The Introduction of Academy Schools to England's Education

Andrew Eyles[*] and Stephen Machin[**]

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* *Centre for Economic Performance, London School of Economics*

** *Department of Economics, University College London and Centre for Economic Performance, London School of Economics*

Abstract

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 education. Academies are independent state funded schools that are allowed to run in an autonomous manner outside of local authority control. Almost all academies are conversions from already existent state schools and so are school takeovers that enable more autonomy in operation than in their predecessor state. Our analysis shows that the first round of academy conversions that took place in the 2000s, where poorly performing schools were converted to academies, generated significant improvements in the quality of pupil intake and in pupil performance.

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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 that are run by local authorities, academies are autonomous, state-funded schools that are managed and run outside local authority control. In almost all cases, they are conversions of pre-existing schools that inherit pupils already enrolled in the school. They are school takeovers that, 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 are free schools, recent additions that are brand new schools (often set up by parent or community groups). A closer US comparison to academies are '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, other countries have pursued education policies 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 disadvantaged urban 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. We consider data on pupils in schools over the school years in the 2000s that permit us to undertake 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 than 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 pupils enrolled in academy schools to outcomes of those enrolled in a specific group of comparison schools - state schools that go on to become academies after our sample period ends. We discuss the rationale for this research design (together with threats to convincingly achieving identification) in more depth below. This approach produces a well-balanced treatment and control group that differences out key observable and some of the 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 who 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 in line to take their final year 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 studies on Catholic schools, voucher-subsidised private schools and on 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.

2. Academy Schools

Academy schools were first introduced to English education in the early 2000s. Looking back with hindsight, their initial introduction can be viewed as a landmark development in the history

[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, 2016; 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).

of education in England.[9] Firstly, changes in school type like those that have taken place for academies, and the scale of the academies programme now, 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 in the early 2000s, 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. The first tranche of academies that we study in this paper 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 which has responsibility for employing all academy staff, agreeing levels of pay and conditions of service and deciding on policies for staffing structure, career development, discipline and performance management. Subsequent developments of the English academies programme that have taken place mean that not all academies now have sponsors, and that following the Academies Act of 2010 that the academies programme has been extended to cover primary schools as well. In this paper, we study the genesis of the programme, studying academy introduction prior to 2010.

[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. In the OECD's Programme for International Student Assessment (PISA) data, this has resulted in England becoming the highest ranked country in school autonomy over resource allocation in the 2012 PISA – see Eyles, Hupkau and Machin (2015) for more detail on this aspect of academisation of English schools.

[10] For example, the anti-academies alliance (see the website at http://antiacademies.org.uk).

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 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, the main impetus of the programme was to replace failing schools with academies in order to generate improvement by moving away from the conventional school type that had populated the English secondary sector in the past.[11] The path to establishing 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.

[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.

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 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, 106 schools became academies for the seven cohorts of conversions we study. An additional two cohorts of schools - comprising a total of 114 schools - were approved

to become academies, with their conversion occurring after the sample period ends in 2008/09, and not being set up under the new regime that arose after the 2010 Academies Act.[12]

Related Literature

Whilst there is a sizeable body of research on the impact of different schooling systems on pupil performance, there are fewer studies that look at what happens when the type of school attended by pupils changes. One study related to this paper that also looks at schools changing status in England is Clark (2009). In common with our work, he looks at what happened when school types changed with a new school type being granted more autonomy to operate than it had before. It is however in a very different setting from the late 1980s and early 1990s, and is a fundamentally different policy change in nature and implementation from the academies programme. Clark looks at a setting when parents with children enrolled at schools were allowed a vote on whether the school could become a grant-maintained (GM) school.[13] Based on the outcome of the vote, he utilises a regression-discontinuity design to show that the narrow GM vote winners experienced a significant improvement in pupil performance (of about a quarter of a standard deviation) compared to the narrow GM vote losers. Thus the change in school type brought about performance improvements, which he argues arose as a consequence of increased school autonomy.

In the US, the work on charter schools[14] is relevant to our analysis because, in some dimensions, charter schools have similarities to academies. However, most charters are new schools, and in this dimension the relevance is reduced. Initial findings from the literature on charters, based upon quasi-experimental research designs produced mixed to negative results.

[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.
[13] GM schools were renamed as foundation schools (see Table 1) in the Schools Act of 1998.
[14] Epple, Romano and Zimmer (2016) provide an in-depth and up-to-date survey of the work on charter schools.

For instance, Betts et al (2006) find that charters perform roughly at a similar level to public schools in the 16 charters they study in San Diego while two studies carried out by CREDO (2009 ,2013) find little average effects when looking at charters across 16 and 27 states.

Concerns with unobservables and that matching does not adequately account for selection into charters subsequently led researchers to look at lottery based estimates of the effect of charter attendance. These studies exploit the fact that some schools use lotteries to allocate places when the school is oversubscribed. The vast majority of these papers find substantial positive test score gains for pupils 'lotteried' in to charters relative to those 'lotteried' out (see Abdulkadiroglu et al, 2011, Angrist et al., 2010, Angrist et al., 2013, Dobbie and Fryer, 2011, Dobbie and Fryer, 2013 and Hoxby Murarka and Kang, 2009, for studies of test score gains; and Angrist et al., 2016, and Dobbie and Fryer, 2014, for evidence of students' longer-run outcomes, including college attendance).

An exception to these papers is Gleason et al. (2010) who use lottery estimates from 36 charters across 15 states and find little evidence of improvements in pupil performance. The difference seems to be due to the charters studied, and may reflect the fact that the high-impact charters that have been the focus of lottery studies are oversubscribed schools represent a minority of the overall charter population. Moreover, whilst Gleason et al. report little in the way of average effects, when they explore heterogeneity they do find performance improvements for disadvantaged children (those on free school meals). Similarly, Angrist, Pathak and Walters (2013) find that when splitting their Massachusetts sample between urban and non-urban charters, gains are positive in urban schools but negative for non-urban charters.

As the majority of the lottery studies are based upon charters serving disadvantaged children in urban areas, such as New York and Boston, this can reconcile the seemingly

disparate findings. Further differences between lottery estimates can also be due to the stronger research design of lottery studies. The potential for observational studies to lead to erroneous inference can be seen in Dobbie and Fryer (2013) where they 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 the other hand, Abdulkadiroglu et al. (2011) find that observational estimates replicate their lottery based findings; interestingly they report observational estimates of the effect of attending a non-oversubscribed charter and report much lower effect sizes suggesting that by focusing on oversubscribed schools lottery estimates may over estimate the average effect of charter attendance.

More pertinent to the case of English academies, a smaller number of studies have looked at conversions of already existing schools to charters (as in the study of school takeovers in Boston and New Orleans by Abdulkadiroglu et al., 2014), as well as the introduction of practices used in charters to US public schools (as in Houston schools studied by Fryer, 2014). These report substantial improvements in test scores in those setting due to the use of methods of 'best practice'.

On academies themselves, there remains little rigorous work. Machin and Wilson (2008) looked at differences in pupil performance between a small sample of the first academy schools and a matched group of schools, finding modest, statistically insignificant, relative improvements. A 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), while a National Audit Office (2010) report on the Labour academies looked at their performance compared to a selected

group of maintained schools, with similar pupil intakes and performance to the academies pre-treatment, finding a significant improvement in pupil performance in the academies. 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).

In our empirical analysis, we separately study intake and performance separately. Because we observe changes in enrolment based upon the prior academic achievement of pupils, pre and post conversion, in the incoming year 7 cohort, 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) 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

[15] The use of pupil-level data throughout and a heavily refined research design are the key innovations compared to the version of this paper circulated earlier (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.

