Changing School Autonomy: Academy Schools and Their Introduction to England's Education[1]

Stephen Machin[*] and James Vernoit[**]

May 2012

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

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

Corresponding author: s.machin@ucl.ac.uk

Abstract:

A small, but growing, literature has begun to explore whether different school types influence pupil achievement. In this paper, we study a high profile example – the introduction of academy schools to the English secondary school sector. Our results indicate that, in some settings, academy conversion generated a significant improvement in the quality of pupil intake and pupil performance. The settings associated with beneficial results arise from heterogeneity in the estimated effects, as improvements only occur for schools that have been established as academies for a sufficiently long period and for those experiencing the largest increase in their school autonomy.

JEL Keywords: Academies; Pupil Intake; Pupil Performance.
JEL Classifications: I20; I21; I28.

[1] Correspondence to be sent to: Stephen Machin.

1. Introduction

Around the world, it is recognised that a highly educated and skilled workforce is one of the key drivers of a country's future progress and prosperity. This has led to a keen interest in the types of educational institutions that deliver better outcomes for their students. Unsurprisingly then, the case of schools, and the policies they pursue, has attracted a large amount of attention.

Some nations have been innovative in their quest for the optimal school type, while others pursue policies with little deviation from the orthodox model of the traditional local or community school. Charter schools – a school type in the US that allows the managing body of the school to gain autonomy levels more like private schools – are one example of a new type of school and their introduction has spread across many states. In England, new types of schools have been introduced: academy schools – the subject of this paper – are probably the most well known example.

A small, but growing, economics of education literature has presented empirical estimates of the impact of various school types on pupil achievement. For example, US work on charter schools finds some evidence of achievement gains associated with charter status.[2] In the UK, a small body of work has identified the impact of specific school types on educational and labour market outcomes.[3]

The gradual introduction of academy schools in England has proven to be a controversial area of schools policy since the first clutch of academies opened in September 2002. Academies are independent, non-selective, state-funded schools that fall outside the control of local authorities. In most cases, they are conversions of already existing predecessor schools. Academies are managed by a private team of independent co-sponsors. The sponsors of the academy school delegate the management of the school to a largely self-appointed board of governors with responsibility for

[2] This literature is not without 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; Angrist et al. 2010; Dobbie and Fryer 2011; Hoxby and Murarka 2009). Wider coverage non-experimental evaluations produce more mixed results (Center for Research on Education Outcomes 2009).

[3] 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).

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 impact of academy school conversion on pupil intake and pupil performance. To enable a before/after comparison, these lines of enquiry are considered over the school years 2001/02 to 2008/09. We bypass the selection bias that could be connected to academy conversion by comparing the outcomes of interest in academy schools to a specific group of comparison schools, namely those state-maintained schools that go on to become academies after our sample period ends. This approach enables us to produce a well-balanced treatment and control group that differences out key unobservable factors linked to conversion to academy status.

To preview our findings, we report results that, in some settings, conversion to an academy generated a significant improvement in the quality of pupil intake and in pupil performance. The settings associated with these beneficial results arise from heterogeneity in the estimated effects, in that improvements only occur for schools that have been established as academies for a sufficiently long period and for those experiencing the largest increase in their school autonomy.

In the next section of the paper, we discuss the structure of the secondary schooling system in England and document the rise of academies. We also describe how schools become academies and present a brief summary of related studies. Section 3 describes the data, the estimation framework that we adopt and uses this discussion to formulate key hypotheses to be tested in the empirical work. 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. We offer conclusions to the paper in section 5.

2. Academy Schools

Academies are a relatively new type of school that were first introduced as secondary schools to the English education system in the early 2000s. In this section, we consider their introduction. We discuss how academies relate to the other secondary school types that are in operation in England and we document the scale of the rise in the number of academy schools.

Secondary School Types in England and Academy Introductions

There are currently 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 school autonomy and governance. This is shown in Table 1. In this Table, we order the different school types by the amount of autonomy that their governing body/management body has to make schooling decisions, ranging from private schools with most autonomy and community schools with the least.

In the time period we study, academy schools were (typically) set up for a number of reasons. The main reason was to replace existing schools (that were often failing), but academies also arose to become an additional school in a particular area, 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). Prior to the Academies Act of 2010, which altered the notion of academy status, the path to establish an academy school in a local authority involved a number of steps.[4]

In Table 2, we show the number 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 2001/2 that we study in this paper. The Table shows that by the 2008/9 school year, there were 133 academies. These were gradually introduced, with the first three opening in 2002/3, and then speeding up in the subsequent school years as follows: 2003/4 - 9; 2004/5 - 5; 2005/6 - 10; 2006/7 - 19; 2007/8 - 37; 2008/9 - 30. The Table shows reductions in the other secondary school types as the share of academies rose to 4 percent of the secondary sector by 2008/9.

[4] For more detail on the nature of this process, see Appendix A of our earlier discussion paper (Machin and Vernoit 2010).

In Table 3, we look in more detail at which types of school converted to academy status. The upper panel of the Table shows all schools that get academy status, whilst the lower Panel shows conversions for the sample of schools on which we have data available pre- and post-academy conversion. The main differences is the small number of new academies (12), for which there is no predecessor school and 5 conversions from independent schools, for which we do not have predecessor school data.

Table 3 shows that the vast majority of academy schools are actually academy conversions from predecessor schools. The Table also shows that (at least) one school from every secondary school type has converted to an academy. However, the majority of academy conversions occur in community schools. There is also a marked increase in the number of foundation schools that convert to academies as the program has matured. Finally, in the period we study, school years 2002/3 through 2008/9, there are seven cohorts of converting academies on which we have data on 97 schools, and there are two cohorts of schools that have signed up to become academies but that convert after our analysis period ends in 2008/9 (i.e. what we refer to as the future academies).

Related Literature

There is a small, but growing body of research, studying the impact of different school types on pupil performance. In terms of context, a study related to the questions we analyse the Clark (2009) analysis of grant-maintained (GM) schools[5] in England in the late 1980s and early 1990s. He utilises a regression-discontinuity design that exploits the fact that schools wishing to become GM schools were required to win the support of the parents with children who were enrolled at the school. He finds that the narrow GM vote winners experienced a significant improvement in pupil performance (of 0.25 standard deviations) compared to the narrow GM vote losers.

