Accepted Manuscript

Does Greater Primary School Autonomy Improve Pupil Attainment? Evidence from Primary School Converter Academies in England

Joseph Regan-Stansfield

PII: S0272-7757(16)30656-2
DOI: 10.1016/j.econedurev.2018.02.004
Reference: ECOEDU 1779

To appear in: *Economics of Education Review*

Received date: 1 December 2016
Revised date: 15 January 2018
Accepted date: 26 February 2018

Please cite this article as: Joseph Regan-Stansfield , Does Greater Primary School Autonomy Improve Pupil Attainment? Evidence from Primary School Converter Academies in England, *Economics of Education Review* (2018), doi: 10.1016/j.econedurev.2018.02.004

Highlights

- The conversion of primary schools into converter academies does not improve reading and maths attainment at ages 7 and 11 for the average pupil
- Although, there is evidence of a slight improvement (0.03 standard deviations) in the attainment of pupils eligible for free school meals
- The composition of the entry-year cohort of primary schools is also unaffected voluntary academy conversion
- These findings are in stark contrast to the reported positive attainment effects of academy conversion for under-performing secondary schools

Does Greater Primary School Autonomy Improve Pupil Attainment? Evidence from Primary School Converter Academies in England

Joseph Regan-Stansfield[1]

Department of Economics, Lancaster University

January 2018

Abstract

A recent English education policy has been to encourage 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 percent of the primary sector. This paper investigates the causal effect of becoming a converter academy 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 a converter academy effect on attainment for the average pupil. Although, there is evidence of a slight positive effect on age 11 attainment for pupils eligible for free school meals. There is no evidence that becoming a converter academy affects the composition of the entry-year intake.

Keywords: School Type, School Autonomy, Primary Education

JEL codes: I20, I21, I28

[1] *E:* j.regan-stansfield@lancaster.ac.uk *P:* Lancaster University Management School, Lancaster, United Kingdom, LA1 4YX. I gratefully acknowledge the comments of Colin P. Green, Maria Navarro Paniagua and Ian Walker. This research uses a data extract from the National Pupil Database provided by the Department for Education.

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 percent of secondary and twenty-one percent of primary schools are now academies.

Some 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). A second intervention, the sponsored academies programme, established 200 sponsored academies between 2002 and 2010 to replace historically underperforming schools. Research suggests that the replacement of these schools with academies led to an improvement in pupils' GCSE attainment (Eyles et al., 2016a). Pupils attending these academies were also more likely to complete a degree following their schooling (Eyles et al., 2016b). These two interventions were different and affected schools with dissimilar performance records, but both increased schools' autonomy and attainment.

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, Eyles et al. (2017) and Worth (2015) both show that attainment in primary converter academies does not improve following academy conversion.

This paper uses a difference-in-differences strategy to exploit the availability of data before and after conversions to identify the effect of becoming a converter academy on pupil attainment in primary schools. This paper also considers whether voluntary academy conversion alters the composition of primary schools' entry-year intake.

This paper finds no evidence that the average pupil performs any better in end-of-primary-school reading and maths tests because of their school becoming a converter academy. However, evidence is uncovered that one sub-group of pupils, those eligible for free school meals, perform slightly better in age 11 maths and reading tests. There is also a small positive effect on age 11 reading attainment for schools that had the least autonomy before becoming a converter academy, but no effect is found for schools that were already relatively autonomous before conversion. No evidence is found that average pupil attainment at age seven is affected by converter academy status. 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 declared 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] While full academisation is no longer a policy priority, schools continue to become academies at a vast rate. The scale and speed of the reform is unprecedented. If the trend continues then, state-

[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.

funded schools will be largely independent of local government, and the English state schooling system will secure its position as the world's most decentralised.

Academies are relatively less prevalent in the primary sector than the secondary sector. Furthermore, the government has already ensured that many of the worst performing primary schools have become sponsored academies. As such, the biggest consequence of further academisation will be a large increase 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 additional recurrent cost per open academy. In 2012/13, this was estimated at £260,000 per annum on average. At a time when the English 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 from academy conversion to the average primary school pupil.

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 local education authorities (LEAs), to whom the government has historically delegated schooling provision. 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 staff, unlike community and voluntary-controlled schools whose staff are 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 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 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 independent of the LEA. These trusts set their own budget and policies, including the length of the school day and year. Academies are effectively the UK equivalent of charter schools in the USA.

Officially academies should not be funded advantageously relative to maintained schools. However, a 2012 National Audit Office survey of converter academy head teachers found that 77 per-cent of academies converted to obtain more funding for front-line education (National Audit Office, 2012). Academies and maintained schools receive comparable

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

Dedicated Schools Grant (DSG) funding which covers mainstream education provision and is the primary 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 to provide these functions.[4] It boosted some academies' budgets more than 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 were secondary schools and most were former maintained schools.[6] Academies founded due to the DfE imposing academy status on failing schools are now referred to as sponsored academies.