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 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

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

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 multiple predecessor schools combine to create a single 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, so that 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. Limiting the sample to pupils in schools that either convert or are set to convert after the sample period enables us to implement the treatment-control comparison across conversion cohorts that we described earlier in the paper.

There are 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

[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.

score of individual year 11 students.[18] Below we also consider robustness of the findings to use of different measures of pupil performance.

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:

$$KS2_{ist} = \alpha_s + \alpha_t + \delta A_{ist} * I(E \geq t = c) + u_{1ist} \quad (1)$$

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. Finally, α_s denotes school fixed effects, α_t denotes year effects and u_1 is an error term.

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:

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

[18] 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.

From (2) we can derive event study estimates of four pre-conversion δ's (from E = c-4 to c-1) that are informative about differential pre-conversion trends and four conversion year and post-conversion δ's (from E = c to c+3).

We further 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.[19] The presumption underpinning this part of our empirical investigation is premised on the observation 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 some important identification issues that need to be considered here that do not apply to the KS2 intake part of our study. Specifically, two aspects of the research design that enable a causal effect of academy conversion on pupil performance to be identified require discussion:

i) We consider children whose parents had already made their decision to enrol their children in the academy before it converted. This focus upon the legacy enrolled pupils ensures that academy conversion was exogenous to enrolment in secondary school. Thus the set up is 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 pupils enrolled in the predecessor school

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

who pre-conversion are lined up 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.

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 exposure varies depending on when conversion took place. Table A1 of the Data Appendix shows the structure of this treatment in more detail.

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:

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

with X denoting a set of control variables.

In (3) estimates of the θ_1 coefficient are 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 (4) estimates of θ_2 show the proportion of the ITT group that stay in the academy and take KS4 exams there. Equation (5) is the reduced form regression of KS4 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.[20] We estimate each of these separately for community and non-community predecessor schools to enable 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 the 106 academies have

[20] 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.

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 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. There is one exception here, as the 12 conversions from City Technology Colleges (CTCs) were already highly autonomous schools that were performing well. We therefore omit these and define the treatment group as the 94 new academies that converted from the four groups of state maintained schools: community, voluntary controlled, foundation and voluntary aided schools.[21] This leaves us with the following numbers per conversion cohort: 2002/3 – 3; 2003/4 – 6; 2004/5 – 2; 2005/6 – 7; 2006/7 – 14; 2007/8 – 25; 2008/9 – 37.

Looking in more detail at these 94 schools that become academies in our sample period and the 114 that become academies later, as shown in Panel B of Table 4, makes it clear that the two groups of schools have similar pre-treatment characteristics. A set of balancing tests is

[21] In fact, some commentators have identified CTCs as the precursors of academies (see West and Bailey, 2013). Almost all CTCs took up the opportunity to become academies when it arose with the introduction of academy schools. They were already highly autonomous schools already, being able to not fully follow the national curriculum, to run their own admissions, and not being maintained by the local authority. One can argue that the autonomy gains they experienced from academy conversion were negligible, unlike for the state maintained schools that converted who we study in this paper.

given in the final row of the Table. For most of the variables considered (the one exception being the proportion white), one cannot reject the null hypothesis that the 94 academies that convert in the sample period and the 114 to be academies have, on average, 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 concerns of mean reversion.

The data structure for our empirical work is based on a balanced panel of treatment and control schools for the school years 2001/02 to 2008/09 with repeated cross-sections of enrolled year 7 (for intake) and year 11 (for performance) pupils. The cross-time variation in the academy conversion programme means that we can set these up in the event study framework detailed above. Table A1 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). In column (2), we estimate heterogeneous (with respect to the number of years post conversion) effects in the event study setting, and in columns (3) and (4) 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 0.109 of a standard deviation (σ) higher than those attending schools yet to attain academy status. Thus on average the composition of newly enrolled pupils did alter after academy conversion.

The event study estimates in column (2) show there to be no pre-conversion differences in trends between pupils in the treatment and control schools. They show a significant conversion year impact (E = c) of 0.085σ. This gradually rises year on year post conversion, becoming strongly significant in statistical terms, before reaching 0.198σ by event year c+3. Thus on conversion, and with bigger shifts in the years after, academies began admitting higher ability pupils.

This positive and growing impact is shown in Figure 1, suggesting important compositional changes occurring in the academies student body over time. Interestingly, the positive intake effects are present both for academies that convert from community predecessor schools (as shown in column (3) of Table 5 where the conversion year impact of 0.093σ is significant and rises to 0.220σ by E = c+3) and from non-community predecessor schools (with comparable impacts of 0.077σ for E = c and 0.140σ by E = c+3). Figure 2 plots the event study estimates by predecessor type, showing there to be a numerically slightly larger impact by E = c+3 for conversions from predecessor community schools.

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 pupil performance from specifications without control variables. Columns (4) to (6) show estimates from value added specifications that net out end of primary school 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 0.113σ to 0.118σ. Adding the prior achievement measure (KS2) and control variables in columns (4) to (6) reduces this by a very small amount to a range of 0.106σ to 0.115σ, with all estimates remaining strongly 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 0.106σ improvement is that KS4 went up by 0.106σ 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 the vast majority - 96.3 percent[22] - did as the highly significant first stage at the bottom of the Table shows). Because of the high rate of compliance, this rises a touch to 0.110σ. 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 there to be 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 0.041σ

[22] The implied degree of pupil mobility in the secondary school years from this 96.3 percent (or 3.7 percent moving) lines up well with pupil mobility numbers for English schools described in Machin, ⁅Telhaj and Wilson (2006).

higher (though statistically insignificant), and this rises to (a statistically significant) 0.305σ four years post-conversion. Figure 3 very clearly shows the significant upturn after treatment and the lack of pre-conversion differences. It 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 effects are more muted (and borderline significant) for the latter. These results reveal that pupils attending schools experiencing the largest increase in autonomy via conversion – those from predecessor community schools – experienced bigger performance improvements. The estimated effects are large, with treatment effects in the IV estimates being 0.075σ in the year of conversion and rising to 0.351σ by c+3. For conversions from non-community schools, the initial year impact is essentially zero (at -0.011σ) and rises only to a smaller 0.207σ by c+3.

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.[23] 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.

[23] It shows the equivalent to the Table 6 column (3) specification separately estimated for four year cohorts of conversions: these comprise the 9 conversions in school years 2002/03 and 2003/4 (3 from 2002/03 and 6 from 2003/04), the 9 conversions from school years 2004/5 and 2005/06 (2 from 2004/05 and 7 from 2005/06), the 39 conversions from 2006/07 and 2007/08 (14 from 2006/07 and 25 from 2007/08) and the 37 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.

Furthermore, the lack of differential pre-treatment trends for all cohorts is highly supportive of the research design we have implemented.

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 but are nevertheless exposed to treatment (i.e. they may attend the academy but leave prior to examinations). We have therefore reformulated the estimated models in terms of years of exposure to an academy. 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 0.304σ in the academy conversions from predecessor community schools.

Next we consider a falsification test. This is a further test of whether the estimated θ coefficients could 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 two schools who do not have GCSE sittings for some of the earlier 1997/98-2000/01 period.[24]

The falsification exercise was conducted over the seven year period between the 1997/98 and 2004/05 academic years. Column (1) of Table 9 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 the Table, we report the original specifications for the same sample of schools. They are very similar to the main KS4 results of the paper.[25] 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 A3-A5, for specifications 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) or pupil performance in GCSE Maths and English alone, 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.

[24] 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 A2.
[25] 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.

