subsequent moves and identify actual academy exposure. We then use ITT exposure to instrument for actual exposure and estimate Instrumental Variable (IV) D-i-D models to identify the causal effect of academy attendance on end-of-secondary school KS4 attainments

In our second approach, we consider the school in which pupils sit for their KS4 examination and compare students that are 'endogenously' enrolled in an academy in their grade 11 to pupils who are enrolled in a school that is not an academy at that point in time – but will convert after the end of our observation period. Endogeneity arises from the fact that individuals might: *(i)* choose to stay in the school where they started secondary education (in grade 7) after it becomes an academy and take their KS4 (grade 11) exams at that school; or *(ii)* choose to move away/move to an academy to sit for their KS4 examination. It should be noted that the time-series of the available data does not allow us to consider the KS4 attainments of pupils who chose to start secondary education in a school that was already an academy when they applied for it.

The first approach described above should yield an unbiased causal estimate of the impact of academy conversion on a randomly picked student. Conversely, the second method yields a potentially biased estimate containing a 'match-specific gain' component – which might however be relevant for parental preferences.

To estimate these models, we use KS4 attainment data on all pupils in England (not just our eight LAs) for the academic years 2005/2006 to 2013/2014. As in Eyles et al. (2017), we identify converter academies as 'operative' for the academic year *t/t+1* if they open by December of year *t* so that they have two full terms of academy teaching before impacting students' KS4 exams in May of *t+1* (still part of the same academic year t/*t+1*).[1] To clarify, consider the following example: a student who starts secondary school in September of 2007 – in the academic year 2007/2008 – who takes his/her KS4 tests in May 2012 – in the academic year 2011/2012 – and whose school converts to academy in December 2010 – i.e., during the academic year 2010/2011. If the student does not change school, he/she will have ITT and actual academic exposures equal to two years. If instead the student changes school in September 2011, he/she will be assigned to two years of ITT exposure, although actual exposure will only be one year. Note that approximately 95% of the pupils we observe in converter academies do not change school.

[1] Academies opened by December of year *t* are essentially all approved by October of year *t*, as in the timeline in our main analysis. Academies that open after December of year *t* – predominantly in April/May of year *t+1* – are deemed 'too late' to influence their students' attainment in that period and assigned to students' KS4 tests for the academic year *t+1/t+2*. Note that we experimented with alternative timelines and found that the results are fairly robust.

Throughout the analysis, we use standardized KS4 test scores as our dependent variable. All regressions control for student demographics (FSM eligibility, SEN status, gender, English as first language and White British ethnicity) and end-of-primary KS2 attainments. Because of this, all specifications are 'lagged dependent variable' models measuring academies' impact on students' test score progression. More restrictive value-added models yielded similar findings. Throughout our analysis, we cluster standard errors by school.

Besides estimating academies' average effectiveness, we investigate heterogeneous impacts by pupil sub-groups – e.g., FSM vs. non-FSM eligible pupils – and across schools. These models are estimated by pooling all pupil observations but interacting all controls included in the specification (including year effects) with the relevant sub-group indicators. In essence, these models only restrict school fixed-effects to be the same for the various strata. Split-sample models yield almost identical estimates. As we found no evidence of significant heterogeneity across pupils with different background, these results are not tabulated or used in our analysis.

We also estimate models (pooled across all pupil types) to recover standardized school-specific policy-on academy effects. These are obtained by interacting each school identifier with a dummy capturing whether the school is open as an academy in time to influence KS4 attainments of that academic year (irrespective of how many academic years the academy has been open for).

On-Line Appendix Figure 1 graphically presents our estimates of the effectiveness of converter academies obtained using the 'legacy enrolment' approach. These are estimated separately for schools rated 'outstanding', 'good' and 'satisfactory/inadequate' at the latest inspection prior to 2010. We follow this approach since different schools had different eligibility criteria to convert to academy depending on their inspectorate rating (see Section 2.4). The top left-hand side plot displays the average impact of converting to academy up to four years after conversion (for 'outstanding' schools; up to three for the other two groups as not enough schools with these ratings convert early enough to impact KS4 attainments in the academic year 2013/2014 – the last year we use in our analysis) and up to five years before conversion – with the omitted group being the year just before academisation. The other diagrams instead present histograms displaying standardized KS4 school-specific policy-on academy effects.