Across the Atlantic, the growing body of work on charter schools is relevant because, at least in some dimensions, charter schools have similarities to academies. Some of the more convincing

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

studies in this literature exploit the fact that some charter schools use lotteries to allocate places when the school is oversubscribed. Examples of this kind include: Abdulkadiroglu et al. (2011), who estimate the impact of charter attendance on student achievement using Boston data; Hoxby and Murarka (2009), which evaluates the effect of charter schools in New York City on their students' test scores; and Angrist et al. (2010), who evaluate the impact of a specific Charter School (in Lynn, Massachusetts) that is run by the Knowledge is Power Program (KIPP) which is targeted at low income students that qualify for free school meals and was set-up by Teach for America veterans.

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

One issue with these studies is that the lotteries only occur in the schools that are oversubscribed. Given that successful schools are more likely to be oversubscribed, estimates that exploit the lottery process are likely to be upper bounds. As an alternative, some studies adopt non-experimental methods to appraise the charter school model. However, they tend to produce results that are more mixed. For example, the Center for Research on Education Outcomes (2009) study uses propensity score matching methods. They find that charter school performance is no better (or worse) than neighbouring traditional public schools. One problem with non-experimental methods is that there are concerns about how well they deal with selection bias compared to the lottery based estimates. An informative study that addresses this issue is by Hoxby and Murarka (2007). They estimate treatment effects for charter schools using both non-experimental methods and lottery based estimates, finding that their non-experimental estimates replicate their lottery-based estimates. Angrist et al. (2011) also compare experimental lottery estimates with observational estimates on a large sample of Massachusetts schools, reporting positive urban charter school effects using both approaches.

On academies themselves, there is very little research work. There are early studies by Machin and Wilson (2008) and the evaluation by Price Waterhouse Coopers (PwC Report 2008), plus a more recent report by the National Audit Office (2010). Machin and Wilson (2008) looked at differences in pupil performance in academy schools compared to the performance of a matched group of schools, and found very modest, statistically insignificant, relative improvements. The PwC Report (2008) reported higher percentage point increases in the results of academies compared to the national average (which is not a good comparison since academies are well below average performers in their predecessor state[6]). It is noteworthy that both Machin and Wilson (2008) and PwC (2008) admitted it was very earlier days in the lives of academies when they conducted their analyses, so drawing any strong conclusions from their results is precipitous.

More recently, a National Audit Office report (National Audit Office 2010) has looked at the performance of academies compared to a selected group of maintained schools for academies converting in the 2002/3 to 2007/8 school years and who had been open for at least two years. Their comparison group is composed of schools with similar pupil intakes and performance to the pre-treatment academies. They report a significant improvement in pupil performance in the academies

[6] See the discussion of Table 4 below.

compared to the comparison group. They argue this result is driven by relatively more advantaged pupils attending the academy as compared to the predecessor school.

3. Data and Conceptual Framework

Data

We use school level administrative data from the Edubase, School Performance Tables (SPT) and Annual School Census (ASC) data sources. All three are collected by the Department for Education (DfE). Edubase contains annual data on a number of school characteristics for all state schools in England and Wales from the 1999/00 academic year onwards. The School Performance Tables contain annual data on several performance measures for all state schools in England from the 1993/1994 academic year onwards. In England, as a requirement of the Education Act of 1996 (see Elias and Jones, 2006), all state-maintained schools also complete an Annual School Census.

In addition to the above, we have also used pupil level data from the National Pupil Database (NPD). The NPD is a centrally collected data source that contains the pupil and school characteristics (school census) combined with the annual National Curriculum key stage attainment data at the pupil level. The school census data contains information on pupil-level background characteristics for all pupils in the English maintained sector. This data has been collected three times per year (January, May and September) from the 2001/2002 school year onwards. For this paper, we only use the year-on-year January collection because this collection is the most available and consistent through time.

In England, compulsory education is organised around four key stages for eleven years of compulsory 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 whether academy conversion impacts on schools, our two outcomes of interest are pupil intake and pupil performance. To study intake, we therefore 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, 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 are known as GCSEs (General Certificate of Secondary Education).

We are able to analyse the pupil-intake of the secondary schools by using the pupil year and the school identifier that is contained in the pupil's school census data. This allows us to identify – for each year - the pupils that enter year 7 of secondary school. We are then able to look at the 'intake quality' of each secondary school – for each year – by matching their year 7 pupils to their KS2 results. That is, 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.

However, academy school treatment is at the school level rather than the pupil level. We therefore perform our analysis at the school-level for the main results of this paper. In order to undertake the analysis at the level of the school, we use the school code information to collapse the 'pupil-intake' dataset to the level of each individual school. We then match this collapsed dataset to the school-level characteristics from the Edubase, School Performance Tables (SPT) and Annual School Census data sources using the school identifier for the 2001/02 to 2008/09 academic years. For our estimation purposes, we keep only schools that are matched to every source in all years. We understand that that there are no differences in the calculation of any of these data sources across school types, and therefore a comparison across school types using the same data sources is appropriate in this instance.

One further issue concerns the schools that convert to academies. There are some examples where a number of schools combine to create one academy school. Where this occurs, we create one hypothetical pre-academy school. This adopts hypothetical characteristics that are a weighted-average – based on their student population at the time of the merge - of the characteristics of the merged schools.

So as to isolate the impact of an academy conversion, we define the academic year that the academy status is awarded as the first academic year that the academy school starts operating (i.e. 'opens for business'). 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 policy effect.

We investigate the impact of academy school conversion on their pupil intake based on the average standardised KS2 total points score[7] (with a population mean of zero and a standard deviation of one) of the pupils who enrol into year 7 of the academy school (the first year of secondary school). We investigate the impact of an academy school conversion on its pupil performance by looking at the KS4 performance of these pupils. The main measure of KS4 performance that we use in this paper is the average standardised proportion of pupils enrolled into the school who achieve five or more GCSEs at A*-C grade (with a population mean of zero and a standard deviation of one).

Modelling Approach

We use a school-level differences-in-differences method to estimate the impact of academy school conversion on pupil intake and pupil performance. We begin with the following equation, where the key parameter of interest is the differences-in-differences coefficient δ:

$$y_{st} = \alpha_s + \alpha_t + \delta A_s * PolicyOn_{st} + \sum_{j=0}^{J} \lambda_{1j} X_{jst} + u_{1st} \qquad (1)$$

In (1) y denotes the outcome of interest for school s in year t, A is a dummy variable that is equal to 1 for academy school in our treatment group and it is equal to 0 for every school in our control group; PolicyOn is a dummy variable equal to 1 for each school s in year t for the year, and the years after, the academy status has been awarded and it is otherwise equal to zero, X denotes a set of control variables, α_s denotes school fixed effects, α_t denotes year fixed effects (included to take account of year effects that are common to all schools) and u is an error term.