We have also produced estimates with a different set of control schools. This design has the same set of treatment schools, but now each cohort is lined up with their own control group of schools converting four years later. Using a control group of schools that convert four years later than the treatment group of converters means that the legacy enrolled children will have finished their KS4 assessments prior to control group children receiving treatment. The treatment-control structure of this design is shown in Table A6 (note that it involves using some of the later conversion cohorts as both treatments and controls in a rotating treatment-control design). Results from this alternative research design are shown in Table 10. Columns (3) and (4) compare legacy enrolled pupils in the 94 treatment schools with their t+4 matched controls. Because some of the controls for later cohorts go beyond the Academies Act conversions, we also show estimates based on this design for only the first five cohorts in columns (7) and (8). This ensure only pre-Academies Act conversions are only considered in the control group. For comparability reasons, we show the results for continuous and dummy variable specifications in the Table, thus comparing to the exposure specifications of Table 8, the key results of which are reproduced in columns (1) and (2) of Table 10 for all cohorts, and for the sub-sample of the first five cohorts in columns (5) and (6). The estimates in Table 10 clearly show that the range of estimated effects is very similar for all the specifications, which offers strong reassurances of the main findings of the paper.

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.[26] Table 11 shows

[26] 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.

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 11 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 12 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 evidence 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? We acknowledge that we are somewhat limited in what we can do with available data here, but offer two main sources of evidence, the first from survey data on academies, the second on data on changes in headteachers and teaching staff before and after conversion.

To begin this 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 collected information on a wide array of changes that may have occurred following

conversion.[27] These are summarised in Table 13 for 23 of the Labour academies we analyse in this paper, and for 148 academies (including the 23) overall.

Table 13 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 13: 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.

Table 14 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

[27] 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.

conversion year. In treatment schools, 63 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 66 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 can be gleaned from the results reported in Table 15. 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 14.

6. Conclusions

The research topic of what new school types can potentially do to alleviate concerns of poor education standards has become a high profile question of significant interest to educators, policymakers and parents. This paper focusses on what has become a high profile example of this – 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.

In undertaking this empirical study, we have offered 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. For example for children attending academies that converted from a community school we find that transformation to an academy raised their educational outcomes by 0.139σ on average, and by more for children receiving more years of treatment (rising to 0.351 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. They also add significantly to this literature as many of the best identified studies of US charters are often focussed on a single city or state setting. The national scope of the effort we study in this paper makes our findings less likely to be driven by context-specific factors than some of that research.

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 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 has become 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.[28] 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.

[28] 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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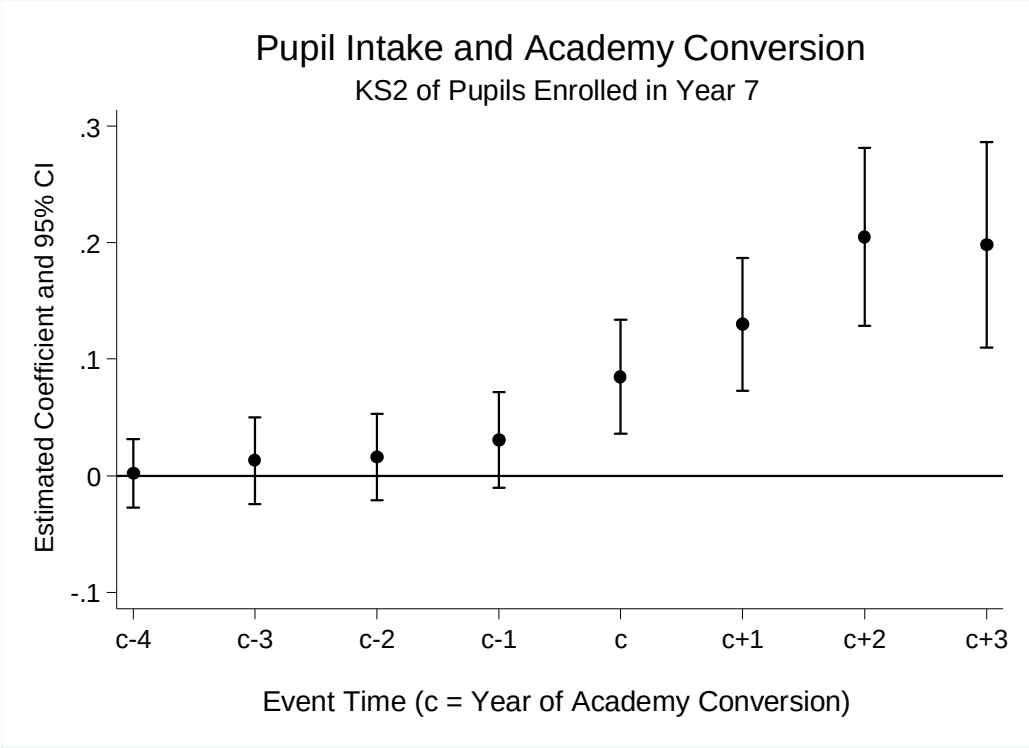
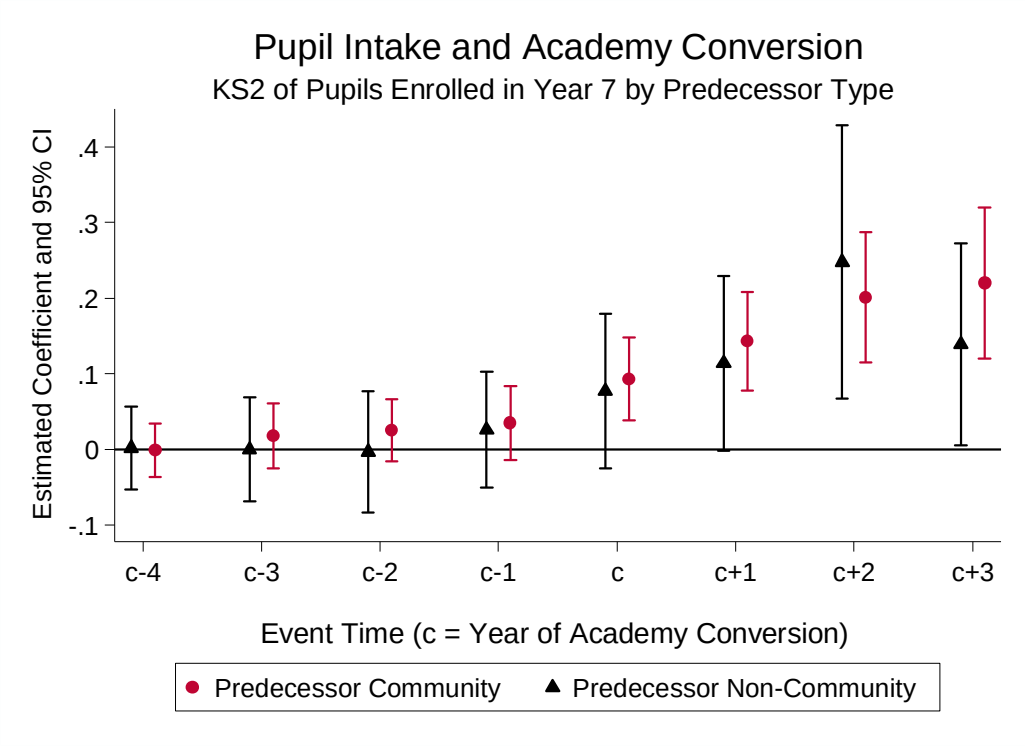
Figure 1: Event Study Estimates of Pupil Intake and Academy Conversion, Key Stage 2, Pupils Enrolled in Year 7



