The University Of Sheffield.

Department Of Economics.

Sheffield Economic Research Paper Series.

The Effect of Primary Converter Academies on Pupil Performance

Emily McDool

ISSN 1749-8368

SERPS no. 2016013
December 2016

THE EFFECT OF PRIMARY CONVERTER ACADEMIES ON PUPIL PERFORMANCE

Emily McDool

Department of Economics, University of Sheffield, UK

December 2016

Abstract

This study analyses the impact of primary converter academies on pupil progress, using data from the National Pupil Database. Adopting a difference-in-differences methodology, a positive influence of converter academies upon pupil outcomes is identified when comparing individuals exposed to academy conversion with those who complete primary school before conversion. Attending a converter academy increases a pupil's ranking within their cohort, according to the average point score, by between 1.1 and 2.6 percentile points. Primary converter academies are found to consistently improve pupil outcomes within areas of low deprivation whereas in areas of high deprivation, the identified effect may be much greater but is more variable across years of conversion and cohorts. White pupils and pupils not in receipt of free school meals are also consistently found to benefit from converter academy attendance.

Key Words: Academies, pupil performance, education policy, children

JEL Codes: I20, I21, I28, J13

Acknowledgements: This research was funded by the ESRC. The National Pupil Database data used were kindly provided by the Department for Education. I would like to thank Sarah Brown, Geraint Johnes, Steve McIntosh and Gurleen Popli along with participants at the WPEG conference at the University of Sheffield in July 2016, for the helpful comments on this paper.

1 Introduction

Introduced by the Labour government in the early 2000s, academy schools present a divergence from the usual provision of state schooling in England. Academies are publicly funded schools that gain greater autonomy by functioning independently of local authorities. Initially, the Academies Programme aimed to replace underperforming secondary schools situated in socially disadvantaged urban areas with a poor GCSE attainment performance record; these schools would be converted into sponsored academies, appointed a new governing body and managed by an academy trust. By 2010, 203 secondary sponsored academies existed within England's education system (Department for Education, 2011). Following the 2010 general election, the coalition government stated its intentions to widen the academies remit, offering primary, secondary and special schools the opportunity to seek academy status. The Academies Act 2010 consequently increased the heterogeneity of new academies by permitting all schools to apply to voluntarily convert to converter academies; this allowed for all schools to benefit from the greater autonomy of an academy without a sponsor[1] (National Audit Office, 2010). Schools rated outstanding by Ofsted, who voluntarily applied to become an academy, were fast-tracked through the process of conversion, allowing for conversion within the same year. 'Weak' schools continued to be advised that improvement could be attainable through conversion to a sponsored academy.

Though the proportion of primary academies is relatively small at 13% relative to the 60% of secondary schools that academies constitute, the number of primary academies exceeds the number of secondary academies (House of commons, 2015), with over half a million pupils attending a primary academy (Department for Education, 2014b). Given the commitment to the expansion of the academy programme, the quantity of all academies is expected to continue to rise (Department for Education, 2016).

It is imperative to understand the impact and usefulness of the large-scale academies programme for which resources and funding is required. It is equally vital to assess the programme as a determinant of primary pupils' outcomes since educational attainment in the early stages of schooling are key determinants of educational outcomes later in life (Dearden et al. 2004). Since one in five children leave primary school unable to read at the level required for secondary school (Department for Education, 2015b), it is vital to understand which policies improve or worsen educational standards in practice. The impact of academy schools upon pupil performance will form the focus of this paper

[1] Sponsored academies are usually previously underperforming schools that convert under a sponsor to become an academy in order to raise attainment. Converter academies are previously successful schools that voluntarily select to become an academy, often to benefit from greater autonomy.

which will examine the effect of primary converter academy schools specifically. Utilising pupil-level data from the National Pupil Database (NPD) a difference-in-differences (DID) methodology is adopted to observe the impact upon pupil progress between Key Stage 1 (KS1), when pupils are aged 7, and Key Stage 2 (KS2)[2], when aged 11.

Evidence of the impact of academy schools upon pupil outcomes is currently limited; this is especially so for post-2010 converter academies and equally, for primary academies, thus prompting the reported need for further research into academy schools (House of Commons, 2015). The existing literature has largely focused on analysing the impact of pre-2010, Labour secondary sponsored academies; promising results have been presented, with a positive relationship with pupil outcomes identified in a number of studies (Department for Education 2012; Hutchings et al. 2014; Eyles and Machin, 2015). In addition, evidence suggests that the quality of the pupil intake and Ofsted ratings improve post-conversion (Eyles and Machin, 2015). However, Wilson (2011) finds that sponsored academies reduce the intake of pupils from deprived backgrounds post-conversion, thus increasing educational inequality. The extent to which some pupils may be able to access the positive benefits of academy attendance may therefore be restricted.

The relevance and generalisability of the sponsored academies literature may, however, be limited when evaluating primary converter academies because prior to 2010, only secondary schools converted to academies. Additionally, Eyles et al. (2015) identify that while Labour academies are characterised by low attainment and a high proportion of disadvantaged pupils from poor family backgrounds, post-2010 academies have a lower share of free school meals (FSM) eligible children and experience little change in the ability composition of their intake. Of greater significance to analyses of primary converter academies, is therefore the post-2010 academies literature which is currently very underdeveloped. One study of post-2010 converter academies by Worth (2014) examines the impact of secondary converter academies on school-level pupil performance measures[3]. Adopting a propensity score methodology, an insignificant difference in the performance of converter academies relative to characteristically similar maintained schools is generally identified, except in 2014 when two-year-old academies were found to significantly outperform maintained schools in gold standard attainment and value-added measures, though differences are small.

Central to the academies programme is school autonomy, which is often quoted as the main benefit of academy status (Cirin, 2014), providing schools with greater freedom in management that aids

[2] Primary education may be split into two stages; infant, known as Key stage 1 (KS1), which caters for children between foundation year and 2 when pupils are aged between 5 and 7, and junior, referred to as Key stage 2 (KS2), which provides education to children in year 3 to year 6, up to the age of 11.
[3] Academies that converted between the 2009/10 and 2011/12 academic years are observed.

innovation and improvement. The higher management quality associated with more autonomous schools may positively influence pupil performance through practises such as accountability to an external governing body and the adoption of a long-term strategy (Bloom et al., 2015). The introduction of more autonomous schools into an education system is commonly associated with a modification to school choice, which, together with autonomy, is argued to assist in increasing educational standards. Greater choice may allow for the needs of pupils to be more closely matched to school provision (Machin and Silva, 2013), while improving standards through market mechanisms, as 'good' schools attract 'good' pupils and therefore financial benefits and incentives for schools. More autonomous school types, such as grant maintained (GM)[4] and faith schools, often form the focus of the autonomy and choice literature in the UK. Clark (2009) identifies positive and significant gains to pupil performance of GM school attendance, when comparing GM schools to schools that narrowly missed out on GM conversion. Allen (2010) correspondingly identifies short-term benefits of GM attendance; however, both authors acknowledge that improved pupil intake and a reduction in socially disadvantaged pupils following conversion may drive these results. More able pupils may therefore be attracted to more autonomous schools; Eyles and Machin (2015) find evidence for this when analysing secondary academy schools, whilst Gibbons and Silva (2011) similarly identify that more able pupils are more likely to attend more autonomous faith primary schools. Such schools are found to offer no advantages, in terms of pupil performance, relative to less autonomous LEA controlled primary schools. Choice and competition in primary education is however, found to provide positive performance gains for primary schools with autonomous governance (Gibbons et al. 2008). Evidence from outside of the UK, from the US and Sweden, where charter schools and free schools benefit from greater levels of autonomy, suggests that greater school autonomy and choice is beneficial for pupil performance (Abdulkadiroglu et al. 2011; Hoxby and Muraka, 2009; Angrist et al. 2010; Böhlmark and Lindahl, 2012).

This paper contributes to the academies literature in two ways; firstly, by investigating the impact of primary academy schools upon pupil progress and secondly, by analysing primary academies that converted between 2011/12 and 2013/14, thereby analysing post-2010 academies that voluntarily converted. By adopting a DID methodology, a treatment and control group are formed; pupils who were enrolled in a primary school prior to, and at the time of conversion, form the treatment group while the control group consists of pupils who attended eventual academies, but left before the primary school converted. The approach addresses the potential issue that a post-conversion improvement in pupil intake quality may drive positive performance findings (Eyles and Machin,

[4] Grant maintained were state secondary schools that existed in England and Wales between 1988 and 1998. They were a product of the Education Reform Act 1988 that allowed existing schools to opt out of local government control.

