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ABSTRACT

The Introduction of Academy Schools to England's Education*

We study the origins of what has become one of the most radical and encompassing programmes of school reform seen in the recent past amongst advanced countries – the introduction of academy schools to English secondary education. Academies are state schools that are allowed to run in an autonomous manner which is free from local authority control. Almost all academies are conversions from already existent state schools and so are school takeovers that enable more autonomy. Our analysis shows that this first round of academy conversions that took place in the 2000s generated significant improvements in the quality of pupil intake and in pupil performance. There is evidence of heterogeneity as improvements only occur for schools experiencing the largest increase in their school autonomy relative to their predecessor state. Analysis of mechanisms points to changes in headteachers and management structure as key factors underpinning these improvements in pupil outcomes.

JEL Classification: I20, I21, I28

Keywords: academies, pupil intake, pupil performance

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* This is a very substantively revised and overhauled version of a paper based only on school-level data previously circulated as "Changing School Autonomy: Academy Schools and Their Introduction to England's Education" (Machin and Vernoit, 2011). It should be viewed as the current version of that paper. We would like to thank the Department for Education for providing access to the pupil-level data we analyse and participants in a large number of seminars and conferences for helpful comments and suggestions.

1. Introduction

The introduction of academy schools to English education is turning out to be one of the most radical and encompassing programmes of school reform seen in the recent past amongst advanced countries. Unlike traditional community schools which are run by local authorities, academies are autonomous, state-funded schools that are managed and run outside the control of local authorities. In almost all cases, they are conversions of already existing predecessor schools and so they inherit pupils already enrolled in the school. They are school takeovers which, because of their nature, enable more autonomy in operation than in their predecessor state.[1] At the time of writing, nearly 2000 of England's secondary schools (or about 63 percent of schools) and a further 2300 (about 15 percent) of primary schools had become academies.[2] The vast majority became academies after a change of government in May 2010 quickly ushered in the 2010 Academies Act, a legislative change that widened the academies remit.[3]

School reforms that have taken place in many countries in the recent past – notably free schools in Sweden, and charter schools in the US - have proven to be an important dimension of the changing education landscape. Change has occurred in the context of some reforming nations being innovative in their attempts to get closer to what they perceive to be the optimal

[1] They are different from most US charter schools which are typically, though not always, set up from scratch. A closer comparison to the typical charter school in England is the free schools which are a recent addition to the education landscape and are new schools (often set up by parent or community groups). The closer US comparison to academies is 'in-district' charters where an already existent public school is converted to a charter as a school takeover – these are less commonplace than US charters as a whole, but there are places where conversions of public schools to charters have taken place (like Boston and New Orleans – see Abdulkadiroglu et al., 2014).

[2] In England, secondary schooling takes place from ages 11-16 and primary schooling from ages 5-11.

[3] Prior to the Act only secondary schools could become academies and to convert they were required to sign up a sponsor. Afterwards, primary schools were permitted to become academies, free schools were introduced and a sponsor was no longer required for conversion to take place. See Eyles, Hupkau and Machin (2015) for more details.

school type. At the same time in other countries education policies have been pursued with little deviation from the orthodox model of the traditional local or community school.[4]

The genesis of the English academies programme is what we study in this paper. The academy school model was initiated under the 1997-2010 Labour government when strong concerns were being expressed that schools in particular local authorities (usually serving urban inner-city disadvantaged neighbourhoods) were not delivering a good enough education to the children attending them. A widespread recognition emerged that something needed to be done, both to try to improve educational standards, and to confront significant behavioural problems, in these schools where it had been said that 'teachers had lost control of the corridors'. The proposed solution was to replace an existing school with a new type of state school to be run outside of local authority control and which was managed by a private team of independent co-sponsors. The sponsors of the new academy school delegate the management of the school to a largely self-appointed board of governors who have responsibility for employing all academy staff, agreeing levels of pay and conditions of service and deciding on the policies for staffing structure, career development, discipline and performance management.

We study the causal impact of academy school conversion on pupil intake and pupil performance. This line of enquiry is aimed at working out how the Labour academy programme functioned and impacted on pupils affected by the policy. To do so we consider data on pupils in schools over the school years 2000/01 to 2008/09 since this facilitates a before/after analysis of the impact of academy conversion.[5] Of course, as the discussion has already made clear, it was pupils in disadvantaged schools that participated in academy conversion and so we need to

[4] This is nowhere better illustrated in the cross-country differences highlighted in discussions of what kinds of schools do better or worse in the international test score data that has been a key resource in recent economics of education research (see OECD, 2011, or Hanushek and Woessmann, 2011).
[5] The school year in England runs from September through July.

define a credible control group of pupils attending schools that did not become academies in the sample period. We do so by comparing outcomes of interest for children enrolled in academy schools to pupils enrolled in a specific group of comparison schools, namely those state schools that go on to become academies after our sample period ends. We discuss the rationale for the empirical credibility of this and our methods in using this research design (together with threats to convincingly achieving identification) in more depth below. It turns out that this approach produces a well-balanced treatment and control group that differences out key observable and unobservable factors linked to conversion to academy status.

Because pupil composition may change before and after conversion to an academy, robust study of the causal impact of academy conversion on pupil performance needs to utilise an empirical strategy that is not contaminated by such change. The approach taken in this paper is to study performance effects for pupils who were already enrolled in the school prior to conversion and are then affected by academy conversion in subsequent years of their secondary schooling. Since the initial enrolment decision was made for the pre-conversion school, academy conversion should be exogenous to these students, and can be set up as in terms of an intention to treat empirical exercise, from which we can obtain a causal estimate of a local average treatment effect. In this setting, the intention to treat group is all pupils enrolled in the predecessor school who, irrespective of whether they actually do, are pre-conversion in line to take their year 11 KS4 exams in the school. The approach has similarities to that taken in Abdulkadiroglu et al. (2014), who study school takeovers in New Orleans, referring to pupils who stay in a converting school as ‘grand-fathered’ pupils.

Whilst we study a school transformation programme that is different in a number of dimensions to those that have been implemented elsewhere in the world, our work fits well with

two strands of economics of education research. The first is a growing literature that presents empirical estimates of the impact of school types on pupil achievement. For example, US work on charter schools tends to find achievement gains associated with charter status, and with the 'injection' of charter school features to public schools.[6] In the UK, a small body of work has identified the impact of specific school types on educational and labour market outcomes.[7] The second is a bigger and by now fairly long established literature on school types in the US and elsewhere. These include many studies on Catholic schools, on voucher-subsidised private schools and many analyses of the impact of school types using international test score data.[8]

In the next section, we discuss the structure of the secondary schooling system in England and document the rise of academies in the period we study. We also present a brief summary of related studies. Section 3 describes the data and the research designs we implement. Section 4 presents the main results on the effects of academy conversion on pupil intake and performance. We also report a number of robustness tests of our key findings. Section 5 hones in on mechanisms through studying the use of academy freedoms that underpin the reported results. We then offer conclusions to the paper in section 6.

[6] This literature is not without its own controversy. Recent, typically small scale, experimental evaluations of charters in or near particular US cities (Boston and New York) find positive impacts on educational achievement (see Abdulkadiroglu et al. 2011, 2014; Angrist et al. 2013; Dobbie and Fryer 2011; Hoxby and Murarka 2009). Wider coverage non-experimental evaluations produce more mixed results (Center for Research on Education Outcomes, 2009). On the injection of charter school features to public schools in Houston, and their beneficial effects, see Fryer (2014).

[7] See, for example, the Clark (2009) paper on schools becoming devolved from local authority control in the late 1980s and early 1990s or the work on private schools by Green et al. (2012).

[8] See, for example, Altonji, Elder and Taber (2005), Neal (1997) or Evans and Schwab (1995) for analysis of US Catholic schools or Hsieh and Urquiola (2006) for an analysis of the private school voucher programme in Chile. For evidence on school effects using international test score data see OECD (2011) and Hanushek and Woessmann, 2011, 2015).

2. Academy Schools

Academies were first introduced to English education in the early 2000s. In hindsight, their introduction can be viewed as a key development in the history of education in England.[9] This is firstly because changes in school type like those that have taken place for academies, and the scale of the academies programme, are rarely seen in education systems across the world. Secondly, the academies programme has been promoted and pursued with almost evangelical fervour by advocates, and run down with an equal lack of enthusiasm and stark criticism by detractors. Lord Adonis' (2012) book eloquently describes this. Adonis was the key player in government in setting up the Labour academies programme, and the more sceptical lines from those who oppose academies[10] make the controversial nature of the debate clear.

The first clutch of academies opened in the school year beginning in September 2002. Academies are independent, non-selective, state-funded schools that fall outside the control of local authorities. In most cases, they are conversions of already existing predecessor schools. Academies are managed by a private team of independent co-sponsors. The sponsors of the academy school delegate the management of the school to a largely self-appointed board of governors with responsibility for employing all academy staff, agreeing levels of pay and conditions of service and deciding on the policies for staffing structure, career development, discipline and performance management.

Secondary School Types in England and Academy Introductions

There are seven different school types that make up the English secondary education system: independent schools, academy schools, city technology colleges (CTCs), voluntary

[9] It is only England, and not in the other nations of the United Kingdom (Northern Ireland, Scotland and Wales) who run their own devolved education systems , where academies have been introduced.
[10] For example, the anti-academies alliance (see the website at http://antiacademies.org.uk).

aided schools, foundation schools, voluntary controlled schools and community schools. Each school type is characterised by a unique set of features regarding their autonomy and governance. This is shown in Table 1. In the Table the different school types are ordered by the amount of autonomy that their governing body/management body has, ranging from those with the most (private fee-paying independent schools that operate outside of the state sector) to those with the least (community schools that are largely operated under the remit of local authority control).

In the time period we study prior to the Academies Act of 2010 (which altered the definition of academy status in some important ways), the main impetus of the programme was to replace failing schools with academies with aim of generating school improvement by moving away from the conventional school type that had populated the English secondary sector in the past.[11] The path to establish an academy school in a local authority involved a number of steps. The key feature was the need to sign up a sponsor, who worked with the local authority (LA) where the school operates, and to complete a formal expression of interest (this made the case that an academy in the proposed area was both needed and feasible). The phase is completed when the LA and sponsor send the expression of interest to the Secretary of State for Education for his or her ministerial approval. After approval the process moves on to the feasibility stage and beyond that to actual conversion of the already existing school to an academy.

Table 2 shows the numbers of state-maintained English secondary schools of each school type in operation at the start and end of the eight year period beginning in the school year

[11] There were some other cases, for example where schools that already had more autonomy than a typical state community school became an academy, or as a means for fee-charging independent schools to broaden their intake of pupils by becoming academies (Department for Children, Schools and Families 2007), but as the numbers discussed below will show, these were the exception rather than the norm.

2001/02. The Table shows that by the 2008/09 school year, there were 133 academies open and operating. These had a gradual introduction, with the first three opening in the 2002/3 school year, and then in the subsequent school years as follows: 2003/04 - 9; 2004/05 - 5; 2005/06 - 10; 2006/07 - 20; 2007/08 - 36; 2008/09 - 50. The Table shows reductions in the other secondary school types as the share of academies rose to a share of 4 percent of the secondary sector by 2008/09.

In Table 3, we look in more detail at which types of school converted to academy status. The upper panel of the Table shows information on all schools that became academies, whilst the lower panel shows information on the school conversions on which we have full data available pre- and post-academy conversion. The main differences between the samples in the upper and lower panel is the small number of new academies (twelve of them), for which there is no predecessor school, and the five conversions from independent schools, for which we do not have predecessor school data.

Table 3 shows that that (at least) one school from every secondary school type converted to become an academy. However, the majority of conversions occurred in community schools - the 'typical' state school operating with the lowest levels of autonomy as outlined in Table 1. In the period we study, for the seven cohorts of converting academies we have data on 106 schools, and there are two cohorts of schools (comprising a total of 114 schools) that were approved to become academies, but their conversion occurs after the sample period ends in 2008/09.[12]

Related Literature

Whilst there is quite a lot of research on the impact of different school types on pupil enrolment and performance, there is much less that studies what happens when the type of

[12] For inclusion in our analysis, the approval of 'future' academies had to have taken place before May 2010, when the government changed and the new coalition introduced the Academies Act.

school attended by pupils changes. One study related this paper (albeit from an earlier time period, and in a rather different setting) is the analysis of grant-maintained (GM) schools[13] by Clark (2009) in England in the late 1980s and early 1990s. He utilises a regression-discontinuity design that exploits the fact that schools wishing to become GM schools were required to win the support of the parents with children who were enrolled at the school. He finds that the narrow GM vote winners experienced a significant improvement in pupil performance (of 0.25 of a standard deviation) compared to the narrow GM vote losers. Thus the change in school type brought about performance improvements.

In the US, the body of work on charter schools is relevant because, at least in some dimensions, charter schools have similarities to academies. Most (although not all) charters are new schools and so in this dimension the relevance is reduced. The more convincing studies in the charter school literature exploit the fact that some schools use lotteries to allocate places when the school is oversubscribed. Examples of this kind include: Abdulkadiroglu et al. (2011), who estimate the impact of charter attendance on student achievement using Boston data; Hoxby and Murarka (2009), which evaluates the effect of charter schools in New York City on their students' test scores; and Angrist et al. (2010), who evaluate the impact of a specific Charter School (in Lynn, Massachusetts) that is run by the Knowledge is Power Program (KIPP) and is targeted at low income and minority students.

The smaller number of studies of conversions of already existing schools to charters (as in the study of school takeovers in Boston and New Orleans by Abdulkadiroglu et al., 2014), or the introduction of practices used in charters to US public schools (as in Houston schools studied by Fryer, 2014) are of more direct relevance to the English case of academies. Some of

[13] GM schools were renamed as foundation schools (see Table 1) in the Schools Act 1998.

these report substantial improvements in test scores due to the use of methods of 'best practice'. In our analysis, we look at mechanisms in the case of academies and, as we will show, some of these overlap with the successful features of innovation in these in-district charters and school conversions/takeovers.