---

[7] This is calculated by totalling (for each pupil) their overall percentage score in English, Maths and Science. We then average to the level of the year 7 school.

We also allow for heterogeneous effects. We have two main interests here. First, some of the US work on charter schools has reported that it takes a while for any beneficial impacts to occur, in that no effects are detected in new openings, but they do emerge in schools that have been running longer (Hoxby 2004; Zimmer et al. 2009). We can thus generalise (1) to allow for cohort specific variations in the academy impact, δ_c, where c denotes the academy cohort:

$$y_{st} = \alpha_s + \alpha_t + \sum_{c=1}^{7} \delta_c A_{sc} * PolicyOn_{st} + \sum_{j=0}^{J} \lambda_{2j} X_{jst} + u_{2st} \qquad (2)$$

For equation (2), we report unrestricted estimates that allow for variations across the full set of seven cohorts, but we also consider groupings into earlier and later academy conversions that are upheld in terms of statistical tests.

The second form of heterogeneity we consider recognises that academies with different forms of predecessor school effectively gain different amounts of autonomy by doing so. We therefore also consider differences by 'autonomy distance' and by placing the different school types that convert to academies (see Table 3) into separate groups. We then estimate a separate δ coefficient for each pre-academy type group, where δ_d denotes the 'autonomy distance' associated with an academy conversion from five different predecessor school types as follows:[8]

$$y_{st} = \alpha_s + \alpha_t + \sum_{d=1}^{5} \delta_d A_{sd} * PolicyOn_{st} + \sum_{j=0}^{J} \lambda_{4j} X_{jst} + u_{4st} \qquad (3)$$

Finally, we allow for heterogeneity by both autonomy distance and early/late academy conversion. This is shown in the following equation:

$$y_{st} = \alpha_s + \alpha_t + \sum_{dg=1}^{10} \delta_{dg} A_{sdg} * PolicyOn_{st} + \sum_{j=0}^{J} \lambda_{5j} X_{jst} + u_{5st} \qquad (4)$$

where dg denotes a group that reflects both the pre-academy type group and the early/late academy cohort group.

[8] We have placed the academies into five pre-academy type groups: a CTC group, a voluntary aided school group, a foundation school group, a voluntary controlled school group and a community school group. We are not able to calculate a separate δ coefficient for the independent schools that convert to academies because we have no pre-conversion data on independent schools.

Definition of Comparison Schools

In Table 4, we compare the average pre-treatment school characteristics of academy schools and other types of maintained English secondary school. The striking observation from the Table is that academies have significantly different characteristics than all the other sorts of schools. This is true for the characteristics of pupils (like the proportions eligible for and taking school meals, the proportion white and the proportion of 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 the Key Stage 2 primary school points score). The issue is that academy status was typically awarded to poorly performing problem 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 all share particular unobservable characteristics (e.g. they have a type of school ethos that is more in line with the academy model).

Looking in more detail within the group of academies it does, however, turn out that the schools that convert to academy status between 2002/3 and 2008/9 have very similar pre-treatment characteristics to the schools that will later become academies. A set of balancing tests is given in the final row of the Table and with the exception of one (the proportion white) one cannot reject the null hypothesis that the 97 academies that convert in the sample period and the 98 future academies have the same sets of characteristics.[9] Our empirical approach (considered in more detail below) thus bases itself on this, modelling the converters during the sample period as the treatment group and the future converters as a balanced comparison group in a difference-in-differences setting.

[9] The one significant difference between the current academies compared to the future academies is that the pre-treatment current academies seem to enrol a significantly higher proportion of non-white pupils compared to the future academies. It is possible that one significant difference out of eleven regressions is spurious, nonetheless we account for the possibility that it is legitimate by presenting results both with and without a number of school-level controls that includes the proportion of non-white pupils in the school. Secondly, we perform a robustness check below that uses a weighting procedure that makes all eleven estimated differences insignificant. To do this we reweight the schools in the control group by their predicted probabilities from a logit of current academy status on pre-treatment school characteristics. Results of the logit regression are provided in Table A1 of the Appendix.

For this research design to yield unbiased estimates of the impact of academy status on the outcomes of interest, we need two crucial assumptions to hold. Firstly, we need there to be no effect of the treatment group on the control group. In other words, we need to be able to rule out the possibility of an academy conversion having an impact control group schools. This is the no spillovers assumption. Secondly, we need the evolution of the outcomes of interest for the treatment group (in the absence of treatment) to behave in an identical manner as the control group. This is the common trends assumption.

The feasibility of the zero spillovers assumption depends on the distance between the academies in the treatment group and the schools that make up the control group. In this paper, the median distance between an individual academy in the treatment group and all the future academies (control group) is one-hundred and seventy-one kilometres. This is probably sufficiently large to mean that any academy effect (if any) on the future academies is likely to be small. However, this is difficult to formally test. To take account of the possibility that there may be a spillover effect influencing the reported results, we performed a robustness check that reduces the sample of control schools by excluding the schools that are located particularly close to an academy school in the treatment group. More specifically, we reduced the sample of control schools by excluding any school from the control group that is within three kilometres of an academy school in the treatment group. This has the effect of reducing the sample of control schools by ten per cent. We report on this robustness check below, after the main results.

To assess the common trends assumption, we looked at whether there are differences between the current academies and future academies in their pre-treatment levels and trends in the two outcomes of interest. The results of this exercise are reported in Table 5. Panel A presents the results from a linear trend model with school fixed effects and clustered standard errors at the school-level. The estimated coefficient on the interaction of the time trend with the current academy indicator indicates whether there is a significant difference in the pre-treatment time trend in the KS2 test scores of the current academies compared to the future academies. The column (1) estimate shows there to be an insignificant trend difference in the KS2 test scores of the year 7 intake in the pre-treatment years. If we look at the pre-treatment KS4 performance of the current academies compared to the future academies (in column (2)), we see that there is no significance difference in the time-trend on their pre-treatment KS4 performance.