Our findings show that 'outstanding' converters display no significant pre-policy effects (i.e., the standard D-i-D parallel-trend assumption is likely to hold) and a significant positive impact on pupils' KS4 attainments in the first three years after opening – before dropping somewhat four years after conversion and becoming insignificant. This dip is most likely explained by the fact that very few schools converted by December 2010 in time to have four full years of impact by the end of the academic year 2013/2014 – and should be somewhat discounted. However, we find a much less neat picture for the other two group types. To begin with, there is some evidence of pre-trends in KS4 in the years leading up to conversion – in particular for good schools – which complicates causal inference. Furthermore, we find little evidence to suggest that converter academies in these two groups improve the attainments of their students following conversion.

The other panels show that average performance measures hide substantial heterogeneity in effectiveness across schools. The average policy-on impact for outstanding schools is 3% of a standard deviation with a standard deviation of 10.6%. On the other hand, the average policy-on impact is 0.4% for 'good' schools and

negative 3.7% for schools in the residual group. Both estimates display substantial amounts of variation with 12.7% and 13.9% standard deviations, respectively.

Finally, On-Line Appendix Figure 3 presents scatter plots of the school-specific estimates we obtain using the 'legacy enrolment' approach against those obtained using the potentially endogenous school where students sit for their KS4 exam. The left-hand side panel presents estimates for all schools in England – once again broken down by OFSTED ratings; the right-hand side panel instead presents estimates for the schools we consider in our sample. As clear from the diagrams, the correlation between the two set of estimates is very high and always above 0.85.

On-Line Appendix Table 1: The impact of conversion to academy on the demand for schools – Additional robustness checks

	(1)	(2)	(3)	(4)	(5)
	Keeps schools within 2.5km from home	Non-movers only	No SEN pupils	No pupils with pref's for sibling school	Extra school controls
Converter Academy	4.579 (1.137)**	0.147 (0.073)**	0.141 (0.062)**	0.142 (0.068)**	0.118 (0.055)**
Log of pupil-school distance	-0.225 (0.0190)***	-0.054 (0.004)***	-0.053 (0.004)***	-0.050 (0.004)***	-0.053 (0.004)***
Mean outcome	*0.1401*	*0.0098*	*0.0098*	*0.0101*	*0.0098*
Implied highest preference effect	*32.7%*	*15.0%*	*14.4%*	*14.1%*	*12.0%*
Implied willingness to travel	*20.3%*	*2.72%*	*2.66%*	*2.84%*	*2.23%*

Note: The table reports coefficients and standard errors in parenthesis (clustered at the school level) on a dummy for academy conversion (multiplied by 100) and on the log of home-to-school distance. All regressions control for a variable identifying whether schools become 'sponsored' academies and for: *(i)* pupil and school effects; *(ii)* school time-varying controls, and *(iii)* school 'academy propensity' × year effects. The dependent variable is a binary outcome denoting the students' highest preference. Column (1) only considers schools within 2.5km of the home address. Column (2) only considers pupils who do not change their postcode of residence between grade 3 (right after their Key Stage 1 test in the third year of primary education) and grade 7 (right after they have entered secondary education). Column (3) drops pupils with Special Education Needs (SEN) with statements. Column (4) drops pupils who express preferences for schools where they have priority admissions because of 'sibling rules'. Additional school controls in Column (5) include: school share of pupils with SEN; ratio of pupil to SEN-support teachers; school percentage of sessions missed because of absences (authorized and unauthorized). Column (1): 51,174 in 42 schools. Column (2): 916,030 in 60 schools. Column (3) 1,427,329 in 60 schools. Column (4): 1,037,258 in 60 schools. Column (5): 1,448,688 in 60 schools. Implied academy effect obtained by rescaling the coefficient by the probability of a school being the highest preference. ***: significant at the 1% level; **: significant at the 5% level; *: significant at the 10% level.