The composition of the academy sector changed dramatically following the formation of the Conservative-Liberal Democrat coalition government in May 2010. The new Secretary of

[4] The grant was known as the Local Authority Central Spend Equivalent Grant.
[5] This requirement was subsequently dropped to encourage more sponsors.
[6] Some academies were new establishments with no predecessor school, some were previously private schools.

State for Education was keen to offer 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 the UK 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, the national school inspections body, originally had their applications pre-approved meaning they could become academies from September 2010. From April 2011, all applications from "well-performing" 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 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.

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 type of state primary school open at the start of every school-year since 2008. Five years after their introduction, converter academies account for 11.1 percent 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

[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.

predecessor school type. Around 120 primary schools converted during the 2010/11 school-year. Since then between 350 and 450 conversions have taken place each school-year. 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 to improve attainment. A well-established literature exists 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 & Fryer, 2011a). The interaction of these factors means that the samples of the studies above 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 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 more 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., 2011). 45 percent of the variation in charter school effectiveness is associated with policies aligned to the *No Excuses* model (Dobbie & Fryer, 2011b).

Evidence on the medium-term effect of charter school attendance is similarly mixed. Teen pregnancy and incarceration are less likely among charter attendees (Dobbie & 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. However, charter school takeovers are considerably less common than start-up charter schools. 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.

Another difference between academies and charter schools is that charter schools are not generally part of a centralised admissions system. They instead require parents to make a separate application to them whereas, academy admissions are handled through the same centralised process as applications to maintained schools. Abdulkadiroğlu et al. (2015) investigates charter school effectiveness in the Denver, CO school district which has a rare unified, centralised admission system incorporating charter schools. The authors find positive attainment effects from charter school attendance similar to Abdulkadiroğlu et al. (2011).

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 & Machin (forthcoming) 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 percent 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 et al., 2016a).

[11] Eyles & Machin (forthcoming) 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 et al. (2017) applies the methodology of Eyles & Machin (forthcoming) to an analysis of primary converters. The authors find no effect of voluntary academy conversion on KS2 attainment. Primary schools that converted between 2010/11 and 2014/15 form the treatment group, while schools that converted in 2015/16 and 2016/17 are the control group. Although, the approval criteria for academy conversion applications weakened significantly in April 2011. In the methodology section (Table 4), I show that in the pre-treatment period of the present study, primary schools converting between 2010 and 2012 had a better attainment record, and educated more advantaged pupils than primary schools that became converter academies after 2012. If these observable differences are accompanied by unobservable differences between primary schools established either side of the approval criteria change, then enrolment in a predecessor school is not a validly excluded instrument for enrolment in a converter academy. To address this, Eyles et al. stratify their sample according to schools' most recent OFSTED rating. This ensures there are no differences in the means of baseline characteristics between their control and treatment schools.

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

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

ability to improve disadvantaged pupils' attainment. Other evidence indicates that sponsored academies in chains perform marginally better than standalone sponsored academies.

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 School Census 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. School Census records can be directly matched to pupils' KS1 and KS2 attainment records. I also use data from the School Level Database (SLD) to facilitate between school comparisons of aggregate pupil demographics and attainment.

State 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 six 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 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 six 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. KS2 attainment is measured using test marks standardised to zero mean, unit standard deviation. However, it is important to note that pupils who are deemed by their schools to be working below the level assessed by the KS2 tests gain exemption from the tests. As such, pupils at the bottom of the attainment distribution are excluded from the analysis. Including these pupils necessitates using NC level point score as the attainment outcome which is far a coarser variable.[14] I use this outcome in a robustness exercise. As teacher assessment exclusively measures KS1 attainment, the outcome variable for KS1 analysis is the NC level point score.

This paper uses a data extract covering school-years 2007/08 to 2014/15. 2014/15 the last school-year before NC assessments undergo significant reform. 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 reception and key stages 1 and 2 in their entirety or schools that cater to special educational or behavioural needs are excluded from the analysis. [15]

[14] The NC level point score is a simple numerical transformation of the NC level. For example, level 1 is coded as 9 points, level 2 as 15 points. The NC level point score does not convey any more detail than the NC level.
[15] In other words, lower and middles schools are excluded from the analysis.

5 Methodology

The causal effects of a primary school opting to become a converter academy 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 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.

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}.

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 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.

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 $AcademyYrX_{ts}$ equals one if the difference between school-year t and the school-year that school s becomes an academy is X school-years, and zero otherwise. 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 $\left(\hat{\beta}_{-4}, \ldots, \hat{\beta}_{-1}\right)$ will be significantly different from zero. Estimates that are not significantly different from zero lend support in favour of the identifying assumption.

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 all KS2 attainment analysis. This means the pre-treatment period spans four schools-years (two either side of the dropped year). As such, I correct the pre-treatment indicators in equation (2) such that, for example, if a school

becomes an academy in 2012/13, then 2008/09 is coded as the third school-year before that school's conversion, and not the fourth school-year prior.

Also, 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 $Academy_{st}$.

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

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. An important sub-group is pupils from disadvantaged backgrounds. I use FSM eligibility to indicate disadvantage. I further estimate equation (4).

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

FSM eligibility is recorded in vector x_{ist}.