Panel B of Table 5 presents the results of a model that compares the level of our outcomes of interest through time. It shows that there is no significant difference in the pre-treatment levels (in any pre-treatment period) for either KS2 test scores of their year 7 intake or the KS4 performance of their pupils. We also report the results of an F-Test that looks at whether the interaction terms in Panel B are jointly equal to zero. The results of the F-Test (for both outcomes of interest) show that we cannot reject the hypothesis that the interaction terms are jointly equal to zero – this further supports the conclusion that there is no significant pre-treatment difference between our current academies and our future academies.

4. Empirical Results

Academies and Pupil Intake

In Table 6, we investigate whether an academy school conversion has an impact on the pupil-intake of the school. We track the pupil-intake quality of each school over the 2001/02 to 2008/09 period by using the average standardised KS2 total points score of their year 7 pupils. This Table uses four different specifications to report estimates of the impact of academy status on its pupil intake. We begin with the raw differences-in-differences estimate in column (1). We add time-varying controls in column (2). In column (3), we estimate heterogeneous effects for different cohorts of academies, and in column (4), we place the first five academy cohorts from the treatment group into an early group and we place the remaining two academy cohorts of academy school into a late group.

The estimated coefficients in the Table show that there has been a significant increase in the KS2 test scores for the year 7 pupils who have enrolled into an academy. This suggests that (on average) the schools that convert to academies experience a sharp and significant increase in the 'quality' of their pupil intake at year 7. Column (1) shows the key stage 2 total points score of the year 7 pupils enrolled into an academy to be 0.16 standard deviations higher following the academy conversion. The intake quality (on average) significantly increases by 0.12 standard deviations when we add the controls in column (2). The estimates in columns (3) and (4) show that the quality of intake measured by primary school test scores seems to have increased by more in the earlier academy conversions. In column (4), the 'early' cohort conversions (cohorts 1 to 5, in school years 2002/3 to 2006/7) saw an increase in the KS2 performance of their year 7 intake by a statistically significant 0.23 standard deviations, as compared to a (statistically insignificant) increase of only 0.01 in the 'later' conversions (cohorts 6 to 7, in school years 2007/8 to 2008/9).

These results suggest that (on average) there has been a step-change in the pupil intake of schools when they convert to academy status. Academies seem to be attracting and admitting higher ability pupils once they convert to academy status. One interpretation of these results is that higher ability pupils may be substituting away from other schools to the academy schools.[10] It is important, however, to note that the pupil intake effect is only significant for the early converters who, in the data period we study, have therefore been academies for longer.

Academies and Pupil Achievement

We next consider whether an academy school conversion has an impact on the KS4 performance of its pupils. This is considered in Table 7. The Table has the same set-up as Table 6 except that we add an additional column that controls for KS2 results. Column (1) shows that an academy school conversion increases the proportion of their pupils who achieve five or more GCSEs at A*-C grade by an insignificant 0.04 of a standard deviation. This (average) performance effect

[10] Indeed, in an earlier version of this paper we looked at the impact on KS2 in neighbouring schools, reporting reductions in the KS2 intake.

changes to an insignificant 0.01 of a standard deviation with the addition of the controls in column (2). The estimates in column (3) and (4), however, reveal a striking finding. They show that the performance improvements due to the academy conversion are substantially higher in the earlier academy conversions. In column (4), the 'early' cohort conversions saw an increase in their KS4 performance by a statistically significant (at the 10 percent level) 0.13 standard deviations, as compared to the 'later' cohort conversions that saw a decrease in their KS4 performance by an insignificant 0.11 standard deviations.[11]

In column (5), we check whether these performance improvements can be (at least partially) explained by the academies admitting a pupil intake with higher ability. That is, we make use of the KS2 test scores of the pupils who are now taking their KS4 exams by including their average standardised KS2 total points as an additional control. We see that the performance improvements for the early cohort conversions remain after we take account of the KS2 test scores of these pupils.

The results of Table 7 therefore suggest that KS4 performance significantly improved for the earlier cohorts that convert to an academy school. On average, there seems to be an increase the proportion of pupils who achieve five or more GCSEs at A*-C grade by around 0.13 to 0.15 standard deviations for the 'early' cohort conversions. Interestingly, in terms of magnitudes, these findings are broadly consistent with the Hoxby and Murarka (2009) results on charter schools.[12]

Variation by Pre-Academy School Type (Autonomy Distance)

We next exploit the fact that five different types of secondary school in the state-maintained sector have converted to an academy school. The managing/governing body in each of these five different school types (prior to the academy conversion) possess different degrees of autonomy (see Table 1). This means that that the amount of autonomy that each type of school gains by converting to an academy will vary. In Table 8, we therefore show coefficient estimates from equations (3) and

[11] See also Hoxby (2004) and Zimmer et al. (2009) for discussions of evidence that beneficial charter school effects in the US only emerge in established schools, with there being little impact in newly opened charters.
[12] Once you take account of the fact that the Hoxby and Murarka estimates are per year spent at the charter school.

(4) where treatment effects vary by autonomy distance. In column (1) and column (2), we look at the impact of an academy conversion on the quality of the pupil-intake of the school. In columns (3) through (6), we look at the impact of an academy school conversion on the KS4 performance of their pupils.

The Table shows there to be considerable variation in the estimated academy effect for the different types of schools that convert to academies. In column (1), we see that (on average) there are positive significant increases in intake quality for the voluntary aided schools, voluntary controlled schools and the community schools that convert to an academy school. However, there is also (on average) a significant reduction in the intake quality for the CTCs and (at the 10% level of significance) for foundations that convert.

In column (2), we also see that there is considerable variation in the estimated effects for the early cohort conversions compared to the later cohort conversions within each school type that has converted to an academy. However, it is important to bear in mind that many more community schools convert to an academy school compared to the numbers from other types of academy school conversions. This means that it will (largely be) the community school conversions that are driving both the overall increase in the KS2 test scores of the pupil intake and the more pronounced increase in the pupil-intake quality for the earlier cohort conversions reported in Table 6.

Considering the KS4 results, the column (3) and (4) results also show heterogeneity by autonomy distance. However, we only see significant positive effects in the final two columns where variation in the early/late conversion status is allowed for. Here positive significant effects (at the 10 percent level) are seen for the cohort 1 to 5 community and voluntary aided schools that convert to academies. There are no significant performance improvements in the other types of schools that convert to an academy. However, it is again important to bear in mind the larger numbers of early community schools that have converted to an academy school compared to the other types of schools that convert to an academy. This again means that it will (largely be) the early community school conversions (i.e. those which involve the biggest increase in autonomy) that are driving the overall performance improvements for the early cohort academy conversions. In column (6), we see that these effects remain after we control for the KS2 results of these pupils.

