**Andrew Eyles, Stephen Machin and Sandra McNally**

# Unexpected school reform: academisation of primary schools in England

## Article (Accepted version) (Refereed)


# Unexpected school reform: Academisation of primary schools in England

Andrew Eyles, Stephen Machin, Sandra McNally

PII: S0047-2727(17)30147-0
DOI: doi: 10.1016/j.jpubeco.2017.09.004
Reference: PUBEC 3809

To appear in: *Journal of Public Economics*

Received date: 3 November 2016
Revised date: 6 September 2017
Accepted date: 7 September 2017

Please cite this article as: Andrew Eyles, Stephen Machin, Sandra McNally , Unexpected school reform: Academisation of primary schools in England, *Journal of Public Economics* (2017), doi: 10.1016/j.jpubeco.2017.09.004

# Unexpected School Reform: Academisation of Primary Schools in England

**Andrew Eyles*, Stephen Machin** and Sandra McNally*****

September 2017 – 2nd Revision

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

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

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

Abstract

The change of government in 2010 provoked a large structural change in the English education landscape. Unexpectedly, the new government offered primary schools the chance to have 'the freedom and the power to take control of their own destiny', with better performing schools given a green light to fast track convert to become an academy school. In England, schools that become academies have more freedom over many ways in which they operate, including curriculum design, budgets, staffing issues and the shape of the academic year. However, the change to allow primary school academisation has been controversial. This paper reports estimates of the causal effect of academy enrolment on primary school pupils. While the international literature provides growing evidence on the effect of school autonomy in a variety of contexts, little is known about the effect of autonomy on primary schools (which are typically much smaller than secondary schools) and in contexts where the converting school is not deemed to be failing or disadvantaged. The key findings are that English primary schools did change their mode of operation after the exogenous policy change, utilising more autonomy and changing spending behaviour, but this did not lead to improved pupil performance.

Acknowledgements

We would like to thank Jonah Rockoff, two referees and the following for helpful comments in seminars and conferences: Gabriel Heller-Sahlgren, Victor Lavy, Olmo Silva and participants at the IFN Stockholm Conference of June 2016, the MILLS Workshop in the Economics of Education in Bocconi University of June 2016 and the CESifo Economics of Education Network Conference of September 2016.

## 1. Introduction

Since 2010, the educational landscape in England has radically altered. By 2017, nearly two-thirds of secondary schools and over a fifth of primary schools are academies. Academy schools are granted considerable operational autonomy by government and have a battery of freedoms they can use that standard state schools cannot. As Michael Gove, the Minister then responsible for education, put it by enabling academisation these schools have been 'given the freedom and the power to take control of their own destiny'.[1]

Although academies were present before then – principally as a school improvement policy for underperforming secondary schools since 2002 - the programme was radically altered and significantly expanded following the election of the new UK government in May 2010. It became a school structure to which all schools were invited to aspire as enabling legislation – the Academies Act of 2010 - was rapidly put in place two months after the election of the new government.[2] For the first time, and through this completely unexpected policy change[3], primary schools were invited to become academies, with better performing schools being given priority to convert. The first batch of such schools converted in the school year beginning in September 2010. This paper reports estimates of the impact of primary school conversion to academy status on their operation and on the performance of enrolled pupils.

This introduction of primary academies took place in an international context where publicly-funded, autonomous schools have become a familiar form of school improvement

[1] Department for Education (2013). Forward by Michael Gove MP.

[2] Most new academies since 2010 are 'converters'. However, some academies are sponsored (i.e. managed by a private team of independent co-sponsors) and these are schools that have been underperforming. The effect of academisation on these schools (which are closer to the original New Labour academies, studied by Eyles and Machin, 2015, and comprise about 30 percent of primary academies) is not considered in this paper, because we want to explore the unexpected dimension of academisation that applied to converters, and especially those rated outstanding prior to the 2010 change in policy.

