

School Choice During a Period of Radical School Reform. Evidence from Academy Conversion in England.

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Abstract:

We study how demand responds to the rebranding of existing state schools as autonomous ‘academies’ in the context of a radical and large-scale reform to the English education system. The academy programme encouraged schools to opt out of local state control and funding, but provided parents and students with limited information on the expected benefits. We use administrative data on school applications for three cohorts of students to estimate whether this rebranding changes schools’ relative popularity. We find that families – particularly higher-income, White British – are more likely to rank converted schools above non-converted schools on their applications. We also find that it is mainly schools that are high-performing, popular and proximate to families’ homes that attract extra demand after conversion. Overall, the patterns we document suggest that families read academy conversion as a signal of future quality gains – although this signal is in part misleading as we find limited evidence that conversion causes improved performance.

Keywords: school reform; choice and autonomy; preference formation; consumer behaviour.

JEL Classification: I21; H75; D12.

1. Introduction

Many countries have enacted reforms that give state-funded schools greater freedom over management, budgets, personnel, curriculum and teaching. In the US, policy changes in this direction started in the 1990s and led to the creation of Charter schools. Similar reforms have occurred in Europe – for example the birth of ‘Friskolor’ in Sweden in the 1990s. These reforms are predicated on the notion that autonomy can improve education quality: autonomous schools can differentiate their educational offer and raise standards through better matching of provision to student needs; autonomous schools also have more scope for changing management, teaching and recruitment practices in ways that promote student achievement.

Generally, these reforms are embedded in quasi-market mechanisms in the process of school choice – with new types of autonomous school providing parents with an alternative to more traditional schools under closer state control. The principle at the core of such systems is that allowing choice and closely linking finance to demand creates incentives for schools to target their offer to their market and improve standards.¹ However, the success of this kind of policy rests on the idea that families actively exercise choice and are drawn to autonomous schools, even when their benefits (and costs) are *ex-ante* largely unknown. In other words, there must be demand from parents for schools packaged in such ‘autonomous’ wrappers – even when the contents of these bundles in terms of education outcomes are not yet clear.

This problem is in essence similar to the one faced by consumers (and firms) in any product market when new brands are introduced or existing products re-designed – and their quality is uncertain. Our key contribution is to study these issues in an unusual setting – the English education market and a far reaching policy shock that involved the conversion of a large share of state schools to autonomous ‘academy’ status. We examine parental demand for this academy brand, and the uncertain changes it implied, using variation in preference rankings for schools in the choice process. Although previous studies have investigated parental preferences for school attributes, we are the first to focus on demand for autonomy – or more precisely, the rebranding of schools as autonomous. We are also the first to identify any such effects by exploiting multiple years of choice and by focussing on the impact of a large supply-side policy shock – while holding other school and market level unobservables fixed.

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. A small number of ‘sponsored’ academies were introduced in the early 2000s by the Labour government as a remedial intervention

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 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-markets in education.

aimed at failing schools, forcing organizational change under the guidance of a private or charitable sponsor. However, from 2010, the Conservative/Liberal Democrat Coalition Government dramatically expanded the academy programme and changed its nature. Under the new policy, existing schools – provided that they met high academic standards – were encouraged 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 occurred rapidly: the new Government was installed May 2010 and the Academies Act 2010 ushering these changes passed in June 2010. Over 1100 secondary schools – 35% of the stock – converted to academy in the first 3 years of the programme – with a further 200 by 2015. Since then, subsequent governments reined in an ambition to ‘make all schools academies’ (an aspiration briefly held in 2016) and the proportion rose only slightly – before becoming stable at below 70%.

Despite the pervasiveness of these ‘converter’ academies, little information or hard evidence was available to parents at the time on the likely structural changes associated with academy status and their potential benefits. From a choice perspective, the only explicit short-run difference was a change in the school name – i.e., a ‘rebranding’. Of course, the label ‘academy’ carries connotations of scholarship and initially only schools judged outstanding in school inspections were allowed to convert – reinforcing the association between the academy brand and quality. Moreover, the Government explicitly promoted the notion that academies would provide excellent education by leveraging their freedom from local government control, and pushed the idea that the new wave of ‘converters’ would be as successful as earlier rounds of ‘sponsored’ academies – notwithstanding the intrinsic differences of the two programmes.² Our study investigates the effect of this ‘rebranding’ of existing state schools as autonomous academies on families’ choice of school.³

In order to carry out our study, we merge 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).

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. From an identification point of view, we treat the introduction of academies as a supply-side shock that allows us to estimate parents’ latent demand for autonomous schools. We do not structurally model the entry of converter

² A growing body of evidence exists on the characteristics and the effects of the early pre-2010 academies. See for example Eyles and Machin (2018) and Eyles et al. (2016). Conversely, limited research has been conducted on the post-2010 ones. Importantly, Eyles et al. (2018) clarify that the two types of academies are radically different, and the lessons learnt from the first ‘batch’ cannot be extrapolated to the second one.

³ Other studies have looked at the effects on demand from the expansion of choice in public service provision; see, for example, Gaynor et al. (2016) on interventions that introduced more choice in the English National Health Service. We do not study the effects of an increase in the ability of parents to make choices amongst a range of alternatives, but look at the effects of changes in the characteristics of these alternatives on the way they are ranked.

academies into the market – unlike Mehta (2017) and Ferreyra and Kosenok (2018) for charter schools in the US. In our reduced-form research design, we take several steps to show that we are estimating how demand for academies increased with a policy-driven shift in supply – rather than how the supply of academies increased in response to a consumer demand shock. The institutional context supports this interpretation. The expansion of the academy programme occurred swiftly and was promoted under a new centre-right government with political aims that were in marked contrast with its centre-left predecessor. Although ostensibly promoted as a policy concerning school quality, it was motivated by a political stance that opposed local government control. In short, the policy was not plausibly an endogenous response to an underlying shift in demand for autonomy in the population. Importantly, all other key elements of the English education market – including admissions and core funding principles – remained unchanged.

In order to alleviate concerns about unobservable confounders, we take several steps. First, the panel nature of our data allows us to control for school fixed-effects to partial out time-fixed school specific unobservables – with estimation based on the change in the demand for a school following its conversion to academy compared to other schools in the choice set that do not convert. Second, we restrict this choice set to schools which become academies during our observation period or right afterwards (up to 2015). Similar ideas using ‘future’ treatment units as controls for current treated units are common in the policy evaluation literature (see Busso et al., 2013). Third, we present balancing tests showing that the timing of conversion is unrelated to school characteristics and that conversion is unrelated to pre-policy trends in school characteristics and parental demand. Finally, we devise an instrumental variable (IV) strategy that uses exogenous variation in the date of conversion arising from the timing of (and the resources dedicated to) the processing of applications by the Government’s Department for Education. In all cases, we control for an extensive set of observable time varying school and student characteristics.

Our key finding is that, on average, families respond positively to academy conversion. Schools that are ‘rebranded’ as academies 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. This headline result masks substantial heterogeneity by family background. Families with higher income – ineligible for Free School Meals (FSM) – are 17% more likely to rank a school first when it converts, while poor families are indifferent. We detect an even bigger difference when comparing White British families with other ethnic groups. Finally, 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 31% more likely to list a school as their first choice if it becomes an academy. An important corollary of these findings is that they underpin a potential increase in segregation across schools – with a higher probability of more advantaged families enrolling at a converting school. This conjecture is borne out by evidence on changes in pupil composition in academies relative other schools – documented both in this paper and in Eyles et al. (2018). These changes are driven by changing demand from families, since academies cannot change their admission code and prioritise more affluent pupils.

We also provide tentative evidence on the mechanisms behind our findings. 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 control for the actual changes in composition and effectiveness following conversion (as in Ferreyra and Kosenok, 2018). 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. A better explanation emerges when we study the influence of pre-existing school characteristics interacted 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 after they become academies – especially for White British, non-poor families. We also find that the probability of ranking a converter academy first is higher when the school is close to home.

Based on these findings, we conjecture that the most plausible explanation of the results is that families from more advantaged backgrounds interpret conversion to academy as a signal of quality and future performance – particularly when combined with pre-policy measures of popularity and academic standards. Furthermore, living close to a converting school might simply make families aware of the academy ‘brand’ – and therefore more likely to rank one first. In short, while there is little new information content on quality in the decision to convert to an academy – given that school inspectorate ratings, popularity, and performance data were publicly available and easily accessible – the academy brand seems to have acted as a salient indicator of these attributes and the change in popularity a rational response to this indicator. Alternatively, the change in family rankings may have just been guided by heuristics and a misinterpretation of the signal. Either way, it turns out that the signal provided by academy conversion was at least in part misleading as these schools do not in general perform better after conversion.