Many of the US experimental and quasi-experimental studies are relatively small scale in that their treatment group is often a small sample of schools (or even a single school in the case of Angrist et al., 2010). Interestingly, they do find positive effects for lotteried in pupils. Abdulkadiroglu et al. (2011) find that the lotteried in pupils experience significant improvements in their English language arts (ELA) scores and math scores at both middle and high schools, with effects being larger for the latter. Hoxby and Murarka (2009) also find that lotteried in pupils experience significant improvements in both their maths scores and reading scores between the third and eighth grade compared to the lotteried out pupils who remain in traditional public schools. Angrist et al. (2010) find that lotteried in students who attend KIPP Academy Lynn, a school that serves students in grades five through to eight, experience significant improvements in their maths scores and ELA scores. In a separate study, Dobbie and Fryer (2011) look at schools in Harlem in New York, with results being broadly similar results to those of Angrist et al. (2010).

An issue with the experimental studies is that lotteries only occur in the schools that are oversubscribed. Given that successful schools are more likely to be oversubscribed, estimates that exploit the lottery process are likely to be upper bounds. As an alternative, some studies adopt non-experimental methods to appraise the charter school model. They tend to produce more mixed results. For example, the CREDO (2009) study uses propensity score matching

methods finding charter school performance to be no better (or worse) than neighbouring traditional public schools.

One problem with non-experimental methods is how well they deal with selection bias compared to the lottery based estimates. An informative study that addresses this issue is by Hoxby and Murarka (2007). They estimate treatment effects for charter schools using both non-experimental methods and lottery based estimates, finding that their non-experimental estimates replicate their lottery-based estimates. Abdulkadiroglu et al. (2011) compare experimental lottery estimates with observational estimates on a large sample of Massachusetts schools, reporting positive urban charter school effects in both cases. However, Dobbie and Fryer (2013) report that observational estimates from New York schools give lower effect sizes than lottery estimates from the same sample of schools suggesting that the use of matching and regression alone may lead to downward bias.

On academies themselves, there remains very little rigorous research work. There are early studies of small numbers of converters by Machin and Wilson (2008) and evaluations by Price Waterhouse Coopers (PwC Report 2008), plus various National Audit Office reports.[14] Machin and Wilson (2008) looked at differences in pupil performance in academy schools compared to the performance of a matched group of schools, finding modest, statistically insignificant, relative improvements. The PwC Report (2008) reported higher percentage point increases in the results of academies compared to the national average (which is not a good comparison since academies are well below average performers in their predecessor state). Both

[14] See, for example, National Audit Office (2010, 2015) for reports on the Labour academies and the full Labour and Coalition academies programmes respectively. There is also some largely descriptive, non-causal school-level empirical work in the education field. See, for example, Gorard (2014) or West and Bailey (2013).

Machin and Wilson (2008) and PwC (2008) admitted their studies were in the early days of academy schools, so drawing any firm conclusions from their results is precipitous.

A National Audit Office (2010) report on the Labour academies looked at their performance compared to a selected group of maintained schools for academies converting in the 2002/03 to 2007/08 school years and who had been open for at least two years. Their comparison group comprised schools with similar pupil intakes and performance to the pre-treatment academies. They report a significant improvement in pupil performance in the academies compared to the comparison group, arguing the result to be driven by more advantaged pupils attending the academy post-conversion.

In our analysis reported on below we separately study intake and performance for this very reason, because we observe changes in the ability composition of pupils in terms of their prior academic achievement entering schools after they become academies. Thus we implement a research design studying performance effects only for children who were enrolled in the converting schools before they became academies (in the terminology of Abdulkadiroglu et al. (2014), who study school takeovers in New Orleans, these are 'grand-fathered' pupils). Since the initial enrolment decision was made for the pre-conversion school, academy conversion should be exogenous to these students, and therefore the study of pupil performance effects can be set up as in terms of an intention to treat empirical exercise, from which we can obtain a causal estimate of a local average treatment effect.

3. Data and Research Designs

Data

Our main source data source is the National Pupil Database (NPD).[15] The NPD is centrally collected census data containing pupil and school characteristics combined with the annual National Curriculum key stage attainment data at the pupil level. The Pupil Level Annual Census data (PLASC) contains information on characteristics of all pupils in the English maintained sector. This has been collected three times per year (January, May and September) from the 2001/02 school year onwards (though pupils can be traced back to earlier years of the key stage attainment data via their unique id). For this paper, we only use the year-on-year January collection because this collection is the most available and consistent over time.[16]

In England, compulsory education is organised around four key stages for years of schooling from ages 5 to 16. These are key stage 1 (in years 1 and 2) and key stage 2 (years 3 to 6) in primary school; and key stage 3 (years 7 to 9) and key stage 4 (years 10 and 11) in secondary school. In studying academy conversion impacts, our two outcomes of interest are pupil intake and pupil performance. To study intake for pupils enrolling in secondary school in year 7, the first year of secondary school, we look at the key stage test scores (KS2) that pupils take at the end of primary school (aged 10/11 at the end of year 6) before they make the transition to secondary school. To study performance in year 11, the final year of compulsory secondary schooling, we look at the key stage 4 (KS4) examinations that pupils take at the end

[15] The use of pupil-level data throughout and refined research design are the key innovations compared to the earlier version of this paper (Machin and Vernoit, 2011). Of course, use of pupil-level data (which the earlier version did not have full access to) makes the analysis more appropriate in that the right level of treatment is the effect of schools on the pupils that attend them compared to schools they would otherwise have attended. Put another way, changing pupil composition due to academy conversion because the demand for places alters compared to the predecessor school can render school-level estimates biased.

[16] See the Data Appendix for a detailed description of the sample constructions we use.

of compulsory schooling (aged 15/16 at the end of year 11). These school leaving exams are known as GCSEs (standing for the General Certificate of Secondary Education).

The impact of academy conversion needs to be analysed at the pupil-level. This is because the underlying composition of students attending schools may change over time (as we show, pupil intake does change post-conversion). To study intake, we match each pupil entering year 7 of a secondary school over the 2001/02 to 2008/09 academic years to their KS2 results over the 2000/01 to 2007/08 academic years. It is important to note that we allow for this intake change when identifying the causal effect of academy attendance on KS4 performance by focusing on pupils already enrolled in an academy pre-conversion thus avoiding endogeneity of the post-conversion enrolment decision.

One further practical issue concerns the definition of schools that convert to academies. There are a small number of examples where more than one predecessor school combines to create one academy school. Where this occurs, we create one hypothetical pre-academy school (see the discussion in the Data Appendix). This adopts hypothetical characteristics that are a weighted-average of the characteristics of the merged schools.

Modelling Approach

A conversion event c is defined as occurring in the school year t that the academy school starts operating (i.e. event year E(t = c) is when it 'opens for business' and admits new pupils as an academy). We then use the academic year that the academy status is awarded (and the years after) as the base that we need to calculate the quasi-experimental before/after conversion effect on the pupil-level outcomes of interest.

We have two outcomes of interest. The first is to study the impact of academy school conversion on the quality of pupil intake, which we measure in terms of ability composition by

the end of primary school standardised KS2 average points score[17] of pupils who enrol into year 7, the first year of secondary school. The second outcome, and the main outcome of interest in the paper, is the KS4 performance of pupils, measured as the standardised best 8 exams points score of individual year 11 students. We also consider robustness of the findings to different measures (the precise measures used for KS2 and KS4 are described in detail in the Data Appendix, together with additional performance results for a range of different KS4 measures).

Research Design – Quality of Pupil Intake

We begin by comparing what happens to pupil intake (measured by KS2 test scores of year 7 enrollers) before and after conversion for pupils attending schools that do and do not convert in the sample period. In the following equation for pupil i enrolled in year 7 in school s in year t, the key parameter of interest is the differences-in-differences coefficient δ:

$$KS2_{ist} = \alpha_s + \alpha_t + \delta A_{ist} * I(E \geq t = c) + \sum_{j=1}^{J} \lambda_{1j} X_{jist} + u_{1ist} \quad (1)$$

In (1) A is a dummy variable equal to 1 if the secondary school attended in the entry year of secondary school is in the treatment group (i.e. will become or is an academy in the sample period) and equals 0 if the school is in the comparison group (schools that do not convert to an academy in the sample period, but convert after the sample period ends). Defining E as an event year, the dummy variable indicator I(E ≥ t = c) takes a value 1 if the pupil enrols in conversion year c or after and X denotes a set of control variables. Finally, α_s denotes school fixed effects, α_t denotes year effects and u_1 is an error term.

[17] This is calculated by totalling (for each pupil) their raw scores in English, Maths and Science. We then average across the three before standardising to have mean zero and standard deviation one.

The specification in (1) imposes an average post-conversion effect across all post-conversion years. A more flexible specification estimates separate treatment effects for pre- and post-conversion years, in an event study setting, as:

$$KS2_{ist} = \alpha_s + \alpha_t + \sum_{e=c-4}^{e=c+3} \delta_e A_{ist} * I(E=e) + \sum_{j=1}^{J} \lambda_{2j} X_{jist} + u_{2ist} \quad (2)$$

We report event study estimates of four pre-conversion δ's (from E = c-4 to c-1) and four conversion year and post-conversion δ's (from E = c to c+3).

We also allow for heterogeneous effects by recognising that academies with different forms of predecessor school gain different amounts of autonomy when they convert. We consider differences by 'autonomy distance' by allowing effects to vary with the type of predecessor school. To do so, we estimate separate versions of (2) for academy conversions from community schools and conversions from non-community schools.[18] The presumption underpinning this is that the autonomy distance is largest for conversions that take place from predecessor community schools (see the earlier discussion around Table 3).

Research Design - Pupil Performance

To study pupil performance effects we look at the Key Stage 4 (KS4) performance of year 11 students. There are important identification issues that need to be considered here that did not apply to the KS2 intake part of our study. Specifically, there are three important dimensions of our empirical strategy that enable us to identify a causal effect of academy conversion on pupil performance:

i) We consider children whose parents made their decision to enrol their children in the academy before it converted. This ensures that academy conversion was exogenous to enrolment in secondary school.

[18] We limit the control group to community/non-community predecessor schools only in the respective regressions.

ii) We limit the event study on pupil performance to a maximum of four years post conversion, including the year of conversion itself. This is because children spend five years in secondary school. Thus children affected by conversion when enrolled in the predecessor school in their first year of secondary school, year 7, could have up to four post-conversion years of education in the academy (i.e. since their full enrolment in the school runs from E = c-1 up to E = c+3). Similarly children affected by conversion when enrolled in the predecessor school in year 8 could have up to three conversion years (to E = c+2), and so on for children in years 9 and 10 in the predecessor school. Thus the length of treatment varies depending on when conversion took place. Table A1 of the Data Appendix shows the structure of this treatment in more detail.

iii) Since the initial enrolment decision was made for the pre-conversion school, academy conversion should be exogenous to these students, and can be set up as in terms of an intention to treat (ITT) empirical exercise, from which we can obtain a causal estimate of a local average treatment effect (LATE). The ITT group is all pupils enrolled in the predecessor school who pre-conversion are lined up to take their year 11 KS4 exams in the school (i.e. year 7 students enrolled 4 years prior to conversion, year 8 students enrolled 3 years prior etc). The approach is similar to that taken in Abdulkadiroglu et al. (2014), who study school takeovers in New Orleans, referring to pupils who stay in a converting school as 'grand-fathered' pupils.

As we are interested in the causal impact of academy conversion on KS4 results we can first operationalise our empirical analysis by means of the following value added equation:

$$KS4_{ist} = \alpha_s + \alpha_t + \theta_1 A_{ist} * I(E \geq t = c) + \sum_{j=1}^{J} \pi_{1j} X_{jist} + \varphi_1 KS2_{ist} + v_{1ist} \qquad (3)$$

In (3) estimates of the θ_1 coefficient is analogous to the KS2 difference-in-difference set up above, but because we now restrict to pupils enrolled in the pre-conversion school there is a

subtle difference. This is that not all pupils who end up taking their KS4 exam at a school that becomes an academy ($A_{ist} = 1$) were enrolled in the school pre-conversion. Conversely, not all students initially enrolled in a school that converted to an academy ($ITT_{ist} = 1$) remain in the school to take their KS4 exams. Thus, ordinary least squares estimates of θ_1 from (3) will not reflect a causal estimate.

Defining the variable indicating treatment by an academy conversion as $Z_{ist} = A_{ist} * I(E \geq t = c)$ we account for selection into and out of treatment by using intention to treat status (ITT_{ist}) as an instrument for Z_{ist}, to estimate a LATE as follows:

$$Z_{ist} = \alpha_s + \alpha_t + \theta_2 ITT_{ist} * I(E \geq t = c) + \sum_{j=1}^{J} \pi_{2j} X_{jist} + \varphi_2 KS2_{ist} + v_{2ist} \quad (4)$$

$$KS4_{ist} = \alpha_s + \alpha_t + \theta_3 ITT_{ist} * I(E \geq t = c) + \sum_{j=1}^{J} \pi_{3j} X_{jist} + \varphi_3 KS2_{ist} + v_{3ist} \quad (5)$$

In the first stage in (4) the estimates of θ_2 show the proportion of the ITT group that stay in the academy and take KS4 exams there. These are the 'grandfathered' pupils that remain in the school. Equation (5) is the reduced form regression of KS4 results on the instrument. The instrumental variable (IV) estimate is the ratio of the reduced form coefficient to the first stage coefficient, θ_3 / θ_2.

Extending this IV setting to the event study framework we are able to estimate separate estimates for the four years from conversion onwards (E = c to c+3) using four instruments for whether a pupil is ITT for event year c, event year c+1 and so on.[19] Further extending to

[19] Formally, an individual enrolled in a treatment school in event year c-i and academic year group k is, for instance, intention to treat for c+1 if c-i + (11-k) is equal to c+1, where 11 is the academic year group in which KS4 exams are sat. The binary instrument in equation (4) is equal to 1 only if any one of the four instruments used for the event study equals 1.

estimate separately for community and non-community predecessor schools enables us to consider the impacts of autonomy distance associated with the conversion.

Comparison Schools

In Table 4, we compare average pre-treatment characteristics of academy schools and other types of maintained English secondary schools. It confirms that academies have significantly different characteristics from the other school types. This is true of pupil characteristics (like the proportion eligible for free school meals, the proportion white and the proportion with special educational needs) and of pupil performance (like the headline school leaving age measure of the proportion getting 5 or more A*-C GCSEs and equivalents and the Key Stage 2 primary school points score).