2015; Wilson, 2011) by only observing individuals who were enrolled in a primary school prior to its conversion; the enrolment decision is therefore exogenous to the academy conversion. The approach also constructs a credible control group by only observing the pupils who attended eventual academies since the characteristics of academies and eventual academies are more comparable than non-converters in the sample period. Finally, treated individuals are compared with control individuals from the same school cohort; the treatment group are therefore subject to the same shocks and cohort-specific trends as individuals within the control group. The analysis is extended to identify whether the characteristics of pupils may determine the extent to which they are influenced by the academies programme. Additionally, the effect is estimated for pupils attending schools in deprived neighbourhoods and compared with non-deprived neighbourhoods. In a similar manner, the extent of a differential impact of primary converter academies by FSM status and ethnicity is explored.

The results provide evidence of a positive influence of converter academies upon the outcomes of primary aged pupils; converter academies are found to increase the percentile rank of pupils' average point scores by between 1.1 and 2.6 percentile points, ceteris paribus. These results are based on a number of models that are adopted within the analysis which allow for differences in the exposure of pupils to converter academies to be considered. The results are consistent across specifications and are robust to a falsified treatment placebo test.

When estimating the effect of primary converter academies by school neighbourhood deprivation, pupil progress is more consistently found to be improved for pupils in the least deprived areas, relative to the most deprived areas, when multiple models varying in the year of academy conversion and pupil cohort are estimated. However, the magnitude of the effect is much smaller than in the most deprived neighbourhoods when a significant impact is identified for both subsamples. Similarly, pupil progress is more consistently significantly influenced by academies for white and non-FSM pupils, than their non-white and FSM counterparts.

This paper will be structured as follows; section 2 will provide a description of the data with the methodology provided in section 3. The main results will be presented in section 4. A summary of the paper's aims, findings and conclusions will be provided in section 5.

2 Data

The National Pupil Database (NPD) is utilised to analyse the impact of converter academies on pupil performance. The NPD is a pupil-level database containing data on all pupils in state schools in England which matches pupil-level data to school characteristics. Pupil characteristics are provided by the dataset alongside attainment data for each pupil from Key Stage 1 to Key Stage 5, allowing for pupils to be tracked over time, across schools and educational institutions. NPD data from 2007-2014 will be utilised to observe three cohorts of pupils; those leaving primary school in the academic year 2011/12 and 2012/13 alongside those leaving in 2013/14, as indicated in Table 1. The panel nature of the NPD data is utilised to allow for individuals to be observed at the end of both KS1 and KS2[5].

Institutional data, provided by the Department for Education, is available for all schools for the 2005/6-2014/15 academic years. School-level data are matched to each pupil in every year, so that changes in the school, for instance the institution type, may be tracked over time and observed once the pupil leaves school, or between observation periods (KS1 and KS2). This allows for pupils of eventual academies to be identified which is central to the control group construction.

Individuals who experience a change in the unique identifier are dropped from the sample[6]. The unique identifier could change in three situations, each of which may impact directly upon pupil performance. Firstly, when a school's local authority experiences a reorganisation; this may have implications for the competition and pupil intake of the pupil's school. Secondly, following the movement of a pupil between schools. Thirdly, if a pupil moves from an infant to junior school; these individuals are likely to account for a large proportion of the dropped sample which are dropped since infant and junior schools are essentially two separate schools. Since in each situation pupil performance may be impacted, it is difficult to disentangle the performance effect of academy schools. Individuals with missing KS1 data are also dropped alongside individuals who start KS1 or KS2 at an unexpected age. A number of institutions' observation years are also dropped since institutions are reported to be both academies and a different institution type within the same academic year; it is therefore difficult to establish the institution type.

Table 2 indicates the year of academy conversion experienced by individuals within the sample, which consists of individuals from the three cohorts who attend academies or eventual academies and leave primary school between the 11/12 and 13/14 academic years.

Table 1 Observed academic years

COHORT	FINAL KS1 ACADEMIC YEAR	FINAL KS2 ACADEMIC YEAR
1	2007/2008	2011/2012
2	2008/2009	2012/2013
3	2009/2010	2013/2014

[5] When pupils are aged 6/7 and 10/11, dependent on the month of birth
[6] This leads to a loss of just over 1,100,000 observations

Table 2 Academic year of academy conversion

YEAR ACADEMY	OBSERVATIONS	PERCENT
2011	13,142	5.32
2012	54,254	21.96
2013	70,262	28.45
2014	60,028	24.30
2015	49,324	19.97
Total	247,010[7]	100

Institutional data is also matched to the Index of Multiple Deprivation (IMD) and the Income Deprivation Affecting Children Index (IDACI) [8] using the Lower Layer Super Output Area (LSOA)[9] of the school, allowing for the neighbourhood deprivation level of the institution to be identified.

The outcome of interest is the percentile rank of the pupil's average point score (APS). The APS is measured at the end of KS1 and KS2[10] and is used to calculate the pupil's rank amongst all other pupils in the same cohort[11]. The APS is often used to estimate the value-added score between KS1 and KS2 which is a widely-adopted measure of progression between different levels of education (Gibbons et al. 2013; Wilson and Piebalga, 2008). The percentile rank of the child in terms of APS is used rather than the raw APS since APS scores may not take account of the differences in test difficulty between the different cohorts[12].

Figure 1 provides a density plot of the APS percentile ranks of the treated and control groups in KS1; overall, the pre-treatment percentile rank distribution is similar for treated and control individuals. The mean percentile rank of the control group is 48.39 which is comparable to the treatment group mean of 48.97. Differences in the distribution of the APS rank between the treatment and control group are apparent in the post-treatment period, as Figure 2 shows, particularly below the 20th percentile and beyond the 60th percentile. Relative to the pre-treatment period, greater differences in

[7] This equates to 123,505 individuals whose schools become converter academies since individuals are observed in two periods; KS1 and KS2

[8] The IMD and IDACI each provide a measure of relative deprivation for every neighbourhood in England. The IDACI measures the proportion of children specifically, that reside in low income households The IMD and IDACI deciles relates to the neighbourhood of the school.

[9] LSOAs are geographic areas containing on average 1,500 residents or 650 households. Within England 32,844 LSOAs exist

[10] In KS1, the APS is calculated as the average score achieved in reading, writing and mathematics obtained from a teacher assessment. In KS2 the APS is based upon the average point score achieved in English, maths and science in the KS2 standardised national tests (SATs) taken at age 11 by all primary school pupils. Though these measures vary slightly in the subject assessed since tests need to be age specific, the scores between KS1 and KS2 are comparable since they provide an indication of the child's overall level of ability; it is likely that there will be a high correlation between the scores achieved in individual subjects.

[11] The percentile rank is based on all pupils within the cohort excluding those who are dropped from the sample due to missing data and other reasons to be specified later in this data section.

[12] Machin and McNally (2008) similarly adopt a percentile outcome measure when analysing the percentile reading score of pupils in the analysis of the literacy hour.

the mean percentile rank of the treated and control groups exist, at 52.47 and 51.06 respectively. The raw statistics therefore indicate that the change in the mean percentile score between KS1 and KS2 is greater for the treated group, with a change of 3.5, than for the control group, with a change in mean percentile score of 2.7.