Our paper contributes to several strands of research. First, it adds to the literature on parental preferences for school attributes, including studies of the capitalisation of school quality in house prices (Black 1999, Gibbons et al., 2014) and studies that have directly analysed parental choices. Amongst others, Hastings et al. (2005) show that better-off parents are more likely to choose high test score schools and willing to travel more to secure their pupils’ attendance to better schools than worse-off families. These patterns are echoed by Burgess et al. (2015) for England and Calsamiglia et al. (2015) for Barcelona. 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. Butler et al. (2013) provide evidence that richer students are more likely to choose charter schools. Our findings are consistent with this evidence.

Second, our work contributes to the literature on the role of information in influencing school choices (Figlio and Lucas, 2004; Hastings and Weinstein, 2008; Gómez et al., 2012; Mizala and Urquiola, 2013; and Imberman and Lovenheim, 2016). In particular, Hussain (2016) provides evidence that parents change their preference in response to credible and simplified information provided by school inspectorate ratings in England.

Third, 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 demand for school attributes (see among others Abdulkadiroglu et al, 2015; Abdulkadiroglu et al., 2017; Agarwal and Somaini, 2014; De Haan et al., 2016; and

Fack et al., 2015).⁴ Closest to our work is Kapor et al. (2018) who show that parents' subjective beliefs – rather than rational expectations – of school admission chances influence behaviour.

Lastly, our findings contribute to research on consumer and investor behaviour in relation to brand identity and product/corporate rebranding (e.g., Bronnenberg et al., 2012 and 2015; and Zhao et al., 2017). In particular, Bronnenberg et al. (2015) show that a significant portion of the premium paid for branded health-related products can be ascribed to consumer misinformation. This result parallels our findings that parents choose the 'academy brand' despite only speculative evidence on the advantages relative to comparable alternatives. Zhao et al. (2017) find positive abnormal stock returns after corporate rebranding, but there is marked heterogeneity in the effects depending on the nature of the rebranding and the competitive position of the firm within its market. These results echo our findings on the heterogeneity in the effects of conversion depending on a school's initial quality and popularity (although in their case it is the least competitive firms that see the biggest gains).

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). At KS2, students take standardised national 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

⁴ Abdulkadiroglu et al. (2017) provide evidence that parents prefer school with high-achieving peers but – conditional on these – do not place any value on selection-corrected measures of school effectiveness recovered by exploiting the New York City's central assignment mechanism.

aspects such as teaching, management and pupil behaviour. Although OSTED is a non-ministerial government department, its inspections are carried out – and its reports published – independently of government. Rules for inspections are set out in an Education Inspection Framework document, and ratings based on a specified set of criteria. School ratings are therefore not open to political manipulation.

2.2. School choice, admission and allocation

Admission to state schools is based on principles of free choice, though 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 operates a centralised school admissions process, collecting parental preferences for all state schools – inside and outside the LA. This means we observe preferences expressed by residents of Birmingham for all state secondary schools. We do not observe preferences for private schools, but this omission is unlikely to have much impact on our analysis because less than 5% of secondary school pupils were privately educated in Birmingham in 2010/11.

The LA contacts parents in late Spring of the academic year before students enrol in secondary education (e.g., May 2009 for secondary school admissions to the academic year 2010/2011) and provides detailed information booklets 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 explain the timeline of the admissions process and families are expected to apply by the beginning of October, though applications close at the end of that 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).⁵ The information provided in the admission booklets

⁵ Note that in 2007 the UK Government banned the use of assignment algorithms that are manipulable – such as the Boston mechanism – and as a result all LAs in England have moved to a DA assignment rule (see Pathak and Sonmez, 2013).

also encourages 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, limits to the length of the choice list imply rankings may not reflect families’ unconstrained preferences because parents are aware of the way schools prioritise 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 a family’s first choice – hence we focus on this in our main estimates. Moreover, given that 71% of the families in our sample rank less than six schools, lack of ‘truth telling’ should not be a major issue. We consider the implications of the limited number of choices in 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 – while holding fixed other aspects of the education market and assignment mechanism.

2.3. Academies: institutional characteristics

There are five types of secondary schools: community, voluntary controlled (VC), foundation, voluntary aided (VA) and academy. Community and 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 and, in the case of VC schools, members of the foundation (usually religious) supporting the school. 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 the following changes after becoming academies: (i) 65% to 75% changed their taught curriculum by focussing on core subjects; (ii) 60% to 70% introduced new systems to monitor pupil and teacher performance more formally, regularly and/or thoroughly; and (iii) around 85% linked more explicitly teachers’ pay and promotions to pupils’ performance. Very few secondary schools reported that they were able to change the length of the school day. 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. Note that these are self-reported retrospective assessments of changes implemented since conversion. Little ‘hard’ evidence has been collected so far 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.

Academy funding is linked to the number of students on roll. However, unlike other schools which receive funds from central Government via the LA, academies receive funding directly from central Government. Academies also acquire more administrative responsibilities and become responsible for maintenance of the premises. Lastly, academies cannot legally 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 from September 2002 to tackle 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 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 by 2015 (at the end of our study period) there were more than 1800 secondary academies out of around 3200 secondary schools. Of these, around 500 were sponsored academies – with almost 300 created during the Labour Government (pre-2010). More than 1300 academies were created between 2010 and 2015 through the converter route – with approximately 85% of this expansion taking place in the first three years. These represented 40% of all secondary schools in 2015 and 80% of the increase in the academy sector in the first five years after the reform. In our analysis, we focus on these converter academies to identify the impact of the ‘autonomous brand’ on parental preferences.⁶ As noted above, the proportion of academies at the time of writing is around 70%, but this figure plateaued after the government pulled back (in 2016) on its ambitions to make all schools academies.

The process of conversion is voluntary and initiated by the head-teacher and/or the school 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

⁶ 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, which could impact parental demand.

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 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 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.

What motivates schools to apply for conversion? A key incentive for a school to convert to an academy is to free up funds previously kept by the LA to provide back-office activities – such as accounting, procurement and site-maintenance. Indeed, the DfE (2013) academies survey reports that the two most frequently cited reasons for converting were ‘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’). Managerial independence and reduced bureaucratic control were also factors, with the fourth most cited reason being ‘to become independent of the LA’. On the other hand, the opportunity to expand enrolment does not seem to be strong incentive, probably because of wide-spread planning constraints – which hinder expansion of the school premises. 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). Set against these potential gains are a number of costs and risks. As discussed previously, student numbers and funding are more closely linked to school popularity and student demand in academy schools – so the school might choose not to convert because of uncertainty with the popularity of the change. Furthermore, the downside of greater freedom and control is that this brings increased administrative burdens and responsibilities on the school, and the school leadership might be unwilling to take this on. Finally, some teaching staff (and teaching unions) were hostile to the idea of academies, which may have been a barrier to conversion.

Two aspects of the conversion process are relevant for the instrumental variable strategy we use later in the analysis: OFSTED ratings and the approval process. First, OFSTED inspection ratings are an important determinant of approval for conversion. Initially, conversion was mainly reserved for ‘outstanding’ schools. Subsequently ‘good’ schools with ‘outstanding features’ (e.g., in teaching or management) were allowed to apply if their attainments and value-added were above average. 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. Schools with other OFSTED ratings could apply for conversion though their applications required additional material to support their case. 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 the Department for Education takes place in two steps. First, ‘Lead Teams’ are assigned to evaluate the proposals from different 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 championed as ‘best-practice’ examples. The objective criteria for approval prevent teams from applying less scrutiny in order to achieve their targets. 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 approve the cases put forward by Lead Teams. 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. The reason why delays happen is 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.

2.5. Academies: what parents knew

The information provided to families when making school applications is that academies 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). Parents might gain additional information through the community consultations that schools are required to undertake as part of the conversion process, and from school visits.

However, survey evidence suggests that parents had little understanding of the academy model. A survey by the consultancy HCSS Education in 2015 on a sample of 1,000 households reports that 58% of parents did not understand what academies do, and 32% thought there was 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 know little or nothing at all about academy schools, with a lower figure for high-income individuals (68%) than for low/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).⁷ This evidence suggests that any changes in parental preference following conversion are more likely to have resulted from a reaction to a school's decision to 'rebrand' as an autonomous academy – rather than hard information on the likely structural changes and benefits associated with the academy model.