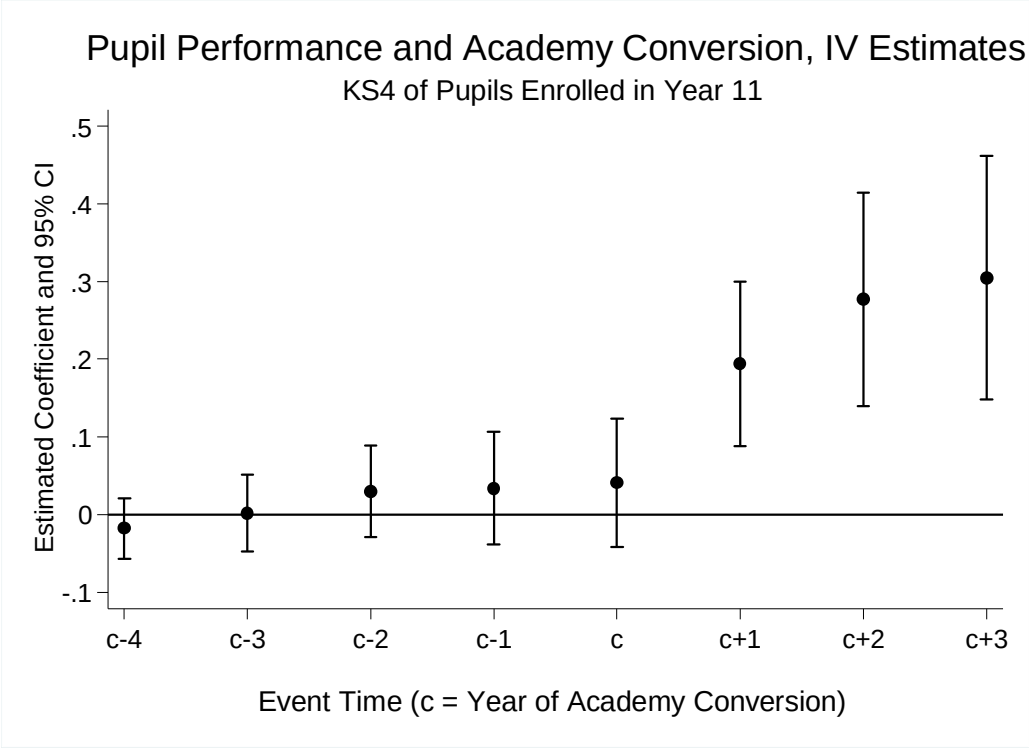
Notes: From column (2) specification of Table 5.

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



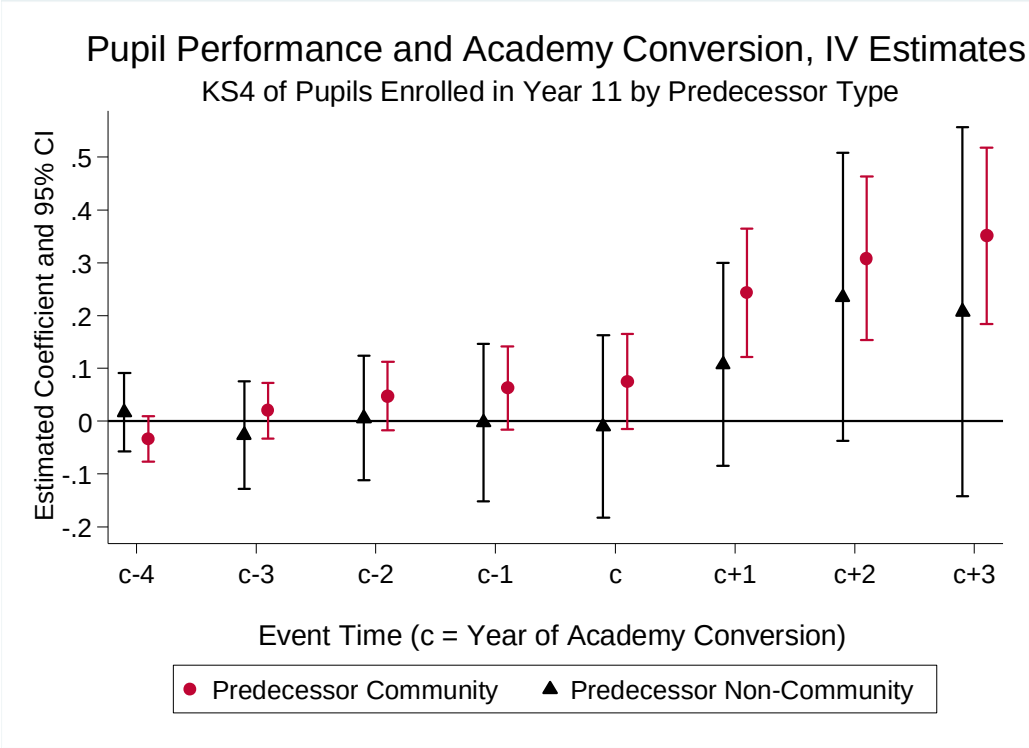
Notes: From columns (3) and (4) specifications of Table 5.

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



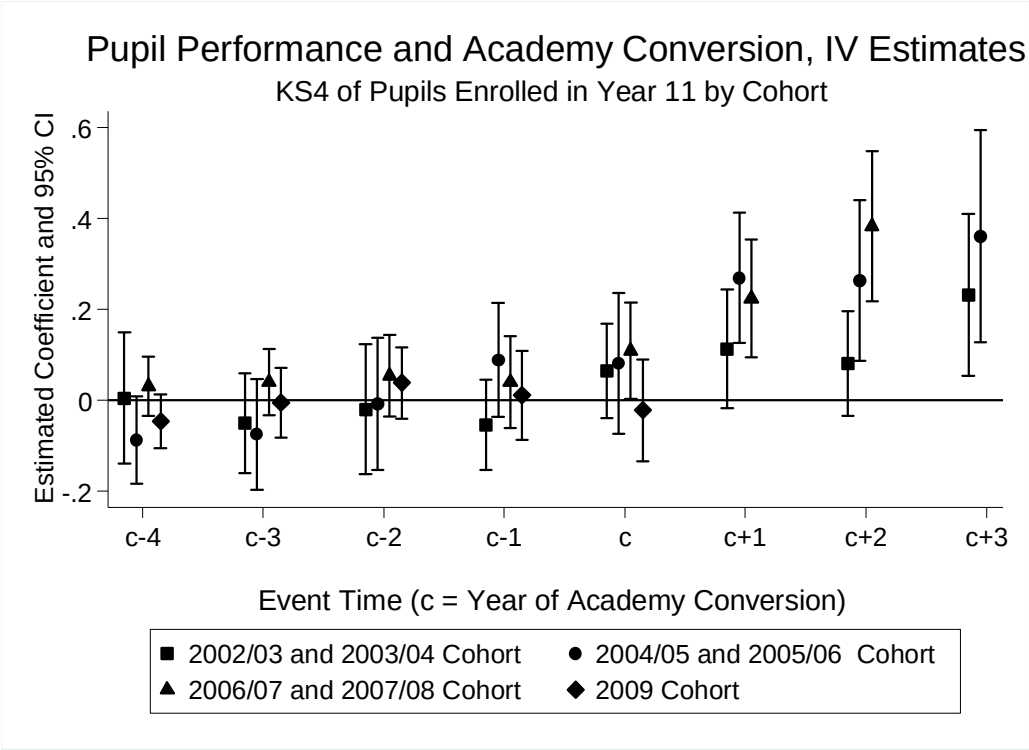
Notes: From column (9) specification of Table 6.