Figure 1 KS1 APS percentile distribution treatment and control groups

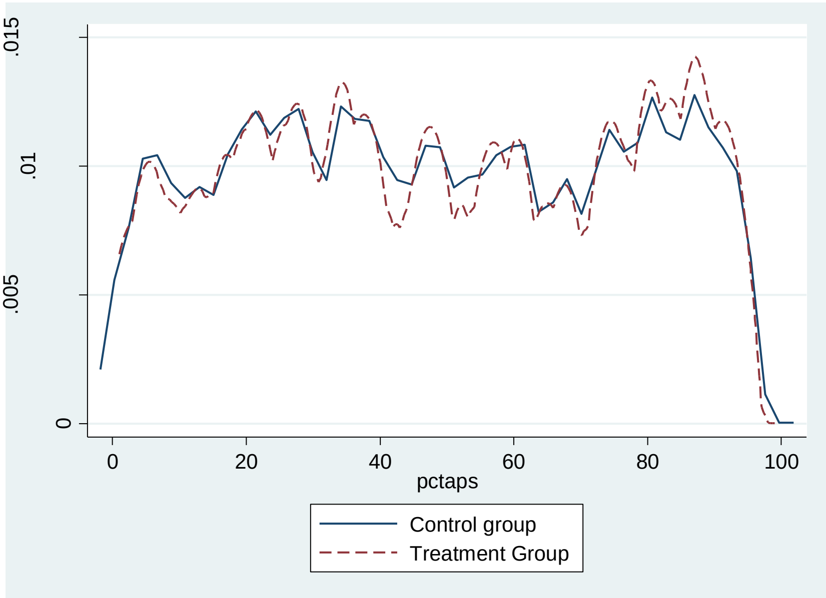
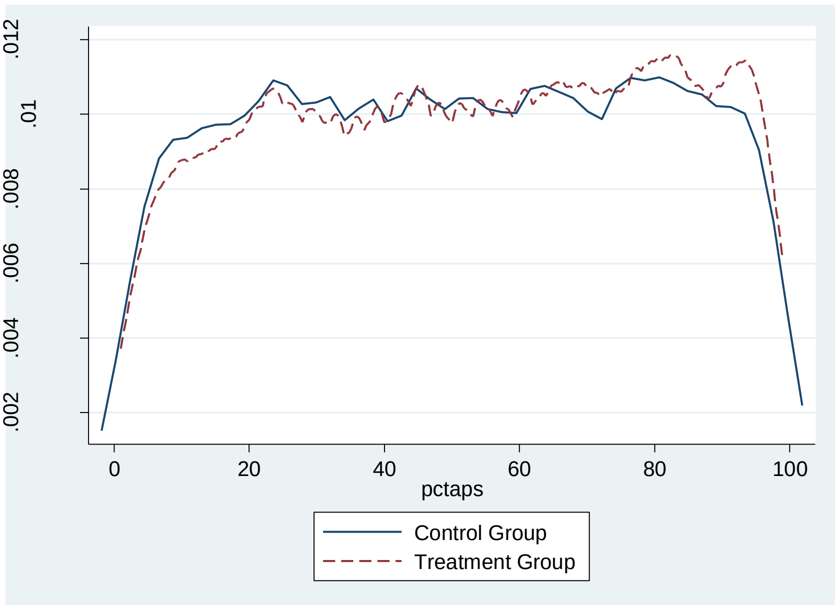


Figure 2 KS2 APS percentile distribution treatment and control groups



3 Methodology

3.1 Difference-in-differences

The difference-in-differences approach allows for the estimation of a treatment effect, specifically, the average treatment effect for the treated when we assume common trends; in the absence of treatment, treated individuals are assumed to be subject to the same trends as untreated individuals who therefore provide the counterfactual outcomes of the treated, should they have not received treatment, thereby overcoming the evaluation problem which occurs as only one outcome per person can be observed at any point in time.

There are three key features of the methodology to highlight; firstly, treated individuals who experience academy conversion are compared with individuals whose schools become academies but once they had left the primary school, these are referred to as eventual academies. As the descriptive statistics to be presented indicate, eventual academies are characteristically more similar to converter academies than schools that do not convert; this is especially so since academy conversion is not random and is a choice made by the school. This is a similar approach to that taken by Eyles and Machin (2015) who employ a control group consisting of pupils in state schools that convert to academies after the study's sample period ends. However, unlike Eyles and Machin (2015) this paper specifies and observes a specific year of conversion in each model for both the treatment and control group; this approach checks the robustness of results alongside the generalisability of the estimated effect of academy schools by year of conversion.

Secondly, also in a similar manner to Eyles and Machin (2015), this study observes only individuals who already attend the primary school before academy conversion so that academy conversion should be exogenous to the school enrolment choice. This approach overcomes the potential issues surrounding the improved pupil intake of academies post-conversion (Eyles and Machin, 2015; Wilson, 2011).

Thirdly, treated and control individuals are compared within the same cohort; individuals are therefore subject to the same cohort specific effects and time effects. This also allows for pupils to be compared within the same percentile rank since the rank relates to the APS scores of pupils in a specific cohort. Whilst this allows for different levels of exposure to academy schools to be examined, this approach also assists in analysing the generalisability of the identified impact of converted academies by cohort as well as by conversion year.

The outcome of interest, the pupil's percentile rank, is measured at two points in time: in KS1 (age 7) before treatment and in KS2 (age 11), in the post-treatment period. Treatment will occur between KS1, when the pupil attends the converting school and KS2, prior to the completion of primary school. The control group consists of individuals from the same cohort as the treated group, whose school converted to an academy after KS2, when they left primary school. Unlike the usual difference-in-differences set up, there is therefore not a single point in time which marks the treatment or the introduction of a policy for all treated individuals since schools converted at different times.

Initially, the impact of academies that converted in 2011/2012 is analysed when compared to eventual academies that convert in the 2012/13 academic year and observing pupils who left primary school in the same academic year; this is referred to as model A. As a check of robustness and to analyse the generalisability of the results to all years and cohorts, these results are then compared with similar models that vary in the years of conversion or the cohort of pupils. Three cohorts that completed KS2 between 2011-12 and 2013/14 are observed in the analysis, as depicted in Figure 3. Table 3 summarises the models to be estimated. The initial four models analyse the impact of converters within the 2011/12 academic year[13]. Individuals in the treatment and control group are from the same cohort and therefore leave primary school in the same year, which will be prior to the academy conversion year for the control group. While multiple years of conversion are observed, different levels of exposure are also examined; while in models A and B for example, treated individuals will experience an academy for up to one full academic year, in model C, exposure is up to two academic years. Pooled models are also examined; while the 12AB model simply pools the control groups from models A and B, the final pooled model includes all treated and control individuals from across the three cohorts and controls for year and cohort specific effects by including cohort dummies in the model. The sample size of each model is provided in Table 4.

[Table 4 here]

[13] The academic year continues to be recognised as September to August. For academy converters however, June to May is considered the same academic year since converters in June, July and August may not be open as academies for an entire month to students until September. E.g. an academy converting in July 2011 would be considered as converting in the 11/12 year rather than the 10/11 academic year.

Figure 3 Cohorts observed

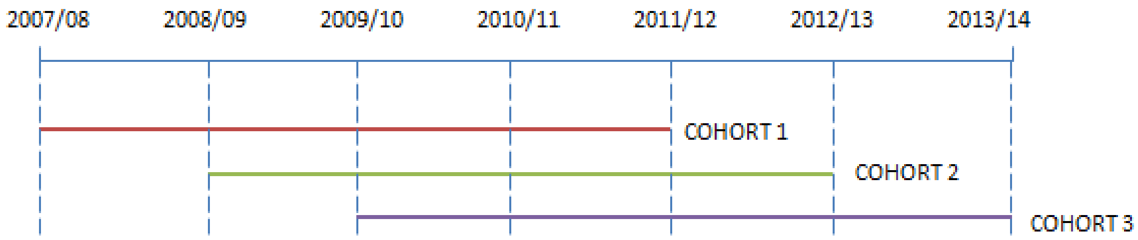


Table 3 Models to be analysed

MODEL	BECAME ACADEMY		YEAR LEFT PRIMARY
	TREATMENT	CONTROL	
A	11/12	12/13	11/12
B	11/12	13/14	11/12
12 AB (A&B pooled)	11/12	12/13 or 13/14	11/12
C	11/12	13/14	12/13
D	12/13	13/14	12/13
POOLED	Any time while at school (11/12-13/14)	After child left school (11/12-13/14	11/12 - 13/14

The first five models may therefore be represented in a general form as follows for pupil *i* in school *s* in year *t:*

(Eq.1)

$$PAPS_{ist} = \beta_0 + \beta_1(Treat_{is}) * (Time_t) + \beta_2(Treat_{is}) + \beta_3(Time_t) + \beta_4 X_{ist} + \beta_5 S_{st} + \varepsilon_{ist}$$

This varies from the single pooled model which may be represented as follows:

(Eq.2)

$$PAPS_{ist} = \beta_0 + \beta_1(Treat_{is}) * (Time_t) + \beta_2(Treat_{is}) + \beta_3(Time_t) + \beta_4 X_{ist} + \beta_5 S_{st} + \beta_6 T_t + \varepsilon_{ist}$$

PAPS gives the percentile rank of pupil i at time t in school s within their cohort according to their APS. $\beta_0$indicates the intercept term; β_1 represents the coefficient on the interaction between the treatment dummy and the time dummy and therefore indicates the treatment effect. β_2 denotes the coefficient on the treatment dummy and indicates, in the absence of treatment, the difference in the percentile rank of individuals in the treatment and control group. The parameter β_3 is the coefficient on the time dummy equalling zero when the child is in KS1 and equalling one when the child in in KS2. The time dummy indicates how the percentile rank of pupils' changes over time, from KS1 to KS2. The parameter β_4 is a vector of coefficients on the characteristics of individual i at time t in school s affecting their distribution in the test score ranks. S_{st} is a vector of school characteristics that may influence pupils' rank in the APS distribution of their cohort. T_t, which appears in equation 3 only represents the cohort dummies which enter the model to control for cohort and year specific time effects in the pooled model. Finally, ε_{ist} denotes the error term. It should be noted that standard errors have been clustered at the individual level[14].