3. Data construction

Data on family preferences for schools comes from administrative records held by Birmingham LA. These data contain information about the preferences expressed by Birmingham residents applying for a place in a secondary school for the academic years beginning in September 2010, 2011 and 2012. The data contain the ordered list of preferences expressed by each family; whether any special criteria for admission (i.e., having

⁷ 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. (2018) 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.

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 and outside the LA. In our data we initially observe a preference for nearly 300 different schools – i.e., schools that were listed as one of a family's choices at least once over three years. Of these, approximately one third is located in Birmingham and two thirds in other 42 LAs. Of the schools outside Birmingham, however, around 130 are listed just once and are clearly not part of a consistently defined and stable choice set. Other schools are listed more than once but in only one of the three years. In order to avoid including these 'outliers', we define our choice set to include only: (i) all Birmingham schools; (ii) LAs other than Birmingham that receive at least 30 preferences from pupils residing in Birmingham in each of the three years of data; and (iii) schools within these LAs that receive 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 schools for males and male-only schools for females. As a result, the choice set counts 110 schools for boys and 115 schools for girls.

Data on pupil attainment and background comes from 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 low income); 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 to residential moves in response to academy conversions.

The data on preferences and the NPD/PLASC databases are linked using a unique pupil identifier (UPN). After dropping cases where the UPN is missing, families did not submit a preference (or submitted late) and background or attainment data is missing, we obtain a final sample 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 all pupils on which we have some information.

We merge additional data on schools from several administrative sources. A school registry (Edubase) provides information about exact location (the postcode), type (e.g. community or academy), gender composition (mixed or male-/female-only), admission procedure (selective or comprehensive), and opening

date – including the date when a school converts to an academy. The opening date for converter academies is spread out across the months. Approximately 60% convert between July and September, with the rest clustered in October, January, April and July. Although opening marks the conclusion of legal aspects of academisation 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 in the application booklets, or at open days held by schools. Therefore, in our analysis, we define schools as academies if they are approved for conversion by the end of October – in time for families to identify them as academies before the deadline for application for admission in the following year. Information on each school’s stage in the conversion process was provided by DfE (up to March 2015). 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).

School composition variables (shares of girls, pupils registered for FSM, pupils with special educational needs, and White British pupils) are obtained by aggregating the NPD/PLASC data to school-by-year cells using all pupils from grade 7/age 12 to grade 11/age 16. Achievement-related variables are school-by-year mean KS2 scores (averaged across English, Mathematics and Science) of pupils in 7th grade (the first year of secondary education), 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 provides an indicator of the school’s educational effectiveness. Other school-level indicators – such as total roll, total number of teachers, and the pupil-to-teacher ratio – are obtained from the School Level Annual School Census. Inspection ratings and dates of inspection are collected from OFSTED.⁸

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 effect of conversion to an autonomous academy on parental demand – inferred from a school’s ranking in the list of preferences that families submit when choosing schools. To do this, we compare the ranking for a school when it converts to that of comparable non-academy schools,

⁸ Data on schools’ expenditure, income and workforce is available from other sources, but missing for some academies, which do not have the same reporting obligations as other state-maintained schools. We therefore only use this information in some extensions and checks.

controlling for other school, pupil and neighbourhood characteristics that affect school demand and correlate with the likelihood of converting to academy. Our baseline specification is as follows:

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

In equation (1), $Pref_{ist}$ measures the ranking that the family of pupil i places on school s at time t ; α_i , σ_s and θ_t capture pupil, school and time specific unobservables; $Academy_{st}$ is a dummy indicating whether school s operates as an academy by time t when pupils/parents 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, potentially correlated with $Academy_{st}$.

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 is the first choice school, and zero for all other schools. In some robustness tests, we use the full list of six ranked preferences, and $Pref_{ist} = 6$ if a school is ranked first, 5 if a school is ranked second – down to zero if a school is not listed in a family's preferences. The second variable, $Academy_{st}$, indicates whether school s at time t is operating as an academy. As discussed in Section 3, we mainly focus on whether the school was approved for conversion to academy at the time when parents expressed their preferences.⁹

In our main specifications, we estimate Equation (1) by OLS – i.e., a linear probability model – and using all pupils and all the alternatives in their choice set (discussed in Section 3). If $Pref_{ist}$ is our indicator that a school is the first choice, then $\hat{\beta}$ estimates the probability that a school is ranked first, conditional on being an academy – $\text{Prob}(\text{Pref}=1|\text{Academy}=1)$ – minus the probability that a school is ranked first, conditional on being a non-academy – $\text{Prob}(\text{Pref}=1|\text{Academy}=0)$. Our estimates thus measure a relative change in the probability of listing a school first when it acquires autonomous status relative to the change in the probability of listing a school first when it does not convert – i.e., a shift in equilibrium first choice ‘market share’. If academy status was randomly assigned (or all unobservables otherwise uncorrelated with academy status), then this estimate would provide an indicator of families’ demand for schools which are academies, compared to schools which are non-academies – i.e., the adjustment of demand to an exogenous shift in the supply of academies.¹⁰

To estimate this parameter consistently, we do not need to formally model the structure of demand or the structure of supply of academy schools as has been done elsewhere in the literature on consumer preferences for differentiated products and services (Lancaster, 1971, McFadden, 1973, Berry et al., 1995) and studies of

⁹ 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).

¹⁰ Conversely, our estimates do not measure an overall increase in demand in the market following conversion to academy of a subset of schools – since families can only list a first choice school once and only attend one school. Although we do not have data on potential substitutions ‘into the market’ from alternative private provision or home-schooling, these represent a very small share of the education sector in England.

the demand for public services, like schools and hospitals (Ferreyra and Kosenok, 2018 and Gaynor et al., 2016). The challenge for us is to ensure that the variation in academy status from which we estimate $\hat{\beta}$ is as good as random, and uncorrelated with unobservables that directly affect families' rankings of schools.

In this respect, there are two main channels that threaten identification. Firstly, there is a potential standard demand/supply endogeneity issue. We want to infer families' underlying preferences for academies and the autonomy they embody from their response to a supply shock. Therefore, we need to rule out a response in supply to an unobserved shift in demand, which would not be revealing about underlying preferences in the absence of any demand shift. Secondly, unobserved fixed and time varying confounders at school level might simultaneously affect the ranking of schools and schools' decision to convert, meaning we might mistake preferences for unobserved quality-related attributes for a preference for academies and autonomy. This issue is similar to the quality-price endogeneity problem characterising standard consumer demand models.

The fact that the academy programme was a national policy, rapidly introduced by a new Government with the aim of freeing schools from local government control (and in marked contrast with the aims of the predecessor policy) suggests that it can plausibly be treated as an exogenous shock to the supply of academies in the extended Birmingham market. Indeed, the Government decision to move to an academy-centred system (and later shelved plans to move to an all-academy education market) was mainly motivated by political priorities and orientations, and met with resistance by some teachers, education practitioners and a sizeable representation of parents. However, we still need to rule out the possibility that the decision of individual schools to convert was partly driven by changes in their local markets, and that parents are not choosing schools that are academies because of unobserved attributes, rather than their academy status.

In order to mitigate these concerns, we take several steps. First, 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. A similar fixed-effect approach is used in Nevo (2001) in his analysis of ready-to-eat cereals, and is suggested by Berry et al. (1995) when micro-level (as opposed to market-level) data is available (as in our case). 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 these 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 ranked preferences for listed schools) or restrict the sample of schools available to pupils (e.g., only schools within 2.5km from home) in ways that generate pupil-level variation in choice sets. We also control for a detailed set of time varying school characteristics, namely: average KS4 attainments; the incidence of pupils eligible for

FSM; the shares of female and of White British students; pupil-teacher ratios; and a dummy indicating whether the school was rated ‘outstanding’ by the inspectorate (OFSTED).

Given the inclusion of these fixed effects, the identifying assumption is that the timing of conversion to academy is ‘as good as random’ and unrelated to time-varying school unobserved shocks that might affect parental preferences and the school propensity to become an academy. In order to deal with these potential time varying 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 right after our observation period – i.e., a control group formed of *future* converters (up to March 2015, when the data collection took place). This approach exploits 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 details of the process of academy conversion. This IV strategy addresses the concern that there might be time-varying quality-related variables that attract applicants and affect a schools’ decision to apply for conversion in a given year, or speed up approval – conditional on applying – such as a change in head-teacher.