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
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	57.804	0.934	0.487	0.968	0.095	0.060	2
Voluntary aided	66.763	43.323	0.578	0.505	0.798	0.126	0.143	502
Foundation	65.516	43.34	0.573	0.522	0.85	0.092	0.146	470
Voluntary controlled	66.827	43.515	0.579	0.51	0.876	0.077	0.153	96
Community	61.983	38.312	0.46	0.503	0.828	0.153	0.188	1933
Academies (Pre-conversion)	57.230	31.689	0.316	0.536	0.725	0.250	0.254	106
B. Academy Schools								
Current academies (treatment group)	55.408	29.619	0.267	0.536	0.804	0.262	0.277	94
Future academies (control group)	56.480	30.912	0.285	0.515	0.812	0.232	0.241	114
Difference	-1.072 (0.798)	-1.293 (0.834)	-0.018 (0.018)	0.021 (0.015)	-0.008 (0.004)	0.031 (0.019)	0.036 (0.019)	

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)
Academy x Post-Conversion (E = c to c+3)	0.109 (0.023)			
Academy x (E = c-4)		0.002 (0.015)	-0.001 (0.018)	0.002 (0.028)
Academy x (E = c-3)		0.013 (0.019)	0.018 (0.022)	0.000 (0.035)
Academy x (E = c-2)		0.016 (0.019)	0.025 (0.021)	-0.003 (0.041)
Academy x (E = c-1)		0.031 (0.021)	0.035 (0.025)	0.026 (0.039)
Academy x (E = c)		0.085 (0.025)	0.093 (0.028)	0.077 (0.052)
Academy x (E = c+1)		0.130 (0.029)	0.143 (0.033)	0.114 (0.059)
Academy x (E = c+2)		0.205 (0.039)	0.201 (0.044)	0.248 (0.092)
Academy x (E = c+3)		0.198 (0.045)	0.220 (0.051)	0.139 (0.068)
School Fixed Effects	Yes	Yes	Yes	Yes
Year Dummies	Yes	Yes	Yes	Yes
R-Squared	0.056	0.056	0.054	0.063
Sample Size	1333322	1333322	1036301	297021
Number of Treatment and Control Schools	208	208	158	50

Notes: E denotes event year and c is the year of conversion. Robust standard errors (clustered at the school level) are reported in parentheses

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)	0.117 (0.029)	0.113 (0.029)	0.118 (0.030)	0.115 (0.030)	0.106 (0.030)	0.110 (0.031)			
Academy x (E = c-4)							-0.017 (0.019)	-0.018 (0.020)	-0.018 (0.020)
Academy x (E = c-3)							0.003 (0.024)	0.002 (0.025)	0.002 (0.025)
Academy x (E = c-2)							0.031 (0.028)	0.030 (0.030)	0.030 (0.030)
Academy x (E = c-1)							0.035 (0.035)	0.035 (0.037)	0.034 (0.037)
Academy x (E = c)							0.044 (0.040)	0.040 (0.042)	0.041 (0.042)
Academy x (E = c+1)							0.196 (0.051)	0.185 (0.051)	0.194 (0.054)
Academy x (E = c+2)							0.284 (0.064)	0.253 (0.063)	0.277 (0.070)
Academy x (E = c+3)							0.288 (0.066)	0.269 (0.069)	0.305 (0.080)
KS2 Standardised Test Score				0.521 (0.006)	0.521 (0.006)	0.521 (0.006)	0.521 (0.006)	0.521 (0.006)	0.521 (0.006)
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	0.075	0.075	0.075	0.472	0.472	0.472	0.472	0.472	0.472
Sample Size	1246598	1246598	1246598	1246598	1246598	1246598	1246598	1246598	1246598
Number of Treatment and Control Schools	208	208	208	208	208	208	208	208	208
First Stage Coefficient on ITT			0.963 (0.003)			0.963 (0.003)			
First Stage Coefficient on ITT x (E = c)									0.986 (0.001)
First Stage Coefficient on ITT x (E = c+1)									0.952 (0.003)
First Stage Coefficient on ITT x (E = c+2)									0.912 (0.007)
First Stage Coefficient on ITT x (E = c+3)									0.880 (0.013)

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 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 need, entered together with KS2 test scores and a dummy variable for pupils for whom KS2 data is unavailable

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)	0.139 (0.035)		0.053 (0.063)	
Academy x (E = c-4)		-0.033 (0.022)		0.017 (0.038)
Academy x (E = c-3)		0.020 (0.027)		-0.026 (0.052)
Academy x (E = c-2)		0.048 (0.033)		0.006 (0.060)
Academy x (E = c-1)		0.063 (0.040)		-0.002 (0.076)
Academy x (E = c)		0.075 (0.046)		-0.010 (0.088)
Academy x (E = c+1)		0.243 (0.062)		0.108 (0.098)
Academy x (E = c+2)		0.308 (0.079)		0.235 (0.139)
Academy x (E = c+3)		0.351 (0.085)		0.207 (0.178)
KS2 Standardised Test Score	0.527 (0.007)	0.527 (0.007)	0.499 (0.012)	0.499 (0.012)
School Fixed Effects	Yes	Yes	Yes	Yes
Control Variables	Yes	Yes	Yes	Yes
Year Dummies	Yes	Yes	Yes	Yes
R-Squared	0.477	0.477	0.456	0. 456
Sample Size	972678	972678	273920	273920
Number of Treatment and Control Schools	158	158	50	50
First Stage Coefficient on ITT	0.962 (0.003)		0.966 (0.005)	
First Stage Coefficient on ITT x (E = c)		0.985 (0.001)		0.988 (0.002)
First Stage Coefficient on ITT x (E = c+1)		0.954 (0.004)		0.950 (0.006)
First Stage Coefficient on ITT x (E = c+2)		0.917 (0.007)		0.894 (0.020)
First Stage Coefficient on ITT x (E = c+3)		0.881 (0.014)		0.877 (0.030)

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	0.077 (0.016)		0.088 (0.017)		0.052 (0.033)	
One Year of Exposure to Academy		0.031 (0.029)		0.054 (0.033)		-0.009 (0.06)
Two Years of Exposure to Academy		0.182 (0.044)		0.218 (0.050)		0.110 (0.077)
Three Year of Exposure to Academy		0.260 (0.060)		0.274 (0.068)		0.236 (0.115)
Four Years of Exposure to Academy		0.279 (0.068)		0.304 (0.072)		0.205 (0.158)
KS2 Standardised Test Score	0.521 (0.006)	0.521 (0.006)	0.527 (0.007)	0.527 (0.007)	0.499 (0.012)	0.499 (0.012)
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	0.472	0.472	0.477	0.477	0.456	0.456
Sample Size	1246598	1246598	972678	972678	273920	273920
Number of Treatment and Control Schools	208	208	158	158	50	50
First Stage Coefficient on ITT Years of Exposure	0.934 (0.005)		0.934 (0.006)		0.935 (0.012)	
First Stage Coefficient on ITT x One Year of Exposure		0.985 (0.001)		0.984 (0.001)		0.987 (0.002)
First Stage Coefficient on ITT x Two Years of Exposure		0.951 (0.003)		0.951 (0.004)		0.949 (0.007)
First Stage Coefficient on ITT x Three Years of Exposure		0.911 (0.007)		0.915 (0.007)		0.892 (0.020)
First Stage Coefficient on ITT x Four Years of Exposure		0.874 (0.013)		0.875 (0.015)		0.872 (0.031)

Notes: As for Table 6.