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

$$y_{st} = \alpha_s + \alpha_t + \beta_1 Academy_{st} + \varepsilon_{st} \#(5)$$

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 (5) 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 based on unobserved trends. I depend on the parallel trends assumption to dismiss this remaining identification threat.

The treatment and control groups should be as similar as possible in observed and unobserved dimensions; this maximises the likelihood that outcomes for the groups share a common time trend in the absence of treatment. While 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 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 treatment schools experience academy status for 21 months on average. The minimum observed pre-treatment period is four school-years.

Panel A of Table 3 compares several measures of attainment and pupil demographics, for the year six cohort averaged at school-level, for the last pre-treatment school-year, between 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 regarding observable factors the school-year before the first treatment schools become converter academies.

Panel B of Table 3 shows the change in the same school level attainment and demographic measures of the year six cohorts between 2007/08 and 2011/12 for the control and treatment groups. Column 3 tests whether the difference in the mean change is equal between the two groups. There are no statistical differences between the groups at typical significance levels. This suggests that the overall trend in these measures in the pre-treatment period do not vary between the groups.

Table 4 compares the means of the same variables averaged over the pre-treatment period for a wider selection of schools. Column 1 shows means for schools that become converter academies in 2010/11 and 2011/12. Column 2 and 3 contains means for the schools that are considered in this analysis. A comparison of Column 1 against columns 2 and 3 suggests that year six pupils in the first schools to become converter academies perform significantly better in the pre-treatment period than those attending the converter academies included in this paper's main analysis sample. The average KS2 reading standardised test mark in the earliest converters in the pre-treatment period is 0.17 standard deviations compared to 0.05 standard deviations for the converter academies in the treatment and control groups. The earliest converter academies also educate more advantaged pupils (based on eligibility for free school meals) than later converter academies. Additionally, unreported results show that the trends in these variables differ in the pre-treatment period between the earliest converters academies and the academies in this paper's sample. This table demonstrates that the first two waves of primary converter academies differ to more recent primary converter academies in observable dimensions in the pre-treatment period. As such, there is reasonable grounds to suspect that they may also differ in unobserved attributes. Consequentially they are not appropriate to include in a research design which exploits time variation in conversion to converter academy status.

6 Results

6.1 KS2 attainment

Table 5 contains estimates from difference-in-differences (DiD) models with a single post-treatment effect. In columns 1 to 3, the outcome is KS2 maths standardised test mark. KS2 reading standardised test mark 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 converter 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, ranging between 0.017 to 0.007 standard deviations. None of the estimates are statistically different from zero at the ten per-cent significance level. This contrasts with the control variable coefficients which are uniformly estimated with high precision, and are statistically different from zero. These estimates do not provide evidence of a converter academy status effect on KS2 attainment. This finding is not sensitive to the measure of KS2 attainment. Appendix Table 1 shows there is no academy status effect when the dependent variable is the point score corresponding to the National Curriculum (NC) level achieved by the pupil (levels range from 1 to 6), or a binary variable indicating if the expected NC level (level 4) is achieved.

Appendix Table 2 shows that this finding is insensitive to an alternative treatment definition and an alternative model specification. The treatment effect estimate may be subject to bias caused by mismeasurement in the treatment variable. The main mismeasurement threat comes from schools not operating as academies until they enter a full school-year as one, despite possibly legally becoming a converter academy midway through the previous school-year. In Panel A, I calculate the treatment variable such that schools are coded as exposed to converter academy status only if they have that status at the start of the school-year. This does not alter the conclusions that can be drawn from Table 5. In Panel B, I add school specific linear time trends to investigate whether the results are being driven by differential trends in KS2 performance between treated and control schools. Estimates of the academy status effect are not sensitive to the inclusion of these trends. In the following tables and figures, 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 validity of the common trends assumption. There should be no "effect" from being in the treatment group before treatment. If an "effect" is consistently found before 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 standardised test mark, while the effect on KS2 reading standardised test mark 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. The F-test statistic corresponding to the null hypothesis that the pre-treatment coefficient estimates are jointly insignificantly different from zero is 1.44 and 1.31 for the maths and reading models respectively. Therefore,

[16] Full tables are available upon request.

there is no evidence of differential trends between the control and treatment groups before treatment. This suggests that the common trends assumption holds.

It is plausible that academy conversion effects on KS2 attainment exist for sub-populations of pupils and schools, despite the seeming lack of an effect for the average school or pupil. Table 6 presents estimates from two models which accommodate heterogeneous treatment effects for disadvantaged pupils, and academies which were relatively autonomous before conversion.

Estimates from models allowing the academy conversion effect to vary by free school meal (FSM) eligibility are presented in Panel A. This is the best available indicator of whether the pupil's background is disadvantaged. Columns 1 and 2 suggest that the KS2 maths and reading attainment of FSM ineligible pupils is not affected by academy conversion. However, there is evidence of a small positive academy conversion effect (0.03 standard deviations) on maths and reading attainment for FSM eligible pupils. This effect is statistically different from zero at the five percent significance level.