A range of individual and school characteristics are controlled for due to their potential influence on pupil performance. Owing to the positive association with cognitive development problems for primary aged children, English as an additional language (EAL) status is controlled for (Sammons et al. 2007), alongside ethnicity, which plays a significant role in pupils' educational trajectories (Dustmann et al. 2010; Wilson et al. 2011). Vignoles and Meschi (2010) identify that children from ethnic minority groups make greater progress than white children, while on average, females do better than males in terms of attainment. Alongside gender and ethnicity, gaps in educational attainment are commonly associated with socio-economic disadvantage (Strand 2014; Kramarz et al. 2008) which will be proxied in this model by free school meal eligibility (FSM)[15]. Similarly, Special educational needs (SEN) enters the model since SEN children characteristically perform worse than non-SEN children (Crawford and Vignoles, 2010; Kramarz et al. 2008). Younger children within the year group are consistently found to be academically disadvantaged, relative to children born at the beginning of the academic year (Campbell, 2013; Crawford et al. 2010); the month of birth is therefore controlled for.

School characteristic controls include the local area deprivation of the school, measured by the Index of Multiple Deprivation (IMD) decile; this is also likely to represent or be highly correlated with the deprivation of the child's and their classmates' neighbourhood. The type of institution may also influence pupil performance at primary school (Silva, 2006), whilst the autonomy gains from conversion may differ by institution type. It is therefore important to control for differences in

[14] If the individual's school converts to become an academy, the treatment variable is often serially correlated as the individual is either deemed treated or untreated throughout the data. The error term is therefore correlated over time if any unobservable characteristics determine whether an individual is treated, thus standard errors are likely to be biased. Clustering standard errors at the individual level is one potential solution to this issue (Cameron and Miller, 2015).
[15] FSM indicates if the child was recorded as eligible for free school meals in a spring census in the last 6 years

institution type before conversion for all individuals and for control individuals in the post-treatment period[16]. Finally, the number of months into the academic year that an academy converted is included in the model to control for any disruption effects alongside differences in exposure since conversion may occur in any month, thus differences in academy exposure may exist within models.

3.2 Deprivation analysis

Unlike Labour academies, post-2010 converter academies are not located exclusively in deprived neighbourhoods, though many converter academies do exist within deprived areas, thus allowing for a differential effect of converter academies by neighbourhood deprivation to be identified. This differential effect may be of policy interest due to the persistent gap between the performance of pupils from deprived and non-deprived areas in England, alongside the existing evidence on the negative impact of neighbourhood deprivation on pupil outcomes (Lindahl 2011; Nicoletti and Rabe, 2010). This relationship, between deprivation and educational outcomes, could potentially be influenced by the academies programme.

A difference-in-differences methodology continues to be implemented once the sample is restricted to individuals who attend schools in the top 30% of deprived neighbourhoods according to the Index of Multiple Deprivation. In a similar manner, the estimation is restricted to individuals in the 30% least deprived neighbourhoods. The effect of primary converter academies may then be compared between the most and least deprived neighbourhoods.

3.3 *FSM and ethnicity*

Gaps in educational attainment by ethnicity and FSM status, which is an indicator of socio-economic disadvantage, are equally apparent in the UK (Department for Education, 2015). In a similar manner, the sample is therefore split according to FSM status and ethnicity to identify whether a differential effect of converter academies is experienced due to these characteristics This allows for the identification of individuals who benefit to a greater extent from the academies programme.

4 RESULTS

4.1 Descriptive Statistics

The descriptive statistics are presented for the treatment and control groups, who are observed in the pooled model, and for the non-academy group which includes individuals excluded from the main

[16] Treated individuals will attend a converter academy in the post-treatment period.

analysis since their school did not convert within the sample period. Descriptive statistics from non-academies are presented to provide justification for the use of eventual academies as the control group within the analysis. Table 5 provides the mean value of the control variables across the sub-samples. Relative to non-academies, eventual academies are more similar to the treated group in terms of individual characteristics such as EAL, ethnicity, FSM and SEN. School characteristics, such as IMD decile and the proportions of community, voluntary controlled and foundation schools[17] are also more comparable for the treatment and control group. Due to large sample sizes, however, small differences in the mean values are significant. Eventual academies therefore bear the greatest similarities with the treated group thus prompting the use of these individuals as the control group.

[Table 5 here]

Amongst the schools that did convert to academies, an almost equal distribution of deprivation, based upon the IMD deciles, is evident in Figure 4; it therefore does not seem that converter academies were mostly schools located in non-deprived neighbourhoods.

[Figure 4 here]

Table 6 provides the raw difference-in-differences estimates for each model; the difference indicates the change in the mean APS percentile between the pre-treatment and post-treatment periods, for both the treatment and control groups. The difference in the changes of the treatment and control group provides the raw difference-in-differences. All difference-in-differences estimates are positive, with the exception of model D which provides a negative raw difference-in-differences estimate, suggesting that the treated group progress between KS1 and KS2 to a lesser extent than the control group whose APS percentile improves more over the observed time period. Before treatment, when both groups were subject to the same untreated state, the treatment and control group in model A are the most dissimilar in terms of KS1 APS percentile with the pooled group providing the most similar treatment and control groups. Any group differences will be accounted for within the difference-in-differences framework.

[Table 6 here]

Table 7 provides the raw differences in the APS scores of the three cohorts within the sample by treatment status. Since the APS percentile is calculated within the cohorts and is therefore only

[17] Community schools refer to state-funded primaries that are controlled by the local council. Voluntary aided schools are state-funded primary schools that receive contributions from a foundation or trust (usually a religious organisation) which has a substantial influence on the school running. Voluntary controlled primaries have less autonomy than voluntary aided schools but are similarly influenced by a foundation or trust.

comparable within cohorts, the mean APS score is provided. Only the mean KS2 APS scores of the control group increase over time with cohort; interestingly, KS1 scores decrease with later cohorts among treated individuals, thus cohort 1 obtains the highest APS score on average both at KS1 and KS2. Individuals from the treatment and control group are most similar in cohort 3. The mean KS1 and KS2 scores are marginally higher for treated individuals, relative to the control group, in every cohort.

[Table 7 here]

4.2 Main results

A summary of the main results is provided in Table 8 with the full results provided in table A1 in the appendix. The results indicate a positive and significant effect of time; in model A, between KS1 and KS2 pupils move 3.7 percentiles up the cohort rank. This finding is consistent across all models. Since the rank is based on the APS scores of the entire cohort and includes all pupils whose school did not become an academy[18], this movement of academy and eventual academy pupils up the rank, must be at the expense of the pupils of non-converters, who move down the cohort rankings.

In model A, the coefficient on the treat variable is positive and significant at the 10% level, indicating that in the absence of treatment, the APS percentile rank of individuals in the treatment group is significantly greater than the rank of individuals within the control group. A similar effect is identified in models 12AB and the pooled model which are significant at the 5% and 1% level respectively. In model D, the treat coefficient is negative and significant indicating that treated individuals are ranked lower than their counterparts in the control group, in the absence of treatment. These differences are controlled for in the model.

[Table 8 here]

Model A identifies a positive and significant impact of primary converter academies upon pupil progress when analysing the impact of converter academies that converted in the 11/12 academic year by observing 11/12 primary leavers and adopting a control group of individuals who attended 12/13 converters. Treated individuals are exposed to a converter academy for a maximum of one academic year since schools may convert at any point in the 11/12 academic year. The difference-in-difference estimate indicates that relative to pupils from the same cohort whose school converted to an academy in the 12/13 academic year, converter academies raised treated pupils' percentile APS scores by 1.9

[18] It excludes individuals who are dropped from the sample (e.g. infant and junior school attendees) as discussed in the data section

percentile points, ceteris paribus. This significant difference-in-difference estimate is greater than the raw difference presented in Table 7, once controls are added.

The positive and significant effect of converter academies is consistently identified in all subsequent models, though the magnitude of this effect varies. The effect identified in model B is slightly greater with a difference-in-difference estimate of 2.1. As in model A, model B analyses 11/12 converters when observing pupils from the 11/12 cohort, though in model B, 13/14 converters constitute the control group; the control group therefore leaves the school two academic years prior to conversion. A very similar result is identified in model 12AB where the control group of models A and B are simply pooled. With an adjustment to the control group, the difference-in-differences estimate remains positive and significant.

A greater impact of primary converter academies is identified in model B, possibly because in model A, the treatment and control schools were more similar as they converted within one academic year of each other. Though the 12/13 converters could not benefit from the actual conversion in the 11/12 academic year, they may have begun to put measures in place for conversion, providing benefits from some aspects of becoming an academy, for example by conversing with other converter academies to share expertise and pool resources (Department for Education, 2014b). Pupil performance may therefore be improved, closing the gap in performance with converter academies. This preparation may be less likely for 13/14 converters, who did not convert for some time after the 11/12 academic year, thus greater gaps in pupil performance may exist.