The results of Table 8 reveal an important finding in terms of the overall interpretation of our results. They suggest that the schools experiencing the largest increase in school autonomy via academy conversion (see Table 1) – community schools – experienced the greatest performance improvements. Such schools gain responsibility for the majority of the curriculum of the school (except the core subjects: English, Maths, Science and IT); the structure and length of the school day; selection of up to 10% of their pupil-intake; the school budget and all staffing decisions (in the case of community schools that convert to academies). In addition to this, we also find a similar relationship between the size of the school autonomy increase (due to the academy conversion) and its impact on the quality of its pupil intake in year 7.

We remain reluctant to draw strong conclusions from the results from column (2) and column (5) for all the school groupings, because some of the estimates are based on only a relatively small number of schools. Nonetheless, we believe that the variation in the estimated academy effects across the different school types is interesting and the results showing the shift from community to academy status (which are based on a big enough sample size) do suggest that the schools that gain the largest increase in autonomy experience the greatest increase in their pupil quality and the greatest increase in their pupil performance.

Robustness Checks

We have also appraised the sensitivity of our results on the impact of an academy conversion on their pupil intake and pupil performance. It is important to test the robustness of the estimated academy effects that we reported in the previous section to see if they can be explained by other factors and that they are not necessarily due to an academy school conversion.

Results from a barrage of tests of robustness are therefore reported in Table 9. In all cases, we compare the estimates to our original specifications in column (4) of Tables 6 and 7. For convenience, we re-report these results in column (1) of Table 9. In Panel A, we report the robustness tests when the dependent variable is the KS2 standardised total points score for the pupil who enrol into year 7 of each school in each year. In Panel B, we report the robustness tests when the dependent variable is the standardised proportion of pupils who achieve five or more GCSEs at A*-C grade for each school in each year.

The first robustness test we consider was already mentioned above in the context of the no spillovers assumption implicit in our research design. This is reported in column (2) of Table 9, where we present the estimates of the δ coefficients where we reduce our sample of control schools (future academies) by approximately 10%. This has the effect of removing all schools in our control group that are within 3km of the treatment group. The procedure is an indirect way of looking at the potential size of any spillover effects from the treatment group to the control group. Reassuringly, the Table shows that this has little impact on either the size or the significance of the estimated δ coefficients.

In column (3), we report results using a kernel matching procedure whereby we reweight the schools in the control group by their predicted probabilities from a logit of current academy status on pre-treatment school characteristics (the logit estimates are in Table A1 of the Appendix). As the results show, this also makes little difference to the estimated academy effects.

Next we consider a falsification test. This is a test of whether the estimated δ coefficients reflect unaccounted pre-existing differences in the outcomes of interest for our treatment group compared to our control group. To do this 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 δ coefficients based on a ‘fake’ year where we pretended schools converted to academies. If the δ 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 would reflect pre-existing differences in the outcomes of interest. To avoid any contamination when schools actually convert to academies and when there 'fake' conversion occurred it is necessary (for each school) for there to be no overlap between their fake post-academy years and their actual post-academy years. This means that we have to shorten the post-treatment fake periods for the first two academy cohorts. Thus the sample size drops.

We conduct the falsification exercise over the eight year period between the 1996/97and 2003/04 academic years, where the actual structure of the falsification test in shown in Table A2 in the Appendix. It shows for each academy school cohort the years in which the 'fake' academy conversion occurs compared to what actually happens. We then use this set-up to estimate the δ coefficients using these fake policy years. Column (4) of Table 9 shows the results, and the estimated δ coefficients for the early academy conversion are very close to zero and become 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 (5) of Table 9, we report the original specifications for the same sample. They are very similar to the results reported in the column (1) specification. In column (6), we look at whether our estimated academy effects hold if we use similar dependent variables and are not specific to the dependent variables that we have chosen to use in our original specification. In Panel A, we change the dependent variable to the KS2 standardised mean points score for English only. In Panel B, we investigate whether one still sees the same effect if we narrow the KS4 measure to be 5 or more good GCSEs but that they have to include English and Mathematics. The column shows that changing the dependent variables in these ways has little impact on either the size or the significance of our estimated δ coefficients.

Finally, in column (7) we compare the short-term academy effects for the early cohort conversions compared to the later cohort conversions. That is, we restrict the post-academy periods for each academy school so that there is a maximum of one post-academy period. For example, we estimate the academy effects for an academy school that opens in the 2005/06 academic year using all pre-treatment periods (in this case, 2001/02 to 2004/05) but only one post-treatment period (in this case, 2005/06 only). We then use this approach to compare the short-run academy effects for the early academy cohort conversions compared to the later academy cohort conversions. This can be thought of as an indirect test of whether the difference in the estimated effects for the early cohort conversions compared to the later cohort conversions is (largely) due to the early cohort academies operating (as academies) for a longer amount of time. That is, if we find that the estimated short term academy effects for the early academy cohort conversions are similar to the estimated short term academy effects for the later cohort conversions then this would suggest that the estimated academy effects are a function of time.

In Panel A, we see that the estimated short term academy effects on their pupil intake are smaller for the early academy cohort conversions at 0.18 of a standard deviation (compared to 0.23 in the baseline specification). The later academy effect remains insignificant and of similar magnitude. Thus, the significant increase in the pupil-intake quality for the early academy cohort conversions, whilst tempered to about 3/4 of the overall effect, still occurs in the short term. This is suggestive that the early academy cohort conversions experience a change in their pupil-intake quality in the short term that is different to the short term change in the pupil-intake quality of the later academy cohort conversions. In essence, this suggests that the reported changes to the pupil-intake quality in our original specification are not due to a function of time and we therefore cannot be confident that the later academy cohort conversions will experience (if given more time) a similar increase in their pupil-intake quality.

In Panel B when we look at performance, however, we see no short term impact. The estimated short term academy effects on their pupil performance for the early academy cohort conversions compared to the later academy cohort conversions are similar in both their size and their significance. This suggests that the early academy cohort conversions experience a change in their pupil performance in the short term that is similar to the short term change in the pupil performance of the later academy cohort conversions. In essence, this suggests that the reported changes to the pupil performance in our original specification are likely to be due the increased operating time of the early academy cohort conversions.