Table 9: Fake Policy Dated (c-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)	-0.031 (0.025)	-0.010 (0.030)	Academy x (E = c-4)	0.000 (0.019)	-0.007 (0.021)	-0.021 (0.019)	-0.034 (0.022)
Academy x (E = c-7)	-0.055 (0.037)	-0.018 (0.039)	Academy x (E = c-3)	0.025 (0.025)	0.036 (0.027)	0.002 (0.024)	0.019 (0.026)
Academy x (E = c-6)	-0.055 (0.044)	-0.016 (0.047)	Academy x (E = c-2)	0.058 (0.029)	0.072 (0.031)	0.029 (0.029)	0.045 (0.031)
Academy x (E = c-5)	-0.056 (0.047)	-0.012 (0.049)	Academy x (E = c-1)	0.050 (0.036)	0.078 (0.039)	0.034 (0.036)	0.060 (0.039)
Academy x (E = c-4)	-0.062 (0.050)	-0.025 (0.053)	Academy x (E = c)	0.090 (0.040)	0.119 (0.042)	0.045 (0.041)	0.073 (0.044)
Academy x (E = c-3)	-0.048 (0.056)	-0.018 (0.06)	Academy x (E = c+1)	0.256 (0.050)	0.300 (0.057)	0.197 (0.051)	0.239 (0.059)
Academy x (E = c-2)	-0.009 (0.064)	0.023 (0.069)	Academy x (E = c+2)	0.336 (0.059)	0.348 (0.067)	0.283 (0.064)	0.300 (0.073)
Academy x (E = c-1)	0.034 (0.075)	0.051 (0.085)	Academy x (E = c+3)	0.387 (0.075)	0.433 (0.081)	0.286 (0.066)	0.326 (0.070)
P-value for test of joint significance of Fake Policy (c-8 to c-1)	0.524	0.920					
KS2 Standardised Test Score						0.521 (0.006)	0.527 (0.007)
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	0.086	0.082		0.130	0.072	0.473	0.477
Sample Size	1320469	1035966		1241175	968292	1241175	968292
Number of Treatment and Control Schools	206	156		206	156	206	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, control variables for specifications (1) and (2) are limited for the Fake Policy time period and comprise solely of gender. Full controls are as for Table 6.

Table 10: Results From Alternative Research Design

	All Cohorts				First Five Cohorts			
	Table 8, Specification (1)	Table 8, Specification (2)	t+4 Research Design,		Table 8, Specification (1)	Table 8, Specification (2)	t+4 Research Design,	
	IV	IV	IV	IV	IV	IV	IV	IV
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Exposure	0.077 (0.016)		0.072 (0.016)		0.093 (0.017)		0.076 (0.019)	
One Year of Exposure		0.031 (0.029)		0.038 (0.024)		0.097 (0.036)		0.057 (0.035)
Two Years of Exposure		0.182 (0.044)		0.177 (0.037)		0.253 (0.052)		0.209 (0.044)
Three Years of Exposure		0.260 (0.060)		0.226 (0.062)		0.287 (0.060)		0.242 (0.061)
Four Years of Exposure		0.279 (0.068)		0.235 (0.071)		0.310 (0.069)		0.251 (0.071)
KS2 Standardised Test Score	0.521 (0.006)	0.521 (0.006)	0.506 (0.005)	0.506 (0.005)	0.527 (0.006)	0.527 (0.006)	0.505 (0.005)	0.505 (0.005)
School Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Control Variables	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year Dummies	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R-Squared	0.472	0.472	0.471	0.471	0.473	0.473	0.469	0.469
Sample Size	1246598	1246598	255638	255638	796488	796488	187401	187401
Number of Treatment and Control Schools	208	208	303	303	146	146	208	208
First Stage Coefficient on ITT Years of Exposure	0.934 (0.005)		0.923 (0.006)		0.922 (0.007)		0.912 (0.007)	
First Stage Coefficient on ITT x One Year of Exposure		0.985 (0.001)		0.985 (0.001)		0.985 (0.002)		0.984 (0.002)
First Stage Coefficient on ITT x Two Years of Exposure		0.951 (0.003)		0.949 (0.003)		0.946 (0.005)		0.946 (0.005)
First Stage Coefficient on ITT x Three Years of Exposure		0.911 (0.007)		0.911 (0.006)		0.911 (0.007)		0.910 (0.006)
First Stage Coefficient on ITT x Four Years of Exposure		0.874 (0.013)		0.875 (0.013)		0.874 (0.013)		0.875 (0.013)

Notes: As for Table 8. The numbers of treatment and control schools for the t+4 research design for specifications (3), (4), (7) and (8) are shown in Appendix Table A6.

Table 11: 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	1	2	0	1	8
	Good	0	2	2	4	11
	Satisfactory	1	3	8	2	14
	Inadequate	2	6	5	0	13
	Total	4	13	15	7	46

Percent Improvement in Ranking = 44
Percent No Change in Ranking = 28
Percent Reduction in Ranking = 28

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, 39 in upper panel, 105 in lower panel.

Table 12: Ordered Probit Estimates of Change in Ofsted Ranking, School Level

	Pr[Change in OfSted Ranking]	
	(1)	(2)
Current Academies (Treatment Group)	0.715 (0.243)	0.705 (0.247)
Control Variables	No	Yes
Sample Size (Number of Treatment and Control Schools)	144	144
Marginal Effects:		
Pr[Change = 2||Treatment=1] – Pr[Change=2|Treatment=0]	0.243 (0.073)	0.257 (0.092)
Pr[Change = 1||Treatment=1] – Pr[Change=1|Treatment=0]	-0.018 (0.028)	-0.018 (0.027)
Pr[Change = 0||Treatment=1] – Pr[Change=0|Treatment=0]	-0.261 (0.091)	-0.239 (0.075)

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 13: 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	148 Sponsored Academies Including the 23 Labour Academies	148 Sponsored Academies Including the 23 Labour Academies
	Percent Making Change	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 14: 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)	-0.039 (0.054)	-0.033 (0.069)	-0.085 (0.092)
Academy x (E = c-3)	-0.003 (0.059)	-0.015 (0.066)	0.003 (0.121)
Academy x (E = c-2)	0.038 (0.059)	0.079 (0.070)	-0.100 (0.108)
Academy x (E = c-1)	0.035 (0.053)	0.040 (0.057)	-0.013 (0.120)
Academy x (E = c)	0.632 (0.058)	0.617 (0.072)	0.656 (0.096)
Academy x (E = c+1)	-0.022 (0.065)	-0.016 (0.072)	-0.049 (0.130)
Academy x (E = c+2)	0.086 (0.092)	0.116 (0.106)	-0.031 (0.204)
Academy x (E = c+3)	0.074 (0.126)	0.072 (0.147)	0.114 (0.265)
P-value for test of joint significance (E = c-4 to c-1)	0.694	0.750	0.759
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	0.319	0.315	0.365
Sample Size	1641	1244	397
Number of Treatment and Control Schools	208	158	50

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 0.359 (0.045).

