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Do Good Primary Schools Perform Even Better as Academies?

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Abstract

A current English education policy is to encourage all state primary schools to become academies: state-funded, non-selective, and highly autonomous establishments. Primary schools have been able to opt-in to academy status since 2010 and academies now account for twenty-one per-cent of the primary sector. This paper investigates the causal effect of voluntary academy conversion on primary school assessment outcomes, and on entry-year intake composition. Unlike existing evidence focused on earlier academies formed from failing secondary schools, no evidence is found of an academy conversion effect on attainment for the average pupil, although pupils with special educational needs do perform better in reading tests after academy conversion. There is no evidence that academy conversion affects the composition of the entry-year intake.

Keywords: School Type, School Autonomy, Primary Education

JEL codes: I20, I21, I28

[1] This research uses a data extract from the National Pupil Database provided by the Department for Education. I gratefully acknowledge the comments of Colin P. Green, Maria Navarro Paniagua and Ian Walker. The author's PhD studies are supported in the form of an ESRC studentship.

1 Introduction

The relentless growth in the number of academies represents arguably the greatest transformation of the English state school sector since the introduction of comprehensive schools in the mid-1960s. First introduced in the early 2000s, academies are state-funded, non-selective, yet highly autonomous schools operating largely without local authority interference. Since the change in UK government in 2010, the Department for Education (DfE) has overseen a process of "mass academisation" whereby all state schools have been encouraged to become academies. 65 per-cent of secondary and twenty-one per-cent of primary schools are now academies.

A number of studies suggest the high priority attached to the mass academisation programme is justified. The conversion of existing secondary schools between 1988 and 1997 into foundation schools, which enjoyed greater autonomy than their predecessors, was estimated to increase the proportion of pupils passing five GCSEs or more by five percentage points on average (Clark 2009). Between 2002 and 2010, around 200 secondary sponsored academies were established. These academies replaced underperforming schools following targeted government intervention. Research suggests that the replacement of these schools with academies led to an improvement in pupils' GCSE attainment (Eyles, Hupkau, *et al.* 2016a). Pupils attending these academies were also more likely to complete a degree following their schooling (Eyles, Hupkau, *et al.* 2016b).

The existing body of research into academies focuses overwhelmingly on secondary sponsored academies established before 2010. Sponsored academies are far less prevalent than converter academies, which are formed by schools that voluntarily elect to become academies. These schools tend to be already well-performing, and educate advantaged pupils. Researchers have only recently turned their attention towards converter academies. For example, Worth (2015) and Eyles, Machin, *et al.* (2016) both show that attainment in primary converter academies does not improve following academy conversion.

Using a difference-in-differences strategy, this paper exploits the availability of data before and after conversions to identify the effect of voluntary academy conversion on pupil attainment in primary schools. Since parents may interpret a school's decision to become an academy as a signal of school quality, this paper also considers whether voluntary academy conversion alters the composition of primary schools' entry-year intake.

Whilst this paper shares much in common with the contemporaneously but independently produced analysis of Eyles, Machin, et al. (2016), the research design differs importantly. This paper uses a different and considerably larger treatment group; and investigates heterogeneous treatment effects at the pupil level. The literature review will argue that the control and treatment groups in their paper are more likely to differ in unobserved time-variant dimensions than the groups defined in this paper.

This paper finds no evidence that the average pupil performs any better in end-of-primary-school reading and maths tests as a result of their school becoming a converter academy. Similarly, no evidence is found that average pupil attainment mid primary school is affected by academy status. However, evidence is uncovered that one sub-group of pupils, those with special educational needs, perform modestly better in reading tests. Lastly, the composition of the entry-year intake does not appear to change with respect to several pupil characteristics following conversion.

This paper informs a lively public debate over the merits of academies, which are opposed by most teacher unions, some local authorities and major opposition political parties. The debate was galvanised by the 2016 government white paper *Education Excellence Everywhere* which declares the DfE's aspiration for every English state school to become an academy (or be in the process of doing so) by 2020 (Department for Education 2016).[2] The scale of the reform is unprecedented, if this ambition is realised then state-funded schools will be largely independent of local government and the English state school system will secure its position as the world's most decentralised.

Academies are relatively less prevalent in the primary sector than the secondary sector, and since the government has already ensured that many of the worst performing primary schools have become sponsored academies, the biggest consequence of full academisation will be an explosion in the number of primary converter academies: the specific academy type studied in this paper.

The conversion process is known to place significant administrative and financial burdens on the DfE, local authorities and schools themselves. For example, the DfE incurred additional costs of £1bn due to the academies programme between April 2010 and April 2012 (National Audit Office 2012). This includes one off costs such as the £25,000 grant paid to schools to facilitate the conversion process, as well as the recurrent additional cost per open academy. In 2012/13, this was estimated as £260,000 per annum on average. At a time when the state school sector is facing resource pressures, such as teacher shortages, and expecting other radical reforms, such as the introduction of a national school funding formula, this timely analysis is unable to provide evidence of any benefit to primary school pupils from academy conversion.

2 Institutional background

There are two broad types of state school in England: maintained schools and academies. Maintained schools receive funding and some professional and pupil-facing services from

z[2] The whitepaper stated that schools would be forced to become academies by 2022 even if this was against schools' wishes. A hostile backlash led to a policy revision whereby state schools would be encouraged but not compelled to become academies by 2022.

local education authorities (LEAs), to whom the government has historically delegated the provision of state schools. These authorities also set, or constrain, the policies and processes of their maintained schools; although the degree of control LEAs have over schools varies between different types of maintained school. The types of maintained school are, from least to most autonomous: community, voluntary-controlled, voluntary-aided and foundation schools. Academies, on the other hand, are funded directly by the DfE and are largely independent of LEAs.

Academies recruit and contract their own staff, unlike community and voluntary-controlled schools whose staff is employed by their LEAs. Academies may impose their own employment terms and can disregard nationally negotiated teacher pay and conditions. They also have considerable freedom in devising their own curriculum which must be "broad and balanced" and include English, maths, science and religious studies (Department for Education 2010). However, they do not have to follow the national curriculum in these subjects unlike maintained schools who are bound to the full national curriculum. Academies set their own admission policy unlike community and voluntary-controlled schools which are subject to an LEA admission policy.[3]

Maintained schools are run by a board of between 9 and 20 governors. In community schools, one-fifth of the governors are appointed by the LEA. In foundation, voluntary-aided and voluntary-controlled schools, a separate charitable (often faith-based) foundation appoints between one-quarter and a majority of the governors, reducing the LEA's control. Academies are governed by private charitable trusts that are independent of the LEA. These trusts set their own budget and policies, including the length of the school day and year. Academies are in effect the UK equivalent of charter schools in the USA.

[3] However, admission policies must comply with the national School Admissions Code which forbids selection by ability.

Officially, academies should not be funded advantageously when compared to maintained schools. However, a 2012 National Audit Office survey of converter academy head teachers suggested that 77 per-cent of academies converted in order to obtain more funding for front-line education (National Audit Office 2012). Academies and maintained schools receive comparable Dedicated Schools Grant (DSG) funding which covers mainstream education provision and is the main source of funding for schools. However, there has been a historical disparity between academies and maintained schools in respect of funding for auxiliary functions. LEAs centrally provide some services to maintained schools that academies need to procure independently. Academies formerly received an additional grant in order to provide these functions.[4] It boosted some academies' budgets in excess of 10 per-cent and was widely considered to overcompensate academies. This grant has now been replaced with the Educational Services Grant (ESG), paid on a common per-pupil rate. Since the 2015/16 school-year academies and maintained schools are financed on a comparable basis (Department for Education 2014).