Panel B 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. However, pupils in former voluntary-controlled and community school academies gain 0.036 standard deviations in KS2 reading on average. This effect is statistically different from zero at the five percent significance level. There is no effect for pupils from former voluntary-aided or foundation schools.

The estimated academy conversion effect will be biased if enrolment in the converting school is sensitive to the conversion. Parents may interpret the conversion decision as a school quality signal and may alter their child's enrolment accordingly. I estimate a DiD model based on school-level data to investigate whether becoming a converter academy influences the composition of the year six cohorts. Table 7 shows that there is no academy conversion effect on the observed average characteristics of the year six cohorts. Evidence that enrolment decisions are not sensitive to academy conversion is found in column 1, which reports that there is no academy conversion effect on the percentage of year six pupils who completed KS1 (year two) in the same school.

6.2 KS1 attainment

Table 8 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. Additionally, KS1 attainment is recorded using teacher assessments which are inherently more subjective. However, there is still good cause to investigate KS1 outcomes. The KS2 value-added models show a strong relationship between attainment at KS1 and KS2. Moreover, the relationship between attainment at KS1 and KS4 (age 16) is far from trivial. The raw correlation between KS1 maths NC curriculum level and GCSE maths point score is 0.624; the correlation between age 7 and 16 English attainment is 0.597[17].

The estimates of the converter 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 percent level. No evidence is found of an academy conversion effect on KS1 attainment. This

[17] Author's own calculations.

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 FSM eligibility) and the school level (by predecessor school type). Similar to the KS2 analysis, I find evidence of a small positive converter academy status effect on reading and maths attainment for FSM eligible pupils, but no effect for FSM ineligible pupils. I also find evidence of a slight, but statistically significant, positive effect on KS1 reading and maths attainment in schools which had the least autonomy before conversion.

6.3 Entry-year intake

Finally, I explore whether the composition of schools' entry-year intake changes following academy conversion. Table 9 reports the findings from a rudimentary DiD model estimated on school-level data where the outcome variables are the percentage of the 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.

7 Conclusion

This paper attempts to quantify the causal effect of the voluntary conversion of English state primary schools into converter 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. However, heterogeneous effects models do find evidence of a small positive, but statistically significant, KS2 attainment effect for free school meals eligible pupils. There is also evidence of a small positive effect in KS2 reading attainment for schools that had the least autonomy before becoming a primary converter academy. 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. Numerous 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 where 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 affect pupil attainment positively. Additionally, these academies were highly susceptible to leadership changes following conversion (Eyles & Machin, forthcoming). Converter academies are not more likely to undergo leadership changes following their conversions (Eyles et al., 2017). 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.[18]

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

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

panacea in and of itself. This is an important finding given the considerable cost of the academies programme.

References

Abdulkadiroğlu, A., Angrist, J. D., Dynarski, S. M., Kane, T. J., & Pathak, P. A. (2011). Accountability and Flexibility in Public Schools: Evidence from Boston's Charters And Pilots. *The Quarterly Journal of Economics*, *126*(2), 699–748. http://doi.org/10.1093/qje/qjr017

Abdulkadiroğlu, A., Angrist, J. D., Hull, P. D., & Pathak, P. A. (2016). Charters without Lotteries: Testing Takeovers in New Orleans and Boston. *American Economic Review*, *106*(7), 1878–1920. http://doi.org/10.1257/aer.20150479

Abdulkadiroğlu, A., Angrist, J. D., Narita, Y., & Pathak, P. A. (2015). *Research Design Meets Market Design: Using Centralized Assignment for Impact Evaluation*. National Bureau of Economic Research (NBER) Working Paper (21705).

Angrist, J. D., Cohodes, S. R., Dynarski, S. M., Pathak, P. A., & Walters, C. R. (2016). Stand and Deliver: Effects of Boston's Charter High Schools On College Preparation, Entry and Choice. *Journal of Labor Economics*, *34*(2), 275–318. Retrieved from http://dx.doi.org/10.1086/683665

Angrist, J. D., Dynarski, S. M., Kane, T. J., Pathak, P. A., & Walters, C. R. (2010). Inputs and Impacts in Charter Schools: KIPP Lynn. *American Economic Review*, *100*(2), 239–243. http://doi.org/10.1257/aer.100.2.239

Angrist, J. D., Pathak, P. A., & Walters, C. R. (2011). Explaining Charter School Effectiveness. *American Economic Journal: Applied Economics*, *5*(4), 1–27. http://doi.org/10.3386/w17332

Autor, D. H. (2003). Outsourcing at Will: The Contribution of Unjust Dismissal Doctrine to the Growth of Employment Outsourcing. *Journal of Labor Economics*, *21*(1), 1–42.

Clark, D. (2009). The Performance and Competitive Effects of School Autonomy. *Journal of Political Economy*, *117*(4), 745–783.

CREDO. (2013). *National Charter School Study*. Stanford, CA: Center for Research on Education Outcomes.

Department for Education. (2010). *The Importance of Teaching, the Schools White Paper 2010*. London: Her Majesty's Stationery Office.

Department for Education. (2014). *The Education Services Grant*. London: Department for Education.