5. Conclusions

In this paper, we study a high profile case – the introduction of academy schools into the English secondary school sector – that has allowed schools to gain more autonomy and flexible governance by changing their school structure. We consider the impact of academy school conversion on their pupil intake and pupil performance. Our results suggest that (on average) schools respond to being granted increased autonomy (through the academy conversion) by sharply increasing the 'quality' of their pupil-intake at year 7. However, this result is (largely) driven by the early cohorts of schools that converted to an academy school. In addition to this, we also find results showing that only the early cohorts of schools that convert to academies experience significant performance improvements. However, these performance improvements do seem to be a function of time as in the case of US work on charter schools that points to more beneficial effects in older schools as compared to new openings (Hoxby 2004; Zimmer et al. 2009). Moreover, there is significant heterogeneity in the effects we see, with in particular the strongest estimates emerging for the community schools that experience the largest increase in the degree of their school autonomy resulting from the academy conversion. We view these findings as complementing the existing work on whether different school types can affect pupil performance and offering new evidence about what happens when poorly performing disadvantaged schools are converted to a new type of state school that is characterised by greater autonomy and more flexible governance.[13]

[13] It is important to place this final conclusion in its appropriate context in the policy discussion in England since the new coalition government was elected in May 2010. Since the election, the academies programme has been massively expanded, with conversions not only being in the secondary sector, but also covering primary schools. The key feature distinguishing these new coalition academies is that they are not characterised by poor performance and disadvantage in their predecessor state like the academies introduced and approved under the previous government which we analyse in this paper.

References

Abdulkadiroglu A, Angrist J, Dynarski S, Kane T, Pathak P (2011) Accountability and Flexibility in Public Schools: Evidence From Boston's Charters and Pilots, Q J Econ 126: 699-748. doi:10.1093/qje/qjr017

Angrist J, Dynarski S, Kane T, Pathak P, Walters C (2010) Inputs and Impacts in Charter Schools. KIPP Lynn, A Econ Rev 100: 239-43. doi: 10.1257/aer.100.2.239

Angrist J, Pathak P, Walters C (2011) Explaining Charter School Effectiveness, National Bureau of Economic Research Working Paper 17332.

Clark D (2009) The Performance and Competitive Effects of School Autonomy, J Polit Econ 117: 745-83. doi: 10.1086/605604

Center for Research on Education Outcomes (2009) Multiple Choice: Charter Performance in Sixteen States, Stanford University, CREDO.

Department for Children, Schools and Families (2007) Academies and Independent Schools: Prospectus http://dera.ioe.ac.uk/6578/1/Academies_Prospectus.pdf. Accessed 14 August 2010

Department for Education and Skills (2007) What are Academies?, http://www.standards.dfes.gov.uk/academies/what_are_academies/?version=1. Accessed 14 August 2010

Dobbie W, Fryer R (2011) Are High Quality Schools Enough to Close the Achievement Gap? Evidence From a Social Experiment in Harlem, A Econ Rev: Appl Econ 3: 158–87. doi: 10.1257/app.3.3.158

Elias P, Jones P (2006) Administrative Data as a Research Source: a Selected Audit, Economic and Social Research Council Regional Review Board Report 43/06, Warwick Institute for Employment Research.

Green F, Machin S, Murphy R and Zhu Y (2012) The Changing Economic Advantage From Private School, Economica, forthcoming.

Hoxby C. (2004) Achievement in Charter Schools and Regular Public Schools in the United States: Understanding the Differences, Harvard Program on Education and Governance Working Paper.

Hoxby C, Murarka S (2007) Methods of Assessing Achievement of Students in Charter Schools. In: Behrens M (ed) Charter School Outcomes. The Analytic Press, New York

Hoxby C, Murarka S (2009) Charter Schools in New York City: Who Enrolls and How They Affect Student Achievement, National Bureau of Economic Research Working Paper 14852.

Machin S, Wilson J (2008) Public and Private Schooling Initiatives in England: The Case of City Academies. In: Chakrabarti R, Peterson P (ed) School Choice International. MIT Press, Cambridge MA, pp 221-241

Machin S, Vernoit J (2010) Changing School Autonomy: Academy Schools and their Introduction to England's Education, Centre for the Economics of Education Discussion Paper 123.

National Audit Office (2010) Department for Education: The Academies Programme. http://www.nao.org.uk/publications/1011/academies.aspx. Accessed 12 March 2011

Price Waterhouse Coopers (2008) Academies Evaluation Fifth Annual Report. Annesley, DCSF Publications

Whitty G, Edwards A, Gerwitz S (1993) Specialisation and Choice in Urban Education: The City Technology College Experiment. Routledge, London.

Zimmer R, Gill B, Booker K, Lavertu S, Sass T, Witte J (2009) Charter Schools in Eight States: Effects on Achievement, Attainment, Integration and Competition. RAND Corporation, Santa Monica.

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

	Characteristics of School Autonomy and Governance				
	Non-LEA Admissions Authority	Majority Sponsor Appointed Governing Body	Maintained by Non-LEA	Governing Body responsible for most School policies	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 Education Authority (LEA), and are fee-charging.

b - Academy schools are all ability independent specialist schools, which do not charge fees, and are not maintained by the Local Education Authority (LEA). Academies only follow the national curriculum in English, Maths, Science and ICT [DfES, 2007]. They are established by Sponsors from business, faith or voluntary groups, who work 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 LEAs for maintained schools serving similar catchment areas.

c - City Technology Colleges are all ability independent schools, which do not charge fees, and are not maintained by the Local Education Authority (LEA). Their curriculum has a strong technological, scientific and practical bias (in addition to following the national curriculum) [see Whitty et al., 1993]. They are established by Sponsors from business, faith or voluntary groups, who work in partnership with central government. Sponsors and the DfE provide 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 LEAs for maintained schools serving similar catchment areas.

d - Voluntary aided schools are maintained by the Local Education Authority (LEA). The foundation (generally religious) appoints most of the governing body. The governing body is then responsible for admissions, employing the school staff, and the foundation will normally own the school's land and buildings (apart from the playing fields which are normally owned by the LEA).

e - Foundation schools are maintained by the Local Education Authority (LEA). The foundation (generally religious) appoints some – but not most – of the governing body. The governing body is then responsible for admissions, employing the school staff, and either the foundation or the governing body will own the school's land and buildings.

f - Voluntary controlled schools are maintained by the Local Education Authority (LEA). The foundation (generally religious) appoints some – but not most – of the governing body. The LA continues to be the admissions authority. The governing body will employ school staff, and the foundation will normally own the school's land and buildings (apart from the playing fields which are normally owned by the LEA).

g - Community schools are maintained by the Local Education Authority (LEA). The LEA 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, 2001/2 and 2008/9

	Number (Percent) of Secondary Schools by Type	
	2001/2	2008/9
Academy	0 (0.0)	133 (4.0)
City technology college	14 (0.4)	3 (0.1)
Voluntary aided	555 (16.0)	537 (16.0)
Foundation	609 (17.5)	560 (16.7)
Voluntary controlled	116 (3.3)	111 (3.3)
Community	2177 (62.7)	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://www.education.gov.uk/rsgateway/DB/SBU/b000796/b02-2008.pdf and Table 2a in http://www.education.gov.uk/rsgateway/DB/SFR/s000925/sfr09-2010.pdf.