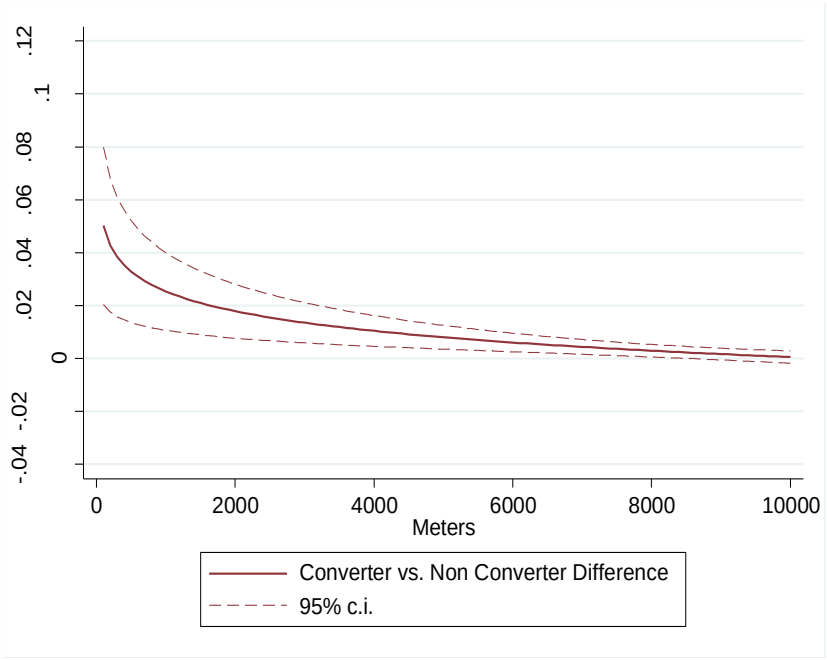
On-Line Appendix Table 2: Conversion to academy, school offers and school attended

	(1a)	(1b)	(2a)	(2b)	(3a)	(3b)
	Pupil is non-FSM	Pupil is FSM	Pupil is White British	Pupil is other ethnicity	Pupil is White British & Non-FSM	Pupil is Non-White British Or FSM
Panel A: School offer						
Converter Academy	0.059 (0.033)*	0.019 (0.060)	0.189 (0.056)***	-0.009 (0.046)	0.202 (0.067)***	0.012 (0.034)
Implied highest preference effect	*7.11%*	*2.29%*	*22.77%*	*-1.08%*	*24.3%*	*1.45%*
Panel A: School attended						
Converter Academy	0.050 (0.036)	0.012 (0.055)	0.170 (0.057)***	-0.011 (0.043)	0.188 (0.073)**	0.006 (0.031)
Implied highest preference effect	*5.95%*	*1.43%*	*20.2%*	*1.31%*	*22.4%*	*0.71%*