Department for Education. (2016). *Educational Excellence Everywhere*. London: Her Majesty's Stationery Office.

Dobbie, W., & Fryer, R. G. (2011a). Are high-quality schools enough to increase achievement among the poor? Evidence from the Harlem Children's Zone. *American Economic Journal: Applied Economics*, *3*(3), 158–187. http://doi.org/10.1257/app.3.3.158

Dobbie, W., & Fryer, R. G. (2011b). Getting beneath the Veil of Effective Schools: Evidence from New York City. *American Economic Journal: Applied Economics*, *5*(4), 28–60. http://doi.org/10.3386/w17632

Dobbie, W., & Fryer Jr, R. G. (2014). The Medium-Term Impacts of High-Achieving Charter

Schools. *Journal of Political Economy*, *123*(5), 985–1037.

Eyles, A., Hupkau, C., & Machin, S. (2016a). Academies, charter and free schools: do new school types deliver better outcomes? *Economic Policy*, *31*(87), 453–501.

Eyles, A., Hupkau, C., & Machin, S. (2016b). School reforms and pupil performance. *Labour Economics*, *41*, 9–19. http://doi.org/10.1016/j.labeco.2016.05.004

Eyles, A., & Machin, S. (2018). Academy Schools and Their Introduction to England's Education. *Journal of the European Economic Association*, forthcoming.

Eyles, A., Machin, S., & McNally, S. (2017). Unexpected school reform: Academisation of primary schools in England. *Journal of Public Economics*, *155*, 108–121. http://doi.org/10.1016/j.jpubeco.2017.09.004

Eyles, A., Machin, S., & Silva, O. (2015). *Academies 2: The New Batch*. Centre for the Economics of Education (LSE) Discussion Paper (1370). Retrieved from http://econpapers.repec.org/RePEc:cep:cepdps:dp1370

Fryer Jr, R. G. (2014). Injecting Charter School Best Practices into Traditional Public Schools: Evidence from Field Experiments. *The Quarterly Journal of Economics*, *129*(3), 1355–1407.

Gleason, P., Clark, M., Tuttle, C., & Dwoyer, E. (2010). *The Evaluation of Charter School Impacts: Final Report*. Washington, DC: US Department for Education National Center for Education Evaluation and Regional Assistance.

Hoxby, C., & Murarka, S. (2009). *Charter Schools in New York City: Who Enrolls and How They Affect Their Students' Achievement*. National Bureau of Economic Research (NBER) Working Paper (14852). Retrieved from http://econpapers.repec.org/RePEc:nbr:nberwo:14852

Hutchings, M., Francis, B., & De Vries, R. (2014). *Chain Effects: The Impact of Academy Chains on Low Income Students*. London: The Sutton Trust.

Hutchings, M., Francis, B., & Kirby, P. (2015). *Chain Effects 2015: The Impact of Academy Chains on Low-Income Students*. London: The Sutton Trust.

Machin, S., & Vernoit, J. (2011). *Changing School Autonomy: Academy Schools and their Introduction to England's Education*. Centre for the Economics of Education (LSE) Disucssion Paper (123). Retrieved from http://econpapers.repec.org/RePEc:cep:ceedps:0123

Machin, S., & Wilson, J. (2009). Public and Private Schooling Initatives in England. In R. Chakrabarti & P. E. Peterson (Eds.), *School Choice International: Exploring Public-Private Partnerships* (pp. 219–241). Cambridge, MA: MIT Press.

National Audit Office. (2010). *The Academies Programme*. London: The Stationery Office.

National Audit Office. (2012). *Managing the Expansion of the Academies Programme*. London: The Stationery Office.

PriceWaterhouseCoopers. (2008). *Academies Evaluation Fifth Annual Report*. Annesley: DCSF Publications.

Todd, P. E., & Wolpin, K. I. (2003). On the Specification and Estimation of the Production Function for Cognitive Achievement. *The Economic Journal*, *113*(485), F3--F33.

Worth, J. (2015). *Analysis of Academy School Performance in 2015*. Slough: National Foundation for Educational Research.