This is not at all surprising. The whole point of Labour's academy programme was to improve poorly performing schools. Thus, a naive comparison between academy schools and all other state-maintained schools is likely to suffer from significant selection bias. A related problem is that schools that go on to become academies may have common unobservable characteristics (e.g. they have a type of school ethos that is more in line with the academy model). Finally there is scope for mean reversion, as academies were badly performing schools in their predecessor state.

Looking in more detail within the group of academies it does, however, turn out that the schools that convert to academy status between 2002/03 and 2008/09 have very similar pre-treatment characteristics to the schools that later become academies. A set of balancing tests is given in the final row of the Table. One cannot reject the null hypothesis that the 106 academies that convert in the sample period and the 114 future academies have the same sets of characteristics. This partially legitimises our use of pupils attending future converters as a

control group in the D-i-D setting. It is further legitimised in the empirical findings we describe below where there are no differential pre-conversion trends in the same school years, thus allaying any concerns of mean reversion.

Thus the data structure we use is a balanced panel of schools for the school years 2000/01 to 2008/09 with repeated cross-sections of enrolled year 7 (for intake) and year 11 (for performance) pupils. Time variation in the academy conversion programme means that we can set these up in the event study framework detailed above. Table A2 of the Data Appendix shows the sample sizes for the different cohorts of academy schools in the KS2 and KS4 analyses that we undertake.

4. Empirical Results

Academies and Pupil Intake

In Table 5, we report results showing the effects of academy school conversion on the quality of pupil intake. The Table reports estimates from five different empirical specifications. We begin with the raw differences-in-differences estimate in column (1). We add time-varying controls in column (2). In column (3), we estimate heterogeneous effects in the event study setting, and in columns (4) and (5) we look at event study estimates for pupils in community and non-community predecessor schools respectively.

The estimated coefficients in the Table show that academies, post-conversion, attract pupils with significantly higher KS2 test scores than those schools that convert after our sample ends. Column (1) shows that, on average, pupils enrolling in an academy at year 7 have a KS2 mean points score that is 0.074 of a standard deviation (σ) higher than those attending schools

yet to attain academy status. The average intake effect falls to 0.058σ with the addition of the controls in column (2).

The event study estimates in column (3) show there to be no pre-conversion differences in trends between pupils in the treatment and control schools. They show a conversion year impact (E = c) of 0.010σ. This gradually rises year on year post conversion, becoming strongly significant in statistical terms, before reaching 0.082σ by event year c+3. These results suggest that (on average) there was a change in the pupil intake of schools when they converted to academy status. On conversion, academies began admitting higher ability pupils.

As shown in Figure 1, this positive impact grows over time, suggesting important compositional changes in the academies student body over time. Interestingly, the positive intake effects are only present for academies that convert from community predecessor schools (as shown in column (4)) where the (bigger) conversion year impact of 0.056σ is significant and rises to 0.200 σ by E = c+3. Figure 2 plots the event study estimates by predecessor type and the clear difference is evident.

Academies and Pupil Performance

Table 6 shows OLS, ITT and IV estimates of the impact of academy conversion on Key Stage 4 pupil performance for year 11 children. Columns (1) to (3) show estimates of the impact of academy conversion on end of secondary school KS4 pupil performance from specifications without control variables. Columns (4) to (6) show estimates from value added specifications that net out end of primary school KS2 pupil performance and include controls while columns (7) to (9) extend the (4) to (6) specifications to the event study setting.

The first point to note is that the estimates are broadly similar regardless of estimation method. The columns (1) to (3) specifications show that being in an academy school increases

pupil's KS4 standardised test scores by a statistically significant 0.082σ to 0.095σ. Adding the prior achievement measure (KS2) and control variables in columns (4) to (6) reduces this a little to 0.073σ to 0.080σ, which remains significant. Thus pupil achievement is significantly higher on average, and so is value added for pupils attending schools that converted to an academy.

The interpretation of the ITT estimate in column (5) of a significant 0.073σ improvement is that KS4 went up by 0.073σ more for children enrolled in a pre-conversion school as compared to children enrolled in control schools in the same school years. The IV estimate in column (6) corrects for the fact that not all ITT children sat their KS4 examinations in the school (in fact 93.2 percent did as the highly significant first stage at the bottom of the Table shows) and this rises to 0.079σ. This is the preferred baseline average impact estimate of academy conversion.

Columns (7)-(9) of Table 6 show the event study D-i-D estimates. These show no discernible pre-treatment trends, but a significant positive, and rising over time, impact after conversion. In the IV estimates of column (9), conversion year test scores are 0.037σ higher (though statistically insignificant), and this rises to (a statistically significant) 0.184σ four years post-conversion. Figure 3 very clearly shows the significant upturn after treatment and the lack of pre-conversion differences. It also makes it clear that academy conversion raised pupil performance, according to the causal IV estimates.

In Table 7 and Figure 4 we show separate IV estimates for pupils attending academies that converted from community and non-community schools respectively. Significant – and sizable – effects are seen for the former, whilst there is no improvement for the latter. These results reveal an important finding in terms of the overall interpretation of our results. They suggest that pupils attending schools experiencing the largest increase in autonomy via

conversion – those from predecessor community schools – were the only ones to experience performance improvements. The estimated effects are large, for example with a year of conversion effect in the IV estimates being 0.097σ that reaches 0.388σ by c+3.

Conversions from community schools enabled a gain of responsibility for the majority of the curriculum of the school (except the core subjects: English, Maths, Science and IT); the structure and length of the school day; the school budget and all staffing decisions. In the next section of the paper we look at which of these underlying mechanisms may have been behind the observed performance improvements. Prior to that, however, we consider some empirical extensions and study the robustness of the key findings.

Extensions and Robustness

Recall that the treatment effect we are estimating is time-varying because academy conversions occur in different school years 2002/03 through 2008/09 . Thus one extension we have considered is to estimate the most detailed KS4 models separately by cohort. Figure 5 plots IV estimates from the models separately by cohort.[20] It is very clear that a null hypothesis of the same average effects across cohorts is not rejected by the data. The gradually rising positive performance effects are seen across the four cohorts of conversions shown in the Figure.

The event study estimates uncover a significant improvement in performance that grows with more years post-conversion. This is not quite the same, though is strongly connected, to the years of exposure to academy treatment that children receive. The reason why is that a small number of pupils do not sit their KS4 exams in an academy school and are not intention to treat,

[20] It shows the equivalent to the Table 6 column (3) specification separately estimated for four year cohorts of conversions: these comprise the 10 conversions in school years 2002/03 and 20030/4 (3 from 2002/03 and 7 from 2003/04), the 12 conversions from school years 20040/5 and 2005/06 (2 from 2004/05 and 10 from 2005/06), the 45 conversions from 2006/07 and 2007/08 (15 from 2006/07 and 30 from 2007/08) and the 39 conversions from school year 2008/09. In each case they are compared to the control group of 114 schools that convert after the study sample period ends.

but are nevertheless exposed to treatment (i.e. they may enter post-conversion and leave prior to examinations). We have therefore reformulated the estimated models in terms of years of exposure to being taught in an academy. This involves defining the ITT variable and the treatment variable as years of exposure. Table 8 shows the results both for continuous and for dummy variable ITT and treatment years of exposure variables. It is evident that more years of exposure produces a bigger impact on pupil performance, and one that is of sizable magnitude for four years of exposure at 0.323σ in the academy conversions from predecessor community schools.

Next we consider a falsification test. This is a test of whether the estimated θ coefficients reflect pre-existing differences in the outcomes of interest for our treatment group compared to our control group. To do the falsification exercise, we altered the year in which each cohort of academy school became an academy to that of an earlier time period. We then re-estimated our models calculating the θ coefficients based on a 'fake' year (four years before) where we pretended schools converted to academies. If the θ coefficients in this falsification exercise give similar results to that of our original specification, then we would worry that the results of our original specifications reflect pre-existing differences in the outcomes of interest. To avoid any contamination when pupils attend schools that actually have converted, as oppose to attending during the 'fake' conversion, it is necessary for there to be no overlap, at the school level, between fake post-academy years and actual post-academy years. This means that we have to shorten the post-treatment fake periods for the first three academy cohorts. Thus the sample size

drops. We also lose 4 schools who do not have GCSE sittings for some of the earlier 1997/98-2000/01 period.[21]

The falsification exercise was conducted over the seven year period between the 1997/98 and 2004/05 academic years. Column (1) of Table 8 shows the results for all conversions, and column (2) just for conversions from community schools. In both cases the estimated θ coefficients for the academy conversion are always close to zero and statistically insignificant. This fake policy experiment does seem to rule out that our results are driven by pre-existing unobservables. However, as already noted, it was carried out on a slightly different sample and so in columns (3) and (4) of Tables 7, we report the original specifications for the same sample of schools. They are very similar to the main KS4 results of the paper.[22] The same is true when the value added specification adding in KS2 (which we are unable to do for the fake policy) is considered in columns (5) and (6).

We have also looked at other measures of KS4 performance. These are shown in Appendix Tables A2-A4. All models are comparable with those in Tables 6 and 7. If, rather than using the total points score, we consider the proportion getting 5 A*-C GCSEs (and their equivalents) and or the proportion getting 5 A*-C GCSEs (and their equivalents) but including GCSEs in Maths and English, we see a very similar pattern of results. It is evident that, with some subtleties, the same overall pattern of results is clear. Estimates that look separately at GCSEs and equivalents are somewhat noisy although marked effects for the equivalent qualifications and GCSEs are found in community converters.

[21] This is because some schools open post 1992/93 and so their first GSCE cohorts are post 1997/98 – sample sizes for the fake policy experiment are shown in Appendix Table A5.
[22] Pre-2002 observations (and hence our fake policy) have limited data on pupil characteristics. Therefore when we re-estimate our original model on the new sample of schools we omit those covariates that are missing pre-2002.

Finally, we considered a different measure of whether academisation under the Labour programme resulted in improved school performance by looking at Ofsted inspections of schools before and after conversion, again relative to control schools.[23] Table 9 shows transition matrices for treatment and control schools in the 2000s. These transitions constitutes movements in inspection rankings (of outstanding, good, satisfactory or inadequate) before and after academy conversion for academies in the early and late 2000s and the same for comparison schools. Not all schools were inspected twice in this period so we are forced to analyse a sub-set of schools.

The descriptive statistics in Table 9 show that academies were, on average, more likely to move up the rankings before and after conversion as compared to comparison schools. Ordered probit estimates reported in Table 10 confirm this and show a statistically significant improvement in inspection rankings of academies. We take this as complementary and corroborative evidence in line with the KS4 performance gains we have already reported.

5. Mechanisms

The above results uncovered evidenced of significant performance improvements for pupils treated by academy conversion. They also showed these improvements to be more pronounced for those attending schools that gained the greatest autonomy. We now address the question - what use of academy freedoms can account for these findings?

To begin the discussion of mechanisms, we first draw on the Department for Education's (2014) survey of academy schools 'Do Academies Make Use of Their Autonomy?'. This survey

[23] Ofsted is the Office for Standards in Education, Children's Services and Skills which is a government department of Her Majesty's Chief Inspector of Schools in England which undertakes inspections of schools as part of the strongly enforced school accountability system that operates in England.

collected information on a wide array of changes that may have occurred following conversion.[24] These are summarised in Table 11 for 23 of the Labour academies we analyse in this paper, and for 148 academies (including the 23) overall.

Table 11 ranks the responses in order of the percent making the particular change considered in the survey. The three most prominent changes, amongst the 23 converters in our sample, were 'changed school leadership', 'procured services that were previously provided by the local authority' and 'changed the curriculum you offer'. Over 75 percent of the schools said they made these changes pursuant to gaining the new academy freedoms. This ranking is broadly consistent with that of the 148 sponsored academies overall.

When asked what the most important change was, two answers dominate - 'changed school leadership' (at 56 percent) and 'changed the curriculum you offer' (at 26 percent). Furthermore, both of these were reported to be linked to improved outcomes (in 73 and 77 percent of cases respectively). Other changes that were notably linked to improved outcomes were 'Increased the length of the school day' (63 percent) and 'Collaborated with other schools in more formalised partnerships' (45 percent).

Looking at differences between treatment and control schools in the D-i-D event study offers further evidence. We can look at three of the important factors identified in Table 11: whether a new headteacher is taken on upon conversion; whether more pupils are enrolled; and whether more teachers are taken on. This is facilitated by the availability of school level data over time on each of these.

[24] In May 2013 the Department for Education sent a questionnaire to all 2919 open academies. Of the 720 respondents, 148 were sponsored academies, with 74 of these being secondary schools. Of the 74, 23 converted pre-May 2010 and thus were academies at some point in our sample period.

Table 12 reports results for headteacher change. Event study estimates show evidence of considerable headteacher turnover when a school converts, and that this is concentrated in the conversion year. In treatment schools, 59 percent more headteacher turnover occurred in the year of conversion c as compared to the control schools. This seems to be a one off change that occurs as the subsequent year treatment effects from c+1 to c+3 are all insignificantly different from zero. The rate of headteacher turnover is a little higher 62 percent in conversions from predecessor community schools, but is also high at 51 percent in predecessor non-community schools, showing that changing headteacher is a general and widespread feature of academy conversions.

Thus a strong feature of academy conversions is to replace the headteacher. There is a more modest turnaround of the rank and file teaching staff, and much of this is due to a need to take on more teachers as more pupils enrol in academies post conversion. This is shown from the results reported in Table 13. The Table shows event study D-i-D estimates of the effect of academy conversion on the number of teachers, number of pupils and the teacher-pupil ratio. Looking at columns (1)-(3) shows that the number of teachers rose gradually for event study years c+1 through c+3, although there was no significant effect in the year of conversion. This is because, as shown in columns (4)-(6), more pupils were enrolled as the academies were up and running, again with an insignificant change in the year of conversion, but with increases in pupil numbers by c+3. Finally, columns (7)-(9) show that the number of teachers increasing was largely due to increased pupil enrolments (except in the conversion year where the teacher-pupil ratio did rise, especially in conversions from predecessor community schools because of a blip down in pupil enrolments that year). Overall, however, the Table shows less clear evidence of

teacher turnover as compared to the very significant evidence of headteacher turnover shown in Table 12.

6. Conclusions

This paper focusses on what has become a high profile case of education policy – the introduction of academy schools into the English secondary school sector. We consider the impact of academy school conversion on pupil intake and performance. Academy conversion is seen to generate a significant improvement in the quality of pupil intake and significant improvements in pupil performance for those who attended schools treated by academy conversion. There is evidence of heterogeneity in the estimated performance effects as they occur only for schools experiencing the largest increase in their school autonomy relative to their predecessor state.