An understanding of the academy sector's expansion is important as academies can be grouped into two very different subcategories. By 2000 it was apparent to the then Labour government that there was a pervasive problem of under-performance, poor behaviour and low aspirations in inner-city secondary schools. The government's solution was to inject innovative management and private sector best practices into these failing schools. The government set about matching selected schools to sponsors – an individual, business or charitable organisation – who would influence the management, ethos, and curriculum of the school as it re-opened as an academy. These original academies would often occupy new or extensively refurbished facilities co-financed by the sponsor.[5] Between 2002 and 2010, 203 such academies were established; all of which were secondary schools and most were former

[4] The grant was known as the Local Authority Central Spend Equivalent Grant.
[5] This requirement was subsequently dropped to encourage more sponsors.

maintained schools.[6] Academies founded as a result of the DfE imposing academy status on failing schools are now referred to as sponsored academies.

The composition of the academy school sector changed dramatically following the formation of the Conservative-Liberal Democrat coalition government in May 2010. The new Secretary of State for Education was keen to extend the opportunity to enjoy academy freedoms to schools that were not failing or located within inner-city or deprived neighbourhoods. In July, the Academies Act 2010 became one of the fastest pieces of education legislation to be adopted by parliament. It gave all schools the option to voluntarily become academies from the 2010/11 school-year, ultimately leading to the first primary academies. Academies formed from schools which voluntarily chose to become academies are known as converter academies.

Schools rated “outstanding” by OFSTED originally had their applications pre-approved meaning they could become academies from September 2010. From April 2011, all applications from “well preforming” schools received priority from the DfE.[7] The application process is relatively swift, with eight months elapsing on average between an initial expression of interest and the actual re-opening of a school as an academy. The approval rate for primary converter academies applications is 90 per-cent, which should allay any fears that schools are “cherry picked” to become academies.[8] It is not uncommon for conversions to take place mid school-year, although many conversions occur over the summer school break.

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[6] Some academies were new establishments with no predecessor school, some were previously private schools.

[7] According to National Audit Office (2012), “well performing” is based on the last three years’ exam results; prior OFSTED inspections, particularly OFSTED judgements on leadership and the capacity to improve; financial management, and any other evidence deemed significant.

[8] Statistic is calculated from the author’s own analysis of the DfE’s *Open Academies and Applications Dec ’15* dataset, and refers to the number of all applications received by the end of December 2015 to be approved.

The DfE continues to identify under-performing schools, match them with sponsors and impose academy status. Weak schools that apply to become converter academies can have their application withdrawn and face a sponsor-led academy takeover thrust upon them.

Table 1 shows the number of each state primary school type open at the start of every school-year since 2005. Five years after their introduction, converter academies account for 11.1 per-cent of the primary school sector. 5.4 per-cent of primary schools are now sponsored academies. Table 2 depicts the number of primary conversions during each school-year by predecessor school type. Around 100 primary schools converted during the 2010/11 school-year. Since then between 350 and 450 conversions have taken place each school-year. Rough calculations suggest that if the government is to meet its stated ambition of full academisation by the start of the 2022 school-year, then the rate of conversions must quadruple. Although a slightly disproportionate number of early converters were community schools, it appears that the overall predecessor school type distribution corresponds to the prevalence of each type in the pre-academy period.

In England pupils start primary school at the age of four or five and complete seven school-years at primary level before joining a secondary school at age ten or eleven. Primary school is split into three stages: reception which lasts a single school-year; key stage 1 (KS1) which covers the second and third years of primary school (known as year 1 and 2), and key stage 2 (KS2) which encompasses the final four years of primary schooling.

At the end of both key stages, schools assess the attainment of their pupils in English, maths and science. Schools have good reasons to encourage their pupils to perform well in the KS2 tests. KS2 assessment performance is an integral component of school league tables and the wider school accountability system. KS2 performance can also affect pupils' secondary school experience if their secondary school tracks students by ability, since KS2 performance is often used by secondary schools to gauge the ability of pupils joining from primary schools.

3 Literature review

3.1 US evidence: charter schools

Other nations have introduced new, more autonomous school types in an attempt to improve attainment. There is a well-established literature on charter schools, which were introduced to the US in 1992. Like academies, charter schools are highly autonomous, fee-free and non-selective. Unlike academies, charter schools tend to be new establishments with no predecessor state school.

The causal effect of charter school attendance is often identified using charter admission lotteries to instrument the number of years spent in a charter school. Identification depends on the lotteries being fair and, by implication, lottery winners and losers not being systematically different. Angrist et al. (2010) find that lottery winners test scores are 0.35σ and 0.12σ higher per year of charter attendance in maths and English language arts (ELA) tests respectively. σ denotes the standard deviation of the test score distribution for a given subject, grade and year. Based on different samples, Abdulkadiroğlu et al. (2011) and Dobbie and Fryer Jr (2011) report quantitatively similar effects for maths test scores, but find ELA test score effects in limited circumstances only.

There are good reasons to interpret these results cautiously. Admission lotteries are held when schools are oversubscribed which is a consequence of good performance. Therefore, the studies pre-condition on school quality. These studies also condition on schools retaining lottery records which might be associated with the efficiency or competence of the school (Dobbie and Fryer Jr 2011). The interaction of these factors means that the samples of the aforementioned studies are small. The sample of eight schools in Abdulkadiroğlu et al.

(2011) is the largest of the three. Hoxby and Murarka (2009) use a larger sample of 42 charter schools located across New York City. They report a much smaller per year of charter school attendance effect of 0.09σ on maths test scores, and a statistically insignificant reading test score effect.

Other lottery based (Gleason *et al.* 2010) and observational (matching) evidence (CREDO 2013) suggests some charter schools are ineffective. Urban charter schools seem to be effective whereas non-urban charters appear to be ineffective or harmful. Angrist et al. (2013) argues that student demographic differences explain a small portion of the urban/non-urban distinction, whereas variation in the policies and practices of urban and non-urban charter schools have greater explanatory power. The *No Excuses* philosophy, incorporating strict discipline, academic rigour and high expectations, may be driving the urban charter school effect (Angrist *et al.* 2013). 45 per-cent of the variation in charter school effectiveness is associated with policies aligned to the *No Excuses* model (Dobbie and Fryer Jr 2013).

Evidence on the medium term effect of charter school attendance is similarly mixed. Teen pregnancy and incarceration are less likely amongst charter attendees (Dobbie and Fryer Jr 2014). Yet charter attendance does not appear to affect the likelihood of high school graduation or college enrollment (Angrist *et al.* 2016).

State to charter school conversions, which are more comparable to England's experience with academy schools, have also been studied. Abdulkadiroğlu *et al.* (2016) focuses on nine charter takeovers of failing New Orleans, LA public schools, and another in Boston, MA. To accommodate selection into and out of takeover schools, the authors use enrolment in the schools pre-takeover to instrument enrolment post-takeover. Takeovers are shown to have significant positive effects on maths and reading test scores. A similar study by Fryer Jr (2014) imposes the freedom and practices associated with effective charter schools on eight randomly selected failing elementary schools in Houston, TX. After two years of exposure, maths test scores in the treated schools improve by 0.15σ on average relative to their closest matched school from the control group.

3.2 English literature: grant maintained and academy schools

The academies programme is not the first initiative to increase the autonomy of England's schools. Between 1988 and 1997, if maintained schools won a majority vote of current parents they could partially opt out of LEA control by becoming a grant maintained (GM) school.[9] One-third of secondary schools held such a vote. Clark (2009) uses a fuzzy regression discontinuity design to estimate the GM conversion effect. GM conversion meant greater autonomy, including control over staffing and admission policies, and more generous capital and current expenditure funding (according to estimates). Clark reports that the percentage of pupils in converters passing five GCSEs or more increased by 4 to 6 percentage points (from a base of 60 per-cent). The prior attainment of the entry year intake increased for converters, and they experienced greater teacher turnover and a net rise in teacher numbers. No evidence is found that schools neighbouring a GM converter were affected by their neighbour's conversion.