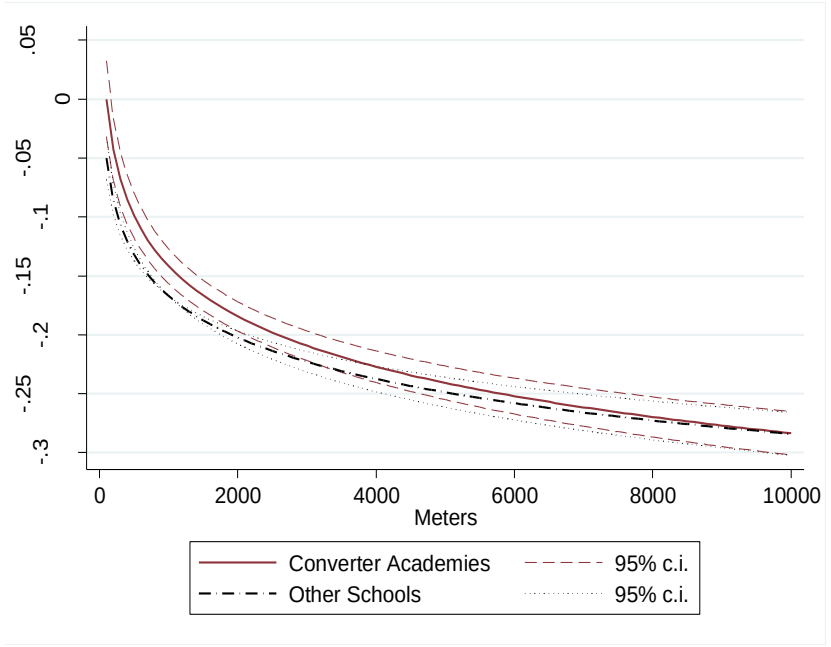
Note: The table reports coefficients and standard errors (clustered at the school level) in parenthesis (multiplied by 100). Coefficients and standard errors multiplied by 100. All regressions control for a variable identifying whether schools become 'sponsored' academies and for: *(i)* pupil and school effects; *(ii)* school time-varying controls, and *(iii)* school 'academy propensity' × year effects. The dependent variable in the top panel is a binary outcome denoting whether the student is offered a place at the school. The dependent variable in the bottom panel is a binary outcome denoting whether the student attends the school. Regressions use schools in the converters sample only (i.e. schools that are already academies at the time when pupils choose and schools that will become academies in the future). All results come from split-sample regressions run separately for the groups of pupils reported in the column headings. Implied academy effect obtained by rescaling the coefficient by the probability of a school receiving an offer or attending a school in converters sample (respectively 0.0083 and 0.0084). Number of pupils as follows (always in 60 schools). Top panel: 963,116 and 466,902 (Columns 1a and 1b); 581,412 and 848,606 (Columns 2a and 2b); 425,183 and 1,004,835 (Columns 3a and 3b). Bottom panel: 942,912 and 464,883 (Columns 1a and 1b); 577,854 and 829,941 (Columns 2a and 2b); 420,890 and 986,905 (Columns 3a and 3b). ***: significant at the 1% level; **: significant at the 5% level; *: significant at the 10% level.

On-Line Appendix Figure 1: The impact of distance for converter academies and other schools

Panel A: Implied academy-distance interaction

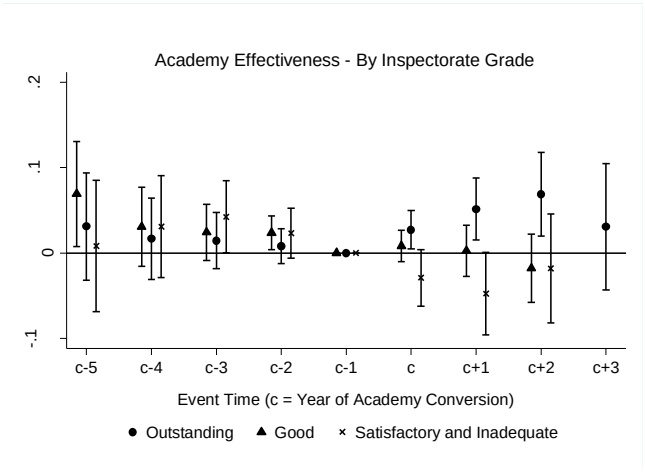


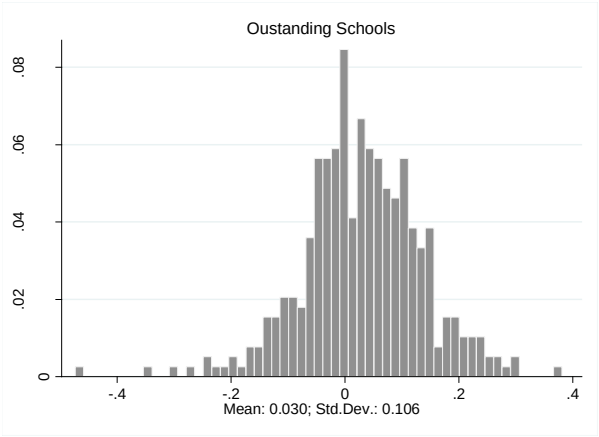
Panel B: Impact of distance by school type effect