Model C investigates whether the benefits of conversion increase with greater exposure to converter academies, as Eyles and Machin (2015) identify. Model C continues to analyse the impact of 11/12 converters but allows for an additional year of exposure by pupils from the 12/13 cohort. The difference-in-difference estimate indicates that treated individuals move 1.1 percentiles further up the APS percentile rank than pupils in the control group, ceteris paribus. Despite the greater level of exposure, the results from model C are actually smaller than those of models A and B, in which, only one year of exposure is observed.

Model D presents the greatest estimated impact of primary converter academies, relative to alternative models; the percentile score of pupils who attend a converter academy is increased by 2.6 relative to the control group, ceteris paribus. Once controls are added, the effect estimated in model D is therefore in the opposite direction to the raw difference-in-difference estimate and is greater in magnitude. When compared with the previous models, this result suggests that the 12/13 converter academies had a greater influence on the progression of pupils between KS1 and KS2; this could arguably be due to the later converters having greater room for improvement. It is possible that some of the fast-tracked 'outstanding' rated early converters may have narrowly missed the 10/11 academic year and may have instead converted in the 11/12 academic years. This is less likely to be so in the 12/13 academic year since it is over a full academic year after the academy programme reform. 12/13 converters may therefore be less likely to be 'outstanding' schools and more likely to be schools rated 'good' by Ofsted. It could therefore be argued that the gains to be made by conversion are simply smaller for schools that are already performing well; once treated, the 12/13 converter academies therefore provided pupils with greater levels of progression than in schools that were soon to become converter academies.

The pooled model includes all individuals who completed primary school between the 11/12 and 13/14 academic years, whose school eventually became a converter academy. The results indicate that converter academies raise pupils' percentile APS by 1.4, relative to individuals who are not exposed to converter academies, ceteris paribus. Primary converter academies are therefore consistently found to have a positive and significant effect upon pupil progress between KS1 and KS2. Given the results of Table 8, it is unlikely that the difference in results between models is due to a differential average ability of the cohorts.

The full table of results is presented within the appendix of this paper in Table A1. All estimates are consistent with the expected effects upon pupil progress, given the existing literature on the determinants of pupil outcomes.

4.3 Deprivation Analysis

The effect of primary converter academies is also estimated by the neighbourhood deprivation of the school attended, based on the IMD decile. The results, which are provided in the appendix in Table A2, indicate that individuals from the least deprived neighbourhoods more consistently benefit from converter academies, with the exception of models C and D which identify a positive but insignificant influence of converter academy attendance. Converter academies improve the APS percentile rank of pupils in schools in the least deprived neighbourhoods by between 1.2 and 3.7 percentiles, ceteris paribus. Unlike the main analysis, the greatest effect of converter academies is identified in model A. Amongst the most deprived neighbourhoods, there is a mostly insignificant effect of converter academies on pupil performance; model B, however, presents a highly significant result, which relative to the least deprived, is a rather large effect; ceteris paribus, converter academies significantly increase the APS percentile score of pupils by 11.8 percentile points. Though the results are not presented, it should be noted that the results are robust to a change in the neighbourhood deprivation measure to the Income Deprivation Affecting Children Index (IDACI).

The results present an unclear picture of the effect of converter academies upon pupils when conditioning on the deprivation level of the school. Converter academies only have a positive influence on the treated from the deprived sample when the 11/12 converters are compared to the 13/14 converters, within the 11/12 cohort. Even when comparing the same year academy converters but varying the cohort observed in model C, an insignificant result is identified. Overall, the results suggest that although in one particular year and cohort, converter academies benefitted individuals attending schools within deprived areas to a greater extent, it is the pupils of converter academies in the least deprived areas that more consistently benefit from conversion.

4.4 Ethnicity and FSM status

In a similar manner to neighbourhood deprivation, the effect of primary converters is estimated by ethnicity and FSM status to identify whether academy conversion benefitted pupil progress differentially. FSM is a proxy for socio-economic disadvantage and provides a measure of pupil-level deprivation; this deprivation measure therefore varies from neighbourhood deprivation which is measured within the school's neighbourhood and consequently reflects neighbourhood and peer disadvantage. Whilst these two measures may be correlated, the analysis by pupil and neighbourhood deprivation is not based on the same sample of individuals.

The results which are provided in the appendix in Table A3 indicate that white children consistently benefit from primary converter academies, whilst non-white pupils are insignificantly impacted by converter academy attendance in all models. White children may therefore be driving the positive and significant results of the main analysis. Children eligible for FSM are identified as benefitting from primary academy attendance to a greater extent than non-FSM children; in model A, attending a converter academy leads to a movement up the cohort ranks by 5 percentiles for FSM pupils, relative to a 1.2 percentile improvement for non-FSM pupils. This conclusion is reached in four models as in models C and D, the effect of converter academies is insignificant for FSM pupils but consistently positive and significant for non-FSM pupils. Within the models in which a significant effect is identified, the progress of deprived pupils, as proxied by FSM status, is therefore improved to a greater extent.

These findings are surprising given the existing, albeit limited, literature on the effectiveness of schools by ethnicity and disadvantage which suggests that there is an insignificant differential impact of school effectiveness by ethnicity or socio-economic status (Strand 2010; Strand 2014). Given this evidence, it may be expected that all pupil outcomes would be improved uniformly by academy attendance. However, McNally (2015) identifies that increases in school resources, as we expect academy schools to experience, are more effective in improving the performance of disadvantaged pupils, thus potentially explaining the greater magnitude of the estimated effect on FSM pupils. Whilst there is little empirical evidence to suggest that the effectiveness of resources is also greater for white pupils, it could be argued that an increase in resources may close the gap between actual and potential performance which may be greater for white pupils. White pupils, on average, make worse progress than other ethnic minority groups (Vignoles and Meschi, 2010), experience lower parental involvement in schooling, are less likely to have high parental educational aspirations and are less likely to receive private tuition than pupils from other ethnic backgrounds[19](Stokes et al. 2015).

4.5 Robustness check

As a check of robustness, placebo estimations from the pre-treatment period are provided. The difference-in-differences model is estimated as in the main analysis, however, the robustness check will be performed based upon a falsified treatment effect using a sample of individuals who left school in the pre-treatment period[20]. The placebo test is a test of whether the impact of converter academies identified may be due to a pre-existing difference in trends between the treatment and control group, prior to treatment, that may be perceived as the treatment effect within the main analysis. If the results are robust, there should be an insignificant difference-in-differences estimate in the placebo model implying that there were no pre-existing differences in the trends of the treatment and control group prior to the actual year of treatment, while pupils could not be impacted by academy conversion, since leaving primary school before their school converted.

The robustness check will be based upon model D from the main analysis since many of the control variables are unavailable prior to the 06/07 academic year thus the 06/07 -10/11 cohort is the earliest cohort that may be included in the placebo estimation[21]. Model D presents a suitable model for the placebo test since treatment occurs in 12/13, two academic years after the placebo treatment. This placebo test therefore avoids any overlap between the falsified treatment and the actual treatment. Treated individuals attend primary schools that converted to academies in the 12/13 academic year while individuals in the control group attend 13/14 converter academies. In the placebo model, all individuals complete KS2 in 10/11[22] and therefore leave primary school before conversion; this contrasts with model D in which individuals complete KS2 in 12/13 (Table A4). Individuals within

[19] From Pakistani, Bangladeshi, Indian and mixed heritage backgrounds
[20] The observed treatment period in this paper is 11/12 – 13/14; the pre-treatment period is therefore conversion in the academic years prior to 11/12
[21] The placebo test could be carried out in a similar manner to models A-C; however, the year of academy conversion within these models is the 11/12 academic year. It is therefore possible that the placebo results may be susceptible to Ashenfelter's dip (Ashenfelter, 1978) which suggests that outcomes may fall directly before the treatment occurs.
[22] These individuals completed KS1 in 06/07

the treatment group in the placebo model therefore receive a falsified treatment. Results are provided in the appendix in Table A5.

The results from the main analysis are robust, since when a falsified treatment is entered into the model in place of an actual treatment, the results fail to identify a significant difference in the outcomes of pupils who experience academies relative to pupils who do not experience a converter academy. The results therefore signify that there are no significant pre-existing differences in the trends of the treatment and control groups that could explain the difference in outcomes that was attributed to academy converters in the main analysis.

5 Conclusions

This paper examines the impact of converter academies upon pupil outcomes at the primary school level to identify how being exposed to a converter academy impacts upon pupils' percentile rank within their cohort, according to their average point scores.