The majority of research into academies is based on the first generation of sponsored academies. An early, government commissioned, evaluation of the academies programme reported that improvements in the GCSE attainment of the first 27 academies exceeded the national average improvement (PriceWaterhouseCoopers 2008). However, this finding may merely reflect mean reversion. These academies replaced some of England's most poorly performing schools and had greater scope for improvement than the average school. A more rigorous early analysis is provided by Machin and Wilson (2009) who compare each academy to a closest matched non-academy twin and also to other secondary schools in the

[9] GM schools are the predecessors to today's foundation schools.

same local authority. They report positive academy effects on GCSE performance, however, their estimates are not statistically significant at standard levels.

A series of papers estimate difference-in-differences models using a treatment group of approximately 100 sponsored academies which opened between 2001/02 and 2008/09. The control group consists of a further 100 sponsored academies which re-opened in later school-years. Using school level data, Machin and Vernoit (2011) find that average GCSE attainment and prior (KS2) attainment of the entry-year intake both increase following an academy takeover. However, these effects take time to materialise. The authors also present evidence that the KS2 attainment of neighbouring schools' entry-year intake decreases, although schools neighbouring the best performing sponsored academies also experience an improvement in their average GCSE performance.

The estimated GCSE attainment effect for sponsored academies could be biased from pupils non-randomly switching into or away from academies in response to sponsored academy takeovers. Indeed, the increase in the prior attainment of the entry-year intake suggests this is a valid concern. Using the same sample of schools, but with pupil level data, (Eyles, Hupkau, *et al.* 2016b), account for this potential source of bias by instrumenting attendance at an academy with attendance at the academy's predecessor school before the takeover.[10] The authors report that the GCSE point score of pupils who attend an academy for one school-year is 0.04σ higher on average; while for those attending an academy for four school-years the average effect is 0.24σ.[11] Only seven per-cent of pupils in the sample attend university, however, each school-year spent in a sponsored academy increases the likelihood of attendance by 0.7 percentage points.

[10] For similar analysis see Eyles, Hupkau, *et al.* (2016a).
[11] Eyles and Machin (2015) suggest that the improvement in GCSE performance is only experienced by sponsored academies which takeover former community schools.

The authors provide a brief insight into the potential mechanisms behind these attainment effects. Sponsored academies are much more likely to undergo a leadership change than control group schools. Academies also add extra pupils and teachers, including unqualified teachers (one of their new freedoms). The teacher-pupil ratio slightly increases.

There are also improvements in the average prior KS2 attainment of the entry-year intake for newer secondary sponsored academies (takeovers after the Academies Act 2010); the magnitude of the effect is comparable to that for older academies (Eyles *et al.* 2015). The same paper finds no evidence of a change in the prior attainment of the entry-year intake of secondary converter academies.

A National Audit Office (2010) evaluation suggests that sponsored academies improve other student outcomes. Sponsored academies are more effective at reducing the percentage of school days lost to absence than comparable maintained schools. Additionally, they are more effective than similar non-academies at reducing the number of their pupils not in education employment or training (NEET) after age 16.

A fundamental challenge with evaluating sponsored academies is disentangling the effects of increased school autonomy, changes in school leadership and heavily refurbished or newly built school buildings. It is not clear how these factors interact to produce a "sponsored academy effect". By comparison converter academies generally experience an increase in the first of these factors, but no change in the latter two.

To date, there are two evaluations of converter primary academies. Worth (2015) uses propensity score matching to compare KS2 performance in the 2014/15 school-year between primary converter academies and matched non-academies. The analysis does not uncover any statistically significant academy status effect on KS2 performance for the average pupil or several sub-groups of pupils. Since this study is cross-sectional, the author is unable to control for any time invariant differences between academies and non-academies.

Eyles, Machin, *et al.* (2016) applies the methodology of Eyles, Hupkau, *et al.* (2016b) to an analysis of primary converters. The authors find no effect of voluntary academy conversion on KS2 attainment. The treatment group consists of primary schools that converted to academies in 2010/11 and 2011/12, whilst the control group consists of schools that converted in 2014/15 and 2015/16 (the study period ends in 2013/14). However, the criteria schools had to satisfy to become converter academies significantly weakened in April 2011. Appendix Table 1 shows that in the pre-treatment period, primary schools converting in 2010-2012 performed better and had more advantaged pupils than primary schools that become academies from 2013 onwards. I argue that there may also be unobservable differences between pre- and post-2012 primary converter academies as a consequence of the change in approval criteria. In this scenario, enrolment in the predecessor school is not a validly excluded instrument for enrolment in the converter academy.

An aspect of the academy programme yet to be fully analysed is academy chains. Half of all academies are a constituent of one of nearly 300 chains: academies linked together through a common sponsor and/or as a single legal entity (typically, a multi-academy trust). The development of chains has been encouraged to mitigate the risks associated with increased autonomy, and to facilitate the sharing of best practice. Focusing on long-established chains, Hutchings *et al.* (2014) offers a descriptive analysis of the effectiveness of chains in the secondary sector.[12] The report reveals persistent variation between and within chains in their ability to improve disadvantaged pupils' attainment. Other evidence indicates that sponsored academies in chains perform marginally better than standalone sponsored academies.

[12] See also Hutchings *et al.* (2015)

4 Data

I use extracts from the Department for Education's National Pupil Database (NPD), a collection of linked administrative datasets providing detailed information on England's state schools and their pupils. The Pupil Level School Census (PLSC) links pupils to the school they attend at a given point in time. It contains rich demographic information such as gender, ethnicity, first language, as well as month and year of birth. Socioeconomic circumstances are captured by proxy variables including free school meals (FSM) eligibility history. Other relevant circumstances such as special educational needs (SEN) status are recorded.

PLSC records include an anonymised matching reference number common to attainment datasets. As such, pupils can be matched to their KS1 and KS2 attainment records with minimal mismatching risk. I also use data from the School Level Database (SLD) to facilitate between school comparisons of aggregate pupil demographics and attainment.

Public primary schools are statutorily required to assess their pupils' attainment using national curriculum (NC) assessments. This includes externally set and marked tests and externally moderated teacher based assessments. Primary schools must register their pupils for these assessments at the end of key stages 1 and 2 (school years 2 and 6).

The KS2 assessments feature mathematics and reading tests, as well as a combined spelling, punctuation and grammar test (since 2012/13). Separately, year 6 pupils undergo teacher assessments in English, mathematics and science. Since 2005, pupils receive a teacher assessment in reading, writing, speaking and listening, mathematics and science at the end of KS1.[13]

Primary NC assessments were graded using a five-point scale (levels 1 to 5) until 2012 when, with the intention of challenging high performing pupils, the government introduced level 6.

[13] Pupils previously also sat KS2 writing and science tests, discontinued in 2012 and 2009 respectively. Before 2005, KS1 attainment was assessed using formal testing.

A pupil achieves level 6 at KS2 in a particular subject if they pass a supplementary test. Consequentially, the grading and difficulty of the level 1 to 5 KS2 tests did not systematically change in 2012. Pupils are expected to be working at level 2 at the end of KS1. Pupils should make two levels worth of progress throughout KS2, therefore, year 6 pupils are expected to attain level 4.

I assess the effects of academy conversion on pupil attainment in reading and maths separately, since academies may on average place greater emphasis on either subject than non-academies following the national curriculum. Attainment is measured using point scores; one NC level corresponds to six points. Table 3 acts as a conversion table between NC levels and point scores. Point scores convey slightly more information than NC levels for two reasons. Firstly, for the purpose of KS1 teacher assessments level 2 is broken down into sublevels 2a, 2b and 2c; each corresponding to a different point score. Secondly, pupils who are working below level 1 are not awarded a level, but they are given a point score. Use of point scores means a more complete sample of students can be used.