Our first instrument exploits a change in the criteria demarking eligibility for conversion occurring between 2010 and 2011 (see Section 2.4 for more details). 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. As already noted, 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. As discussed in Section 2.2, inspectorate reports are not open to political manipulation and we measure ratings prior to the introduction of the converter academy policy. Furthermore, the timing of the changes in the eligibility rules is unlikely to be driven by school-specific considerations – in

particular, considerations that are specific to schools in our sample. This should ensure that our instrument is not endogenous to a school's decision to convert to academy.

Our second instrument predicts whether – conditional on having applied for conversion 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. 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. 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. Furthermore, by using the number of applications from areas other than the extended Birmingham market we consider, we circumvent the possibility that schools' decisions to apply for conversion are affected by strategic considerations about the behaviour of other schools in the same market. This is because it is unlikely that schools in our investigation area would have information about the patterns of application and conversion to academy in geographically removed locations with which they do not interact or compete.

In addition to these steps outlined above, we present a number of other checks on whether institutional features of the admission process might confound our results – e.g., the possibility that families move closer to schools which prioritise applicants by distance to gain admission to academies. We also restrict the choice set available to parents in various ways to address concerns regarding the inclusion of irrelevant alternatives – e.g., schools that are not close substitutes to those that convert and are too far away to be likely to be chosen. These modifications are discussed in the results sections.

The simple OLS linear probability estimation of Equation (1) based on first choice preferences might raise concerns regarding efficiency and for the fact that it deviates from common practice in related fields. To alleviate such worries, we estimate a conditional logit specification of school choice using the same data structure. 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), and 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 infeasible schools which a family might not have listed – but which they in fact might have preferred – to allay concerns about the ‘truthfulness’ of rankings when the number of options is limited.

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 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, where a diverse and relatively deprived population resides. 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 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 of FSM-eligible and White British pupils are 24% and 55% respectively. This is less than in the

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

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 for a discussion) with the exception of: (i) 15 female-only school excluded from the 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. 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. Standard errors are clustered at school level to allow for arbitrary correlation of unobservables within school groups. We also experimented with two-way clustering – at the pupil and school level – and found virtually identical results. 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. 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 table note and described in Table 1). Our results show

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.

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%.

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.

Table 3 presents the results from the instrumental variable (IV) strategy described in Section 4. Note that our IV specifications drop the additional controls added in Columns (5) and (6) of Table 2. 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 better 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. The positive sign of the first-stage coefficient on this instrument 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

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.

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. The implied effect from the IV estimate is remarkably similar to the one obtained in Columns (5) and (6) in Table 2. 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) of Table 2, which we regard as our favourite.

The use of the eligibility instrument based schools' initial OFSTED ratings might raise concerns about the validity of the exclusion restriction (e.g., being a good or outstanding school might have direct effects on *changes* in families' rankings of schools – time-invariant differences in levels are controlled for by the school fixed effects). In practice, this does not appear to be a major concern: the Hansen-Sargan test for the over-identifying restrictions in Column (2) of Table 3 has a p-value over 0.49, implying that we do not reject the hypothesis that the separate IV estimates associated with the two instruments are equal. However, to further probe our results, we used the two IVs separately. When we use only 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 imprecisely estimated effect. This reflects the fact that this second instrument alone does not generate enough variation in the timing of approval and is too weak to yield a reliable estimate of the impact of conversion on preferences.

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 this impact 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 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 4.

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 IVs used in Table 3. 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 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 4, 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 Tables 2 and 3.

Importantly, 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 (measured in 2009): 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

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 Appendix Table 3. To facilitate a comparison with the findings presented in Table 2, we discuss magnitudes in terms of MWT.

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 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) and Abdulkadiroglu et al. (2015), 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 student’s home address.¹⁴ 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 priority groups (e.g., 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. To assess the validity of this approximation, we perform our analysis dropping students who have priority based on other criteria – such as ‘being 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%. Note however that given the average distance in this sample, this still corresponds to approximately 550 metres.¹⁵ 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. (2015). First, we note that just above 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. 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.

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

¹⁴ 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.

¹⁵ For comparison, OLS estimation of the model in Column (6) of Table 2 on the ‘feasible school’ set yields a MWT of almost 29%.

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 academies focussing on income and ethnicity – measured by FSM/non-FSM status and White British/other ethnic group respectively. The table present the effect of 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 controls) 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 is not statistically significant (p-value: 0.443), it is sizeable. 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 as a first preference by this group 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 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 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).¹⁶ 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 suggest 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., 2018).

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 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.¹⁷

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.

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,

¹⁶ 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.

¹⁷ We also estimated the impact of 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. Furthermore, we investigated differences by gender and prior achievement but found no systematic patterns.

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 composition between socioeconomic groups. However, the majority of previous studies 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 institutional 'rebranding' by exploiting the panel dimension in the data and focussing on a large policy shock – with significant heterogeneity in demand, and the potential for sorting and segregation.

6.2. Heterogeneous patterns: what explanations?

In this section, we investigate some possible explanations for the 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 characteristics (possibly unobservable) 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. Similarly, we found that our results do not change if we only consider the set of 'feasible schools'.

Next, we consider the possibility that parents of different background have different awareness about school conversions – and so the patterns 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 discount this channel, we run regressions that only consider schools in the Birmingham LA. As discussed, the LA sends booklets to parents containing information for schools in the LA. Although these booklets contain information for schools in other LAs, the level of detail is 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 confirm our

previous findings. All in all, our evidence suggests that the differential availability of information to different groups is not a strong candidate to 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. (2015) 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 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 potential (but unknown) 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 from the BHPS 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). 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 7.2 and in the Conclusions where interpret our overall 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. While the findings we discuss below cannot

be interpreted as causal, we think they provide some useful descriptive evidence to shed light on why more affluent parents respond to the academy ‘branding’ and to the offer of increased school autonomy.

7.1. Expectations about changes in school composition and effectiveness

We start by investigating the most likely explanation: families expect the characteristics and performance of schools that become academies to improve following conversion – and choose on the basis of these expected changes. Here we focus on two aspects of schooling – peer group quality and academic effectiveness (value-added) given that these factors are acknowledged as objects of preference – both anecdotally and according to empirical evidence (e.g., Gibbons et al., 2014; Burgess et al., 2015). To test for such possibility, we assume families have perfect foresight and their expectations are correct – and so we estimate the impact that controlling for *actual* school-specific changes in characteristics *ex-post* has on the patterns of demand for academies observed in Tables 2-5. This approach follows Ferreyra and Kosenok (2018).¹⁸ 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).

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 heterogeneity in demand presented in Table 5 is mirrored by evidence that academies are more likely to make offers and enrol well-off students – leading to a more advantaged and stratified intake (see also Eyles et al., 2018). 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. (2017) 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. 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 non-academy. 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. In both cases, we recover school-specific estimates about changes in effectiveness by interacting an

¹⁸ Note that these variables are potentially ‘bad controls’ – in the sense of Angrist and Pischke (2009). Even then, it is instructive to study if the coefficient on academy conversion is attenuated once we control for specific-school attributes that could explain the academy effect on choices.

‘academy on’ dummy for the years after conversion with school-specific indicators. Moreover, since different schools had different eligibility criteria (see Section 2.4) to convert depending on their inspectorate rating, we estimate all our models separately for schools rated ‘outstanding’, ‘good’ and ‘satisfactory/inadequate’ at the latest inspection prior to 2010. Our results show that ‘outstanding’ have on average a positive and significant effect on students’ KS2-to-KS4 value added following academisation. However, we find no evidence that ‘good’ or ‘satisfactory/inadequate’ school affect their students’ outcomes after conversion (neither positively nor negatively). More details about our methods and findings are provided in On-Line Appendix 2.

The most striking finding from Panels B and C 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 about the effect that academies will have on these attributes *ex-ante* do not coincide with reality *ex-post*. Assuming the earlier literature is correct – i.e., families are attracted to schools with good peer groups and high value-added (see Gibbons et al., 2014) – our results in Table 7 suggest that families have too little information to form correct 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 likely benefits of academisation.

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).¹⁹ 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 fixed pre-existing school characteristics. These are selected to represent school attributes observable by parents – mostly through school league tables or

¹⁹ We find little evidence that academies introduce significant changes along these dimensions. Regarding curriculum, academies offer 1.25 more subjects (out of an average of 25 subjects), but this estimate is not significant. Similarly, we find no evidence of changes in expenditure and limited evidence of meaningful differences in schools’ workforces after conversion. The only significant estimate concerns the change in head-teacher, showing an increase after academisation.