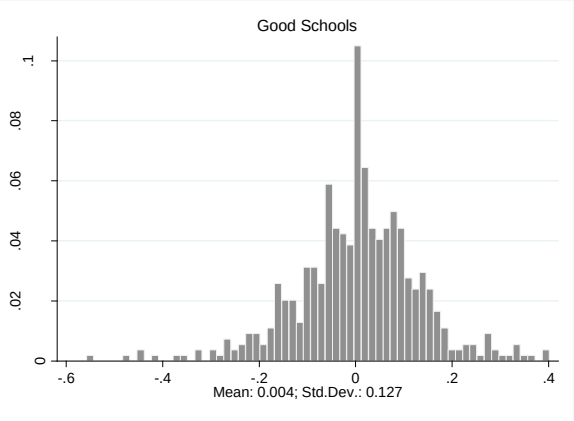


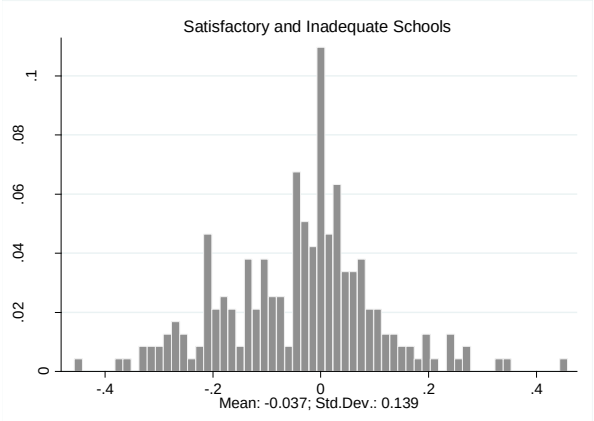
Note: The right hand side figure (Panel A) shows the effect of distance from schools on the probability that a school is top ranked for converters and non-converters. The left hand side figure (Panel B) shows the implied (interaction) effect of converter academies at varying pupil-to-school distances. Figures obtained from specification that: use the converters sample; including all controls and school 'academy propensity'× year effects; and add an interaction between school academy conversion and the log of pupil-to-home straight line distance. Estimation sample only includes non-movers. 95% confidence intervals come from standard errors clustered at the school level. Median distance for schools chosen (ranked) by students: approximately 2.5km. Median distance for highest preference school: 1.7km.

On-Line Appendix Figure 2: Academy effectiveness and variation in ITT policy-on effects – by OFSTED inspection grade