This paper uses a data extract covering school-years 2007/08 to 2014/15. 2014/15 is the most recent school-year for which data is currently available, and is also the last school-year in which NC levels are used to assess pupils.[14] I use data on every year 2 and year 6 pupil in each of these school-years to determine how academy status may affect pupil attainment. Separately, I use data on every reception pupil (the entry-year) to explore whether academy status affects the composition of the entry-year intake. Primary schools that do not cover

[14] Significant NC assessment reform introduced in the 2015/16 school year replaced NC levels with "scale scores". Any future long term evaluation of primary academy conversions would be unable to measure pupil attainment using NC levels or point scores. The only pupil attainment metric consistently available either side of the 2015/16 school year will be raw test scores.

reception and key stages 1 and 2 in their entirety or schools that cater to specialist educational or behavioural needs are excluded from the analysis. [15]

5 Methodology

The causal effects of conversion to academy status are estimated using difference-in-differences (DiD) models. The baseline estimating equation is

$$y_{ist} = \alpha_s + \alpha_t + \beta_1 Academy_{st} + \gamma' x_{ist} + \varepsilon_{ist} \#(1)$$

where i, s and t are pupil, school and school-year identifiers respectively. y_{ist} refers generically to an attainment measure. α_s is a school fixed effect and α_t is a school-year (time) effect. Binary variable $Academy_{st}$ equals 1 if school s is a primary converter academy in school-year t and 0 otherwise. Conversion is an "absorbing" state since no academies revert back to maintained school status. The parameter of interest is β_1 representing the estimated average causal effect of treatment on the treated (ATT). This is the estimated average change in attainment in converter academies caused by conversion to academy status. x_{ist} is a vector of time-varying control variables. Under the parallel trends assumption the error term, ε_{ist}, is orthogonal to $Academy_{st}$. I assume this term has a school/school-year specific component that is likely to exhibit serial correlation over time. Therefore, I estimate robust standard errors clustered at the school level, as advocated by Bertrand et al. (2004). There are approximately 1,300 clusters which exceeds the standard minimum number of clusters required to estimate robust clustered standard errors (Cameron and Miller 2015).

When outcome y_{ist} is a measure of KS2 attainment, a value-added model can be estimated using prior KS1 attainment. This model is motivated by the lack of observed historical school and parental inputs. These important unobserved inputs are proxied using prior attainment. I include prior KS1 attainment in vector x_{ist}, which assumes the effects of historical inputs

[15] In other words, lower and middles schools are excluded from the analysis.

experience a common rate of geometric decay. The alternative case where y_{ist} is equal to the difference of current and prior attainment assumes prior inputs are as relevant as current inputs.

The value-added model does not account for contemporaneous changes in parental inputs. Parents may interpret a school's decision to become a converter academy as a positive or negative signal of the school's quality, and may adjust their parental inputs accordingly. Therefore, the estimated treatment effects are net of the average parental response to their child's school becoming an academy. Value-added models are thoroughly critiqued in Todd and Wolpin (2003), which also discusses the unavoidable restrictions that such models place on the underlying education production function.

26 per-cent of primary schools participated in a boycott of KS2 assessments tests in May 2010. Since participation in the boycott was non-random and widespread, the 2009/10 school-year is dropped from the panel for KS2 attainment analysis. This means the pre-treatment period spans four schools-years (two either side of the dropped year).

I extend equation 1 in several ways to accommodate different forms of treatment effect heterogeneity. Equation 1 imposes a constant average treatment effect for every school-year following academy conversion. It is unlikely that academies fully realise and exploit the implications of their enhanced independence straight after conversion. Instead there may be an adjustment period during which academies gradually implement changes that would not have been possible as a maintained school. It is appropriate to adopt a specification that allows the treatment effect to vary according to the length of time elapsed since conversion occurred. A more flexible variant of equation (1) is

$$y_{ist} = \alpha_s + \alpha_t + \sum_{\tau=-4}^{\tau=2} \beta_\tau Academy\,Yr\,\tau_{ts} + \gamma' x_{ist} + \varepsilon_{ist} \#(2)$$

where $Academy\ Yr - 4_{ts}$ to $Academy\ Yr\ 2_{ts}$ are binary variables equal to 1 if school-year t corresponds to between four school-years before and two school-years after school s becomes an academy. This is sometimes referred to as the leads and lags DiD estimator and attributed to Autor (2003). If the control and treated groups have differential trends in the absence of treatment, then the pre-treatment beta estimates $(\hat{\beta}_{-4}, \dots, \hat{\beta}_{-1})$ will be significantly different from zero. Estimates that are not significantly different from zero lend support in favour of the identifying assumption.

In addition, equation (1) does not allow the treatment effect to vary between academies with different predecessor school types, despite academies experiencing varying degrees of autonomy before conversion. As schools experience differential increases in autonomy following conversion to academy status, there is an element of treatment intensity which could be captured. I interact a binary variable equal to 1 if an academy was previously a community or voluntary-controlled school (CVC_s) and 0 otherwise, with $Academy_{st}$. In equation (3), $\beta_1 + \beta_2$ is the average treatment effect for academies which were previously community or voluntary-controlled schools, whereas β_1 is the average treatment effect for academies whose predecessor school was another maintained school type.

$$y_{ist} = \alpha_s + \alpha_t + \beta_1 Academy_{st} + \beta_2(Academy_{st} \times CVC_s) + \gamma' x_{ist} + \varepsilon_{ist} \#(3)$$

Certain sub-groups of the pupil population may be affected differently by academy conversion than the average pupil. The autonomy accompanying academy status may allow academies to redirect their attention and resources towards or away from certain pupil groups. Two important sub-groups are pupils from disadvantaged backgrounds and SEN pupils. I use FSM eligibility to indicate disadvantage. I further estimate equations 4 and 5.

$$y_{ist} = \alpha_s + \alpha_t + \beta_1 Academy_{st} + \beta_2(Academy_{st} \times FSM_i) + \gamma' x_{ist} + \varepsilon_{ist} \#(4)$$

$$y_{ist} = \alpha_s + \alpha_t + \beta_1 Academy_{st} + \beta_2(Academy_{st} \times SEN_i) + \gamma' x_{ist} + \varepsilon_{ist} \#(5)$$

FSM eligibility and SEN status are recorded in vector x_{ist}. In equation 4, β_1 is the ATT for pupils who are illegible for FSM, while $\beta_1 + \beta_2$ is the ATT for FSM pupils. In equation 5, the ATT for SEN pupils is $\beta_1 + \beta_2$ and β_1 is the ATT for non-SEN pupils. Equations 1 to 5 are estimated using pupil level data.

For the entry-year intake analysis, the baseline estimating equation is

$$y_{st} = \alpha_s + \alpha_t + \beta_1 Academy_{st} + \gamma' x_{st} + \varepsilon_{st} \#(6)$$

where y_{st} refers to the entry-year cohort average of a certain pupil characteristic for school s in school-year t. The interpretation of the equation's remaining components is the same as in the preceding equations. β_1 is the ATT estimate which is the estimated average change in the cohort average of a certain attribute of the entry-year intake experienced by schools when they become academies.

The β estimates in equations 1 to 6 provide unbiased treatment effect estimates if the parallel trend assumption holds conditional on the control variable vector x_{ist}. The school fixed effect controls for differences in time invariant characteristics between treatment and control schools. It remains a possibility that schools become academies on the basis of unobserved trends. I depend on the parallel trends assumption to dismiss this remaining identification threat.