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

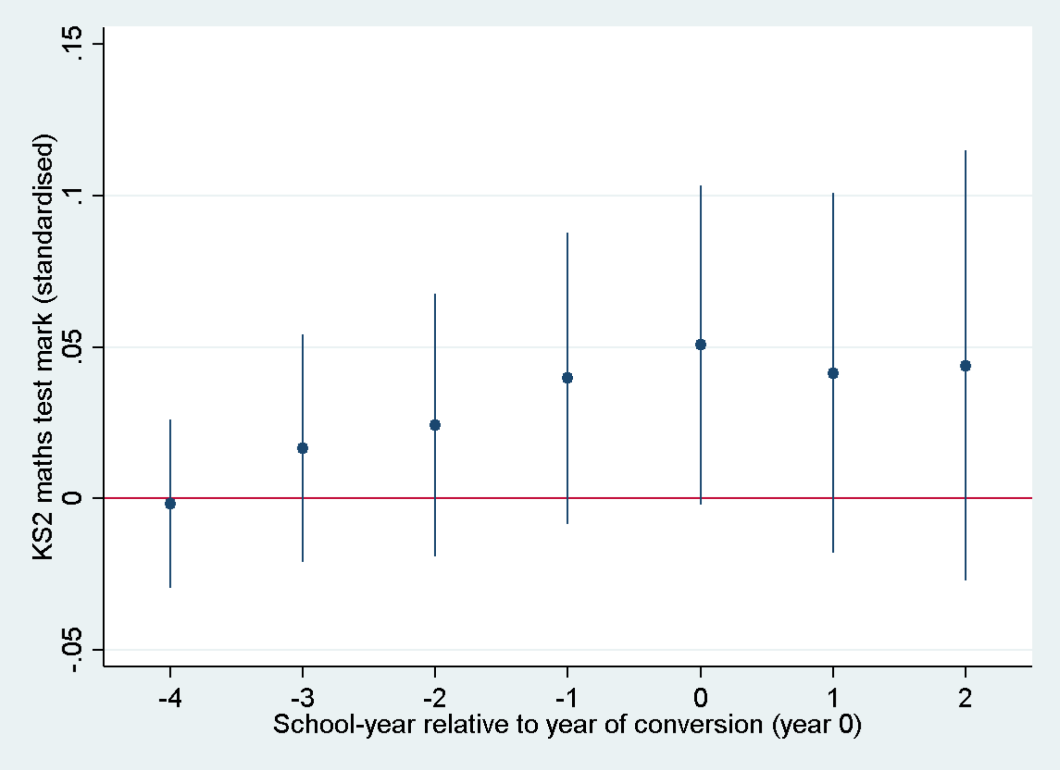

Figure 1b: Pre- and post-treatment effect estimates for KS2 reading test mark (point estimates and 95% confidence interval)

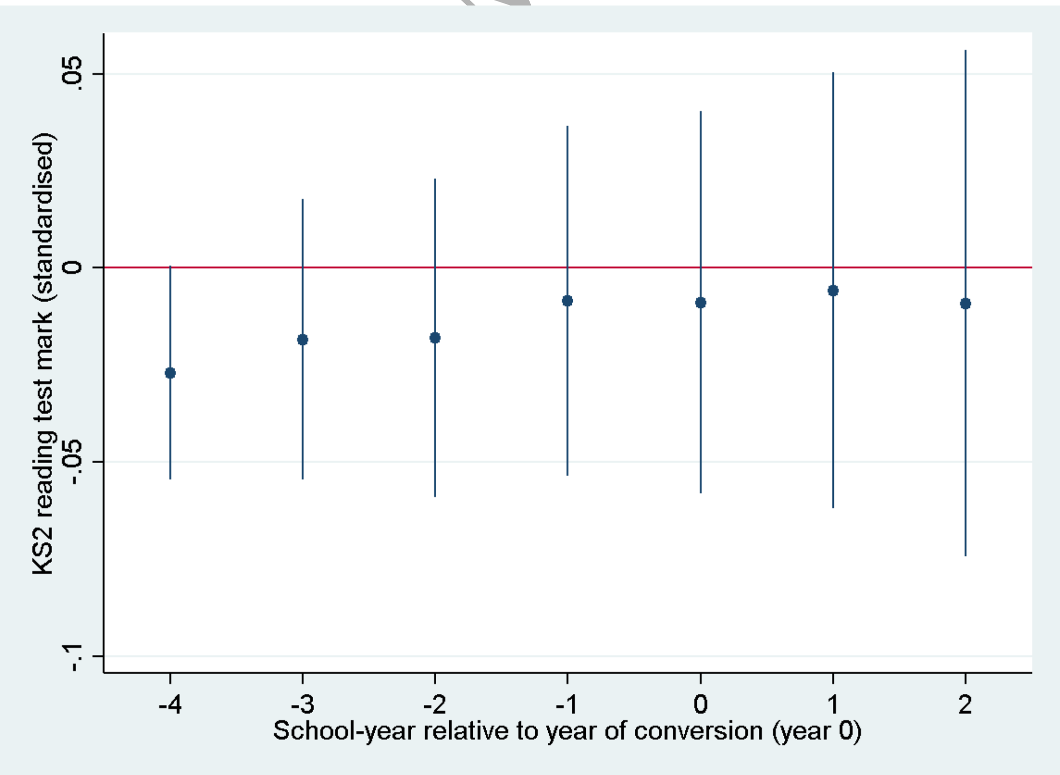

Table 1: The composition of English state primary schools at the start of the school-year

	2008	2009	2010	2011	2012	2013	2014	2015
Converter academy	0	0	6	265	647	1,069	1,462	1,859
Sponsored academy	0	0	0	5	115	391	685	898
Free school	0	0	0	2	34	37	92	117
Community school	9,893	9,803	9,727	9,491	9,111	8,624	8,166	7,842
Foundation school	911	913	911	863	819	768	734	698
Voluntary aided school	3,747	3,738	3,730	3,684	3,606	3,479	3,326	3,148
Voluntary controlled school	2,465	2,459	2,455	2,427	2,384	2,313	2,234	2,155
Grand Total	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: Tests of mean equality between treatment and control groups in attainment and pupil characteristics