[3] The introduction of 'Free schools' and education reform were issues raised in the manifesto of the new government prior to their election; however, there was no mention of large-scale expansion of the academies programme. Free schools are completely new schools that can be set up by interested parties (e.g. parents, or community members). By 2016/17, there were 139 free primary schools open or approved.

policy, most notably through charter schools in the US and free schools in Sweden. Research on the former tends to find achievement gains associated with charter status and with the 'injection' of charter school features to public schools, particularly in urban settings where the schools typically enrol disadvantaged students.[4] In the Swedish context, there is some evidence of positive short and long term effects of the free school program, but these are found to work primarily through competition (see Bolhmark and Lindahl, 2015).

The policy studied here differs from most others in the literature in three important respects. Firstly, it involves conversion of existing schools rather than the creation of new schools.[5] Secondly, it is about the voluntary conversion of better performing schools and not the forced conversion of failing schools. These better performing schools very clearly have a lower proportion of children from disadvantaged backgrounds. Thirdly, the focus is on young children (aged 7-11) who attend primary schools, which are much smaller than secondary schools.[6] Although there have been studies of elementary schools in the charter school context, these are less prevalent than studies of middle and high schools. Similarly, studies of autonomy in the context of the English education system have focused on particular subsets of secondary schools; specifically, advantaged secondary schools voluntarily gaining greater autonomy (Clark, 2009), disadvantaged secondary schools (Eyles and Machin, 2015), and

[4] Epple, Romano and Zimmer (2016) provide an overview of the literature. While something of a consensus has emerged, there is also some controversy within the charter school research. Recent experimental studies of charters in or near particular US cities (Boston and New York) find positive impacts on educational achievement (see Abdulkadiroglu et al. 2011, 2014; Angrist et al. 2010, 2013; Dobbie and Fryer 2011, 2013; Hoxby and Murarka 2009). Wider coverage evaluations have produced more mixed results (Betts et al. 2006; Center for Research on Education Outcomes, 2009, 2013; Gleason et al. 2010). Similarly, there is no consensus on the longer term effects of charters. Angrist et al. (2016) and Dobbie and Fryer (2014) fine that charter attendance improves longer run outcomes such as college attendance. In later work, Dobbie and Fryer (2016) find negative earnings returns for those attending charters that are ineffective at raising test scores and no returns for charters that are successful at raising test scores.

[5] While the majority of school autonomy studies focus on newly set up autonomous schools (e.g. the majority of US charters are new schools), there are some examples of studies where existing schools become more autonomous. Clark (2009) and Eyles and Machin (2015) study English secondary schools gaining more autonomy, while Abdulkadiroglu et al. (2016) study the conversion of traditional public schools in New Orleans to (in-district) charters. Alongside these Steinberg (2014) studies the granting of greater operational freedom to a subset of principals in already existing Chicago Public Schools.

[6] While the majority of charter papers focus on middle and high schools, some papers do include results for elementary schools (Dobbie and Fryer 2011, 2013; and Hoxby et al. 2009).

secondary schools in relatively disadvantaged local authorities (e.g. Birmingham in the case of Bertoni et al., 2017).

Upon conversion, academy schools gain autonomy over many process and personnel decisions. This greater freedom may have positive effects on student outcomes because of superior information held by local decision makers (Hanushek and Woessmann, 2011). Indeed, the first secondary schools in England to become academies (in the early 2000s) did seem to deliver positive effects on student outcomes (Eyles and Machin, 2015; and Eyles at al. 2016a, 2016b). However, the context was one in which a couple of hundred (previously significantly underperforming) secondary schools became academies. It is not necessarily the case that these positive effects carry through to better performing schools and/or to (much smaller) primary schools.

If the autonomy offered within the academies model was unambiguously advantageous for schools, one would imagine that all schools would want to become academies. However, recently the UK government has had to back out of a policy to force all schools in England to become academies by the end of 2022 because of fierce hostility to this by the educational establishment (although the current government vision is still to encourage all schools to become academies).