To maximise the likelihood that the outcomes of the treatment and control groups share a common time trend in the absence of treatment, the two groups should be as similar as possible in all dimensions, observed and unobserved, other than treatment status. Whilst the application procedure and criteria for approval for academy conversion changed during the 2010/11 school-year, it has not significantly changed since. As such, schools that later become academies should be quite similar to already opened academies.

The treatment group is defined as all schools that become converter academies in the school-years 2012/13 to 2014/15. The control group is schools that become converter academies during the 2015/16 school-year. The treatment group includes schools that experience one to three school-years of academy status. The implication of this research design for the main outcome of interest, value-added at KS2, is that I observe cohorts who spend between one and three school-years of KS2 (which spans four school-years) at an academy. The minimum observed pre-treatment period is four school-years.

Table 4 compares average pupil characteristics and attainment (at school level) over the period 2007/08 to 2011/12 (the pre-treatment period) for the control and treatment groups. Column 3 tests the equality of means between the two groups. The means are not significantly different at conventional levels of significance, providing good evidence that the groups are alike in terms of observable factors. The table also shows the mean of each variable for treatment group academies split by year of conversion. This illustrates that the composition of the treatment group does not systematically vary by year of conversion.

Appendix Table 2 tests whether the groups share a common time trend in the pre-treatment period. Maths and reading KS2 attainment are regressed on a school fixed effect, either a linear time trend or a set of time effects and an interaction between the time trend/effects and a treatment group indicator. The null hypothesis – the interactions between the treatment group indicator and time effects for pre-treatment periods are jointly equal to zero – cannot be rejected at significance levels below 66 per-cent.

In Appendix Table 1, I compare the means of the same variables over the pre-treatment period for various school types. This shows that primary schools which became converter academies during the first two school-years of the converter academy programme had better KS2 results and more advantaged pupils than schools that later became academies. These schools are not included in the treatment group because of these significant differences in pre-treatment characteristics.

6 Results

6.1 KS2 attainment

Table 5 contains the estimates from difference-in-differences (DiD) models with a single post-treatment effect. In columns 1 to 3, the outcome is a pupil's KS2 maths test point score. KS2 reading test point score is the outcome variable for columns 4 to 6. Columns 1 and 4 feature estimates from a DiD model without any control variables. I add control variables in columns 2 and 5, and then add prior attainment in each subject in columns 3 and 6 to create a value-added model. The academy coefficient estimate (found in the first row) corresponds to the estimated effect of academy conversion. The estimates are relatively consistent as control variables and then KS1 attainment are added to the model, ranging between -0.01 to 0.06 points (recall that 6 points is equivalent to 1 NC level). None of the estimates are statistically different from zero at the ten per-cent significance level. This is in contrast to the control variable coefficients which are uniformly estimated with high precision, and are statistically different from zero. These estimates do not provide evidence of an academy status effect on KS2 attainment. This finding is not sensitive to the measure of KS2 attainment. Appendix Table 3 shows there is no academy status effect when the dependent variable is the standardised raw test score or a binary variable indicating if the expected NC level (level 4) is achieved. In the following tables, I present estimates from the preferred specification (columns 3 and 6) only; estimates are not sensitive to specification choice.[16]

Figure 1 plots estimates from models with pre- and post-treatment effects. I estimate the effect of being in the treatment group in the years leading up to and following treatment. This allows the treatment effect to vary by length of exposure, and can also be used to assess the

[16] Full tables are available upon request.

validity of the common trends assumption. There should be no "effect" from being in the treatment group prior to treatment. If an "effect" is consistently found prior to treatment, then this raises concerns about the research design. In Figure 1, the coefficient estimate for school-year 0 corresponds to the estimated academy status effect during the conversion year. Coefficient estimates for school-years less than 0 correspond to pre-treatment effect estimates. Figure 1a plots the estimated treatment effects on KS2 maths point score, while the effect on KS2 reading point score is depicted in Figure 1b. The findings from Figure 1 are consistent with those from Table 5; no statistically significant treatment effect is found for attainment in either subject in any treated school-year conditional on the control variables and prior attainment. Aside from one pre-treatment effect estimate at the fringes of statistical and economic significance, there is no evidence of differential trends between the control and treatment groups prior to treatment. This is indicative of the common trends assumption holding.

It is plausible that academy conversion effects on KS2 attainment exist for sub-populations of pupils and schools, in spite of the seeming lack of an effect for the average school or pupil. Table 6 presents estimates from three DiD models which accommodate heterogeneous treatment effects for special educational need (SEN) pupils, disadvantaged pupils, and academies which were relatively autonomous before conversion.

Panel A contains estimates from a model accommodating heterogeneous effects by SEN status. Column 1 indicates that neither SEN nor non-SEN pupils' KS2 maths point scores are affected by academy conversion. However, it does appear that SEN and non-SEN pupils' KS2 reading point scores are affected differently by academy conversion. Non-SEN pupils experience a 0.14 point average reduction in their reading point score. This is equivalent to 1 in 43 pupils achieving one less NC level in reading as a result of their school becoming an academy. This effect is statistically significant as is the 0.55 point increase in the reading point score of SEN pupils. This corresponds to 1 in 10 SEN pupils gaining one extra NC level in reading.

Estimates from models allowing the academy conversion effect to vary by free school meal (FSM) eligibility are presented in Panel B. This is the best available indicator of whether the pupil's background is disadvantaged. Column 1 indicates there is no evidence that KS2 maths attainment is affected by academy conversion irrespective of FSM eligibility. When considering reading attainment, there is no apparent academy conversion effect on FSM ineligible pupils; although FSM eligible pupils experience a 0.28 point gain in their reading point scores on average. This translates into 1 in 21 FSM eligible pupils achieving one NC level higher in their KS2 reading assessment.

Panel C investigates school level heterogeneity, the reported model allows the academy conversion effect to vary between former community and voluntary-controlled schools, which had the least autonomy before becoming an academy, and voluntary-aided and foundation schools which were relatively more autonomous. The academy conversion effect on KS2 maths attainment is insignificantly different from zero regardless of the school's previous structure. Pupils in former voluntary-controlled and community school academies gain 0.046 points in KS2 reading on average, whilst pupils from other formerly maintained schools lose 0.11 points on average. These effects are statistically significant at the 10% level. However, they are trivial, for example the gain experienced in former voluntary-controlled and community schools is comparable to 1 in 130 pupils gaining one additional reading NC level.

6.2 KS1 attainment

Table 7 presents estimates of the effect of academy conversion on KS1 maths attainment (see the first two columns) and KS1 reading attainment (see the last two columns). Since KS1 is the first formal assessment of pupils there is no opportunity to implement a value-added model. This increases the scope for bias from unobserved confounders relative to the KS2 value-added models. Because of this and the more subjective nature of teacher assessments, less weight should be placed on this second set of estimates. The estimates of the academy coefficient are stable following the inclusion of control variables, but are insignificantly different from zero both statistically and economically; whereas every control variable coefficient is precisely estimated at the one per-cent level. No evidence is found of an academy conversion effect on KS1 attainment. This finding is consistent with an unreported dynamic DiD model, in which pre- and post-treatment effect estimates are insignificantly different from zero.

Again, it is possible that the zero average treatment effect on KS1 attainment is masking non-zero treatment effects for school and pupil sub-populations. In an unreported exercise, I investigate heterogeneous treatment effects at the pupil level (by SEN status and FSM eligibility) and at the school level (by predecessor school type).