Data from the National Pupil Database is utilised from 2008-2014 which covers three main cohorts of pupils who completed primary school in the summer of 2012, 2013 or 2014. A difference-in-difference methodology is adopted in line with existing papers within the surrounding relevant literature (Eyles and Machin, 2015; Wilson, 2011; Böhlmark and Lindahl 2012), allowing for the evaluation problem to be overcome. The treatment group comprises of individuals who attended primary schools that convert to academies while in attendance, whilst pupils who completed KS2 before their primary school converted to become an academy form the control group. Relative to schools that never converted, eventual converters provide a more suitable control group since they are more characteristically similar to converter academies. The approach attempts to overcome the possible endogeneity issue by only observing individuals who had already enrolled in the primary school; the enrolment decision is therefore exogenous to the academy conversion.

The main analysis involved the estimation of several models which varied by the cohort observed or academy conversion year of the treatment or the control group. The models predominantly examine the impact of converter academies that converted in the 11/12 academic year though the impact of 12/13 converters is also examined and a pooled model analyses converters at any time between 2012 and 2014.

The results indicate a positive and significant impact of converter academies upon pupil progress; converter academies increase the percentile rank of pupils' average point scores by between 1.1 and 2.6 percentile points, ceteris paribus. A positive influence is consistently identified throughout the analysis, regardless of the year of conversion and cohort observed in each model. This finding mirrors that of Eyles and Machin (2015) who analyse secondary Labour academies. The results also correspond with studies from the school autonomy literature which finds a positive relationship between school autonomy and pupil outcomes (Gibbons et al. 2008; Abdulkadiroglu et al. 2011; Clark, 2009). These results are also found to be robust across specifications and to a placebo test which is performed based upon a falsified academy conversion treatment effect for individuals who completed primary school in 10/11 and therefore left prior to conversion. The placebo test found an insignificant impact of converter academies upon the APS percentile rank of pupils therefore signifying that there were no significant, pre-existing differences in the outcomes of the treatment and control group that could have been perceived as the effect of converter academies.

The findings identify 12/13 converters as providing a greater positive impact than 11/12 converters. This could potentially be due to the sample of 12/13 converters containing fewer of the fast-tracked ‘outstanding’ schools. This speculation does require future research, however, as this study is a pupil-level analysis which finds little evidence that the outcomes of pupils were worse in later cohorts.

The effect of primary converter academies is also estimated by neighbourhood deprivation to identify whether the pupils attending schools within the least deprived neighbourhoods benefit differentially to those in the most deprived areas. The results more consistently identify a positive impact of converter academies within the least deprived neighbourhood sample; a large positive and significant effect is also identified within the most deprived neighbourhoods but this finding is inconsistent across models. Schools in deprived neighbourhoods therefore do not seem to benefit from academy conversion in all periods. The effect of primary converter academies is also estimated by FSM status and ethnicity to identify whether these individual characteristics may explain differentially effects of converter academy attendance. Non-FSM pupils and white pupils are found to consistently benefit from academy attendance in all models. Though inconsistent across specifications, the effect of converter academies is found to be greater for FSM pupils; an insignificant effect is identified for non-white pupils in every model.

To my knowledge, this paper contains the first non-descriptive results from the investigation of the impact of post-2010 primary converter academies on pupil outcomes. The results overall suggest a positive role of the academies programme in improving the progress of primary school children. The change in policy made by the coalition government, allowing for all schools to become academies, therefore seems a positive transformation which has, at least in the case of the 11/12 and 12/13 converter academies, begun to assist in improving the future outcomes of pupils, by advancing their progress between KS1 and KS2.

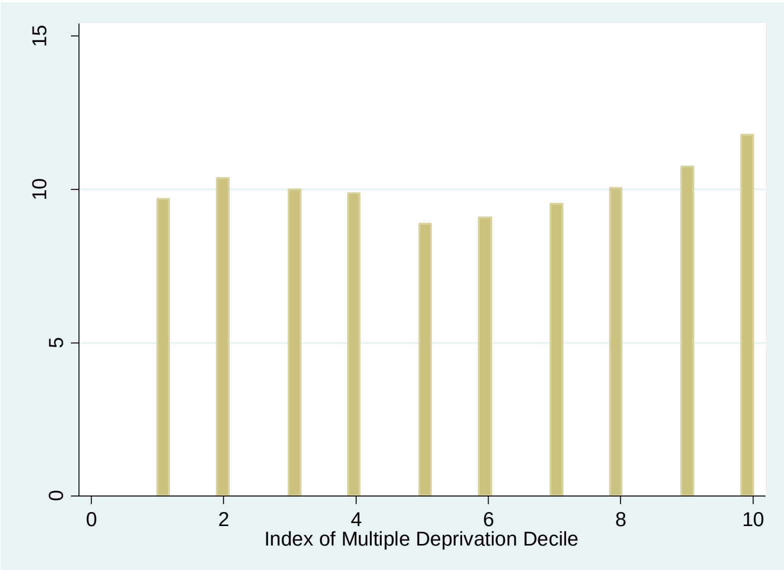
Future research should make use of additional years and cohorts of primary pupils; since at the time of analysis, the latest pupil outcomes available related to 2014, there was little scope to analyse the most recent converter academies. Due to the pronounced expansion of primary converter academies within England, it is imperative to identify the impact of this particular policy primarily since converter academies represent the greatest proportion of academy schools in England. However, sponsored academies continue to exist and continue to be opened thus the impact of these academies should also be examined.

Table 4 Sample size of each model of analysis

MODEL:	CONTROL (0)	TREATED (1)
A	22,767	17,482
B	19,307	17,482
12AB	42,075	17,482
C	19,688	17,417
D	19,688	22,845
Pooled	110,783	134,278

Sample sizes presented for the treatment and control group of each model to be estimated

Figure 4 IMD decile of eventual academy converters



Higher values denote a lower level of deprivation

Table 5 Mean of control variables

VARIABLE	TREATED	CONTROL	NON-ACADEMIES	P-VALUE (Treat & control)	P-VALUE (Treat & non-acad.)
EAL	0.123	0.107	0.162	0.000	0.000
White	0.819	0.843	0.790	0.000	0.000
FSM	0.244	0.236	0.272	0.000	0.000
SEN	0.196	0.207	0.234	0.000	0.000
Female	0.489	0.492	0.490	0.347	0.927
Month of birth	6.531	6.561	6.548	0.031	0.087
IMD decile	5.722	5.445	5.344	0.000	0.000
Conv. month in acad. yr.	3.188	0	0	0.000	0.000
Community school	0.325	0.562	0.625	0.000	0.000
Voluntary aided school	0.106	0.294	0.228	0.000	0.000
Voluntary controlled school	0.039	0.093	0.112	0.000	0.000
Foundation school	0.11	0.06	0.023	0.000	0.000

Sample means for treated, control and non-academies provided in initial three columns. P-values of the difference in means between treatment and control group and treatment and non-academies groups provided in the final two columns respectively.

Table 6 Raw before, after and difference-in-difference estimates by model

	BEFORE	AFTER	DIFFERENCE	DID	P-Value
Model A control	47.62	51.29	3.67		
Model A treated	50.55	54.72	4.17	0.5	0.049
Model B control	48.46	51.13	2.67		
Model B treated	50.55	54.72	4.17	1.5	0.000
Model 12AB control	48	51.22	3.22		
Model 12AB treated	50.55	54.72	4.17	0.95	0.000
Model C control	48.51	51.26	2.75		
Model C treated	51.11	55.15	4.04	1.29	0.000
Model D control	48.51	51.26	2.75		
Model D treated	47.43	51.35	3.92	1.17	0.000
Pooled model control	48.39	51.08	2.69		
Pooled model treated	48.97	52.47	3.5	0.81	0.000

Sample means are provided before and after treatment alongside the difference between these means. DID gives the difference in the before-after difference. between the treatment and control group with the p-values given for the DID.

Table 7 Mean pupil KS1 and KS2 scores by cohort and treatment status

COHORT		KS1APS	KS2APS
1 (11/12 leavers)	Control	15.39	29.04
	Treated	15.70	29.53
	All cohort	15.49	28.18
2 (12/13 leavers)	Control	15.58	29.15
	Treated	15.60	29.38
	All cohort	15.59	29.30
3 (13/14 leavers)	Control	15.52	29.38
	Treated	15.55	29.48
	All cohort	15.54	29.46

Mean Average Point Scores (APS) from Key stage 1 (KS1) at age 7 and Key Stage 2 (KS2) at age 11 for the treated, control groups and the full cohort of pupils including pupils attending non-academies.