Table 3: The Nature of Academy Conversions

A. 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 academies, 2001/2-2008/9	133	12	5	12	10	15	1	78
Future academies, after 2008/9	111	0	0	0	8	19	1	83

B. 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	195	0	0	12	16	30	1	136
Become academies, 2001/2-2008/9	97	0	0	12	10	13	1	61
Future academies, after 2008/9	98	0	0	0	6	17	0	75

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-Academy Conversion School Characteristics and Balancing Tests

	Proportion eligible for free school meals	Proportion taking up free school meals	Proportion white	Proportion getting 5 or more A*-C GCSEs	Key stage 2 points score	Full-time equivalent pupils	Full-time equivalent qualified teachers	Proportion special educational needs, with statement	Proportion special educational needs, no statement	Sample size
City technology college	0.080	0.076	0.970	0.919	73.781	1204.036	81.643	0.005	0.055	2
Voluntary aided	0.126	0.098	0.807	0.637	66.142	956.570	53.633	0.018	0.128	455
Foundation	0.097	0.074	0.857	0.618	65.794	1125.021	61.270	0.019	0.137	430
Voluntarily controlled	0.081	0.058	0.904	0.636	66.622	1145.994	61.008	0.023	0.124	72
Community	0.160	0.117	0.860	0.499	61.404	1038.934	58.689	0.025	0.171	1575
All academies	0.267	0.200	0.836	0.301	56.102	926.112	53.812	0.029	0.243	195
Become academies, 2001/2-2008/9	0.272	0.204	0.795	0.320	56.698	900.882	52.255	0.030	0.235	97
Future academies, after 2008/9	0.264	0.190	0.865	0.288	55.696	943.297	54.873	0.028	0.248	98
Treatment-control gap (Standard error)	0.008 (0.020)	0.014 (0.016)	-0.070 (0.025)	0.032 (0.022)	1.002 (0.772)	-42.415 (45.105)	-2.617 (2.679)	0.001 (0.002)	-0.013 (0.015)	

Table 5 – Pre-Treatment Trends

	Key Stage 2 Test Scores	Key Stage 4 Performance
	(1)	(2)
A. Time trends		
Time trend	-0.732 (0.056)	0.005 (0.002)
Academy X time trend	0.138 (0.095)	0.002 (0.003)
School fixed effects	Yes	Yes
R-squared	0.816	0.886
Sample size	1220	1220
Number of schools	195	195
B. Levels		
Academy X 2003	-0.688 (0.638)	-0.019 (0.019)
Academy X 2004	-0.631 (0.645)	-0.012 (0.017)
Academy X 2005	-0.406 (0.623)	-0.005 (0.017)
Academy X 2006	-0.405 (0.604)	0.002 (0.017)
Academy X 2007	-0.341 (0.539)	0.000 (0.015)
Academy X 2008	0.520 (0.588)	-0.005 (0.016)
Year dummies	Yes	Yes
School fixed effects	Yes	Yes
R-squared	0.897	0.891
Sample size	1220	1220
Number of schools	195	195

Notes: Robust standard errors (clustered at the school level) are reported in parentheses.

Table 6 - Academy Schools and Pupil Intake
(Key Stage 2 Standardised Fine Point Score)

	Key Stage 2 Test Scores			
	(1)	(2)	(3)	(4)
Academy	0.158 (0.063)	0.117 (0.057)		
Academy, cohort 1			0.453 (0.120)	
Academy, cohort 2			0.485 (0.246)	
Academy, cohort 3			0.411 (0.089)	
Academy, cohort 4			0.077 (0.189)	
Academy, cohort 5			0.182 (0.090)	
Academy, cohort 6			0.070 (0.089)	
Academy, cohort 7			-0.094 (0.079)	
Academy, early				0.225 (0.088)
Academy, late				0.005 (0.066)
School fixed effects	Yes	Yes	Yes	Yes
Control variables	No	Yes	Yes	Yes
Year dummies	Yes	Yes	Yes	Yes
R-squared	0.863	0.871	0.874	0.872
Sample size	1560	1560	1560	1560
Number of schools	195	195	195	195

Notes: Robust standard errors (clustered at the school level) are reported in parentheses. Control variables are: proportion of pupils eligible for Free-School-Meals (FSM), proportion of pupils taking Free-School-Meals (FSM), proportion of pupils who are White-Ethnic, the ratio of total pupils to qualified teachers, proportion of pupils with Special Educational Needs (SEN) with a statement, proportion of pupils with Special Educational Needs(SEN) without a statement. Early comprises cohorts 1-5. Late comprises cohorts 6-7.

Table 7 - Academy Schools and GCSE Performance
(Standardised Proportion of Pupils who Gain Five or More GCSEs at A*-C grade)

	Key Stage 4 Test Scores				
	(1)	(2)	(3)	(4)	(5)
Academy	0.037 (0.051)	0.013 (0.051)			
Academy, cohort 1			0.344 (0.246)		
Academy, cohort 2			0.261 (0.147)		
Academy, cohort 3			0.237 (0.209)		
Academy, cohort 4			0.194 (0.107)		
Academy, cohort 5			-0.010 (0.127)		
Academy, cohort 6			-0.124 (0.099)		
Academy, cohort 7			-0.103 (0.072)		
Academy, early				0.133 (0.072)	0.153 (0.071)
Academy, late				-0.111 (0.069)	-0.041 (0.062)
School fixed effects	Yes	Yes	Yes	Yes	Yes
Control variables	No	Yes	Yes	Yes	Yes
Year dummies	Yes	Yes	Yes	Yes	Yes
KS2 control	No	No	No	No	0.179
R-squared	0.870	0.872	0.875	0.874	0.88
Sample size	1560	1560	1560	1560	1560
Number of schools	195	195	195	195	195

Notes: As for Table 6.