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.

Panel A interacts the 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 is linked to 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.²⁰ Finally, Panel C introduces an interaction between academy status and an indicator that the latest school inspection rating prior to 2010 was ‘outstanding’.

The initial picture from Table 8 for the whole pupil population (Column 1) is that there is only a marginal difference between high-quality/popular schools and lower-quality/less popular ones in the effect of conversion on family choices. The coefficients are sometimes higher for the better quality or oversubscribed schools, but the differences are not statistically significant. However, the heterogeneity along the quality/over-subscription dimension becomes more marked once we look at non-FSM families in Column (2), White-British families (Column 3), and especially 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 affecting 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). These 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 two other proxies for quality pre-dating conversion (results not tabulated): school ‘contextual value added’ (CVA, averaged over 2007-2009)²¹; and 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 (this information is taken from the admissions booklet sent out by LAs). Using these measures, we find similar patterns of heterogeneity: high quality and oversubscribed schools prior to conversion experience significantly larger changes in demand following conversion than other schools.²²

Overall, the evidence in Table 8 suggests that families treat the academy ‘branding’ in conjunction with pre-existing quality measures as markers of excellence – and use this information as a signal of a school’s future

²⁰ Note that 81%/43% of current converters and 44%/33% future converters are high quality/overcapacity, respectively.

²¹ CVA is a proxy for school effectiveness (given intake) published in school league tables and it is basically the residual from a regression of KS4 test scores on KS2 test scores and student characteristics.

²² We also checked whether changes in demand following conversion depend on whether schools were mostly ranked first, second or third and below prior to becoming academies. We found no evidence of significant heterogeneity.

performance and suitability for their children (the exception being prior ‘outstanding’ ratings). Stated differently, academy conversion seems to have acted as a salient indicator of ‘excellence’ – and the change in demand a rational response to it – despite the fact that there is little novel information on quality in the decision to convert to an academy (because data on popularity and performance are publicly available and easily accessible). As it turned out, the signal provided by academy conversion was somewhat misleading as these schools do not generally perform any better after conversion. While schools rated ‘outstanding’ prior to conversion improve their effectiveness following conversion (as discussed above), the same is not true for ‘good’ and ‘satisfactory’ schools. Moreover, the correlation between post-conversion changes in effectiveness and pre-conversion KS4/GCSE attainments is positive but very small, at 0.078; the same holds for pre-conversion popularity. 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.

This ‘salience’ interpretation is also consistent with our evidence on the role played by proximity in amplifying the impact of conversion: living close to a converting school simply make families aware of the academy ‘brand’ – and therefore more likely to rank one first.

These results could of course be given 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 we cannot observe. While plausible, this explanation does not account for the significant interaction effects 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 used pupil level information about school preferences at a time of a rapid expansion of the academy sector in the England to uncover how families respond to policy changes that inject significant school autonomy within the state sector. As this expansion was occurring, relatively little information was available to parents on the details of the organisational changes stemming from academy conversion – or the

likely educational benefits (and costs) for children. We therefore used this setting to study how families value the offer of freedom and the ‘branding’ of schools as autonomous academies.

Our results show that – on average – families have a significant preference for schools that convert to academies and opt-out of local authority control. 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 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 (around £22,000 in 2010) or 1.8%-4.2% of average annual secondary school expenditure per pupil (at around £6000 in 2010). While not very large, these sums are non-negligible.

Our investigations of some possible mechanisms found no evidence that the change in demand for schools converting to academies is explained by anticipation of their ex-post changes in academic performance or student composition: conditional on these ‘future’ changes, families still rank converting academies higher. Our investigations show instead that the magnitude of the effect of academy conversion on White, higher-income families’ school rankings varies with measures of schools’ quality and popularity prior to conversion. These families appear to use the event of an academy ‘re-branding’ in conjunction with information on prior quality and popularity to form expectations about schools’ future performance and suitability for their children. Unfortunately, our analysis does not tell us about *why* families with more advantaged backgrounds respond more to academy conversion than less advantaged groups. Evidently, these groups are responding to the same information in very different ways – but we are unable to conclusively say whether this is because of differences in preferences or differences in the way they interpret the same information. We also found evidence that converter schools close to pupils’ homes experience the biggest increases in demand – most likely because parents are just more aware of the conversion happening.

Although these decisions are rational responses to these signals – given the limited information available – the signals provided by academy conversion appear to have been at least partially misleading: there were few signs of general performance improvements in schools that converted to academies in the years after conversion. Obviously, we cannot exclude that parents simply prefer converters because the academy ‘brand’ offers direct utility benefits – irrespective of the education benefits it might confer. However, this explanation seems hard

to square with the fact that it is only the proximate, popular and highly-quality schools that rank higher in parents' preferences following conversion.²³

Besides advancing our understanding of parental preferences for schools and choice behaviour in the context of significant school reforms, our findings carry clear and important policy implications. Growing evidence – from the UK, Sweden and the US – shows 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 regulations do not allow open 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 autonomy are heterogeneous along dimensions of family background. Demand-side policies aimed at raising awareness of any potential 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.

²³ One alternative interpretation of this behavioural response is that it is an example of the ‘representativeness’ heuristic discussed by Tversky and Kahneman (1974). Families may take the observed high probability of conversion, conditional on being a high quality school, as a signal that a school will be high quality in the future, conditional on converting.

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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.
<i>Panel A: Pupil level information</i>				
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	--	--
<i>Panel B: School level information</i>				
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
<i>Panel C: Choice level information</i>				
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)
	Full Sample	Full Sample	Full Sample	Converters Sample	Converters Sample	Converters Sample
	OLS	OLS	OLS	OLS	OLS	OLS
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)**
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)***
<i>Implied highest preference effect</i>	7.95%	8.07%	7.72%	9.32%	12.09%	13.52%
<i>Marginal willingness to travel</i>	1.55%	1.48%	1.42%	1.72%	2.23%	2.49%
<i>F-Test first stage</i>	--	--	--	--	--	--
School effects	Yes	Yes	Yes	Yes	Yes	Yes
Year and gender effects	Yes	No	No	No	No	No
Pupil effects	No	Yes	Yes	Yes	Yes	Yes
School time-varying controls	No	No	Yes	Yes	Yes	Yes
School time-averaged controls × Year effects	No	No	No	No	Yes	No
School ‘academy propensity’ × Year effects	No	No	No	No	No	Yes

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 (6): 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. ***, significant at the 1% level; **, significant at the 5% level; *, significant at the 10% level. Source: authors’ calculations.

Table 3. The impact of conversion to academy on the demand for schools –
Converters sample, instrumental variables estimates.

	(1)	(2)
	First stage. Dependent variable: Converter academy	Second stage. Dependent variable: Students' highest preference
	OLS	IV
School meets eligibility criteria to apply for conversion	0.235 (0.90)**	--
Number of contemporaneous applications received by the DfE from other LAs	0.0038 (0.0004)***	--
Converter Academy	--	0.136 (0.069)*
Log of pupil-school Distance	--	-0.053 (0.004)***
<i>Implied highest preference effect</i>	--	13.93%
<i>Marginal willingness to travel</i>	--	2.57%
<i>F-Test first stage</i>	41.04	--
<i>Over-identification test (p-value)</i>	--	0.498
School effects	Yes	Yes
Year and gender effects	No	No
Pupil effects	Yes	Yes
School time-varying controls	Yes	Yes
School time-averaged controls	No	No
× Year effects	No	No
School 'academy propensity'	No	No
× Year effects	No	No

Note: Column (1): first-stage OLS regression for academy conversion. Instruments are defined 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). We only report coefficients and standard errors (clustered at the school level) on the two instruments. The first stage F test is also reported. Column (2): second-stage IV regression for students' highest preference. We only report coefficients and standard errors (clustered at the school level) on a dummy for academy conversion (multiplied by 100) and on the log of home-to-school distance. Implied academy effect obtained by rescaling the coefficient by the probability of a school being the highest preference (0.0097). The p-value for the Sargan-Hansen J test for over-identification is also reported. Number of observations: 1,448,688 (60 schools and 37,140 pupils). 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. ***: significant at the 1% level; **: significant at the 5% level; *: significant at the 10% level. Source: authors' calculations.