For children attending academies that converted from a community school we find that transformation to an academy raised their educational outcomes by 0.14σ on average, and by more for children receiving more years of treatment (rising to around 0.39 of a standard deviation three years post-conversion). These findings complement existing work from different settings like that on US charter schools (both newly set up and more closely to takeovers of public schools) on whether different school types can affect pupil performance.

In undertaking this empirical study, we offer new evidence about what happens when poorly performing disadvantaged schools convert to a new type of state school characterised by greater autonomy and flexible governance. Possible mechanisms underpinning the estimated pupil performance effects are investigated, both by studying survey evidence and by generating direct evidence from our samples of pupils and schools. Evidence suggests that factors

underpinning the improvements in pupil performance following conversion to academies are headteacher replacement/changes in management structure and curriculum change.

Before finishing, it is appropriate to place these findings into their policy context, especially given the very big and rapid education reforms that have occurred recently in England. We study the sponsored academies set up under the Labour government's programme, which set up 133 academies in the school years we study and which had 203 up and running in May 2010 when a new coalition government was voted in. Since then, the academies programme has been massively expanded and taken on a new direction, with the number of conversions skyrocketing and with new convertors not only being in the secondary sector, but also covering primary schools, and even reaching outside the state sector to some private schools. Moreover, the new coalition academies need not have a sponsor when they are converted. Mass academisation seems to be the order of the day in English education.

It is noteworthy that a key feature distinguishing these new coalition academies is that, on average, they are not characterised by poor performance and disadvantage in their predecessor state like the sponsored academies introduced and approved under the previous Labour government which we analyse in this paper.[25] The way some of them are run is also different with, for example, some of the post May 2010 academies being run as chains of schools by major sponsors. It will be an important future research challenge to determine whether or not these new convertor and chain run academies are able to deliver the kinds of performance improvements for students enrolling in them that the Labour programme we study here seemed to do.

[25] See Eyles, Machin and Silva (2015) for an empirical analysis of the different nature of pre- and post-May 2010 academies.

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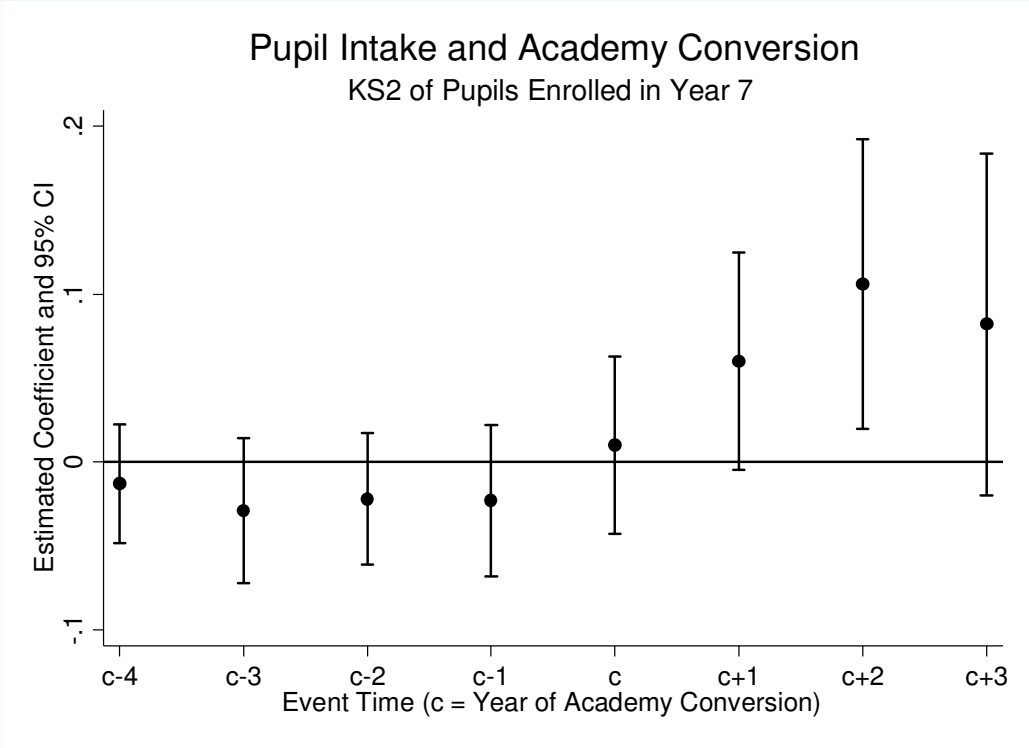
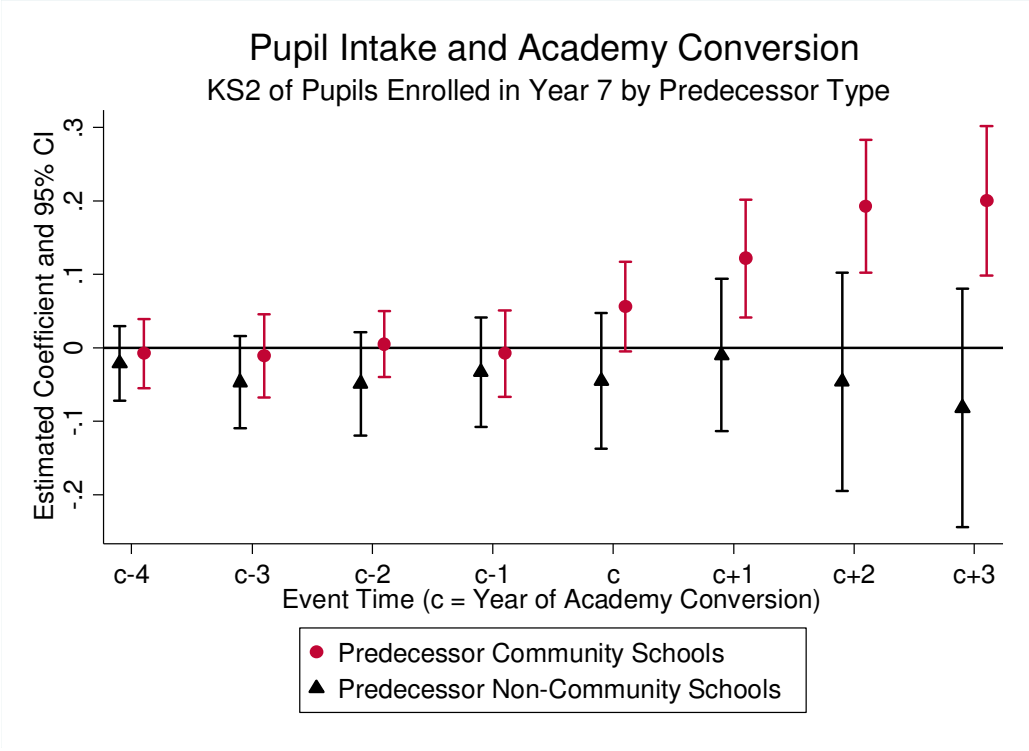
Figure 1: Event Study Estimates of Pupil Intake and Academy Conversion, Key Stage 2, Pupils Enrolled in Year 7



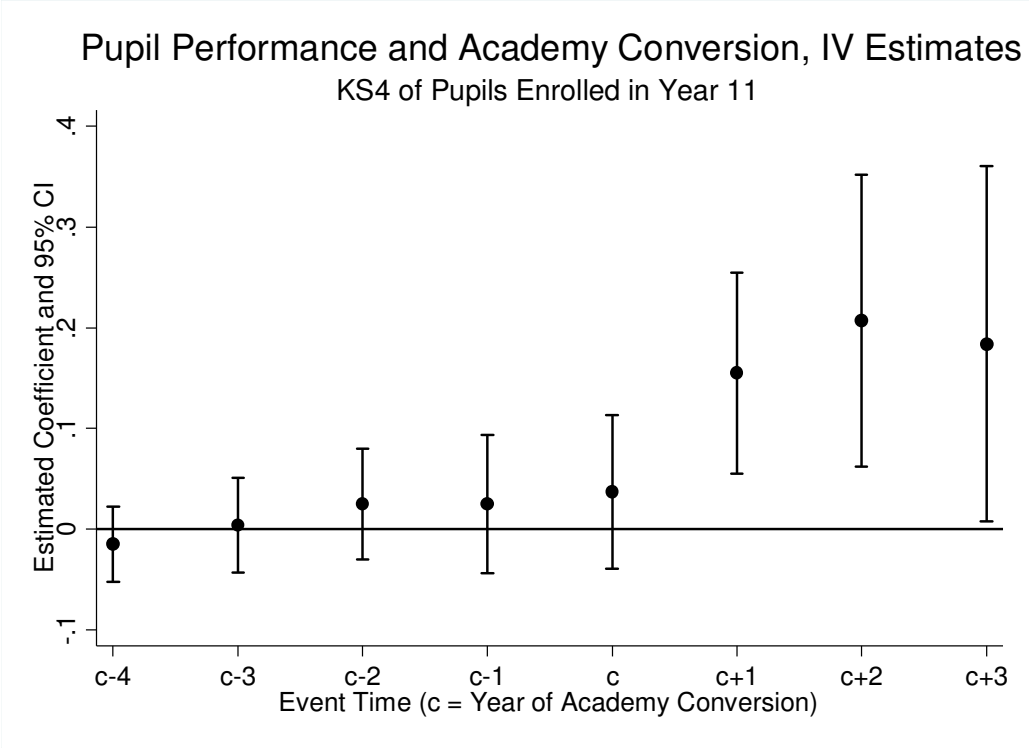
Notes: From columns (3) specification of Table 5.

Figure 2: Event Study Estimates of Pupil Intake and Academy Conversion, Key Stage 2, Pupils Enrolled in Year 7 By Predecessor Type



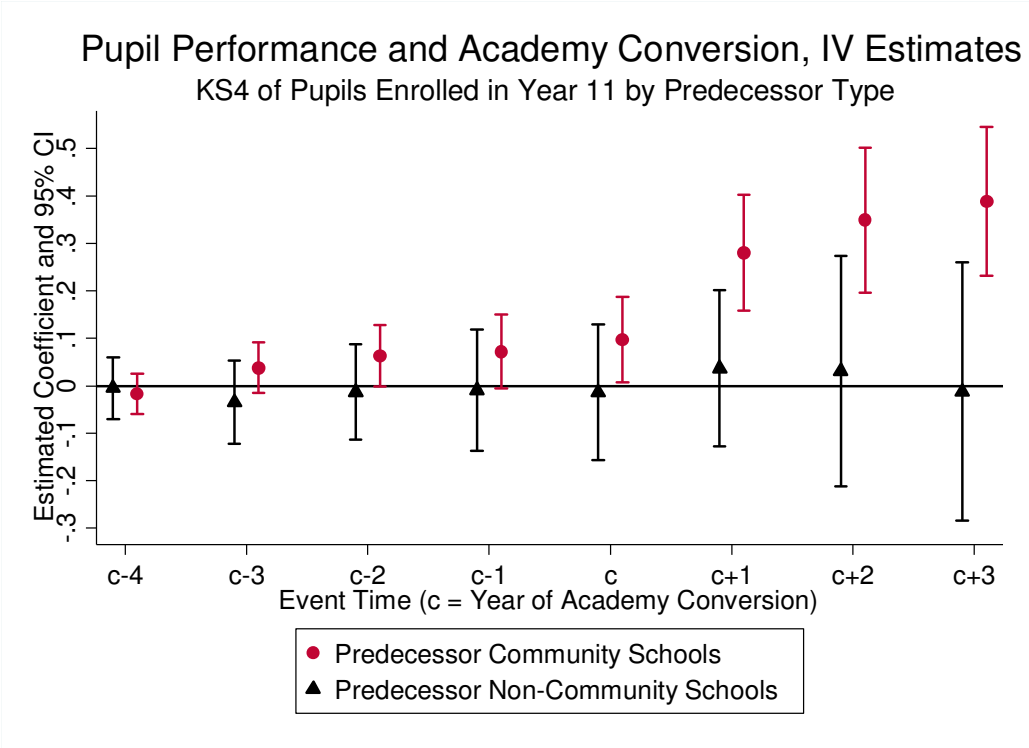
Notes: From columns (4) and (5) specifications of Table 5.

Figure 3: Event Study Instrumental Variable Estimates of Pupil Performance and Academy Conversion, Key Stage 4, Year 11 Pupils



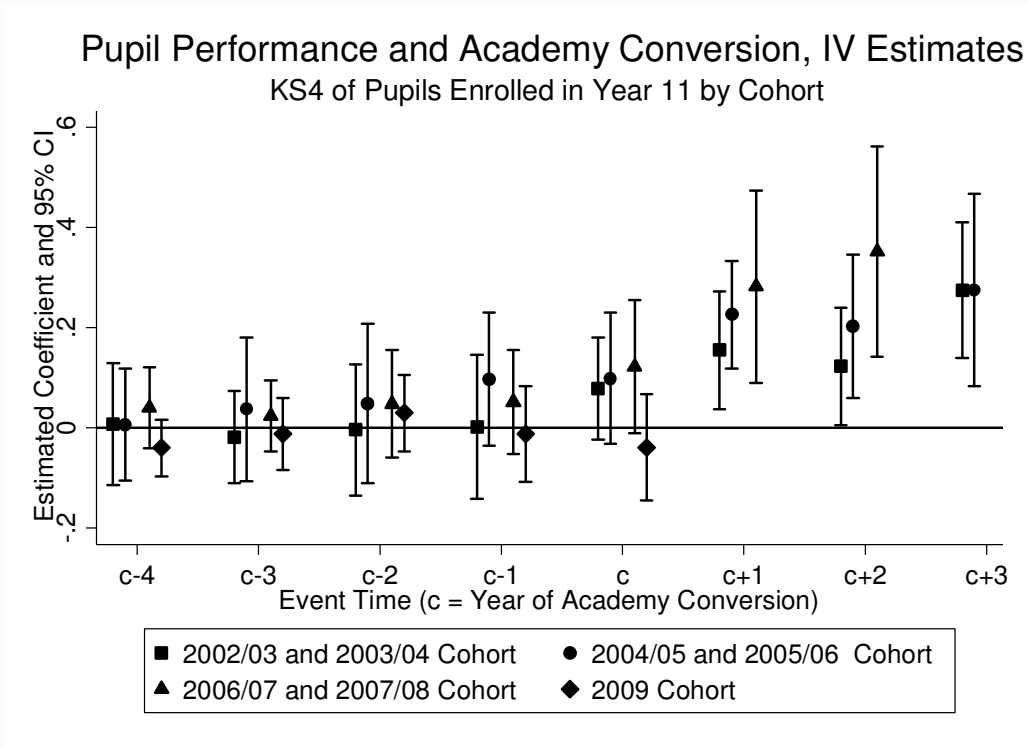
Notes: From column (9) specification of Table 6.