I find evidence of a positive effect on KS1 reading point score for SEN pupils of 0.19 points, and a negative effect on the same outcome for non-SEN pupils of 0.08 points. There is no apparent effect on KS1 reading for FSM ineligible pupils, but there is a small positive effect for FSM eligible pupils. I also find evidence of a positive effect on KS1 reading for schools that were the least autonomous before conversion, and a negative effect for the most autonomous schools before conversions. The nature of heterogeneous effect estimates for the KS1 reading analysis mirrors the KS2 reading analysis closely, however, the magnitude of the estimates are much smaller. In terms of KS1 maths heterogeneous effects, I find evidence of trivial positive effects for SEN pupils and FSM eligible pupils.

6.3 Entry-year intake

Finally, I explore whether the composition of schools' entry-year intake changes following academy conversion. Table 8 reports the findings from a rudimentary DiD model estimated on school level data where the outcome variables are the percentage of entry-year cohort: eligible for FSM; with SEN; whose first language is English, and who are white. The academy coefficients in columns 1 to 3 are insignificantly different from zero suggesting the composition of the entry-year intake for schools is not affected by becoming an academy in three of the four characteristics investigated. However, column 4's estimate suggests that academies experience a 0.6 percentage point decline in the proportion of their entry-year intake that is white. 81 per-cent of entry-year pupils are white in the sample. It is unusual that the composition of the new intake would change in this dimension only. Given that the size of the effect is modest at best, I opt to place little emphasis on this finding.

Since very few pupils switch primary schools outside of the entry-year, the lack of evidence of a systematic change in the composition of the entry-year cohort suggests it is unlikely the composition of other year groups is systematically affected by academy conversion.

7 Conclusion

This paper attempts to quantify the causal effect of the voluntary conversion of English state primary schools into academies on pupil attainment, and the composition of the entry-year intake. To this end, the staggered nature of academy conversions across schools and the availability of a rich administrative dataset are exploited in a battery of difference-in-differences models.

Estimates from these models consistently find no evidence of an academy conversion effect on KS2 maths and reading test point scores for the average pupil. Similarly, heterogeneous effects models do not find any effect on pupil and school sub-groups, with the exception of

SEN pupils. One in ten SEN pupils achieve one NC level higher in KS2 reading subsequent to academy conversion. KS1 teacher assessments and the composition of the entry-year intake are seemingly unaffected by academy conversion.

Although these results are consistent with prior research into primary converter academies, studies of secondary sponsored academies have found academy status effects on attainment. A number of reasons may explain this discrepancy. Firstly, converter academy pupils tend to be more advantaged and academically meritorious than their sponsored academy peers. If the marginal effect of school inputs is diminishing, and academy status improves school inputs comparably in converter and sponsored academies, then academy status will be more effective in sponsored academies were pupils' attainment is at a lower base level.

However, academy status means different things for sponsored and converter academies. First-generation sponsored academies often enjoyed new or extensively refurbished facilities, which is likely to positively affect pupil attainment. Additionally, these academies were highly susceptible to leadership changes following conversion (Eyles and Machin 2015). Converter academies are not more likely to undergo leadership changes following their conversions (Eyles, Machin, *et al.* 2016). Leadership changes may partially explain the difference in the effectiveness of converter and sponsored academy conversions. Suppose underperforming schools are unattractive to effective head-teachers. If sponsored academy status increases the attractiveness of an underperforming school to effective head-teachers, then sponsored academies may improve pupil attainment through attracting a higher calibre of head-teacher. Converter academies might already be attractive to quality school leaders due to their record of good performance. These schools may not attract better leaders following conversions, and, therefore, might not experience attainment improvements.[17]

[17] If this hypothesis is true, then the effectiveness of sponsored academy status should diminish as the sponsored academy sector expands.

The difference in estimated academy status effects may be explained by differences in the stages of schooling. Primary schools are usually smaller than secondary schools, implement different teaching methods, and have different educational goals. The freedom of academies to set their own curriculum may be more consequential for attainment in secondary schools, since secondary pupils are formally assessed in a wider range of subjects (partially determined by the school); whereas, primary school pupils are predominately assessed in numeracy and literacy. Secondly, if the financial benefit from becoming an academy results in increased availability of effective school resources, then academy status may be more effective at secondary level, as these schools face greater per-pupil costs than primary schools.

Irrespective of the mechanisms driving the differences between the effectiveness of sponsored and converter academy status, the lack of evidence of an improvement in attainment of primary converter academies suggests that increasing school autonomy is not a panacea in and of itself. This is an important finding given the considerable cost of the academies programme.

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Figure 1a: Pre and post-treatment effect estimates for KS2 maths point score (point estimates and 95% confidence interval)

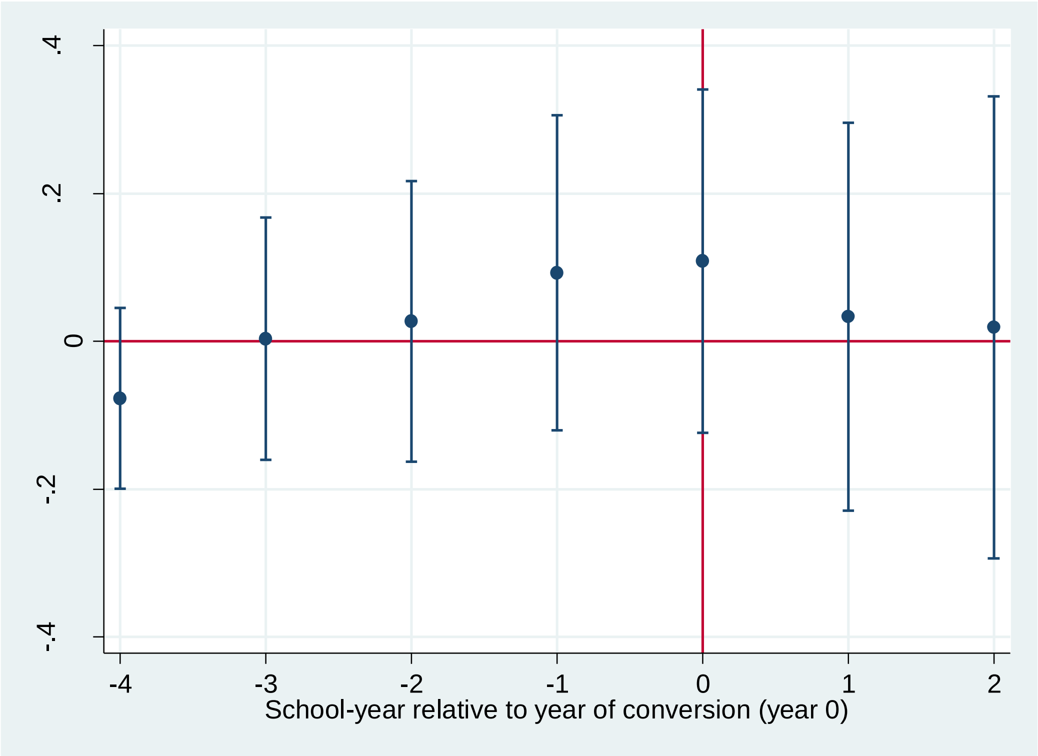

Figure 1b: Pre and post-treatment effect estimates for KS2 reading point score (point estimates and 95% confidence interval)