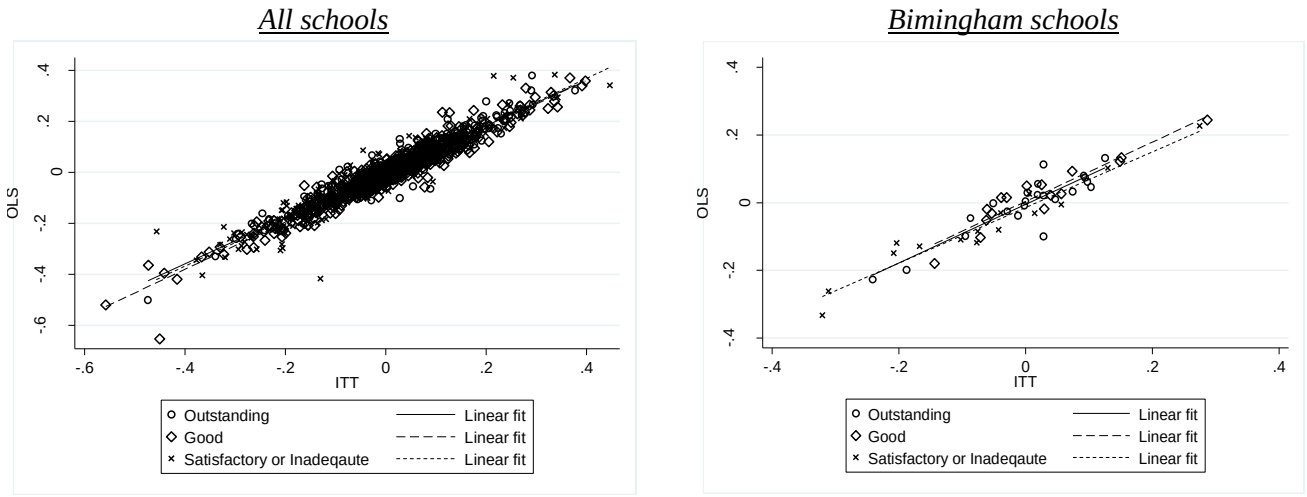






Note: Plots present estimates of the ITT impact of academy conversion on KS4 attainments. Regressions run at the pupil level with standard errors clustered at the school level. Regressions consider pupil KS4 outcomes in the academic year 2005/2006 to 2013/2014. School are considered operating as academies if they open before December of year *t* and impact on KS4 outcomes in May of year *t+1* (e.g. open by December 2011 and impact on KS4 in May 2012 – both dates referring to the academic year 2011/2012). Sample only includes: *(i)* converter academies open between September 2010 and December 2013 (treated) and converter academies open between January 2014 and March 2016 (controls); *(ii)* pupils enrolled in these schools before academy conversion (legacy enrolment). Number of pupils and schools as follows. Outstanding schools: 564,340 pupils in 395 (380 treated and 15 control) schools. Good schools: 803,039 pupils in 566 (516 treated and 50 control) schools. Satisfactory and inadequate schools: 346,684 pupils in 259 (220 treated and 39 control) schools. Top, left-hand side plot presents results for academy impact at time of conversion and up to four years after (c to c+3); and prior to conversion (c-2 to c-5). Omitted group: c-1 (year prior to conversion). There are no good schools/too few satisfactory and inadequate schools converting in the first year (up to December 2010) to present estimates for c+3 for these groups. The other plots present school-specific estimates of academy effectiveness. These are obtained from a school-specific 'policy-on' dummy indicating whether the school was open as academy at that time.

On-Line Appendix Figure 3: Academy effectiveness –
Correlation between estimates obtained using 'legacy' enrolment (ITT) and endogenous KS4 school (OLS)



Note: Plots present school-specific estimates of the impact of academy conversion on KS4 attainments. Regressions run at the pupil level with standard errors clustered at the school level. Regressions consider pupil KS4 outcomes in the academic year 2005/2006 to 2013/2014. School are considered operating as academies if they open before December of year *t* and impact on KS4 outcomes in May of year *t+1* (e.g. open by December 2011 and impact on KS4 in May 2012 – both dates referring to the academic year 2011/2012). Sample only includes: *(i)* converter academies open between September 2010 and December 2013 (treated) and converter academies open between January 2014 and March 2016 (controls); *(ii)* pupils enrolled in these schools before academy conversion (legacy enrolment). Number of pupils and schools as follows. Outstanding schools: 564,340 pupils in 395 (380 treated and 15 control) schools. Good schools: 803,039 pupils in 566 (516 treated and 50 control) schools. Satisfactory and inadequate schools: 346,684 pupils in 259 (220 treated and 39 control) schools. ITT school effectiveness is estimated comparing the performance of actual converters to the performance of future converters using only 'legacy pupils' enrolled in schools prior to conversion. OLS school effectiveness is estimated using pupils enrolled in the school at KS4 (instead of 'legacy' pupils). This approach uses the endogenous location of students at the time when they are sitting for their exams and following conversion (and captures potential 'match specific' gains). See Appendix A for more details. Left-hand side plot presents results for all schools in England. Right-hand side plot presents results for Birmingham schools only.