Table 4: Timing of academy approval and selected school and neighbourhood characteristics –
Converters sample, balancing evidence

	(1)	(2)
	Cross-sectional	Within-school
<i>Panel A: Time-varying shocks</i>		
<u>School:</u>		
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)
<u>Neighbourhood:</u>		
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)
<i>Panel B: Initial conditions and pre-trends 2007-2009</i>		
<u>School:</u>		
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)	--
<u>Neighbourhood:</u>		
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 and year dummies. 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 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)***
<i>Implied highest preference effect</i>	17.3%	6.0%	25.7%	9.8%	31.3%	12.9%	12.4%	5.5%
<i>Marginal willingness to travel</i>	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’ \times 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 (2b): 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
<i>Panel A: Only pupils in the catchment area of at least one academy</i>						
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)
<i>Panel B: Controlling for school \times distance effects</i>						
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)
<i>Panel C: Only Birmingham schools</i>						
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’ \times 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
<i>Panel A: Controlling for expected composition (intake) changes</i>				
Converter	0.120	0.149	0.223	0.264
Academy	(0.059)**	(0.057)**	(0.074)***	(0.089)***
<i>Panel B: Controlling for expected changes in effectiveness (KS2 to KS4 value-added – ‘legacy’ enrolment)</i>				
Converter	0.131	0.169	0.249	0.301
Academy	(0.061)**	(0.064)***	(0.083)***	(0.097)***
<i>Panel C: Controlling for expected changes in effectiveness (KS2 to KS4 value-added – endogenous KS4 enrolment)</i>				
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.

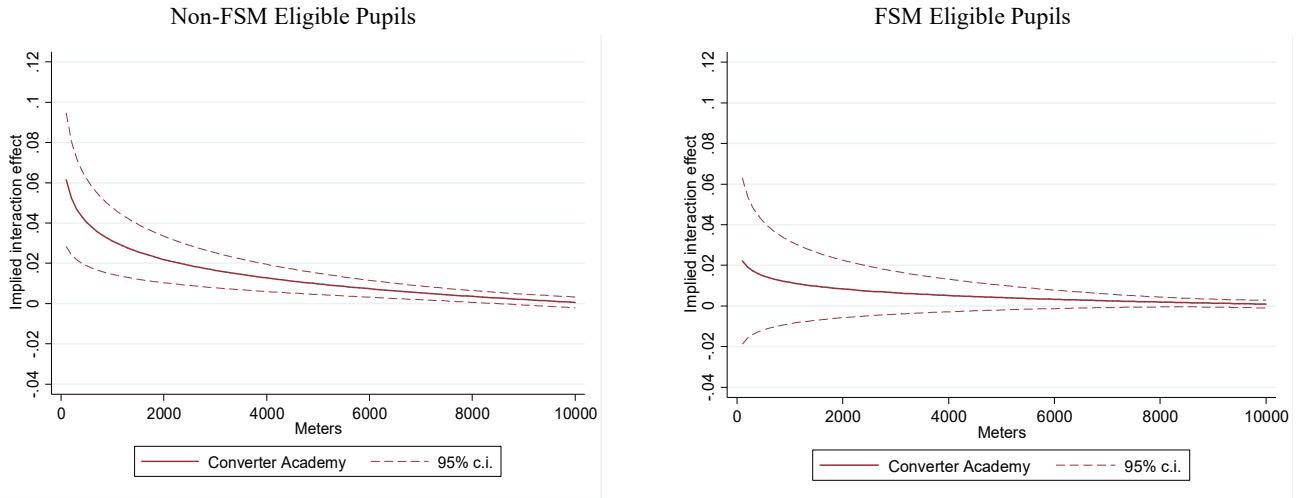
Table 8: The mechanisms behind the impact of conversion to academy –
Pre-conversion school quality and popularity

	(1)	(2)	(3)	(4)
	All pupils	Pupil is non-FSM	Pupil is White British	Pupil is White British & non-FSM
<i>Panel A: % 5 A*-C KS4/GCSEs (average 2007-2009)</i>				
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)
<i>P-value - significance of difference</i>	0.8588	0.4901	0.0243	0.0143
<i>Panel B: School overcapacity (2007-2009)</i>				
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)*
<i>P-value - significance of difference</i>	0.2146	0.0579	0.0188	0.0125
<i>Panel C: OFSTED rating (latest available up to 2009)</i>				
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)***
<i>P-value - significance of difference</i>	0.990	0.838	0.860	0.868

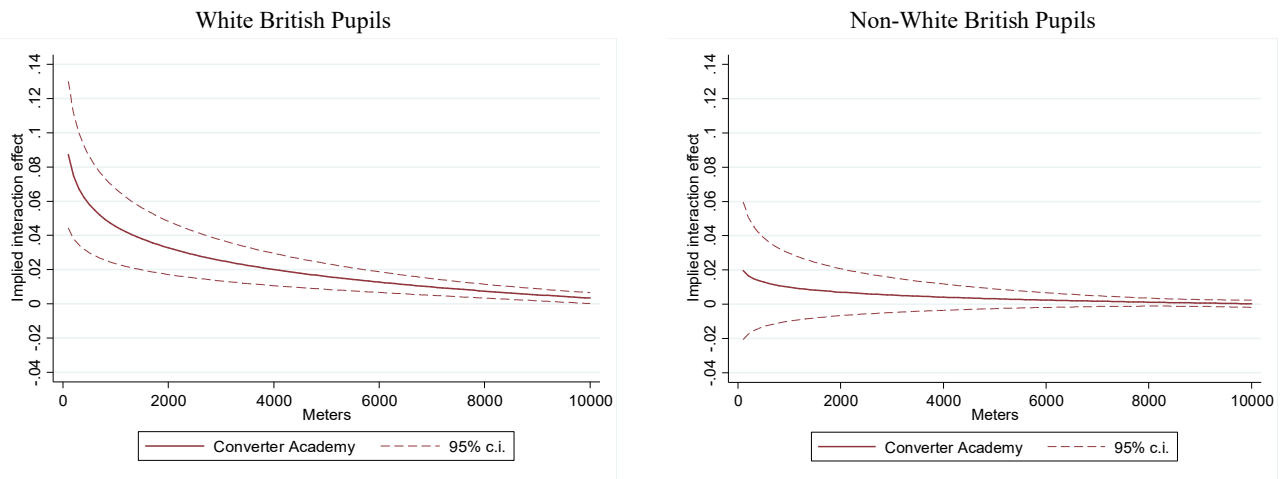
Note: The table reports coefficients and standard errors in parenthesis (clustered at the school level) on a dummy for academy conversion and its interaction with a proxy for ‘salience’ (multiplied by 100). All regressions control for a variable identifying whether schools become ‘sponsored’ academies 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 academy conversion over distance

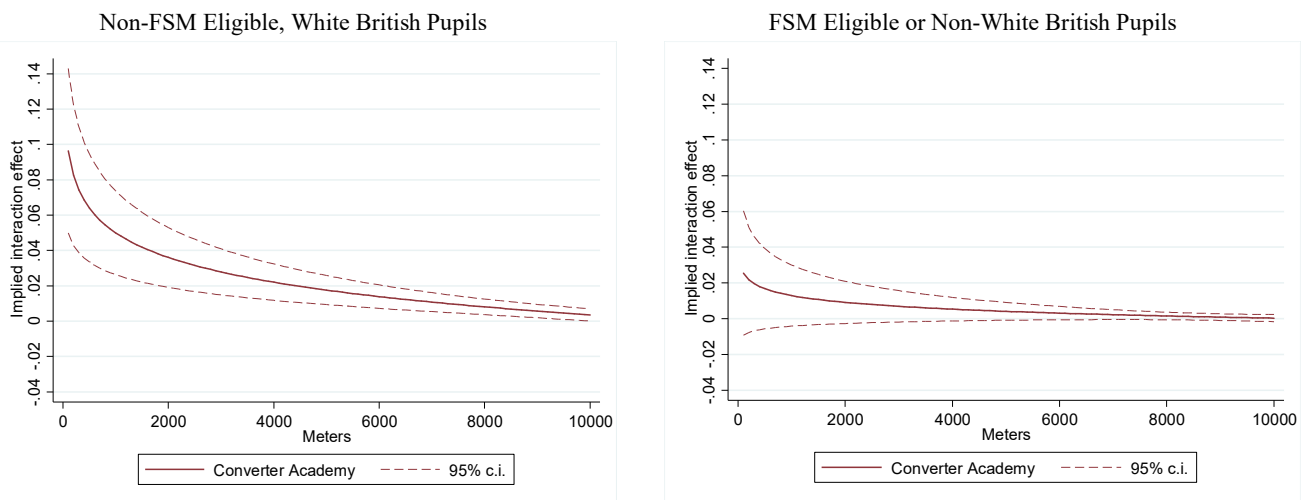
Panel A: by pupil FSM eligibility



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' \times 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)
<i><u>Panel A: Prevalence of academies</u></i>			
Converter academies	0	3 (2.4%)	41 (32.8%)
Other schools	125	122 (97.6%)	84 (67.2%)
<i><u>Panel B: Pupil preferences: % of students choosing first</u></i>			
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)
	Cross-sectional	Within-school
<i>Panel A: Instrument is changes in eligibility criteria</i>		
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)
<i>Panel B: Instrument is total number of applications</i>		
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)	--

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 and year dummies. 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.