Figure 4: Event Study Instrumental Variable Estimates of Pupil Performance and Academy Conversion, Key Stage 4, Year 11 Pupils By Predecessor Type



Notes: From columns (2) and (4) specifications of Table 7.

Figure 5: Event Study Instrumental Variable Estimates of Pupil Performance and Academy Conversion, Key Stage 4, Year 11 Pupils By Cohort



Notes: From cohort specific estimates of column (9) specification of Table 6.

Table 1 - Characteristics of Autonomy and Governance in English Secondary Schools

	Non-LA Admission Authority	Maintained by Non-LA body	Not obliged to follow National Curriculum	Fee Charging
Registered independent school[a]	✓	✓	✓	✓
Academy[b]	✓	✓	✓	×
City technology college[c]	✓	✓	✓	×
Voluntary-aided[d]	✓	×	×	×
Foundation[e]	✓	×	×	×
Voluntary-controlled[f]	×	×	×	×
Community[g]	×	×	×	×

Notes:
a - Registered independent schools are independent of the local authority (LA), and are fee-charging.
b - Academy schools (prior to 2010/11): all ability independent specialist schools, which do not charge fees, and are not maintained by the local authority; established by sponsors from business, faith, HE institutions or voluntary groups, working in partnership with central government. Sponsors and the DfE provide the capital costs for the Academy. Running costs are met by the DfE in accordance with the number of pupils, at a similar level to that provided by local authorities for maintained schools serving similar catchment areas.
c - City Technology Colleges: all ability independent schools, which do not charge fees, and are not maintained by the local education authority. Their curriculum has a particular focus on science and technology education (see West and Bailey, 2013). They were established by sponsors from business, faith or voluntary groups. Sponsors and the DfE provided the capital costs for the CTC. Running costs are met by the DfE in accordance with the number of pupils, at a similar level to that provided by local authorities for maintained schools serving similar catchment areas.
d – Voluntary-aided schools are maintained by the local authority. The foundation (generally religious) appoints most of the governing body. The governing body is responsible for admissions and employing the school staff. Land at voluntary-aided schools is usually owned by trustees, although the local authority often owns any playing field land (DfE, 2012).
e - Foundation (formerly grant-maintained) schools are maintained by the local authority. The governing body is responsible for admissions, employing the school staff, and either the foundation or the governing body owns the school's land and buildings (DfE, 2013).
f – Voluntary-controlled schools are maintained by the local authority. These are mostly religious schools where the local authority continues to be the admission authority. Land at voluntary-controlled schools is usually owned by trustees, although the local authority often owns any playing field land (DfE, 2013).
g - Community schools are maintained by the local authority. The local authority is responsible for admissions, employing the school staff, and it also owns the school's land and buildings.

Table 2 - Number (Percent) of Secondary Schools in England, 2001/02 and 2008/09

	Number (Percent) of Secondary Schools by Type	
	2001/02	2008/09
Academy	0 (0.0)	133 (4.0)
City technology college	14 (0.4)	3 (0.1)
Voluntary aided	549 (15.8)	537 (16.0)
Foundation	501 (14.4)	560 (16.7)
Voluntary controlled	129 (3.7)	111 (3.3)
Community	2278 (65.6)	2017 (59.9)
Total	3471	3361

Notes: Source – School Census. Includes middle schools. Excludes special schools. This is partially available from Tables 2.1 and 2.2 in http://webarchive.nationalarchives.gov.uk/20120504203418/http://education.gov.uk/rsgateway/DB/VOL/v000359/dfes_schools_final.pdf and Table 2a in http://www.education.gov.uk/rsgateway/DB/SFR/s000925/sfr09-2010.pdf.

Table 3: The Nature of Academy Conversions

All Schools								
Pre-Academy School Type								
	All	New	Independent	City technology college	Voluntary aided	Foundation	Voluntary controlled	Community
All academies	244	12	5	12	18	34	2	161
Become academies, up to 2008/09	133	12	5	12	10	15	1	78
Future academies, after 2008/09	111	0	0	0	8	19	1	83
All Schools With Full Data (Pre- and Post-Academy Conversion)								
Pre-Academy School Type								
	All	New	Independent	City technology college	Voluntary aided	Foundation	Voluntary controlled	Community
All academies	220	0	0	12	15	33	2	158
Become academies, up to 2008/09	106	0	0	12	10	15	1	68
Future academies, after 2008/09	114	0	0	0	5	18	1	90

Notes: Source for upper panel, same as Table 2. Source for lower panel, own calculations from Edubase, School Performance Tables and Annual Schools Census.

Table 4: Pre-Conversion Characteristics and Tests of Balancing

	Key stage 2 points score (mean)	Key stage 4 points score (mean)	Proportion getting 5 or more A*-C GCSEs or equivalents(mean)	Proportion male	Proportion white	Proportion eligible for free school meals	Proportion special educational needs	Number of Schools
A. All Schools								
City technology college	74.786	58.983	0.934	0.483	0.945	0.077	0.108	2
Voluntary aided	66.687	44.079	0.590	0.505	0.791	0.149	0.166	504
Foundation	65.453	43.766	0.580	0.524	0.835	0.113	0.184	473
Voluntary controlled	66.676	43.999	0.579	0.510	0.885	0.104	0.169	97
Community	61.997	38.684	0.464	0.505	0.833	0.186	0.219	1975
Academies (Pre-conversion)	57.201	32.140	0.346	0.530	0.753	0.323	0.277	106
B. Academy Schools								
Current academies (treatment group)	57.201	32.140	0.346	0.530	0.753	0.323	0.277	106
Future academies (control group)	56.589	31.344	0.313	0.517	0.813	0.288	0.263	114
Difference	0.612 (0.920)	0.796 (1.011)	0.033 (0.025)	0.013 (0.015)	-0.060 (0.035)	0.035 0.022)	0.014 (0.020)	

Notes: Standard errors clustered at school level reported in parentheses. Both panels refer to characteristics in the 2001/02 school year. The top panel is maintained schools in the UK, which do not convert to academies prior to, or in, the academic year 2008/09. Minor discrepancies between this Table and Table 2 are due to the removal of approximately 300 middle schools. All variables with the exception of KS4 points score and the proportion achieving five or more A*-Cs refers to characteristics of the incoming 2001/02 cohort i.e. incoming pupils in the school year 2001/02, before any academies had opened.

Table 5: Pupil Intake, Key Stage 2, Enrolled in Year 7, 2000/01 to 2008/09

	Key Stage 2 (Standardised)				
		Pupils in All Schools		Pupils in Community Predecessor School	Pupils in Non-Community Predecessor School
	(1)	(2)	(3)	(4)	(5)
Academy x Post-Conversion (E = c to c+3)	0.074 (0.023)	0.058 (0.021)			
Academy x (E = c-4)			-0.013 (0.018)	-0.008 (0.024)	-0.021 (0.026)
Academy x (E = c-3)			-0.029 (0.022)	-0.011 (0.029)	-0.047 (0.032)
Academy x (E = c-2)			-0.022 (0.020)	0.005 (0.023)	-0.049 (0.036)
Academy x (E = c-1)			-0.023 (0.023)	-0.008 (0.030)	-0.033 (0.038)
Academy x (E = c)			0.010 (0.027)	0.056 (0.031)	-0.045 (0.047)
Academy x (E = c+1)			0.060 (0.033)	0.122 (0.041)	-0.010 (0.053)
Academy x (E = c+2)			0.106 (0.044)	0.193 (0.046)	-0.046 (0.076)
Academy x (E = c+3)			0.082 (0.052)	0.200 (0.052)	-0.082 (0.083)
School Fixed Effects	Yes	Yes	Yes	Yes	Yes
Control Variables	No	Yes	Yes	Yes	Yes
Year Dummies	Yes	Yes	Yes	Yes	Yes
R-Squared	0.087	0.332	0.332	0.310	0.376
Sample Size	353077	353077	353077	250222	102855
Number of Treatment and Control Schools	220	220	220	158	62

Notes: E denotes event year and c is the year of conversion. Robust standard errors (clustered at the school level) are reported in parentheses. Control variables are dummies for whether the pupil is male, the pupil's ethnicity group, whether they are eligible for free school meals and whether they have special educational needs.

Table 6: Pupil Performance, Key Stage 4, Year 11, 2000/01 to 2008/09

	Key Stage 4 Points Score (Standardised)								
	OLS	ITT	IV	OLS	ITT	IV	OLS	ITT	IV
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Academy x Post-Conversion (E = c to c+3)	0.095 (0.026)	0.082 (0.024)	0.088 (0.026)	0.080 (0.027)	0.073 (0.025)	0.079 (0.027)			
Academy x (E = c-4)							-0.016 (0.018)	-0.025 (0.018)	-0.015 (0.019)
Academy x (E = c-3)							0.002 (0.024)	-0.010 (0.022)	0.004 (0.024)
Academy x (E = c-2)							0.023 (0.028)	0.009 (0.026)	0.025 (0.028)
Academy x (E = c-1)							0.023 (0.034)	0.008 (0.032)	0.025 (0.035)
Academy x (E = c)							0.029 (0.038)	0.022 (0.034)	0.037 (0.039)
Academy x (E = c+1)							0.146 (0.050)	0.128 (0.043)	0.155 (0.051)
Academy x (E = c+2)							0.219 (0.071)	0.172 (0.061)	0.207 (0.074)
Academy x (E = c+3)							0.192 (0.080)	0.148 (0.074)	0.184 (0.090)
KS2 Standardised Test Score				0.523 (0.005)	0.523 (0.005)	0.523 (0.005)	0.523 (0.005)	0.523 (0.005)	0.523 (0.005)
School Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Control Variables	No	No	No	Yes	Yes	Yes	Yes	Yes	Yes
Year Dummies	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R-Squared	0.123	0.123	0.123	0.482	0.482	0.482	0.482	0.482	0.482
Sample Size	362424	362424	362424	362424	362424	362424	362424	362424	362424
Number of Treatment and Control Schools	220	220	220	220	220	220	220	220	220
First Stage Coefficient on ITT			0.932 (0.007)			0.932 (0.007)			
First Stage F-Test			8115			3973			
First Stage Coefficient on ITT x (E = c)									0.952 (0.004)
First Stage F-Test									33071
First Stage Coefficient on ITT x (E = c+1)									0.905 (0.007)
First Stage F-Test									4034
First Stage Coefficient on ITT x (E = c+2)									0.885 (0.011)
First Stage F-Test									1733
First Stage Coefficient on ITT x (E = c+3)									0.875 (0.015)
First Stage F-Test									619

Notes: E denotes event year and c is the year of conversion. Robust standard errors (clustered at the school level) are reported in parentheses. Control variables included are the same as from the Table 5 regressions, although in specifications including KS2 test scores we now additionally include a separate intercept for pupils for whom KS2 data is unavailable. For children who move out of treatment or control schools to take their KS4, school fixed effects (1714) for the school they move to are also included.

Table 7: Pupil Performance, Key Stage 4, Year 11, 2000/01 to 2008/09, by Predecessor School Type

	Key Stage 4 Points Score (Standardised)			
	Pupils in Community Predecessor School		Pupils in Non-Community Predecessor School	
	IV	IV	IV	IV
	(1)	(2)	(3)	(4)
Academy x Post-Conversion (E = c to c+3)	0.143 (0.032)		0.017 (0.043)	
Academy x (E = c-4)		-0.017 (0.022)		-0.005 (0.033)
Academy x (E = c-3)		0.038 (0.027)		-0.035 (0.045)
Academy x (E = c-2)		0.063 (0.033)		-0.013 (0.051)
Academy x (E = c-1)		0.072 (0.040)		-0.009 (0.065)
Academy x (E = c)		0.097 (0.046)		-0.014 (0.073)
Academy x (E = c+1)		0.280 (0.062)		0.037 (0.084)
Academy x (E = c+2)		0.349 (0.078)		0.031 (0.124)
Academy x (E = c+3)		0.388 (0.080)		-0.012 (0.139)
KS2 Standardised Test Score	0.533 (0.006)	0.533 (0.006)	0.495 (0.009)	0.495 (0.009)
School Fixed Effects	Yes	Yes	Yes	Yes
Control Variables	Yes	Yes	Yes	Yes
Year Dummies	Yes	Yes	Yes	Yes
R-Squared	0.454	0.455	0.521	0.521
Sample Size	258409	258409	104015	104015
Number of Treatment and Control Schools	158	158	62	62
First Stage Coefficient on ITT	0.926 (0.009)		0.937 (0.012)	
First Stage F-Test	2342		7648	
First Stage Coefficient on ITT x (E = c)		0.947 (0.005)		0.960(0.005)
First Stage F-Test		21044		24646
First Stage Coefficient on ITT x (E = c+1)		0.893 (0.010)		0.918 (0.010)
First Stage F-Test		2099		11290
First Stage Coefficient on ITT x (E = c+2)		0.879 (0.014)		0.893 (0.016)
First Stage F-Test		883		3235
First Stage Coefficient on ITT x (E = c+3)		0.863 (0.019)		0.885 (0.022)
First Stage F-Test		333		1137

Notes: As for Table 6.