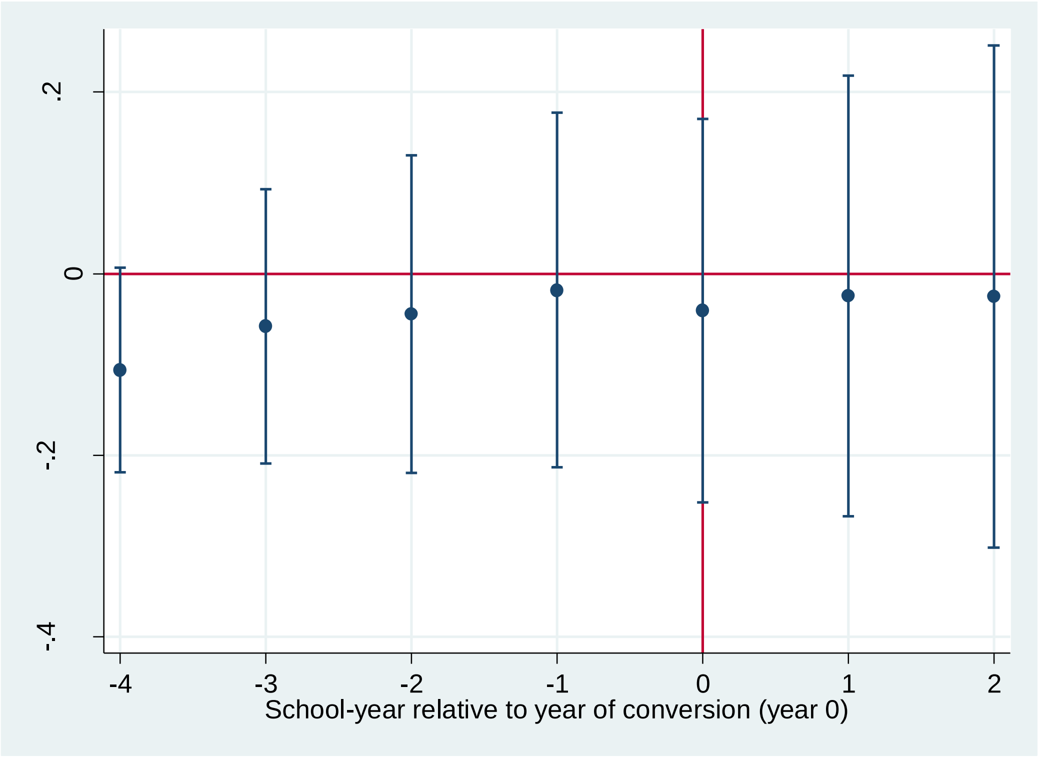

Table 1: The composition of English public primary schools at the start of the school-year

	2005	2006	2007	2008	2009	2010	2011	2012	2013	2014	2015
Converter academy	0	0	0	0	0	6	265	647	1,069	1,462	1,859
Sponsored academy	0	0	0	0	0	0	5	115	391	685	898
Free school	0	0	0	0	0	0	2	34	37	92	117
Community school	10,272	10,145	10,016	9,893	9,803	9,727	9,491	9,111	8,624	8,166	7,842
Foundation school	902	903	909	911	913	911	863	819	768	734	698
Voluntary aided school	3,780	3,769	3,757	3,747	3,738	3,730	3,684	3,606	3,479	3,326	3,148
Voluntary controlled school	2,481	2,481	2,468	2,465	2,459	2,455	2,427	2,384	2,313	2,234	2,155
Grand Total	17,435	17,298	17,150	17,016	16,913	16,829	16,737	16,716	16,681	16,699	16,717

Notes: each column shows the number of schools of each type open on September 1st of that year. *Source:* author's analysis of EduBase data.

Table 2: Primary converter academy schools by school-year of conversion to academy status and predecessor school type

	2010/11	2011/12	2012/13	2013/14	2014/15	2015/16	Total
Community school	77	226	252	232	166	203	1,156
Voluntary controlled school	7	41	43	47	50	58	246
Voluntary aided school	5	82	110	125	128	93	543
Foundation school	31	55	42	18	21	20	187
Multiple or no predecessor school	2	0	0	0	0	2	4
Total	122	404	447	422	365	376	2,136

Notes: school-year is defined as 1st August to 31st July the following calendar year. *Source:* author's analysis of EduBase data and DfE's 'Open Academies' monthly data release.

Table 3: National curriculum level to point score conversion table

National curriculum level	Point score
6	39
5	33
4	27
3	21
2a	17
2b	15
2c	13
1	9
Working below 1	9

Notes: NC level 6 was introduced in 2011/12. Level 2 with no sub-level is equivalent to level 2b.

Table 4: Test of mean equality between treatment and control groups in averaged attainment and pupil characteristics

	Control	Treatment [All]	Difference (SE)	Treatment [2012/13 Converters]	Treatment [2013/14 Converters]	Treatment [2014/15 Converters]
KS2 math points	27.66	27.86	-0.20* (0.11)	27.88	27.82	27.90
KS2 reading points	28.66	28.81	-0.15 (0.11)	28.78	28.81	28.85
Prior KS1 math points	15.88	15.93	-0.05 (0.07)	15.85	15.91	16.06
Prior KS1 reading points	15.65	15.69	-0.04 (0.09)	15.58	15.67	15.85
Headcount	262.45	268.68	-6.23 (9.71)	293.62	264.87	241.86
% white British	80.80	82.04	-1.24 (1.51)	79.54	82.84	84.22
% English speakers	89.32	90.21	-0.88 (1.21)	88.60	90.25	92.17
% FSM eligible	14.51	14.91	-0.40 (0.87)	16.06	14.89	13.47
Observations	268	1,056		384	367	305

Notes: variables are school level averages between 2007/08 and 2011/12. *** denotes significance at 1% level, ** at 5% level, * at 10% level.

Table 5: KS2 maths and reading point score DiD models with common treatment effect

	(1) KS2 maths points	(2) KS2 maths points	(3) KS2 maths points	(4) KS2 reading points	(5) KS2 reading points	(6) KS2 reading points
Academy	0.0579	0.0156	0.0349	0.0258	-0.0138	0.0039
	(0.0539)	(0.0541)	(0.0532)	(0.0507)	(0.0503)	(0.0462)
Female		-1.1435***	-0.6838***		0.3652***	0.0565***
		(0.0162)	(0.0129)		(0.0145)	(0.0125)
English is first language		0.3051***	-0.8743***		1.2620***	-0.3355***
		(0.0548)	(0.0368)		(0.0590)	(0.0327)
White ethnicity		-0.1814***	-0.1144***		-0.2593***	0.1347***
		(0.0462)	(0.0285)		(0.0437)	(0.0252)
SEN		-5.1568***	-1.7652***		-4.9810***	-2.1543***
		(0.0327)	(0.0234)		(0.0392)	(0.0296)
FSM eligible		-1.1388***	-0.4664***		-0.9887***	-0.4232***
		(0.0283)	(0.0206)		(0.0281)	(0.0220)
KS1 math points			0.8253***			
			(0.0037)			
KS1 reading points						0.5700***
						(0.0032)
Constant	27.7026***	29.4518***	16.1938***	29.1760***	29.3314***	21.0918***
	(0.0294)	(0.0581)	(0.0723)	(0.0292)	(0.0545)	(0.0625)
School fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
School-year effects	Yes	Yes	Yes	Yes	Yes	Yes
No. of academies	1,062	1,062	1,062	1,062	1,062	1,062
No. of future academies	269	269	269	269	269	269
Observations	350,005	347,162	333,974	350,189	347,348	331,185
Adj. R-Square	0.083	0.275	0.543	0.069	0.278	0.459

Notes: robust standard errors clustered at school level in parentheses. *** denotes significance at 1% level, ** at 5% level, * at 10% level.