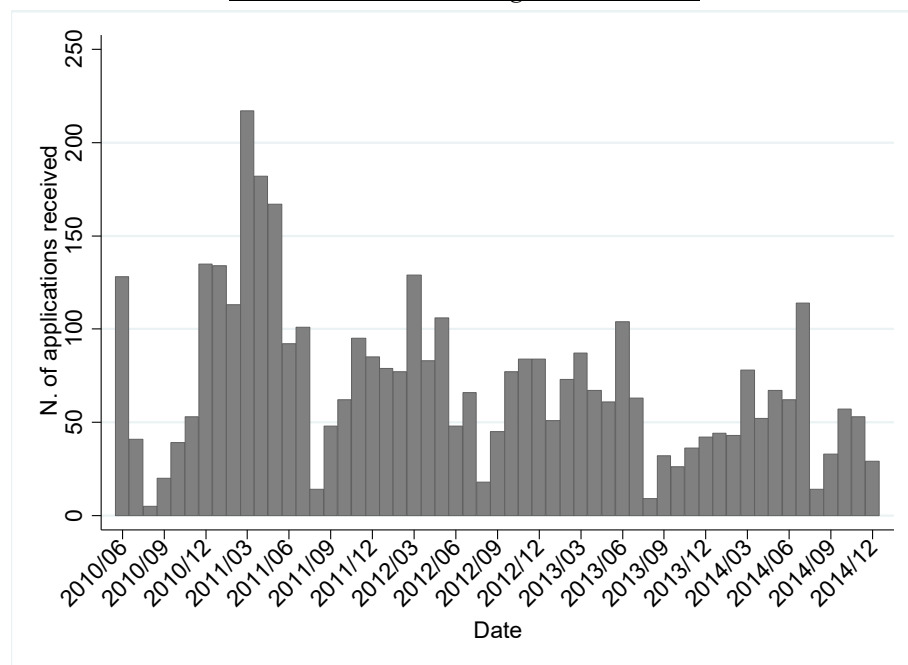
Appendix Table 3: The impact of conversion to academy on the demand for schools –
Specification checks and ‘truth telling’

	(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)***
<i>Marginal willingness to travel</i>	<i>13.2%</i>	<i>4.46%</i>	<i>21.0%</i>	<i>17.9%</i>

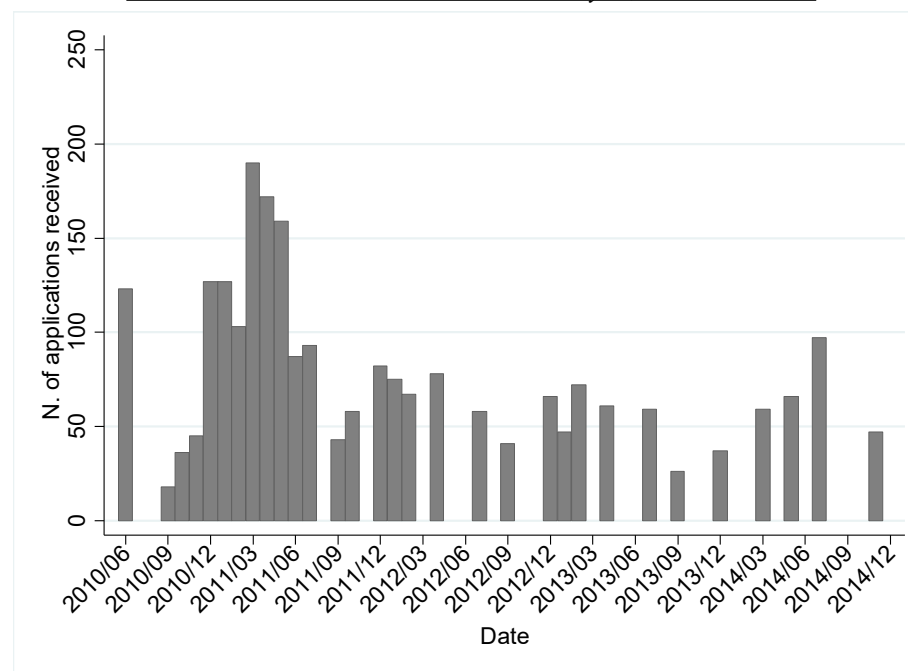
Note: The table reports coefficients and standard errors in parenthesis on a dummy for academy conversion and on the log of home-to-school distance. All regressions control for a variable identifying whether schools become ‘sponsored’ academies and for: (i) pupil and school effects; (ii) school time-varying controls, and (iii) school ‘academy propensity’ \times 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.

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 to 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; on average 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 do so, 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 stage of primary school) and grade 7 (at the beginning of secondary school). As shown in Column (2), dropping these potentially strategic movers does not affect our conclusions.

The next two columns 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 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 also 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 enrolled for secondary education (grade 7) in 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 above should yield an unbiased causal estimate of the impact of academy conversion on a randomly picked student. 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.

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

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.

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 (pooling all pupil types) that allow us 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 school-specific effectiveness. The average school-specific 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 school-specific 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)***
<i>Mean outcome</i>	<i>0.1401</i>	<i>0.0098</i>	<i>0.0098</i>	<i>0.0101</i>	<i>0.0098</i>
<i>Implied highest preference effect</i>	<i>32.7%</i>	<i>15.0%</i>	<i>14.4%</i>	<i>14.1%</i>	<i>12.0%</i>
<i>Implied willingness to travel</i>	<i>20.3%</i>	<i>2.72%</i>	<i>2.66%</i>	<i>2.84%</i>	<i>2.23%</i>

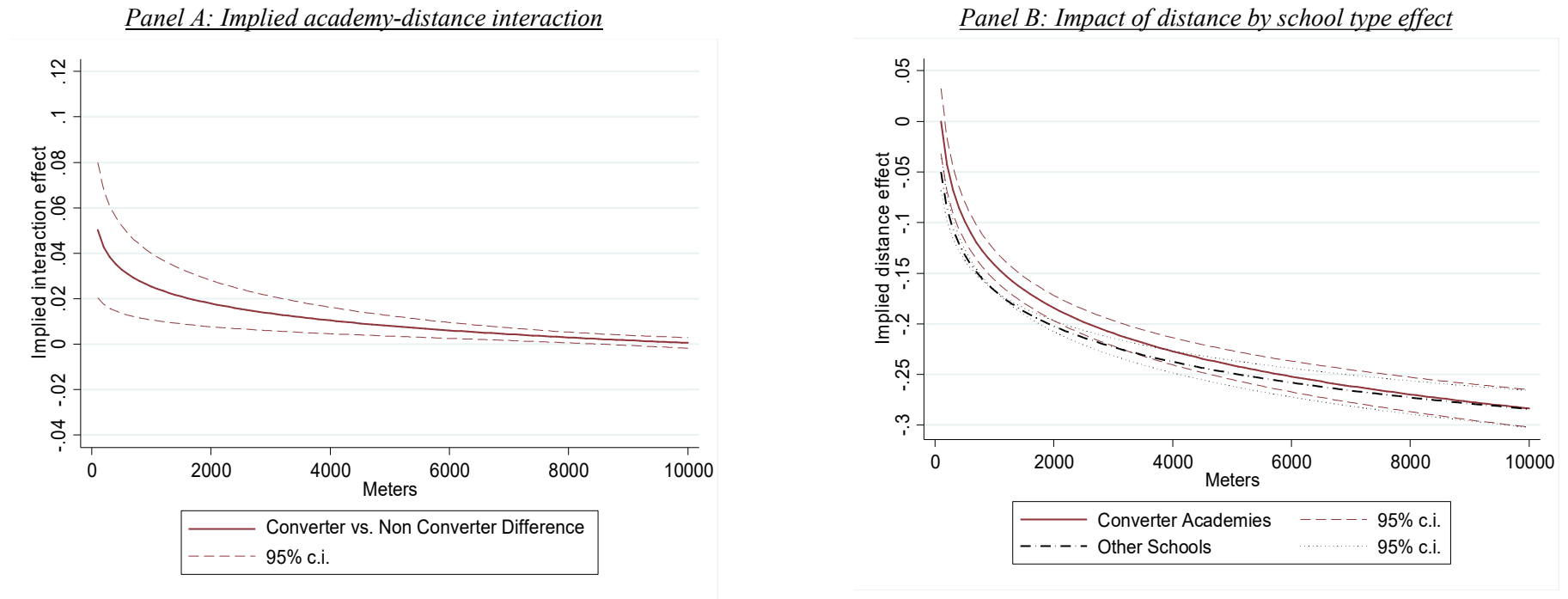
Note: The table reports coefficients and standard errors in parenthesis (clustered at the school level) on a dummy for academy conversion (multiplied by 100) and on the log of home-to-school distance. All regressions control for a variable identifying whether schools become 'sponsored' academies and for: (i) pupil and school effects; (ii) school time-varying controls, and (iii) school 'academy propensity' \times 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
<i>Panel A: School offer</i>						
Converter	0.059	0.019	0.189	-0.009	0.202	0.012
Academy	(0.033)*	(0.060)	(0.056)***	(0.046)	(0.067)***	(0.034)
<i>Implied highest preference effect</i>	7.11%	2.29%	22.77%	-1.08%	24.3%	1.45%
<i>Panel A: School attended</i>						
Converter	0.050	0.012	0.170	-0.011	0.188	0.006
Academy	(0.036)	(0.055)	(0.057)***	(0.043)	(0.073)**	(0.031)
<i>Implied highest preference effect</i>	5.95%	1.43%	20.2%	1.31%	22.4%	0.71%