Table 15: 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)	0.008 (0.011)	0.015 (0.012)	-0.003 (0.022)	-0.002 (0.009)	-0.003 (0.010)	-0.002 (0.020)	0.011 (0.009)	0.018 (0.018)	-0.001 (0.019)
Academy x (E = c-3)	0.006 (0.018)	0.030 (0.020)	-0.055 (0.038)	-0.018 (0.015)	-0.015 (0.017)	-0.029 (0.029)	0.024 (0.016)	0.045 (0.018)	-0.026 (0.032)
Academy x (E = c-2)	-0.003 (0.024)	0.026 (0.026)	-0.073 (0.049)	-0.038 (0.022)	-0.032 (0.024)	-0.061 (0.048)	0.035 (0.019)	0.057 (0.022)	-0.013 (0.035)
Academy x (E = c-1)	-0.004 (0.033)	0.031 (0.036)	-0.111 (0.066)	-0.061 (0.031)	-0.057 (0.035)	-0.096 (0.063)	0.057 (0.050)	0.088 (0.027)	-0.015 (0.053)
Academy x (E = c)	0.028 (0.043)	0.055 (0.048)	-0.083 (0.083)	-0.059 (0.042)	-0.060 (0.048)	-0.097 (0.086)	0.087 (0.031)	0.116 (0.037)	0.014 (0.057)
Academy x (E = c+1)	0.084 (0.053)	0.097 (0.062)	-0.018 (0.092)	-0.009 (0.052)	-0.013 (0.058)	-0.064 (0.103)	0.092 (0.037)	0.110 (0.044)	0.046 (0.070)
Academy x (E = c+2)	0.096 (0.073)	0.129 (0.078)	-0.030 (0.161)	0.026 (0.067)	0.030 (0.070)	-0.031 (0.151)	0.070 (0.042)	0.098 (0.049)	0.001 (0.091)
Academy x (E = c+3)	0.229 (0.076)	0.250 (0.084)	0.153 (0.135)	0.157 (0.074)	0.151 (0.073)	0.131 (0.185)	0.071 (0.061)	0.096 (0.072)	0.021 (0.117)
P-value for test of joint significance (E = c-4 to c-1)	0.757	0.530	0.391	0.195	0.288	0.569	0.207	0.033	0.814
P-value for test of joint significance (E = c to c+3)	0.004	0.038	0.000	0.000	0.000	0.008	0.059	0.036	0.920
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	0.899	0.886	0.937	0.921	0.918	0.948	0.603	0.593	0.653
Sample Size	1641	1244	397	1641	1244	397	1641	1244	397
Number of Treatment and Control Schools	208	158	50	157	158	50	208	158	50

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 0.059 (0.031), for (4) for all schools is 0.031 (0.027) and for (7) for all schools is 0.029 (0.016).

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. As shown in Table 4, this 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 (of which we use the 94 who were not CTCs in their predecessor state) 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, but convert by September 2010.

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 208 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[29], 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 2001/02 – 2008/09 while our results analysis focuses on those who sit exams, are ITT or receive exposure in one of our 94 treatment schools or sit exams in one of our 208 control schools over the same period.

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, in our fake policy results the only covariate, aside from year dummies and school fixed effects, is 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.

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-1 (where c denotes conversion year) and academic year group j an individual is expected to sit their exams in c-1 + (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 '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.

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

[29] 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.

school that they sit their KS4 exams in. In estimated specifications, standard errors are clustered on this unique number resulting in 208 clusters in Tables 6, 7 and 8.

4. Attainment Measures

The main variable in our analysis of intake is an average score across three subject specific key stage 2 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 as a raw test score. The raw test score is graded 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 Maths 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 from 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 between adjacent grades as follows:

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

New scale

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

Grade	Points	Grade	Points
A*	8	D	4
A	7	E	3
B	6	F	2
C	5	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*.[30] 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	Grade	Points
A*	10	D	4
A	8	E	3
B	7	F	2
C	6	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.[31] 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

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

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

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 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 A3-A5) 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 GCSE qualifications of at least grade C.

We present results for all our main performance specifications in Tables A3-A5 using different dependent variables.

5. Ofsted Reports[32]

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.[33] 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.

[32] 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.
[33] Overall effectiveness ratings have been awarded since 2000.

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.

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 39 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 6^{th} 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-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							398	532	537	588	522	3
2003/04						758	800	1061	1065	1138	1081	6
2004/05					264	245	265	340	378	375	379	2
2005/06				903	887	860	806	1026	1058	1158	1183	7
2006/07			2429	2433	2251	2203	2148	2207	2272	2488		14
2007/08		4166	4146	4151	3794	3823	3622	3835	4229			25
2008/09	7509	7493	7191	6671	6360	5961	5662	5765				37

Key Stage 4 (Year 11)

E =	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							399	498	446	397	315	3
2003/04						894	948	957	713	718	630	6
2004/05					263	302	300	258	237	221	229	2
2005/06				847	922	948	912	810	785	714	681	7
2006/07			2191	2366	2450	2341	2274	2183	1967	1883		14
2007/08		3849	3902	4077	3953	3956	4048	3907	3492			25
2008/09	6486	6840	6857	7073	7039	7090	6839	6272				37

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: 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	451	399	3
2003/04						844	901	889	939	894	948	6
2004/05					248	214	259	286	263	302	300	2
2005/06				777	749	759	776	847	922	948	912	7
2006/07			2161	2199	2159	2315	2191	2366	2450	2341		14
2007/08		3463	3453	3550	3619	3737	3789	3953	3810			24
2008/09	6307	6372	6280	6515	6486	6840	6857	7073				37

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. Two schools drop out of the fake policy sample – one is in the 2007/08 cohort and the other is a control school converting in 2010/11.

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

	Key Stage 4 A*-C			GCSE 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	-0.007	-0.007	0.002	0.008	0.008	-0.016	-0.012	-0.012	-0.019	-0.027	-0.027
	(0.01)	(0.01)	(0.010)	(0.018)	(0.019)	(0.019)	(0.02)	(0.021)	(0.021)	(0.047)	(0.048)	(0.048)
Academy x (E = c-3)	0.019	0.008	0.008	0.008	0.014	0.014	-0.006	-0.002	-0.002	0.008	0.000	0.000
	(0.013)	(0.014)	(0.014)	(0.02)	(0.021)	(0.021)	(0.024)	(0.025)	(0.025)	(0.063)	(0.066)	(0.066)
Academy x (E = c-2)	0.034	0.022	0.022	0.032	0.039	0.039	0.024	0.028	0.028	0.043	0.035	0.035
	(0.016)	(0.017)	(0.017)	(0.027)	(0.029)	(0.029)	(0.029)	(0.03)	(0.03)	(0.067)	(0.071)	(0.071)
Academy x (E = c-1)	0.037	0.024	0.024	0.073	0.08	0.08	0.048	0.053	0.053	-0.007	-0.016	-0.016
	(0.019)	(0.020)	(0.020)	(0.03)	(0.032)	(0.032)	(0.031)	(0.033)	(0.032)	(0.077)	(0.081)	(0.081)
Academy x (E = c)	0.054	0.036	0.037	0.087	0.094	0.096	0.004	0.008	0.008	0.083	0.069	0.069
	(0.022)	(0.022)	(0.022)	(0.036)	(0.038)	(0.038)	(0.036)	(0.037)	(0.038)	(0.085)	(0.089)	(0.09)
Academy x (E = c+1)	0.137	0.107	0.112	0.162	0.168	0.176	0.016	0.026	0.027	0.309	0.275	0.289
	(0.025)	(0.026)	(0.027)	(0.049)	(0.049)	(0.051)	(0.042)	(0.043)	(0.045)	(0.112)	(0.118)	(0.124)
Academy x (E = c+2)	0.194	0.142	0.155	0.185	0.185	0.202	0.052	0.056	0.061	0.429	0.375	0.410
	(0.034)	(0.032)	(0.035)	(0.049)	(0.053)	(0.058)	(0.052)	(0.056)	(0.062)	(0.125)	(0.127)	(0.139)
Academy x (E = c+3)	0.177	0.120	0.136	0.247	0.258	0.293	0.148	0.160	0.182	0.324	0.257	0.291
	(0.031)	(0.031)	(0.035)	(0.064)	(0.063)	(0.071)	(0.071)	(0.071)	(0.081)	(0.133)	(0.139)	(0.157)
KS2 Standardised Test Score	0.238	0.238	0.238	0.617	0.617	0.617	0.561	0.561	0.561	0.166	0.166	0.166
	(0.003)	(0.003)	(0.003)	(0.006)	(0.006)	(0.006)	(0.008)	(0.008)	(0.008)	(0.012)	(0.012)	(0.012)
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	0.355	0.354	0.355	0.532	0.532	0.532	0.514	0.514	0.514	0.195	0.195	0.195
Sample Size	1246598	1246598	1246598	1246598	1246598	1246598	1246598	1246598	1246598	1246598	1246598	1246598
Number of Treatment and Control Schools	208	208	208	208	208	208	208	208	208	208	208	208