	(A) School averages in 2011/12			(B) Change in school averages between 2011/12 and 2007/08		
	Control	Treatment	Difference (SE)	Control	Treatment	Difference (SE)
KS2 maths test mark	0.033	0.065	-0.032 (0.024)	-0.019	0.018	-0.036 (0.027)
KS2 reading test mark	0.078	0.059	0.019 (0.026)	0.012	0.003	0.009 (0.028)
KS1 maths TA points	15.840	15.853	-0.013 (0.089)	-0.131	-0.159	0.027 (0.096)
KS1 maths TA points	15.681	15.686	-0.006 (0.106)	0.188	0.189	-0.001 (0.105)
% female	0.487	0.497	-0.010 (0.008)	0.004	0.004	-0.001 (0.010)
% English is first language	0.891	0.897	-0.006 (0.013)	-0.023	-0.024	0.001 (0.005)
% White ethnicity	0.848	0.861	-0.013 (0.015)	-0.027	-0.019	-0.008 (0.005)
% FSM eligible	0.159	0.163	-0.004 (0.010)	0.027	0.031	-0.004 (0.007)
% with SEN	0.242	0.240	0.001 (0.009)	0.008	0.008	0.000 (0.010)
Cohort size	34.643	36.941	-2.298 (1.474)	-3.398	-2.703	-0.694 (0.586)
Observations	269	1,062		269	1,062	

Notes: variables are school-level averages for the year six cohorts. *** denotes significance at 1% level, ** at 5% level, * at 10% level.

Table 4: Mean pupil attainment and characteristics averaged over pre-treatment period by school type

	(1) Converter academies '11 and '12 openers	(2) Converter academies '13, '14 & '15 openers: treat. group	(3) Converter academies '16 openers: control group	(4) Sponsored academies	(5) Community schools	(6) Voluntary-controlled schools	(7) Voluntary-aided schools	(8) Foundation schools	(9) All schools
KS2 maths test mark	0.191	0.049	0.027	-0.341	-0.012	0.076	0.111	0.016	0.003
KS2 reading test mark	0.173	0.055	0.054	-0.349	-0.026	0.126	0.148	0.029	0.009
KS1 maths TA points	16.237	15.931	15.882	14.990	15.684	16.207	16.134	16.011	15.833
KS1 maths TA points	16.047	15.692	15.648	14.472	15.374	16.050	16.019	15.781	15.577
% female	0.491	0.493	0.488	0.490	0.488	0.491	0.499	0.490	0.491
% English is first language	0.912	0.911	0.902	0.846	0.861	0.961	0.887	0.896	0.883
% White ethnicity	0.870	0.872	0.862	0.801	0.816	0.937	0.839	0.851	0.841
% FSM eligible	0.116	0.147	0.146	0.260	0.186	0.093	0.131	0.124	0.162
% with SEN	0.220	0.243	0.240	0.311	0.257	0.229	0.225	0.228	0.248
Cohort size	46.301	38.325	36.422	39.251	39.522	25.057	30.113	45.922	36.153
Observations	447	1,062	269	910	6,660	1,615	2,707	215	13,885

Notes: variables are school-level averages for the year six cohorts between 2007/08 and 2011/12.

Table 5: KS2 maths and reading test mark DiD models with common treatment effect

	(1) KS2 maths test mark	(2) KS2 maths test mark	(3) KS2 maths test mark	(4) KS2 reading test mark	(5) KS2 reading test mark	(6) KS2 reading test mark
Converter academy	0.0141	0.0144	0.0169	0.0087	0.0104	0.0067
	(0.0119)	(0.0119)	(0.0121)	(0.0116)	(0.0116)	(0.0113)
Female		-0.1314***	-0.0882***		0.2061***	0.0769***
		(0.0034)	(0.0029)		(0.0036)	(0.0029)
English is first language		-0.0985***	-0.1677***		0.0484***	-0.0643***
		(0.0111)	(0.0085)		(0.0106)	(0.0081)
White ethnicity		-0.0317***	-0.0248***		-0.0400***	0.0096
		(0.0088)	(0.0064)		(0.0084)	(0.0063)
FSM eligible		-0.3848***	-0.2098***		-0.3620***	-0.1679***
		(0.0075)	(0.0054)		(0.0071)	(0.0050)
Cohort size		-0.0026***	-0.0020***		-0.0018***	-0.0011**
		(0.0007)	(0.0006)		(0.0005)	(0.0005)
KS1 math level			1.0740***			
			(0.0048)			
KS1 reading level						0.8965***
						(0.0042)
Constant	0.0819***	0.5990***	-1.9516***	0.0880***	0.2676***	-1.7700***
	(0.0069)	(0.0365)	(0.0371)	(0.0069)	(0.0315)	(0.0322)
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	326,835	326,835	326,835	324,369	324,369	324,369
Adj. R-Square	0.070	0.104	0.414	0.078	0.116	0.398

Notes: test marks are standardised to mean zero, standard deviation one. Columns (2), (3), (5), and (6) include month of birth effects. 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 test mark DiD models with heterogenous treatment effects