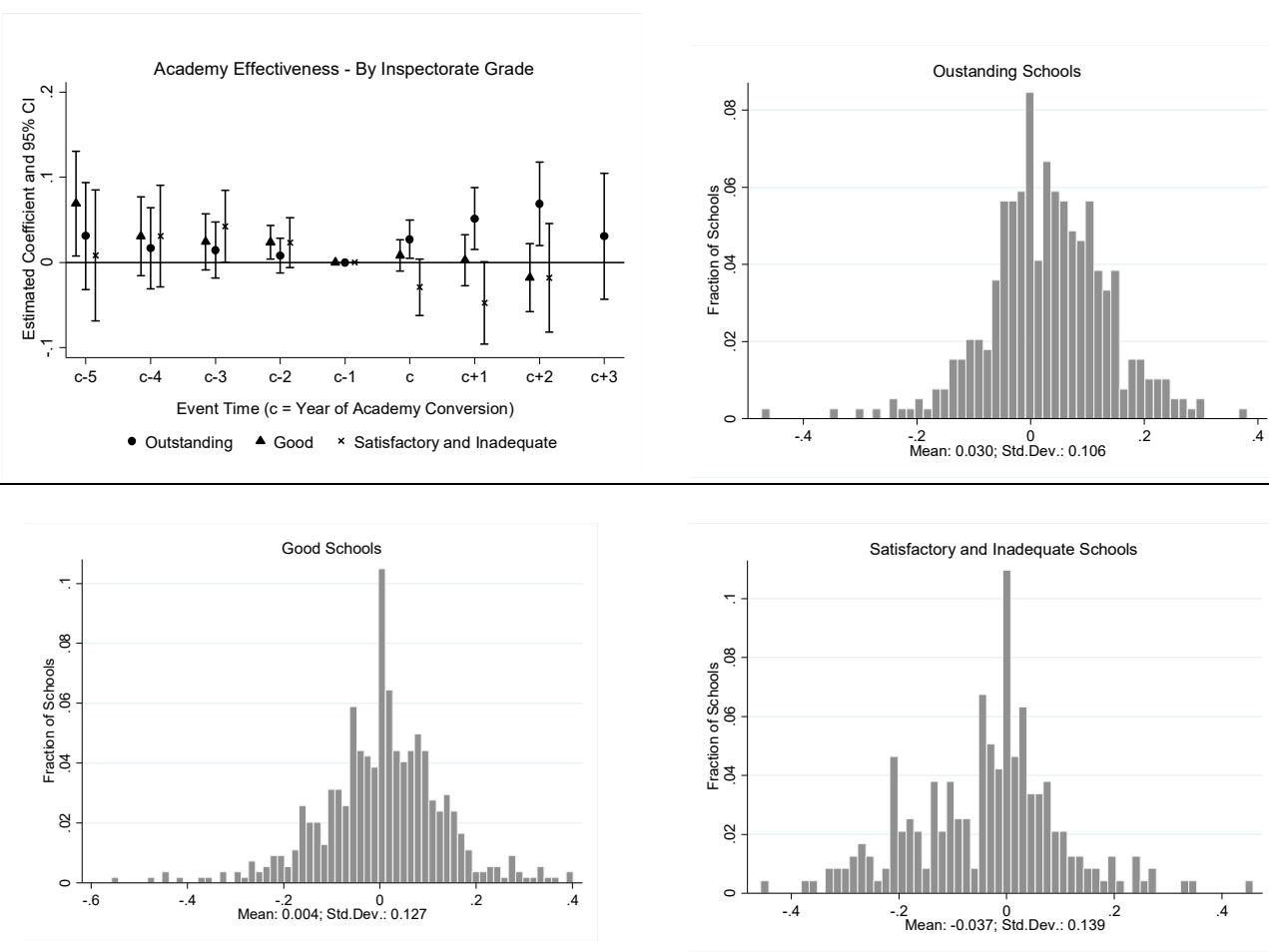
Note: The table reports coefficients and standard errors (clustered at the school level) in parenthesis (multiplied by 100). Coefficients and standard errors multiplied by 100. All regressions control for a variable identifying whether schools become 'sponsored' academies and for: (i) pupil and school effects; (ii) school time-varying controls, and (iii) school 'academy propensity' \times 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



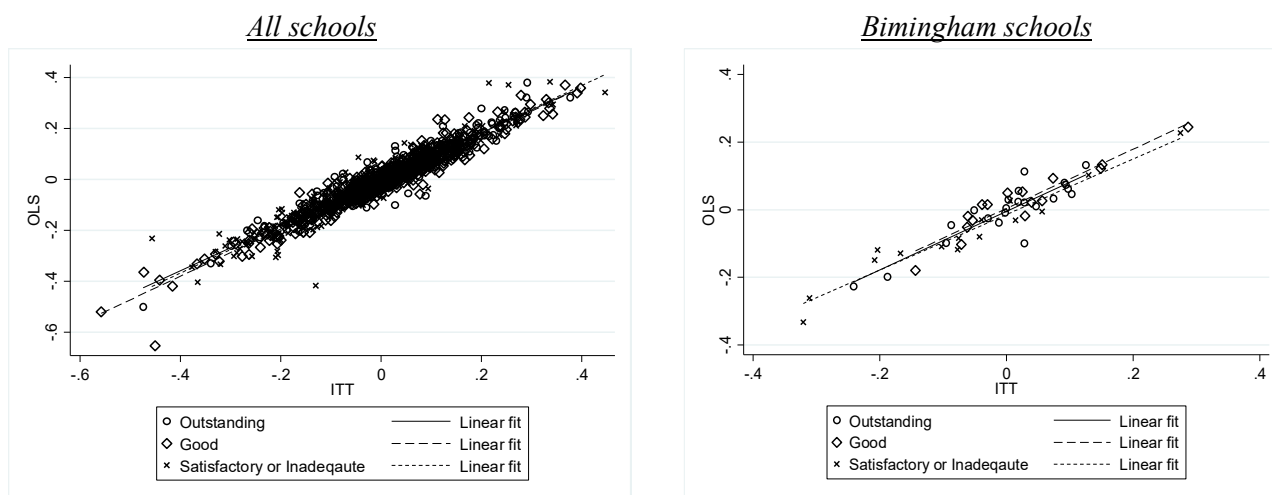
Note: The right hand side figure (Panel A) shows the effect of distance from schools on the probability that a school is top ranked for converters and non-converters. The left hand side figure (Panel B) shows the implied (interaction) effect of converter academies at varying pupil-to-school distances. Figures obtained from specification that: use the converters sample; including all controls and school 'academy propensity' \times 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. Schools 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.