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

	Key Stage 4 A*-C			GCSE 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)	-0.006	-0.015	-0.015	-0.006	0	0	-0.016	-0.01	-0.01	-0.07	-0.079	-0.079
	(0.01)	(0.011)	(0.011)	(0.022)	(0.022)	(0.022)	(0.025)	(0.025)	(0.025)	(0.047)	(0.05)	(0.05)
Academy x (E = c-3)	0.023	0.014	0.014	0.022	0.028	0.028	0.007	0.014	0.014	0.006	-0.004	-0.004
	(0.015)	(0.016)	(0.015)	(0.024)	(0.025)	(0.025)	(0.031)	(0.032)	(0.032)	(0.078)	(0.082)	(0.082)
Academy x (E = c-2)	0.039	0.028	0.028	0.022	0.029	0.029	0.028	0.035	0.035	0.045	0.034	0.034
	(0.018)	(0.019)	(0.019)	(0.032)	(0.033)	(0.033)	(0.035)	(0.037)	(0.037)	(0.079)	(0.085)	(0.084)
Academy x (E = c-1)	0.042	0.031	0.031	0.071	0.078	0.078	0.053	0.061	0.061	0.016	0.005	0.004
	(0.021)	(0.022)	(0.022)	(0.032)	(0.034)	(0.034)	(0.037)	(0.039)	(0.039)	(0.089)	(0.095)	(0.094)
Academy x (E = c)	0.06	0.044	0.044	0.094	0.103	0.104	0.037	0.044	0.045	0.064	0.047	0.047
	(0.024)	(0.025)	(0.025)	(0.037)	(0.038)	(0.039)	(0.042)	(0.043)	(0.044)	(0.099)	(0.105)	(0.106)
Academy x (E = c+1)	0.145	0.119	0.125	0.215	0.214	0.224	0.047	0.058	0.061	0.308	0.273	0.287
	(0.029)	(0.03)	(0.031)	(0.042)	(0.044)	(0.046)	(0.053)	(0.054)	(0.057)	(0.132)	(0.138)	(0.145)
Academy x (E = c+2)	0.193	0.146	0.159	0.181	0.186	0.202	0.04	0.057	0.062	0.445	0.382	0.416
	(0.039)	(0.037)	(0.04)	(0.052)	(0.056)	(0.061)	(0.061)	(0.066)	(0.072)	(0.144)	(0.148)	(0.161)
Academy x (E = c+3)	0.185	0.132	0.15	0.279	0.285	0.323	0.136	0.157	0.178	0.339	0.261	0.295
	(0.031)	(0.033)	(0.037)	(0.059)	(0.058)	(0.065)	(0.081)	(0.081)	(0.093)	(0.143)	(0.153)	(0.172)
KS2 Standardised Test Score	0.238	0.238	0.238	0.623	0.623	0.623	0.565	0.565	0.565	0.169	0.169	0.169
	(0.004)	(0.004)	(0.004)	(0.007)	(0.007)	(0.007)	(0.009)	(0.009)	(0.009)	(0.013)	(0.013)	(0.013)
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	0.359	0.359	0.359	0. 538	0. 538	0.538	0.510	0.510	0.510	0.178	0.178	0.178
Sample Size	972678	972678	972678	972678	972678	972678	972678	972678	972678	972678	972678	972678
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 IV specifications are the same as those reported in Table 7.

Table A5: Pupil Performance, Different Key Stage 4 Dependent Variables, Year 11, 2000/01 to 2008/09, Non-Community Predecessor School

	Key Stage 4 A*-C			GCSE 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)	0.029	0.014	0.014	0.024	0.032	0.033	-0.008	-0.009	-0.009	0.092	0.092	0.092
	(0.018)	(0.019)	(0.019)	(0.031)	(0.032)	(0.032)	(0.034)	(0.035)	(0.035)	(0.097)	(0.096)	(0.095)
Academy x (E = c-3)	0.019	0.005	0.005	-0.018	-0.009	-0.009	-0.031	-0.032	-0.032	0.045	0.044	0.045
	(0.027)	(0.027)	(0.027)	(0.04)	(0.043)	(0.043)	(0.035)	(0.037)	(0.037)	(0.102)	(0.105)	(0.103)
Academy x (E = c-2)	0.031	0.016	0.016	0.072	0.08	0.08	0.018	0.017	0.018	0.079	0.078	0.078
	(0.032)	(0.033)	(0.032)	(0.057)	(0.062)	(0.061)	(0.054)	(0.057)	(0.056)	(0.119)	(0.124)	(0.122)
Academy x (E = c-1)	0.041	0.026	0.026	0.123	0.132	0.132	0.055	0.054	0.054	-0.023	-0.023	-0.023
	(0.038)	(0.039)	(0.038)	(0.076)	(0.081)	(0.08)	(0.058)	(0.062)	(0.061)	(0.147)	(0.154)	(0.152)
Academy x (E = c)	0.055	0.033	0.033	0.113	0.122	0.123	-0.064	-0.067	-0.068	0.183	0.179	0.181
	(0.043)	(0.043)	(0.043)	(0.09)	(0.095)	(0.095)	(0.065)	(0.069)	(0.069)	(0.161)	(0.169)	(0.169)
Academy x (E = c+1)	0.135	0.093	0.097	0.083	0.112	0.117	-0.048	-0.038	-0.041	0.367	0.341	0.359
	(0.047)	(0.048)	(0.051)	(0.115)	(0.119)	(0.124)	(0.066)	(0.069)	(0.072)	(0.209)	(0.22)	(0.230)
Academy x (E = c+2)	0.217	0.149	0.166	0.26	0.244	0.273	0.142	0.105	0.117	0.393	0.373	0.416
	(0.066)	(0.063)	(0.071)	(0.111)	(0.121)	(0.133)	(0.092)	(0.1)	(0.11)	(0.245)	(0.243)	(0.273)
Academy x (E = c+3)	0.165	0.095	0.107	0.147	0.181	0.205	0.215	0.202	0.231	0.355	0.337	0.382
	(0.081)	(0.073)	(0.084)	(0.135)	(0.144)	(0.163)	(0.122)	(0.121)	(0.134)	(0.312)	(0.313)	(0.359)
KS2 Standardised Test Score	0.235	0.235	0.235	0.596	0.596	0.596	0.549	0.549	0.549	0.153	0.153	0.153
	(0.009)	(0.009)	(0.009)	(0.016)	(0.016)	(0.016)	(0.014)	(0.014)	(0.014)	(0.031)	(0.031)	(0.031)
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	0.341	0.341	0.341	0.513	0.513	0.513	0.527	0.527	0.527	0.253	0.253	0.253
Sample Size	273920	273920	273920	273920	273920	273920	273920	273920	273920	273920	273920	273920
Number of Treatment and Control Schools	50	50	50	50	50	50	50	50	50	50	50	50

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

Table A6: Number of Academy Conversions by Year For Alternative Research Design

	2002/3	2003/4	2004/5	2005/6	2006/7	2007/8	2008/9	All
Treatment and Control Schools								
Number of Treatment Schools	3	6	2	7	14	25	37	94
Number of Control Schools Who Convert Four Years Later	14	25	37	58	56	41	54	295

Notes: Source is <https://www.gov.uk/government/uploads/system/uploads/attachment_data/file/175360/academies_annual_report_2010-11.pdf at school level reported in parentheses.