Table 8 Summary of main results from difference-in-difference analysis for all models

	(1) MODEL A	(2) MODEL B	(3) MODEL 12AB	(4) MODEL C	(5) MODEL D	(6) POOLED MODEL
DID (Time * treat)	1.887^{***} (0.669)	2.085^{***} (0.741)	2.073^{***} (0.638)	1.077^{*} (0.563)	2.604^{***} (0.812)	1.431^{***} (0.251)
Time	3.735^{***} (0.167)	2.715^{***} (0.184)	3.265^{***} (0.124)	2.753^{***} (0.182)	2.744^{***} (0.182)	2.733^{***} (0.077)
Treat	0.616^{*} (0.329)	0.419 (0.348)	0.583^{**} (0.293)	0.375 (0.347)	-1.023^{***} (0.314)	0.894^{***} (0.145)
N	40,249	36,789	59,557	37,105	42,533	245,061

Robust standard errors in parentheses *** p<0.01, ** p<0.05, * p<0.1. Controls include: EAL, ethnicity, FSM, SEN, gender, school type, month of birth, school neighbourhood IMD decile, school open month. Pooled model also includes cohort controls.

References

Abdulkadiroglu, A. Angrist, J., Dynarski, S., Kane, T. & Pathak, P. (2011) Accountability and flexibility in public schools: evidence from Boston's charter and pilots. *Quarterly Journal of Economics* 126, 699-748

Allen, R. (2010) *Does school autonomy improve educational outcomes? Judging the performance of foundation secondary schools in England.* IOE DoQSS Working Paper No. 10-02.

Angrist, J., Dynarksi, S., Pathak, P. & Walters, C. (2010) Inputs and impacts in charter schools. KIPP Lynn. *American Economic Review* 100, 239-43

Bloom, N., Lemos, R. Sadun, R. & Van Reenan, J. (2015) Does management matter in schools? *The Economic Journal* 125(584):647-674

Böhlmark, A. & Lindahl, M. (2012) Independent Schools and Long-run Educational Outcomes: Evidence from Sweden's Large-scale Voucher Reform. IZA discussion paper 6683

Cameron, C. & Miller, D. (2015) A Practitioner's Guide to Cluster-Robust Inference. Journal of Human Resources. 50 (2) 317-372

Campbell, T. (2013) *In-school ability grouping and the month of birth effect: preliminary evidence from the Millennium Cohort Study*. Centre for Longitudinal Studies (CLS) working paper 2013/1

Cirin, R. (2014) *Do academies make use of their autonomy?* DfE research report 366

Clark, D. (2009) The performance and competitive effects of school autonomy. *Journal of Political Economy* 117: 745-783

Crawford, C. Dearden, L. & Meghir, C. (2010) *When you are born matters: the impact of date of birth on educational outcomes in England*. Institute for fiscal studies working paper w10/06

Crawford, C. & Vignoles, A. (2010) An analysis of the educational progress of children with special educational needs. DoQSS Working Paper No. 10-19, London: Institute of Education.

Department for Education (2011) *Academies Annual Report. Academic Year 2011/2012.* Department of Education

Department for Education (2012). Attainment at Key Stage 4 by Pupils in Academies 2011 (DfE Research Report 223)

Department for Education (2014a) Performance of converter academies: an analysis of inspection outcomes 2012 to 2013. Research report

Department for Education (2014b) Phonics screening check and national curriculum assessments at key stage 1 in England, 2014.

Department for Education (2015) Ethnicity, deprivation and educational achievement at age 16 in England: trends over time.

Department for Education (2016) Next steps to spread educational excellence everywhere announced. [Press release]. [Accessed 25 May 2016]. Available from https://www.gov.uk/government/news/next-steps-to-spread-educational-excellence-everywhere-announced

Dustmann, C., S. Machin and U. Schonberg (2010) ‘Ethnicity and Educational Achievement in Compulsory Schooling’ *Economic Journal*, 120 272-297.

Eyles, A. & Machin, S. (2015) The introduction of Academy schools to England’s education. IZA discussion paper no.9276

Eyles, A., Machin, S. & Silva, O. (2015) Academies 2: The new Batch, CEP mimeo

Gibbons, S. & Silva, O. (2011) Faith primary schools. Better schools or better pupils? *Journal of Labor Economics* 29(3), 589-635

Gibbons, S., Machin, S. & Silva, O. (2008) Choice, competition and pupil achievement. *Journal of the Europena Economic Association* 6(4): 912-947

Gibbons, S., Machin, S., & Silva, O. (2013) Valuing school quality using boundary discontinuities. *Journal of Urban Economics* 75:15-28

House of Commons (2015) Academies and free schools: Fourth Report of Session 2014–15.

Hoxby, S., & Muraka, S. (2009) *Charter schools in New York Cit: who enrols and how they affect student achievement.* National Bureau of Economic research working paper 14852

Hutchings, M., Francis, B. & De Vries., R. (2014*). Chain Effects: The Impact of Academy Chains on Low Income Students.* Retrieved 02/2016 from http://www.suttontrust.com/wp-content/uploads/2014/08/chain-effects-july-14-final-1.pdf

Kramarz, F., Machin, S. & Ouazas, A. (2008) What Makes a Test Score? The Respective Contributions of Pupils, Schools, and Peers in Achievement in English Primary Education. IZA discussion paper No. 3866

Lindahl, L. (2011) A comparison of family and neighbourhood effects on grades, test scores, educational attainment and income- evidence from Sweden. *The Journal of Economic Inequality 9: 207-226*

Machin, S. & McNally, S. (2008) The literacy hour. *Journal of Public Economics*. 92: 1441-1462

Machin, S. & Silva, O. (2013) *School structure, school autonomy and the tail.* Centre for Economic Performance special paper no. 29.

McNally, S. (2015) *Schools: the evidence on academies, resources and pupil performance*. Centre for Economic Performance paper EA203

National Audit Office (2010) *The academies programme*. London.

Nicoletti, C. and Rabe, B. (2010) *Inequality in pupil's educational attainment: how much do family, sibling type and neighbourhood matter*? ISER Working Paper Series: 2010-26

Sammons, P., Sylva, K., Melhuish, E., Siraj-Blatchford,I. Taggart, B., Barreau, S. & Grabbe, Y. (2007) *Influences on Children's Development and Progress in Key Stage 2:Social/behavioural outcomes in Year 5*. Department for Children, Schools and Families. Research Report DCSF-RR007

Strand, S. (2010) Do some schools narrow the gap? Differential school effectiveness by ethnicity, gender, poverty, and prior achievement. *School effectiveness and school improvement.* 21(3):289-314

Strand, S. (2014) School effects and ethnic, gender and socio-economic gaps in educational achievement at age 11. *Oxford review of education* 40(2):223-245

Stokes, L., Rolfe, H., Hudson-Sharp, N., Stevens, A. (2015) *A compendium of evidence on ethnic minority resilience to the effects of deprivation on attainment.* Department for Education

Vignoles, A. & Meschi, E. (2010) *The Determinants of Non-Cognitive and Cognitive Schooling Outcomes*. Centre for the Economics of Education

Wilson, J. (2011). *Are England's Academies More Inclusive or More 'Exclusive'? The Impact of Institutional Change on the Pupil Profile of Schools*. CEE Discussion Paper 125

Wilson, D., Burgess, S., & Briggs A. (2011). The dynamics of school attainment of England's ethnic minorities. *Journal of Population Economics,* 24, 681–700.

Wilson, D. & Piebalga, A. (2008) *Accurate performance measure but meaningless ranking exercise? An analysis of the English school league tables.* Centre for market and public organisation working paper 07/176

Worth, J. (2014) Analysis of academy school performance in GCSEs 2014. NFER. Final report