Table 8: Pupil Performance, Key Stage 4, Year 11, 2000/01 to 2008/09, Years of Exposure

	Key Stage 4 Points Score (Standardised)					
	Pupils in All Schools		Pupils in Community Predecessor School		Pupils in Non-Community Predecessor School	
	IV	IV	IV	IV	IV	IV
	(1)	(2)	(3)	(4)	(5)	(6)
Total Years of Exposure to Academy	0.054 (0.017)		0.095 (0.016)		0.012 (0.026)	
One Year of Exposure to Academy		0.025 (0.024)		0.061 (0.031)		-0.001 (0.040)
Two Years of Exposure to Academy		0.141 (0.041)		0.237 (0.050)		0.050 (0.057)
Three Year of Exposure to Academy		0.189 (0.064)		0.297 (0.067)		0.048 (0.101)
Four Years of Exposure to Academy		0.161 (0.079)		0.323 (0.064)		0.005 (0.115)
KS2 Standardised Test Score	0.523 (0.005)	0.523 (0.005)	0.533 (0.006)	0.533 (0.006)	0.495 (0.009)	0.495 (0.009)
School Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Control Variables	Yes	Yes	Yes	Yes	Yes	Yes
Year Dummies	Yes	Yes	Yes	Yes	Yes	Yes
R-Squared	0.481	0.481	0.454	0.454	0.521	0.523
Sample Size	363116	363116	258989	258989	104127	104127
Number of Treatment and Control Schools	220	220	158	158	62	62
First Stage Coefficient on ITT Years of Exposure	0.880(0.013)		0.869 (0.017)		0.894 (0.021)	
First Stage F-Test	1070		813		1620	
First Stage Coefficient on ITT x One Year of Exposure		0.951 (0.004)		0.946 (0.005)		0.961 (0.005)
First Stage F-Test		9443		5445		6994
First Stage Coefficient on ITT x Two Years of Exposure		0.907 (0.008)		0.895 (0.010)		0.924 (0.011)
First Stage F-Test		861		482		1238
First Stage Coefficient on ITT x Three Years of Exposure		0.900 (0.011)		0.893 (0.015)		0.914 (0.015)
First Stage F-Test		452		323		451
First Stage Coefficient on ITT x Four Years of Exposure		0.913 (0.014)		0.905 (0.019)		0.924 (0.019)
First Stage F-Test		322		235		297

Notes: As for Table 6. The sample size differences compared to Tables 6 and 7 are because a small number of pupils do not sit their KS4 exams in an academy school and are not intention to treat but are nevertheless exposed to treatment i.e. they may enter post-conversion and leave prior to examinations. Restricting the sample to pupils appearing in both the exposure and event study samples gives us 362412 observations. Running the specifications in Tables 6 and 7 on this sample makes no difference to the reported results.

Table 9: Fake Policy (t-4) For Pupil Performance, Key Stage 4, Year 11

	Key Stage 4 Points Score						
	Fake Policy, 1997/98 to 2004/05			. Original Specifications For Pupils in Fake Policy Sample of Schools, 2000/01 to 2008/09			
	Pupils in All Schools	Pupils in Community Predecessor School		Pupils in All Schools	Pupils in Community Predecessor School	Pupils in All Schools	Pupils in Community Predecessor School
	(1)	(2)		(3)	(4)	(5)	(6)
Academy x (E = c-8)	-0.022 (0.019)	-0.023 (0.026)	Academy x (E = c-4)	-0.012 (0.018)	-0.013 (0.022)	-0.014 (0.018)	-0.019 (0.022)
Academy x (E = c-7)	-0.030 (0.022)	-0.041 (0.027)	Academy x (E = c-3)	0.006 (0.025)	0.036 (0.027)	0.006 (0.024)	0.036 (0.027)
Academy x (E = c-6)	-0.024 (0.026)	-0.033 (0.030)	Academy x (E = c-2)	0.030 (0.028)	0.060 (0.032)	0.022 (0.028)	0.054 (0.033)
Academy x (E = c-5)	-0.011 (0.029)	-0.012 (0.033)	Academy x (E = c-1)	0.013 (0.035)	0.052 (0.038)	0.018 (0.035)	0.058 (0.040)
Academy x (E = c-4)	-0.021 (0.032)	-0.029 (0.035)	Academy x (E = c)	0.042 (0.038)	0.092 (0.043)	0.027 (0.038)	0.078 (0.045)
Academy x (E = c-3)	0.000 (0.037)	0.005 (0.042)	Academy x (E = c+1)	0.162(0.050)	0.260 (0.059)	0.145 (0.051)	0.250 (0.063)
Academy x (E = c-2)	0.014 (0.044)	0.001 (0.051)	Academy x (E = c+2)	0.240 (0.063)	0.316 (0.068)	0.227 (0.072)	0.335 (0.078)
Academy x (E = c-1)	0.035 (0.053)	-0.035 (0.050)	Academy x (E = c+3)	0.293 (0.073)	0.415 (0.081)	0.225 (0.073)	0.367 (0.071)
P-value for test of joint significance of Fake Policy (c-8 to c-1)	0.663	0.707					
KS2 Standardised Test Score						0.524 (0.005)	0.534 (0.006)
School Fixed Effects	Yes	Yes		Yes	Yes	Yes	Yes
Limited Control Variables	Yes	Yes		Yes	Yes	Yes	Yes
Full Control Variables	No	No		No	No	Yes	Yes
Year Dummies	Yes	Yes		Yes	Yes	Yes	Yes
R-Squared	0.140	0.079		0.130	0.072	0.480	0.456
Sample Size	301675	218171		357411	256534	357411	256534
Number of Treatment and Control Schools	216	156		216	156	216	156

Notes: E denotes event year and c is the year of conversion. Robust standard errors (clustered at the school level) are reported in parentheses. Compared to OLS estimates in Tables 6 and 7, control variables for specifications (1) and (2) are limited for the Fake Policy time period and comprise solely of gender.

Table 10: Ofsted Inspection Ratings Transition Matrices, Inspections in the 2000s

Current Academies

		Post-Conversion (Second Inspection)				
		Outstanding	Good	Satisfactory	Inadequate	Total
Before Conversion (First Inspection)	Outstanding	5	2	0	1	8
	Good	2	2	4	3	11
	Satisfactory	1	3	8	2	14
	Inadequate	2	6	5	0	13
	Total	10	13	17	6	46

Percent Improvement in Ranking = 41
Percent No Change in Ranking = 33
Percent Reduction in Ranking = 26

Future Academies

		Post-Conversion (Second Inspection)				
		Outstanding	Good	Satisfactory	Inadequate	Total
Before Conversion (First Inspection)	Outstanding	2	1	1	0	4
	Good	0	5	27	6	38
	Satisfactory	1	9	28	6	44
	Inadequate	1	6	10	2	19
	Total	4	21	66	14	105

Percent Improvement in Ranking = 26
Percent No Change in Ranking = 35
Percent Reduction in Ranking = 39

Notes: For schools with two OfSted inspections in the 2000s, 46 in upper panel, 105 in lower panel.

Table 11: Ordered Probit Estimates of Change in Ofsted Ranking, School Level

	Pr[Change in OfSted Ranking]	
	(1)	(2)
Current Academies (Treatment Group)	0.661 (0.219)	0.724 (0.221)
Control Variables	No	Yes
Sample Size (Number of Treatment and Control Schools)	151	151
Marginal Effects:		
Pr[Change = 2|Treatment=1] – Pr[Change=2|Treatment=0]	0.239 (0.081)	0.261 (0.081)
Pr[Change = 1|Treatment=1] – Pr[Change=1|Treatment=0]	-0.012 (0.023)	-0.017 (0.025)
Pr[Change = 0|Treatment=1] – Pr[Change=0|Treatment=0]	-0.226 (0.069)	-0.244 (0.069)

Notes: The dependent variable is coded as 0 for a reduction in Ofsted rating, 1 for no change and 2 for an improvement. Robust standard errors in parentheses. The control variables included in specification (2) are proportion male, proportion white, proportion of pupils eligible for free school meals and the proportion of pupils with special educational needs al measured in the year of first inspection. Year of inspection dummies are also included.

Table 12: Department of Education Survey of Changes After Academy Conversion, 23 Labour Academies and 148 Sponsored Academies

	23 Labour Academies	148 Sponsored Academies Including the 23 Labour Academies		
	Percent Making Change		Percent Say Most Important Change	Percent Making Change Say Linked to Improved Attainment
Changed school leadership	87	72	56	73
Procured services that were previously provided by the LA	78	83	5	17
Changed the curriculum you offer	74	61	26	77
Changed the performance management system for teachers	74	70	3	39
Collaborated with other schools in more formalised partnerships	70	68	8	45
Introduced savings in back-office functions	70	55	0	12
Added non-teaching positions	70	50	3	31
Reconstituted your governing body	65	76	0	26
Changed your pattern of capital expenditure	65	54	1	19
Increased the number of pupils on roll	61	41	0	12
Hired teachers without qualified teacher status (QTS)	48	24	0	14
Introduced or increased revenue-generating activities	48	34	0	8
Changed your admission criteria	43	20	0	7
Increased the length of the school day	39	18	0	63
Changed staff pay structures	30	24	0	9
Sought to attract pupils from a different geographical area	13	12	0	11
Changed the length of school terms	9	6	0	22
Reduced the number of pupils on roll	4	3	0	0

Notes: Taken from Department for Education (2014).

Table 13: Change in Headteacher Before and After Academy Conversion

	Pr[Change in Headteacher]		
	All Schools	Community Predecessor School	Non-Community Predecessor School
	(1)	(2)	(3)
Academy x (E = c-4)	-0.052 (0.052)	-0.033 (0.069)	-0.129 (0.085)
Academy x (E = c-3)	-0.037 (0.056)	-0.015 (0.066)	-0.101 (0.101)
Academy x (E = c-2)	0.006 (0.056)	0.079 (0.070)	-0.167 (0.093)
Academy x (E = c-1)	0.014 (0.050)	0.040 (0.057)	-0.072 (0.101)
Academy x (E = c)	0.594 (0.058)	0.617 (0.072)	0.508 (0.101)
Academy x (E = c+1)	-0.033 (0.060)	-0.016 (0.072)	-0.100 (0.106)
Academy x (E = c+2)	0.057 (0.084)	0.116 (0.106)	-0.119 (0.144)
Academy x (E = c+3)	0.025 (0.108)	0.072 (0.147)	-0.084 (0.166)
P-value for test of joint significance (E =c-4 to c-1)	0.530	0.562	0.437
P-value for test of joint significance (E = c to c+3)	0.000	0.000	0.000
School Fixed Effects	Yes	Yes	Yes
Control Variables	Yes	Yes	Yes
Year Dummies	Yes	Yes	Yes
R-Squared	0.317	0.315	0.353
Sample Size	1735	1244	491
Number of Treatment and Control Schools	220	158	62

Notes: E denotes event year and c is the year of conversion. Robust standard errors (clustered at the school level) are reported in parentheses. Control variable are percentage of year 7 intake male, white-origin, free school meal status and special educational needs status. A pooled Academy x Post-Conversion (E = c to c+3) estimate and associated standard error (in parentheses) comparable to (1) for all schools is 0.345 (0.042).

Table 14: Numbers of Teachers and Pupils Before and After Academy Conversion

	Log(Number of Teachers)			Log(Number of Pupils)			Log(Teachers Per Pupil)		
	All Schools	Community Predecessor School	Non-Community Predecessor School	All Schools	Community Predecessor School	Non-Community Predecessor School	All Schools	Community Predecessor School	Non-Community Predecessor School
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Academy x (E = c-4)	0.005 (0.009)	0.008 (0.011)	0.003 (0.018)	-0.003 (0.008)	-0.002 (0.009)	-0.003 (0.018)	0.007 (0.008)	0.010 (0.010)	0.006 (0.014)
Academy x (E = c-3)	-0.002 (0.015)	0.016 (0.017)	-0.034 (0.028)	-0.018 (0.013)	-0.016 (0.016)	-0.023 (0.026)	0.015 (0.013)	0.032 (0.016)	-0.010 (0.020)
Academy x (E = c-2)	-0.004 (0.020)	0.013 (0.023)	-0.034 (0.036)	-0.033 (0.02)	-0.031 (0.023)	-0.044 (0.041)	0.028 (0.016)	0.044 (0.020)	0.010 (0.022)
Academy x (E = c-1)	-0.006 (0.027)	0.015 (0.032)	-0.064 (0.048)	-0.051 (0.028)	-0.055 (0.032)	-0.073 (0.051)	0.045 (0.020)	0.069 (0.026)	0.008 (0.035)
Academy x (E = c)	0.025 (0.038)	0.034 (0.047)	-0.038 (0.060)	-0.042 (0.037)	-0.059 (0.044)	-0.060 (0.068)	0.067 (0.026)	0.093 (0.035)	0.022 (0.037)
Academy x (E = c+1)	0.081 (0.047)	0.072 (0.060)	0.025 (0.071)	0.009 (0.045)	-0.012 (0.054)	-0.026 (0.082)	0.072 (0.031)	0.085 (0.041)	0.051 (0.050)
Academy x (E = c+2)	0.088 (0.064)	0.097 (0.075)	0.008 (0.115)	0.039 (0.059)	0.030 (0.067)	-0.007 (0.116)	0.049 (0.037)	0.067 (0.046)	0.015 (0.066)
Academy x (E = c+3)	0.207 (0.066)	0.220 (0.081)	0.112 (0.110)	0.159 (0.066)	0.155 (0.069)	0.089 (0.137)	0.048 (0.051)	0.065 (0.068)	0.023 (0.075)
P-value for test of joint significance (E =c-4 to c-1)	0.938	0.880	0.455	0.236	0.218	0.600	0.203	0.091	0.567
P-value for test of joint significance (E = c to c+3)	0.002	0.053	0.032	0.000	0.000	0.050	0.074	0.098	0.770
School Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Control Variables	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year Dummies	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R-Squared	0.896	0.881	0.937	0.921	0.912	0.948	0.599	0.575	0.677
Sample Size	1955	1402	553	1955	1402	553	1955	1402	553
Number of Treatment and Control Schools	220	158	62	157	158	62	220	158	62

Notes: E denotes event year and c is the year of conversion. Robust standard errors (clustered at the school level) are reported in parentheses. Control variable are percentage of year 7 intake male, white-origin, free school meal status and special educational needs status. A pooled Academy x Post-Conversion (E = c to c+3) estimate and associated standard error (in parentheses) comparable to (1) for all schools is 0.062 (0.028), for (4) for all schools is 0.039 (0.024) and for (7) for all schools is 0.023 (0.015).

Appendix

Data Description

1. Data on Academy Schools

We first identified all schools that became academies over the school years 2002/03 to 2010/11. Our sources for this are Department for Education extracts that give information on all academies that have opened or are in the process of doing so. The extract gives the opening date of the academy, its URN (a unique identifier for the school allowing us to identify it in various governmental data sources such as the National Pupil Database and the Pupil Level Annual Census data), DFE number (a second unique identifier combining school specific and local authority specific numbers) and the URN number of the predecessor school.