	(1) KS2 maths test mark	(2) KS2 reading test mark
Panel A: Heterogeneity by FSM eligibility		
Converter academy	0.0121	0.0019
	(0.0119)	(0.0111)
Converter academy x FSM eligible	0.0300**	0.0303**
	(0.0120)	(0.0119)
FSM eligible	-0.2176***	-0.1758***
	(0.0063)	(0.0059)
Adj. R-Square	0.414	0.398
Panel B: Heterogeneity by predecessor school type		
Converter academy	0.0013	-0.0192
	(0.0162)	(0.0150)
Converter academy x community or voluntary-controlled predecessor school	0.0217	0.0361**
	(0.0170)	(0.0156)
Adj. R-Square	0.414	0.398
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	326,835	324,369

Notes: test marks are standardised to mean zero, standard deviation one. Control variables are the same as in the Table 5. Robust standard errors clustered at school level in parentheses. *** denotes significance at 1% level, ** at 5% level, * at 10% level.

Table 7: Year 6 cohort composition DiD model with school-level data

	(1) Same school since KS1	(2) FSM Eligible	(3) SEN	(4) English is first language	(5) White ethnicity	(6) KS1 maths points	(7) KS1 reading points
Converter academy	0.0023	-0.0004	-0.0118**	-0.0014	0.0012	0.0139	0.0290
	(0.0057)	(0.0034)	(0.0047)	(0.0027)	(0.0027)	(0.0443)	(0.0498)
Constant	0.7398***	0.1314***	0.2322***	0.9199***	0.8789***	16.0040***	15.4963***
	(0.0042)	(0.0019)	(0.0029)	(0.0014)	(0.0016)	(0.0280)	(0.0300)
School fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
School year effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
			1062				
No. of academies	1,062	1,062	269	1,062	1,062	1,062	1,062
No. of future academies	269	269	9317	269	269	269	269
Observations	9,317	9,317	0.385	9,317	9,317	9,317	9,317
Adj. R-Square	0.761	0.768	-0.0118**	0.927	0.935	0.511	0.554

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

Table 8: 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
Converter academy	0.0236	0.0238	0.0048	-0.0001
	(0.0385)	(0.0378)	(0.0382)	(0.0375)
Female		-0.0093		1.1031***
		(0.0118)		(0.0125)
English is first language		0.3777***		0.8330***
		(0.0349)		(0.0429)
White ethnicity		-0.2238***		-0.6356***
		(0.0274)		(0.0330)
FSM eligible		-1.4576***		-1.7383***
		(0.0223)		(0.0269)
Cohort size		0.0017		0.0018
		(0.0016)		(0.0017)
Constant	15.9076***	16.9645***	15.7741***	16.2225***
	(0.0236)	(0.0864)	(0.0255)	(0.0917)
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	432,669	432,669	432,659	432,659
Adj. R-Square	0.071	0.131	0.077	0.151

Notes: columns (2) and (4) include month of birth dummies. Robust standard errors clustered at school level in parentheses. *** denotes significance at 1% level, ** at 5% level, * at 10% level.

Table 9: Entry-year intake composition DiD model with school-level data

	(1) FSM Eligible	(2) SEN	(3) English is first language	(4) White ethnicity
Converter 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: Common treatment effect DiD models with alternative KS2 attainment measures

	(1) KS2 maths NC point score	(2) KS2 reading NC point score	(3) KS2 maths level 4+	(4) KS2 reading level 4+
Converter academy	0.0451	0.0185	0.0025	0.0028
	(0.0547)	(0.0473)	(0.0033)	(0.0023)
Constant	14.2102***	19.1126***	0.4742***	0.7318***
	(0.1713)	(0.1410)	(0.0134)	(0.0074)
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	333,974	331,185	324,653	319,049
Adj. R-Square	0.529	0.433	0.146	0.092

Notes: the outcome variable in columns (1) and (2) is the point score equivalent to the national curriculum (NC) level achieved in the correspondent subject: level 1 is equal to 9 points, level 6 is 39 points (one level corresponds to 6 points). Dependent variable in columns (3) and (4) are equal to one if the pupil achieves NC level 4 in maths/reading and zero otherwise. Control variables are the same as in Table 5. Robust standard errors clustered at school level in parentheses. *** denotes significance at 1% level, ** at 5% level, * at 10% level.

Appendix Table 2: Robustness checks

	(1) KS2 maths test mark	(2) KS2 reading test mark
Panel A: Only consider full school-years as an Academy as exposure to academy status		
Converter academy	0.0051	0.0109
	(0.0123)	(0.0113)
Adj. R-Square	0.414	0.398
Panel B: Introduce school-specific trends to the model		
Converter academy	0.0179	0.0018
	(0.0120)	(0.0114)
Adj. R-Square	0.429	0.409
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	326,835	324,369

Notes: test marks are standardised to mean zero, standard deviation one. Control variables are the same as in Table 5. Robust standard errors clustered at school level in parentheses. *** denotes significance at 1% level, ** at 5% level, * at 10% level.