Table 8 – Heterogeneity by Autonomy Distance and Early/Late Conversion

	Key Stage 2 Test Scores		Key Stage 4 Performance			
	(1)	(2)	(3)	(4)	(5)	(6)
Academy, CTC	-0.269 (0.128)		-0.115 (0.100)	-0.071 (0.101)		
Academy, CTC, early		-0.446 (0.180)			-0.103 (0.159)	-0.113 (0.155)
Academy, CTC, late		-0.117 (0.138)			-0.144 (0.120)	-0.043 (0.126)
Academy, voluntary aided	0.317 (0.145)		0.080 (0.123)	0.138 (0.120)		
Academy, voluntary aided, early		0.144 (0.145)			0.227 (0.137)	0.294 (0.127)
Academy, voluntary aided, late		0.638 (0.172)			-0.184 (0.138)	-0.142 (0.141)
Academy, foundation	-0.218 (0.119)		-0.283 (0.140)	-0.179 (0.110)		
Academy, foundation, early		-			-	-
Academy, foundation, late		-0.235 (0.119)			-0.298 (0.140)	-0.191 (0.109)
Academy, voluntary controlled	0.080 (0.046)		-0.370 (0.048)	-0.349 (0.046)		
Academy, voluntary controlled, early		-			-	-
Academy, voluntary controlled, late		0.068 (0.047)			-0.376 (0.049)	-0.352 (0.047)
Academy, community	0.220 (0.063)		0.077 (0.063)	0.111 (0.061)		
Academy, community, early		0.416 (0.086)			0.166 (0.087)	0.181 (0.085)
Academy, community, late		0.011 (0.071)			-0.028 (0.090)	0.029 (0.084)
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
KS2 control	No	No	No	Yes	No	Yes
R-squared	0.876	0.880	0.874	0.888	0.876	0.889
Sample size	1560	1560	1560	1560	1560	1560
Number of schools	195	195	195	195	195	195

Notes: As for Table 6.

Table 9 - Robustness Checks – Pupil Intake and GCSE Performance

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
A. KS2 Test Scores	Original Specification	Dropping 10% of control academies based on distance.	Kernel Matching, Logit	Fake Policy, T-5	Original Specification, fake policy sample of schools	Original Specification, KS2 test score in English (standardised)	Limit all cohorts of academies to a max of 1 year post treatment
Academy, early	0.225	0.219	0.235	0.012	0.239	0.222	0.175
	(0.088)	(0.088)	(0.091)	(0.073)	(0.124)	(0.084)	(0.091)
Academy, late	0.005	0.004	0.002	-0.105	0.128	0.007	0.007
	(0.066)	(0.066)	(0.067)	(0.048)	(0.067)	(0.065)	(0.070)
School fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Control variables	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year dummies	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R-squared	0.872	0.873	0.868	0.906	0.870	0.855	0.878
Sample size	1560	1504	1552	1488	1488	1560	1415
Number of schools	195	188	194	186	186	195	195
B. KS4 Performance	Original Specification	Dropping 10% of control academies based on distance.	Kernel Matching, Logit	Fake Policy, T-5	Original Specification, fake policy sample of schools	Original Specification, 5 or more A*-C GCSEs, including English and Maths	Limit all cohorts of academies to a max of 1 year post treatment
Academy, early	0.153	0.158	0.126	0.001	0.192	0.160	0.038
	(0.071)	(0.071)	(0.072)	(0.001)	(0.067)	(0.061)	(0.067)
Academy, late	-0.041	-0.039	-0.058	0.000	0.079	-0.005	-0.008
	(0.062)	(0.063)	(0.065)	(0.001)	(0.087)	(0.059)	(0.061)
School fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Control variables	Yes	Yes	Yes	Yes	Yes	Yes	Yes
KS2 control	Yes	Yes	Yes	No	Yes	Yes	Yes
Year dummies	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R-squared	0.888	0.888	0.884	0.852	0.886	0.888	0.890
Sample size	1560	1504	1552	1488	1488	1560	1415
Number of schools	195	188	194	186	186	195	195

Notes: As for Table 6.

Appendix

Table A1: Pre-Treatment Logit Specification for Main Analysis

Explanatory Variables	Dependent Variable: Academy Indicator (= 1 for current academies, = 0 for future academies)
Total pupils	-0.026 (0.022)
Full-time equivalent qualified teachers	-0.001 (0.015)
Total qualified teachers	0.038 (0.014)
Pupil-Teacher ratio	0.139 (0.026)
Proportion taking FSM	2.618 (1.264)
Proportion SEN, with statement	2.953 (3.522)
Proportion SEN, no statement	-1.362 (0.570)
Proportion eligible for FSM	-2.620 (1.046)
Proportion white	-1.502 (0.282)
Full-time equivalent pupils	0.023 (0.022)
Sample size	1220
Number of schools	195

Notes: Robust standard errors are reported in parentheses.

Table A2: The Structure of the Falsification Exercise as a Robustness Check

	Policy Case	1997	1998	1999	2000	2001	2002	2003	2004	2005	2006	2007	2008	2009
Cohort 1	Actual	P	P	P	P	P	P	A	A	A	A	A	A	A
	Fake	P	A	A	A	A	A	A	A					
Cohort 2	Actual	P	P	P	P	P	P	P	A	A	A	A	A	A
	Fake	P	P	A	A	A	A	A	A					
Cohort 3	Actual	P	P	P	P	P	P	P	P	A	A	A	A	A
	Fake	P	P	P	A	A	A	A	A					
Cohort 4	Actual	P	P	P	P	P	P	P	P	P	A	A	A	A
	Fake	P	P	P	P	A	A	A	A					
Cohort 5	Actual	P	P	P	P	P	P	P	P	P	P	A	A	A
	Fake	P	P	P	P	P	A	A	A					
Cohort 6	Actual	P	P	P	P	P	P	P	P	P	P	P	A	A

	Fake	P	P	P	P	P	P	A	A					
Cohort 7	Actual	P	P	P	P	P	P	P	P	P	P	P	P	A
	Fake	P	P	P	P	P	P	P	A					