Using performance tables data from the Department for Education (DfE) we match in predecessor school types. The data gives 244 schools that became academies between the first 3 academy openings in 2002/03 and those that gained academy status by September 2010 (the beginning of the academic school year). We omit those that were previously independent schools due to pupils in these schools not having exam information at KS4. Similarly, we omit new schools as they have no predecessor school.

In order to have a balanced panel we focus on academies that have some form of predecessor school open from at least 1996 onwards. Any later and the school will not have KS4 results for 2001. In order for our sample to be balanced for intake we exclude academies who do not enrol pupils in year 7. The final sample contains 106 treatment schools (those that opened as academies prior to, or in, September 2008) and 114 control schools with observations ranging over the years 2000/01-2008/09. None of our control schools become academies during these sample years.

2. Pupil Level Data

We use data from PLASC (pupil level annual schools census) and the NPD (national pupil database). The NPD contains information on all key stage 2 (KS2) and key stage 4 (KS4) exams sat at the end of primary and secondary school respectively. Each pupil is identified by a unique reference number and the data gives the unique URN of the school in which they sat the exam. While the NPD reports on pupils in examination years PLASC has a record for every pupil for each year that they are in the maintained school sector. PLASC data gives the pupil, year group and school as well as demographic variables such as ethnicity, gender, free school meal eligibility and special educational needs status. We can track pupils through secondary school using the unique pupil identifier. This identifier is common to the NPD enabling us to merge NPD and PLASC data. This gives a panel of pupils with their demographic information, their KS2 and KS4 test results and the school(s) that they attended from year 7 (first year of compulsory secondary education) through to year 11 (final year of compulsory education). We then extract those pupils who attended the 220 treatment and control schools at some point over the sample period. We can now see which schools pupils attended in every secondary

compulsory year of schooling[26], their demographic information and their exams results at KS4 and KS2. Our intake analysis focuses on those who enter as a year 7 student in 2000/01 – 2008/09 while our results analysis focuses on those who sit exams, are ITT or receive exposure in one of our 106 treatment schools or sit exams in one of our 220 control schools over the same period.

The raw data contains a small number of duplicate observations for pupils.[27] Duplicates at the level of KS2 results are easy to deal with as we randomly delete one entry when pupils records are duplicated in all aspects apart from the primary school they attended (as primary school does not matter to us). When there are two entries with differing exam scores we keep the record with the most information (i.e. if one entry has the pupil missing most exams while the second has scores for these exams we keep the latter).[28] There are also small number of multiple records of KS4 attainment for pupils. Our analysis focuses on the year 11 record giving us a dataset of pupils who have completed their GCSEs (as oppose to those who have sat some exams early). When multiple records exist in a single year we delete those whose scores are not included in national or school level calculations – often these are those who switch schools and so take exams in one school but are coded as attending another.[29] In a few cases pupils are flagged as not to be included in the school level calculations (so their attainment would not be used to calculate performance tables school level data) despite the fact that their information is not duplicated and nothing appears to be wrong with their attainment data. We include these pupils in the final dataset. However, all results are robust to omitting these pupils.[30]

The sample sizes for year 7 and year 11 pupils are given in Table A1.

Finally it is worth noting that PLASC does not cover years prior to 2002. For our observations before then we do still have NPD data on KS2 and KS4 performance (we have these going back to 1997 for KS4 and 1996 for KS2). Therefore these observations are missing all demographic covariates with the exception of gender. Similarly in our fake policy results the only covariates, aside from year dummies and school fixed effects, are pupil gender. This is why, in Table 9, we reproduce our main specification without covariates so as to make the fake and actual policy results comparable.

For our the intake analysis we assume that those identified as being in year 8 in 2002 in PLASC were year 7 pupils in the same school the previous year – we therefore retain demographic variables for these pupils.

[26] Strictly speaking this is not true. Some pupils enter the schooling system either from another country or from independent schools. We observe when the pupils enter but not precisely where they came from. These pupils are retained in our analysis.
[27] That is, multiple records in a single year.
[28] This may be the case when a pupil misses exams through illness and retakes at a later date.
[29] Variables in the NPD identify whether the pupil's achievement were used in school/national level calculations.
[30] Unless otherwise stated all further robustness checks mentioned in this Appendix are available upon request form the authors.

3. Notes on Treatment and Clustering

Treatment for the pupil intake KS2 analysis is simple. A pupil is defined in treatment group if they enrol in an academy school in their first year of secondary school - year 7 – after conversion to an academy has occurred.

Intention to treat for the KS4 performance analysis is defined as follows. For an individual in pre-enrolment year c-i (where c denotes conversion year) and academic year group j an individual is expected to sit their exams in c-i + (11-j). A person is then ITT if the preceding term is equal to c, c+1, c+2 or c+3. To see why an individual cannot be ITT in year c+4 note that the 'biggest' pre-enrolment year is c-1 and the 'smallest' academic year group is 7 thus the preceding term cannot exceed c+3.

The exposure variable for Table 8 is defined cumulatively therefore we simply sum the number of academic years an individual spends in an academy school post conversion. ITT is then defined as above. We limit this sample to those who spend at most 4 years in an academy post conversion so as to be consistent with Tables 6 and 7.

A final note relates to how we define 'school'. For each of our treatment and control schools we assign a unique number. It is possible that two pupils from different schools are given the same number should the two differing schools later become the same academy. We identify when schools merge by looking at linked schools in edubase (this is a Department For Education database of all open and closed maintained schools in England). In one case a single school becomes two separate academies (North Westminster Community School splits into Paddington Academy and Westminster Academy in 2006). Pupils attending the predecessor school are randomly assigned one of the two numbers given to the two academies that open later. Students who leave the sample but are ITT or receive exposure are given a unique number equal to the school that they sit their KS4 exams in. In estimated specifications, standard errors are clustered on this unique number resulting in 1714 clusters in Tables 6 and 7 and 1720 in Table 8.

4. Attainment Measures

The main variable in our analysis of intake is an average score across three subjects specific tests: English, Maths and Science. Test scores are reported in two ways: firstly, a level from 2-5 is awarded in each subject and secondly, a raw test score. The raw test score is out of 80 for science and is the sum of two separate science papers each marked out of 40 while the English test score is marked out of 100 and is composed of the sum of two separate test scores, each marked out of 50, in reading and writing. Finally math is composed of two marks out of 50 with one of the tests being in mental arithmetic. The levels are based upon these underlying test scores but are not always consistent. For instance, after an initial level is assigned after grading the test there may be a review of the pupil's test score resulting in a higher or lower level being awarded even if the underlying raw test mark is not altered. Similarly the mark required for any one level varies both between subjects and within subjects across years. For these reasons we use standardised raw test scores as our main dependent variable in KS2 regressions.

When pupils are not awarded a test mark or are performing at a level below the level of the test we award pupils a mark of 0. Those who miss the tests are excluded from our sample for the purposes of the KS2 regressions but are included in our KS4 regressions where we include a dummy for those who do not have a KS2 record or who miss KS2 exams. Our KS4 results are robust to re-running our regressions omitting those without a KS2 record and those whose scores are below test levels.

The main KS4 qualification in the UK is the GCSE (General Certificate of Secondary Education). GCSEs are graded A*-G. The current points score calculations give an A* a score of 58 and a G a score of 16 with grades in between going up in increments of 6 as follows:

Grade	Points
A*	58
A	52
B	46
C	40
D	34
E	28
F	22
G	16

New scale

Prior to this an A* was given a score of 8 and a G a score of 1 with scores going up in increments in 1.

Grade	Points
A*	8
A	7
B	6
C	5
D	4
E	3
F	2
G	1

Old scale

As well as GCSEs there are a wide range of equivalent qualifications focusing on more vocational subjects. These include GNVQs and BTecs. Depending upon the type of equivalent these are often worth multiple GCSEs and are often graded as a combination of GCSE grades i.e. a distinction in an intermediate GNVQ is equivalent to gaining two GCSEs with one at grade

A and the other at grade A*.[31] The points score given to the qualification reflects the underlying GCSE grades that it is based upon so that under the new scoring system the aforementioned qualification would be given a score of 110.

The points system we use is as follows:

Grade	Points
A*	10
A	8
B	7
C	6
D	4
E	3
F	2
G	1

Scale used in the paper

The points system we use addresses some of the concerns expressed pertaining to the 16-58 and 1-8 scales used over the course of our sample.[32] The non-linearity reflects the fact that it appears hardest to jump from grades D to C and from A to A*.

We cap points scores at best 8 qualifications. To do this we normalize raw point scores by their GCSE equivalent i.e. a qualification worth 4 GSCEs and 208 points (under the 16-58 scale) is normalized to be worth 52 points. We then convert these points to our new measure and rank them highest to lowest. We then add up the grade weightings (in terms of GCSEs), taking fractions of qualifications if need be, until we reach 8. All those in the top 8 are then multiplied through by their weight and summed to give the points score.

Our decision to cap at 8 is motivated by two concerns. Total points scores have the problem that pupils can appear to do well by entering many exams and performing poorly in them. Similarly using, for instance, 5 best means that those who focus very narrowly on a small set of exams may appear better than those who perform well over a larger selection of subjects/qualifications. Our decision to cap at 8 balances these two concerns.

Finally, it is worth noting that our point measures create some notable discrepancies with the official method. For instance, an equivalent qualification worth two GCSEs graded CD is worth 74 points under the 16-58 scale meaning that it is worth more than a A* at GCSE. Using our system such a qualification is worth 10 points (the sum of the points scores for grades of C and D) – the equivalent of a GCSE at grade A*. A further example is a BTEC that is worth 76 points on the old scale and equivalent to 4 GCSEs. This is the same as achieving grades of 2 Fs and 2

[31] Most equivalents are graded as pass, merit or distinction but the Department for Education equates these categories, combinations of, A*-G grades.

[32] We are grateful to Tim Leunig and Mike Treadaway for very helpful correspondence on this.

Gs. In our system this is equivalent to a point score of 6. Thus our points mean the qualification is the same as getting a C at GCSE whereas the old measure means that the qualification is again worth more than an A*. In general our system reduces the relative points scores of equivalent qualifications compared to the official method. Despite this our results remain unchanged when using the (standardized) old (1-8) and new (16-58) points systems and when using total rather than capped scores.

The threshold measures (results for which are reported in Tables A2-A4) are relatively simple. In these, an equivalent qualification is seen as being at least a C if its normalized points score is greater than or equal to that score given to a grade C at GCSE. Thus a qualification worth N GCSEs whose normalized point score is at least 6 equates to N qualifications of at least grade C.

We present results for all our main performance specifications in Tables A2-A4 using different dependent variables.

5. Ofsted Reports[33]

Ofsted is a government department that carries out inspections of maintained schools in England and Wales and reports to Parliament. Inspectors give schools minimal prior warning of inspection and proceed to inspect the school based upon a pre-set criteria before awarding the school and overall effectiveness rating.[34] Overall effectiveness is based upon many criteria such as the achievement of pupils, the effectiveness of management and the level of well-being and personal development of the pupils.

Post 2005 there are 4 possible inspection ratings – outstanding, good, satisfactory and inadequate. Prior to 2005 the possible ratings given were excellent, very good, good, satisfactory, unsatisfactory, poor and very poor. To measure whether academies improve over time we equate the 7 ratings given prior to 2005 into the 4 categories given post 2005 in the following manner:

Prior to 2005	Post 2005
Excellent, very good	Outstanding
Good	Good
Satisfactory	Satisfactory
Unsatisfactory, poor, very poor	Inadequate

Our main interest is whether schools converting to academies are more likely to improve their rating relative to the control schools.

[33] Throughout this and the other mechanisms section school refers to the variable school that we cluster on as described in the treatment section of the appendix – all mechanism regressions are performed at this level.
[34] Overall effectiveness ratings have been awarded since 2000.

To do this we use Ofsted ratings for the years 2000-2010. We limit the sample to the years 2000-2010 as post 2010 all the schools in our sample have converted to academies making any comparisons between converters and those yet to convert impossible.

For our estimates we use the first and last inspections for each school in our sample. For treatment schools the first inspection must be prior to conversion while the last must be post conversion. These restrictions results in our sample of treatment school falling to 46 with the first three cohorts not represented in our sample at all. For controls schools we omit those that only have a single inspection over the period thus reducing our sample of control schools to 105. For this sample we define a variable equal to 0 if the school's first inspection is worse than its last, 1 if the inspections are the same and 2 if the latter inspection is an improvement on the first. We use first and last inspections so that there is an equivalence in how we select relevant inspections for treatment and control schools. There are no cases when schools have multiple reports in the same year.

As a robustness check we replicate the results using the following two conversions for Ofsted scores:

Conversion 1

Prior to 2005	Post 2005
Excellent	Outstanding
Very good, good	Good
Satisfactory	Satisfactory
Unsatisfactory, poor, very poor	Inadequate

Conversion 2

Prior to 2005	Post 2005	New Scale
Excellent, very good	Outstanding	Good
Good	Good	Good
Satisfactory	Satisfactory	Good
Unsatisfactory, poor, very poor	Inadequate	Bad

Our results prove robust to these changes.

6. Data on Mechanisms

As well as considering Ofsted reports we study mechanisms by looking at survey results from the Department for Education (2014), head teacher change and teacher turnover.

We collect data on head teachers using edubase and match a head teacher to each of our schools for each year (excluding 2001 for which data are not available) in our sample. For each year we define a binary variable equal to 1 if this year's head teacher is different from last years. When two schools merge we set this variable to 1 only if the head is not the head of either of the predecessors. When two separate schools are defined as being the same (with respect to the

clustering variable) we set this variable to 1 if either school change their head teacher in that year. Controls in this linear model are the same as those reported in Table 10.

For the teacher and pupil analysis we use data from the annual schools census. The data gives us the number of qualified and unqualified teachers at all maintained secondary schools for the years 2001-2009. We weight the total number of teachers, at the school level, by the number of pupils of compulsory secondary schooling age (11-15) relative to the total number of pupils in the school. This prevents a potentially spurious relationship between the number of teachers and academy conversion caused by many schools opening 6th forms post-conversion. The weighted number of teachers, total pupils in compulsory secondary schooling along with the ratio of these two variables form the dependent variables in Table 14. Controls are the same as those reported in Table 11.