Table 6: KS2 maths and reading point score DiD models with heterogonous treatment effects

	(1) KS2 maths points	(2) KS2 reading points
Panel A: Heterogeneity by SEN status		
Academy	0.0278	-0.1375***
	(0.0532)	(0.0447)
Academy x SEN	0.0347	0.6867***
	(0.0510)	(0.0608)
SEN	-1.7731***	-2.3132***
	(0.0257)	(0.0325)
Adj. R-Square	0.543	0.459
Panel B: Heterogeneity by FSM eligibility		
Academy	0.0416	-0.0425
	(0.0537)	(0.0455)
Academy x FSM eligible	-0.0402	0.2802***
	(0.0485)	(0.0507)
FSM eligible	-0.4560***	-0.4962***
	(0.0239)	(0.0263)
Adj. R-Square	0.543	0.459
Panel C: Heterogeneity by predecessor school type		
Academy	0.0144	-0.1050*
	(0.0731)	(0.0601)
Academy x community or voluntary-controlled predecessor school	0.0286	0.1514**
	(0.0763)	(0.0626)
Adj. R-Square	0.543	0.459
Control variables	Yes	Yes
Value-added model	Yes	Yes
School fixed effects	Yes	Yes
School year effects	Yes	Yes
No. of academies	1,062	1,062
No. of future academies	269	269
Observations	333,974	331,185

Notes: robust standard errors clustered at school level in parentheses. *** denotes significance at 1% level, ** at 5% level, * at 10% level.

Table 7: Mid primary school (KS1) maths and reading point score DiD models with common treatment effect

	(1) KS1 maths points	(2) KS1 maths points	(3) KS1 reading points	(4) KS1 reading points
Academy	0.0279	-0.0120	0.0070	-0.0402
	(0.0386)	(0.0377)	(0.0384)	(0.0379)
Female		-0.4627***		0.5810***
		(0.0109)		(0.0117)
English is first language		0.4382***		0.9008***
		(0.0335)		(0.0422)
White ethnicity		-0.1337***		-0.5315***
		(0.0248)		(0.0298)
SEN		-3.9451***		-4.5413***
		(0.0263)		(0.0297)
FSM eligible		-0.9363***		-1.1372***
		(0.0183)		(0.0215)
Constant	15.9077***	16.7296***	15.7732***	16.1480***
	(0.0236)	(0.0384)	(0.0255)	(0.0425)
School fixed effects	Yes	Yes	Yes	Yes
School year effects	Yes	Yes	Yes	Yes
No. of academies	1,146	1,146	1,146	1,146
No. of future academies	292	292	292	292
Observations	436,464	432,669	436,453	432,659
Adj. R-Square	0.071	0.258	0.077	0.295

Notes: robust standard errors clustered at school level in parentheses. *** denotes significance at 1% level, ** at 5% level, * at 10% level.

Table 8: Entry-year intake composition DiD model with school-level data

	(1) FSM Eligible	(2) SEN	(3) English is first language	(4) White ethnicity
Academy	-0.0056	-0.0031	-0.0017	-0.0059**
	(0.0035)	(0.0027)	(0.0051)	(0.0029)
Constant	0.1008***	0.0453***	0.8971***	0.8702***
	(0.0023)	(0.0016)	(0.0032)	(0.0020)
School fixed effects	Yes	Yes	Yes	Yes
School year effects	Yes	Yes	Yes	Yes
No. of academies	1,137	1,137	847	961
No. of future academies	288	288	214	245
Observations	11,400	11,400	8,488	9,648
Adj. R-Square	0.715	0.333	0.750	0.898

Notes: robust standard errors clustered at school level in parentheses. *** denotes significance at 1% level, ** at 5% level, * at 10% level.

Appendix Table 1: Mean pupil attainment and characteristics averaged over pre-treatment period by school type

	Converter academies '11 and '12 openers	Converter academies '13, '14 & '15 openers: treat. group	Converter academies '16 openers: control group	Sponsored academies	Community schools	Voluntary-controlled schools	Voluntary-aided schools	Foundation schools	Total
KS2 maths points	28.54	27.87	27.66	25.95	27.53	28.06	28.19	27.76	27.68
KS2 reading points	29.36	28.81	28.66	26.74	28.35	29.17	29.24	28.68	28.60
Prior KS1 maths points	16.24	15.93	15.88	14.99	15.68	16.21	16.13	16.01	15.83
Prior KS1 reading points	16.05	15.69	15.65	14.47	15.37	16.05	16.02	15.78	15.58
Headcount	306.43	268.95	261.87	276.77	275.40	168.00	217.87	281.70	252.12
% white British	81.56	82.04	80.81	73.77	76.38	89.92	76.79	80.15	78.61
% English speakers	90.39	90.22	89.32	83.34	85.18	95.65	87.72	88.66	87.46
% FSM eligible	11.78	14.89	14.54	25.90	18.11	9.58	13.19	12.94	16.07
Observations	447	1,062	269	910	6,660	1,615	2,706	215	13,884

Notes: variables are school level averages between 2007/08 and 2011/12.

Appendix Table 2: Test for differential time trend in KS2 attainment between groups

	(1) KS2 maths points	(2) KS2 reading points
Panel A: Time effects		
2007/08	-1.6400***	-0.2068
	(0.1456)	(0.1306)
2008/09	-1.5520***	-0.5035***
	(0.1377)	(0.1287)
2010/11	-1.4103***	-0.8528***
	(0.1254)	(0.1149)
2011/12	-0.6869***	-0.2279**
	(0.1275)	(0.1104)
2012/13	-0.2524*	-0.3799***
	(0.1290)	(0.1099)
2013/14	-0.1392	-0.0780
	(0.1207)	(0.0930)
Treatment x 2007/08	-0.1767	-0.1021
	(0.1587)	(0.1428)
Treatment x 2008/09	-0.0680	0.0159
	(0.1516)	(0.1417)
Treatment x 2010/11	-0.0118	0.0172
	(0.1394)	(0.1288)
Treatment x 2011/12	0.0291	-0.0456
	(0.1407)	(0.1231)
Treatment x 2012/13	-0.0582	-0.1055
	(0.1419)	(0.1212)
Treatment x 2013/14	0.0977	0.1505
	(0.1326)	(0.1042)
Constant	29.4416***	29.5023***
	(0.0371)	(0.0320)
F test	0.598	0.331
p-value	0.664	0.857
Adj. R-Square	0.548	0.491
Panel B: Linear time trend		
School year	0.2656***	0.0510***
	(0.0181)	(0.0173)
School year x Treatment	0.0255	0.0132
	(0.0200)	(0.0189)
Constant	25.2589***	28.4522***
	(0.0897)	(0.0817)
Adj. R-Square	0.548	0.491
School fixed effects	Yes	Yes
Observations	9,268	9,268

Notes: robust standard errors clustered at school level in parentheses. The F-test null hypothesis is that the treatment/time effect interactions up to 2011/12 are jointly insignificant. *** denotes significance at 1% level, ** at 5% level, * at 10% level.

Appendix Table 3: Common treatment effect DiD models with alternative KS2 attainment measures

	(1) KS2 maths test total mark	(2) KS2 reading mark	(3) KS2 maths level 4+	(4) KS2 reading level 4+
Academy	0.0119	0.0025	0.0008	0.0017
	(0.0118)	(0.0111)	(0.0032)	(0.0023)
Constant	-1.5423***	-1.4586***	0.6049***	0.8141***
	(0.0136)	(0.0126)	(0.0057)	(0.0035)
Control variables	Yes	Yes	Yes	Yes
Value-added model	Yes	Yes	Yes	Yes
School fixed effects	Yes	Yes	Yes	Yes
School year effects	Yes	Yes	Yes	Yes
No. of academies	1,062	1,062	1,062	1,062
No. of future academies	269	269	269	269
Observations	326,835	324,369	324,653	319,049
Adj. R-Square	0.472	0.435	0.208	0.133

Notes: standard errors clustered at school level in parentheses. Outcome variables in columns 1 and 2 are standardized test marks with zero mean and unit standard deviation. Dependent variable in columns 3 and 4 are equal to one if the pupil achieves NC level 4 in maths or reading and zero otherwise.*** denotes significance at 1% level, ** at 5% level, * at 10% level.