Appendix

Table A1 Full model results with all controls

	(1) Model A	(2) Model B	(3) Model 12AB	(4) Model C	(5) Model D	(6) Pooled model
DID	**1.887***** **(0.669)**	**2.085***** **(0.741)**	**2.073***** **(0.638)**	**1.077*** **(0.563)**	**2.604***** **(0.812)**	**1.431***** **(0.251)**
Time	**3.735***** **(0.167)**	**2.715***** **(0.184)**	**3.265***** **(0.124)**	**2.753***** **(0.182)**	**2.744***** **(0.182)**	**2.733***** **(0.077)**
Treat	**0.616*** **(0.329)**	**0.419** **(0.348)**	**0.583**** **(0.293)**	**0.375** **(0.347)**	**-1.023***** **(0.314)**	**0.894***** **(0.145)**
EAL	-3.073*** (0.746)	-2.190*** (0.848)	-2.650*** (0.632)	-2.936*** (0.792)	-3.557*** (0.705)	-2.667*** (0.305)
White	-0.387 (0.627)	-1.561** (0.708)	-0.749 (0.535)	-2.713*** (0.671)	-2.089*** (0.615)	-1.581*** (0.258)
FSM	-8.726*** (0.377)	-8.742*** (0.402)	-8.664*** (0.306)	-8.127*** (0.395)	-8.660*** (0.351)	-8.513*** (0.152)
SEN	-32.649*** (0.347)	-32.256*** (0.367)	-32.305*** (0.285)	-32.779*** (0.378)	-31.902*** (0.345)	-31.996*** (0.145)
Female	0.186 (0.302)	0.758** (0.316)	0.505** (0.248)	1.092*** (0.318)	0.982*** (0.297)	0.903*** (0.124)
Community	0.679 (0.537)	0.089 (0.638)	0.611 (0.484)	0.611 (0.453)	1.185 (0.730)	0.020 (0.214)
Voluntary aided	2.689*** (0.607)	1.688** (0.699)	2.075*** (0.532)	1.525*** (0.535)	1.638** (0.773)	2.208*** (0.240)
Voluntary controlled	2.863*** (0.752)	0.267 (0.848)	1.985*** (0.641)	0.195 (0.724)	2.132** (0.885)	0.481 (0.303)
MOB Feb	-0.605 (0.753)	-0.865 (0.786)	-0.612 (0.618)	-1.321* (0.787)	-1.062 (0.737)	-1.235*** (0.311)
MOB March	-0.501 (0.727)	-2.321*** (0.767)	-1.409** (0.604)	-2.963*** (0.769)	-2.074*** (0.725)	-1.872*** (0.303)
MOB April	-2.507*** (0.733)	-2.393*** (0.773)	-2.663*** (0.602)	-3.144*** (0.778)	-3.095*** (0.728)	-2.896*** (0.304)
MOB May	-2.632*** (0.727)	-2.574*** (0.754)	-2.882*** (0.592)	-3.684*** (0.766)	-3.785*** (0.710)	-3.801*** (0.299)
MOB June	-4.192*** (0.725)	-4.407*** (0.769)	-4.058*** (0.598)	-5.524*** (0.770)	-4.847*** (0.718)	-4.919*** (0.301)
MOB July	-4.767*** (0.737)	-5.213*** (0.769)	-4.857*** (0.605)	-5.232*** (0.768)	-5.655*** (0.720)	-5.449*** (0.298)
MOB August	-5.539*** (0.722)	-5.846*** (0.750)	-5.710*** (0.591)	-7.239*** (0.755)	-8.079*** (0.713)	-6.681*** (0.297)
MOB September	5.463*** (0.723)	4.506*** (0.753)	5.148*** (0.595)	3.716*** (0.761)	3.567*** (0.716)	4.770*** (0.300)
MOB October	3.552*** (0.740)	3.260*** (0.775)	3.326*** (0.608)	3.176*** (0.767)	2.744*** (0.726)	3.181*** (0.302)
MOB November	2.242*** (0.728)	2.129*** (0.774)	2.296*** (0.602)	0.399 (0.772)	1.719** (0.719)	2.304*** (0.307)
MOB December	1.534** (0.736)	1.551** (0.776)	1.787*** (0.606)	0.808 (0.792)	1.283* (0.734)	1.432*** (0.307)
IMD 2	1.595** (0.726)	-0.294 (0.746)	0.906 (0.553)	-2.209*** (0.729)	-0.391 (0.596)	1.402*** (0.271)
IMD 3	-0.928 (0.728)	-0.679 (0.746)	-0.552 (0.552)	-0.600 (0.748)	0.579 (0.592)	1.377*** (0.278)
IMD 4	0.888 (0.737)	1.769** (0.754)	0.773 (0.569)	0.441 (0.738)	-1.687*** (0.632)	0.607** (0.278)

IMD 5	0.963	-0.029	0.625	-0.709	0.801	1.950***
	(0.721)	(0.821)	(0.577)	(0.810)	(0.665)	(0.289)
IMD 6	2.532***	3.822***	2.829***	1.221	1.607**	3.006***
	(0.749)	(0.766)	(0.580)	(0.743)	(0.664)	(0.291)
IMD 7	2.181***	2.287***	1.815***	2.102***	1.896***	3.387***
	(0.772)	(0.775)	(0.594)	(0.775)	(0.671)	(0.289)
IMD 8	3.991***	4.054***	4.185***	3.421***	3.886***	4.673***
	(0.788)	(0.731)	(0.577)	(0.731)	(0.644)	(0.285)
IMD 9	5.375***	3.445***	4.379***	3.655***	3.126***	5.230***
	(0.741)	(0.741)	(0.574)	(0.724)	(0.677)	(0.283)
IMD 10	6.135***	5.750***	6.043***	4.522***	5.771***	6.769***
	(0.715)	(0.737)	(0.557)	(0.726)	(0.641)	(0.278)
Conv. month in acad. yr.	-0.040	-0.028	-0.032	0.094	-0.023	-0.027
	(0.062)	(0.062)	(0.062)	(0.062)	(0.059)	(0.024)
Cohort 2						-0.759***
						(0.157)
Cohort 3						-1.907***
						(0.167)
Constant	54.946***	57.159***	55.619***	59.264***	57.781***	56.504***
	(1.110)	(1.205)	(0.919)	(1.087)	(1.147)	(0.449)
N	40,249	36,789	59,557	37,105	42,533	245,061
Adjusted R^2	0.320	0.312	0.315	0.301	0.300	0.299

Robust standard errors in parentheses *** p<0.01, ** p<0.05, * p<0.1
Base categories: Non-EAL, Non-white, non-FSM, non-SEN, male, institution type: foundation primary, January month of birth, most deprived neighbourhood decile, never converted (month into=0), 11/12 cohort (cohort 1).

Table A2 Summary of results from difference-in-difference analysis by school neighbourhood deprivation according to IMD.

TREAT:	30% LEAST DEPRIVED	N	30% MOST DEPRIVED	N
A	3.747***	13,658	1.454	10,703
	(1.306)		(1.700)	
B	2.022*	14,067	11.844***	9,583
	(1.051)		(2.954)	
12AB	2.761***	19,985	2.605	17,636
	(0.971)		(1.641)	
C	0.546	14,108	1.424	9,782
	(0.923)		(1.418)	
D	2.008	12,429	-2.136	15,079
	(1.393)		(1.515)	
Pooled	1.163***	79,843	-0.312	73,673
	(0.437)		(0.490)	

Robust standard errors in parentheses *** p<0.01, ** p<0.05, * p<0.1. The time*treat coefficient is provided only Controls include: EAL, ethnicity, FSM, SEN, gender, school type, month of birth, school open month. Pooled model also includes cohort controls

Table A3 Summary of results from difference-in-difference analysis by ethnicity and FSM

	A	B	12AB	C	D	POOLED
WHITE	2.344***	1.924**	2.388***	1.072*	2.206**	0.265***
	(0.722)	(0.783)	(0.688)	(0.605)	(0.920)	(0.038)
N	33,491	31,310	49,762	31,161	34,818	203,477
NON-WHITE	0.511	2.869	0.279	0.159	0.637	0.004
	(1.779)	(2.285)	(1.712)	(1.519)	(1.742)	(0.084)
N	6758	5479	9795	5944	7715	41,584
FSM	4.980***	4.935**	5.366***	-0.539	0.988	0.187**
	(1.594)	(2.008)	(1.490)	(1.221)	(1.633)	(0.077)
N	8792	8118	13,790	8486	11,410	58,669
NON-FSM	1.245*	1.514*	1.322*	1.448**	3.034***	0.293***
	(0.740)	(0.805)	(0.709)	(0.634)	(0.932)	(0.039)
N	31,457	28,671	45,767	28,619	31,123	186,392

Robust standard errors in parentheses *** p<0.01, ** p<0.05, * p<0.1. The time*treat coefficient is provided only Controls include: EAL, SEN, gender, school type, month of birth, school open month in both models. FSM is controlled for within the ethnicity model only; ethnicity is controlled for within the FSM model. The pooled model also includes cohort controls

Table A4 Placebo test sample summary

MODEL	BECAME ACADEMY		YEAR LEFT PRIMARY
	TREATMENT	CONTROL	
D	12/13	13/14	12/13
PLACEBO	12/13	13/14	10/11

Table A5 Placebo test results summary

VARIABLES	PLACEBO
DID (Time * treat)	0.366
	(0.140)
Time	2.769***
	(0.184)
Treat	0.234
	(0.309)
N	41,232

Robust standard errors in parentheses *** p<0.01, ** p<0.05, * p<0.1**.** Controls include: EAL, ethnicity, FSM, SEN, gender, school type, month of birth, school neighbourhood IMD decile, school open month. Pooled model also includes cohort controls