Additional Tables

Table A1: Pupil Sample Sizes by Academy Cohort for KS2 and KS4 Analyses

Key Stage 2 (Year 7)

E =	c-8	c-7	c-6	c-5	c-4	c-3	c-2	c-1	c	c+1	c+2	c+3	Number of Conversions
2002/03							441	398	532	537	588	523	3
2003/04						964	1002	1070	1325	1327	1422	1342	7
2004/05					269	264	245	265	340	385	375	379	2
2005/06				1475	1497	1472	1460	1396	1625	1658	1772	1775	10
2006/07			2608	2588	2591	2411	2358	2332	2363	2440	2654		15
2007/08		5048	5080	5060	5057	4682	4742	4538	4772	5200			30
2008/09	7623	7912	7904	7628	7102	6820	6396	6092	6191				39

Key Stage 4 (Year 11

E =	c-8	c-7	c-6	c-5	c-4	c-3	c-2	c-1	c	c+1	c+2	c+3	Number of Conversions
2002/03							427	399	515	482	451	401	3
2003/04						1020	1128	1115	1233	1068	1091	1108	7
2004/05					278	263	302	301	265	256	261	280	2
2005/06				1282	1418	1513	1529	1484	1430	1440	1452	1483	10
2006/07			2341	2346	2520	2603	2499	2426	2390	2240	2282		15
2007/08		4409	4673	4724	4920	4796	4821	4903	4876	4659			30
2008/09	6749	6858	7220	7227	7468	7425	7482	7264	6823				39

Notes: E denotes event year and c is the year of conversion. Sample sizes and number of academy conversions by cohort for the KS2 and KS4 analysis. The KS4 stop in E = c+3 to ensure that the post-academy new Year 7 intake are not included in the Year 11 samples ensuring treatment to conversion is for children enrolled in the school before conversion year E = c. See the discussion in the main text of the paper for reasons for this chosen research design.

Table A2: Pupil Performance, Different Key Stage 4 Dependent Variables, Year 11, 2000/01 to 2008/09

	Key Stage 4 A*-C			Key Stage 4 A*-C With English and Maths			Key Stage 4 GCSE Points Only			Key Stage 4 GCSE Equivalents		
	OLS	ITT	IV	OLS	ITT	IV	OLS	ITT	IV	OLS	ITT	IV
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Academy x (E = c-4)	-0.008	-0.012	-0.007	0.006	0.004	0.008	-0.004	-0.007	-0.001	-0.100	-0.101	-0.099
	(0.009)	(0.009)	(0.009)	(0.006)	(0.006)	(0.006)	(0.019)	(0.018)	(0.019)	(0.068)	(0.065)	(0.068)
Academy x (E = c-3)	0.004	-0.001	0.005	0.006	0.004	0.009	0.015	0.011	0.018	-0.097	-0.099	-0.095
	(0.013)	(0.012)	(0.013)	(0.007)	(0.007)	(0.008)	(0.023)	(0.022)	(0.023)	(0.083)	(0.078)	(0.083)
Academy x (E = c-2)	0.014	0.008	0.015	0.011	0.009	0.014	0.039	0.034	0.043	-0.071	-0.073	-0.070
	(0.015)	(0.014)	(0.016)	(0.008)	(0.007)	(0.008)	(0.029)	(0.027)	(0.029)	(0.089)	(0.082)	(0.089)
Academy x (E = c-1)	0.012	0.006	0.014	0.017	0.013	0.020	0.062	0.057	0.066	-0.127	-0.130	-0.126
	(0.018)	(0.016)	(0.018)	(0.010)	(0.009)	(0.010)	(0.030)	(0.028)	(0.031)	(0.093)	(0.086)	(0.094)
Academy x (E = c)	0.024	0.021	0.029	0.021	0.019	0.026	0.025	0.023	0.032	-0.072	-0.063	-0.063
	(0.020)	(0.018)	(0.020)	(0.011)	(0.010)	(0.011)	(0.033)	(0.030)	(0.034)	(0.104)	(0.094)	(0.105)
Academy x (E = c+1)	0.076	0.069	0.082	0.054	0.050	0.060	0.043	0.040	0.054	0.089	0.084	0.096
	(0.024)	(0.021)	(0.025)	(0.014)	(0.012)	(0.014)	(0.040)	(0.036)	(0.042)	(0.111)	(0.098)	(0.114)
Academy x (E = c+2)	0.111	0.092	0.109	0.054	0.048	0.060	0.094	0.071	0.09	0.137	0.120	0.135
	(0.013)	(0.030)	(0.036)	(0.016)	(0.014)	(0.016)	(0.049)	(0.042)	(0.050)	(0.136)	(0.119)	(0.142)
Academy x (E = c+3)	0.069	0.053	0.068	0.073	0.072	0.087	0.148	0.139	0.165	-0.036	-0.072	-0.072
	(0.039)	(0.036)	(0.044)	(0.018)	(0.015)	(0.019)	(0.056)	(0.053)	(0.064)	(0.153)	(0.138)	(0.164)
KS2 Standardised Test Score	0.226	0.226	0.226	0.225	0.225	0.225	0.554	0.554	0.554	0.145	0.145	0.145
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.006)	(0.006)	(0.006))	(0.008)	(0.008)	(0.008)
School Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Control Variables	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year Dummies	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R-Squared	0.374	0.374	0.374	0.352	0.352	0.352	0.527	0.527	0.527	0.268	0.268	0.268
Sample Size	362424	362424	362424	362424	362424	362424	362424	362424	362424	362424	362424	362424
Number of Treatment and Control Schools	220	220	220	220	220	220	220	220	220	220	220	220

Notes: E denotes event year and c is the year of conversion. Robust standard errors (clustered at the school level) are reported in parentheses. Control variables included are the same as in Tables 6, 7 and 8. First stage results for the IV specifications are the same as those reported in Table 6.

Table A3: Pupil Performance, Different Key Stage 4 Dependent Variables, Year 11, 2000/01 to 2008/09, Community Predecessor School

	Key Stage 4 A*-C			Key Stage 4 A*-C With English and Maths			Key Stage 4 GCSE Points Only			Key Stage 4 GCSE Equivalents		
	OLS	ITT	IV	OLS	ITT	IV	OLS	ITT	IV	OLS	ITT	IV
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Academy x (E = c-4)	-0.012	-0.018	-0.009	0.005	0.001	0.007	-0.004	-0.012	-0.003	-0.083	-0.094	-0.073
	(0.010)	(0.010)	(0.001)	(0.007)	(0.007)	(0.007)	(0.024)	(0.023)	(0.024)	(0.041)	(0.038)	(0.042)
Academy x (E = c-3)	0.019	0.011	0.023	0.016	0.011	0.019	0.016	0.007	0.017	-0.008	-0.022	0.005
	(0.015)	(0.013)	(0.015)	(0.009)	(0.008)	(0.009)	(0.030)	(0.028)	(0.029)	(0.071)	(0.065)	(0.072)
Academy x (E = c-2)	0.030	0.020	0.034	0.021	0.015	0.024	0.039	0.028	0.040	0.023	0.006	0.036
	(0.018)	(0.017)	(0.019)	(0.009)	(0.009)	(0.009)	(0.035)	(0.033)	(0.035)	(0.074)	(0.066)	(0.075)
Academy x (E = c-1)	0.028	0.017	0.033	0.026	0.020	0.029	0.063	0.051	0.064	-0.019	-0.038	-0.005
	(0.021)	(0.020)	(0.022)	(0.012)	(0.011)	(0.012)	(0.037)	(0.035)	(0.038)	(0.085)	(0.076)	(0.085)
Academy x (E = c)	0.044	0.038	0.053	0.031	0.027	0.036	0.056	0.047	0.061	0.025	0.022	0.050
	(0.024)	(0.022)	(0.024)	(0.012)	(0.011)	(0.012)	(0.041)	(0.038)	(0.042)	(0.095)	(0.084)	(0.096)
Academy x (E = c+1)	0.122	0.107	0.131	0.083	0.074	0.090	0.083	0.071	0.090	0.249	0.226	0.279
	(0.030)	(0.025)	(0.030)	(0.016)	(0.015)	(0.017)	(0.052)	(0.046)	(0.055)	(0.128)	(0.111)	(0.13)
Academy x (E = c+2)	0.163	0.137	0.168	0.074	0.062	0.079	0.112	0.076	0.099	0.381	0.346	0.417
	(0.015)	(0.033)	(0.040)	(0.018)	(0.016)	(0.019)	(0.063)	(0.054)	(0.065)	(0.139)	(0.122)	(0.144)
Academy x (E = c+3)	0.151	0.130	0.164	0.108	0.097	0.119	0.200	0.173	0.208	0.285	0.239	0.307
	(0.031)	(0.026)	(0.033)	(0.02)	(0.017)	(0.02)	(0.071)	(0.067)	(0.082)	(0.135)	(0.118)	(0.143)
KS2 Standardised Test Score	0.231	0.231	0.231	0.220	0.220	0.220	0.555	0.555	0.555	0.151	0.151	0.151
	(0.003)	(0.003)	(0.003)	(0.004)	(0.004)	(0.004)	(0.007)	(0.007)	(0.007)	(0.009)	(0.009)	(0.009)
School Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Control Variables	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year Dummies	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R-Squared	0.351	0.351	0.351	0.317	0.317	0.317	0.492	0.492	0.492	0.179	0.179	0.179
Sample Size	258409	258409	258409	258409	258409	258409	258409	258409	258409	258409	258409	258409
Number of Treatment and Control Schools	158	158	158	158	158	158	158	158	158	158	158	158

Notes: As for Table A2 except first stage results for the IVspecifications are the same as those reported in Table 7.

Table A4: Pupil Performance, Different Key Stage 4 Dependent Variables, Year 11, 2000/01 to 2008/09, Non-Community Predecessor School

	Key Stage 4 A*-C			Key Stage 4 A*-C With English and Maths			Key Stage 4 GCSE Points Only			Key Stage 4 GCSE Equivalents		
	OLS	ITT	IV	OLS	ITT	IV	OLS	ITT	IV	OLS	ITT	IV
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Academy x (E = c-4)	-0.004	-0.005	-0.004	0.004	0.006	0.007	0.003	0.006	0.011	-0.125	-0.119	-0.131
	(0.016)	(0.016)	(0.017)	(0.011)	(0.011)	(0.011)	(0.030)	(0.028)	(0.030)	(0.155)	(0.150)	(0.154)
Academy x (E = c-3)	-0.019	-0.020	-0.019	-0.013	-0.011	-0.009	0.020	0.024	0.030	-0.200	-0.193	-0.209
	(0.023)	(0.022)	(0.023)	(0.014)	(0.013)	(0.014)	(0.039)	(0.037)	(0.041)	(0.169)	(0.162)	(0.168)
Academy x (E = c-2)	-0.007	-0.008	-0.006	-0.009	-0.007	-0.004	0.046	0.051	0.059	-0.165	-0.156	-0.175
	(0.026)	(0.024)	(0.026)	(0.015)	(0.014)	(0.015)	(0.054)	(0.049)	(0.054)	(0.179)	(0.171)	(0.18)
Academy x (E = c-1)	0.001	0.000	0.002	0.002	0.004	0.007	0.075	0.080	0.088	-0.220	-0.210	-0.231
	(0.031)	(0.029)	(0.031)	(0.019)	(0.018)	(0.020)	(0.057)	(0.052)	(0.058)	(0.182)	(0.172)	(0.182)
Academy x (E = c)	0.007	0.007	0.009	0.002	0.005	0.008	-0.013	-0.002	0.006	-0.125	-0.109	-0.134
	(0.034)	(0.031)	(0.035)	(0.020)	(0.019)	(0.021)	(0.062)	(0.055)	(0.063)	(0.201)	(0.186)	(0.203)
Academy x (E = c+1)	0.033	0.034	0.037	0.010	0.016	0.020	-0.002	0.014	0.025	-0.018	-0.008	-0.032
	(0.039)	(0.034)	(0.040)	(0.023)	(0.021)	(0.025)	(0.073)	(0.063)	(0.073)	(0.178)	(0.158)	(0.179)
Academy x (E = c+2)	0.041	0.031	0.034	0.016	0.022	0.028	0.089	0.090	0.109	-0.170	-0.172	-0.213
	(0.023)	(0.051)	(0.060)	(0.032)	(0.028)	(0.032)	(0.087)	(0.075)	(0.089)	(0.228)	(0.192)	(0.222)
Academy x (E = c+3)	-0.021	-0.025	-0.025	0.021	0.037	0.044	0.095	0.119	0.141	-0.342	-0.349	-0.406
	(0.067)	(0.061)	(0.072)	(0.030)	(0.027)	(0.033)	(0.096)	(0.092)	(0.110)	(0.238)	(0.198)	(0.229)
KS2 Standardised Test Score	0.214	0.214	0.214	0.241	0.241	0.241	0.552	0.553	0.553	0.122	0.122	0.121
	(0.008)	(0.007)	(0.008)	(0.008)	(0.008)	(0.008)	(0.011)	(0.011)	(0.011)	(0.019)	(0.019)	(0.019)
School Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Control Variables	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year Dummies	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R-Squared	0.401	0.401	0.401	0.399	0.399	0.399	0.579	0.579	0.579	0.370	0.370	0.370
Sample Size	104015	104015	104015	104015	104015	104015	104015	104015	104015	104015	104015	104015
Number of Treatment and Control Schools	62	62	62	62	62	62	62	62	62	62	62	62

Notes: As for Table A2 except first stage results for the IV specifications are the same as those reported in Table 7.

Table A5: Structure of Fake Policy Sample

	Key Stage 4 (Year 11)											
E =	c-11	c-10	c-9	c-8	c-7	c-6	c-5	c-4	c-3	c-2	c-1	Number of Conversions
2002/03							459	468	475	427	399	3
2003/04						1072	1090	1086	1020	1128	1115	7
2004/05					248	214	259	278	263	302	301	2
2005/06				1011	995	1006	986	1069	1149	1200	1145	8
2006/07			2313	2352	2310	2341	2346	2520	2603	2499		15
2007/08		4308	4246	4378	4305	4561	4611	4796	4653			29
2008/09	6655	6706	6635	6749	6858	7220	7227	7468				39

Notes: E denotes event year and c is the year of conversion. Sample sizes and number of academy conversions by cohort for the fake policy sample used in columns (1) and (2) of Table 9.