Whether such radical upheaval is in the interests of students is an empirical question. Most schools yet to convert are primary schools, which represent the vast majority of schools in England. One might hypothesise that schools which volunteered to convert to academy status early-on are those that were most amenable to academy status, anticipating positive benefits. If effects are not found for such schools, one might question whether it is such a good idea to extend it to schools that are less enthusiastic.

An important feature of the policy being studied here is that it was in no way anticipated by schools or parents. This gives leverage to identify causal effects since the

conversion was exogenous to pupils already enrolled in the school. Thus, the sample studied is restricted to these "legacy enrolled" pupils who can be observed before and after academisation takes place. The importance of estimating effects for pupils who were already enrolled in the school prior to conversion emerges because student mobility post-conversion is potentially endogenous to the policy itself. For example, parents may be attracted by the idea of academy status and be more likely to enrol their students to newly converted primary schools. Exit from the school post-conversion might also be non-random (for example, if schools change policies in a way that is less attractive to certain students or their parents). However, in the empirical work discussed below, a very strong first stage estimate (of the effect of pre-conversion enrolment on the probability of attending an academy) suggests that a causal effect of academy attendance is identified for the majority of eligible pupils in the school.

In practical terms, the empirical strategy adopted in this paper first involves selection of treatment and control groups of schools. The treatment group consists of primary schools that converted to academy status between 2010/11 and 2014/15. In each case, the control groups are those that converted in later academic years, but before 2016/17. Under certain conditions, these treatment and control schools are shown to have similar pre-trends in outcome variables. Further, enrolment in the primary school prior to conversion is used as an instrument for actual attendance in the academy in grade 6 when national tests in reading and maths take place. The legacy enrolment strategy mirrors that used in Eyles and Machin (2015) in their study of the first underperforming English secondary schools to become academies in the early 2000s. It also draws on Fryer (2014) who looks at the effect of injecting charter school practices into traditional public schools and Abdulkadiroglu et al. (2016), who study school takeovers in New Orleans, referring to pupils who stay in converting schools as 'grand-fathered' pupils

The rest of the paper is structured as follows. Section 2 describes primary education in England and offers a discussion of the institutional features characterising the introduction of academy schools. Section 3 describes the data and research strategy. Section 4 reports results from the first part of the empirical analysis, looking at whether primary schools that became academies did in fact change their mode of operation upon conversion. Section 5 report the legacy enrolment results looking at causal effects of academy conversion on pupil performance. Conclusions are given in Section 6.

## 2. Primary Education in England and Academy Schools

*Primary Education*

In England, children start school in the September after they reach the age of 4. Most children attend a primary school up to age 11, after which they go to secondary school.[7] Schooling in England is organised into Key Stages. At the end of Key Stage 1 (age 7), pupils are assessed by their teachers in English and maths according to national guidelines. At the end of Key Stage 2 (age 11), they undertake national tests in English and maths.[8] These tests are used to construct Performance Tables for primary schools, which are publicly available. There is next to no grade repetition within the system.

Up until the introduction of academies in 2010, schooling had been organised at the local level into Local Education Authorities (LEAs). There are 152 LEAs in England and around 15,000 primary schools. The LEA main functions in relation to primary schools are in building and maintaining schools, providing support services (e.g. for children with special needs), and acting in an advisory role to the head teacher regarding school performance and implementation of government initiatives.

[7] There is a small number of infant schools and middle schools in parts of the country. They are not included in this analysis unless they are 'linked', meaning that students at an infant school are prioritised for places at the junior school; in these cases, the proportion of infant school attendees switching to the linked junior school is very high and the two linked schools are treated as though they were one single school.

[8] Prior to 2010, students were also assessed in science.

LEAs also have an important role in the funding allocations of schools. The bulk of schools funding comes from the dedicated schools grant which is given to LEAs and then distributed according to the each LEAs own funding formula. The funding allocated to the LEA is based on a historically determined formula which is mainly driven by the numbers of pupils, 'additional educational needs' and local conditions. These local conditions include population sparsity, measures of deprivation, and wage costs in the area (Roberts and Bolton, 2017).

As well as allocating funding, the LEA also appoints one or two representatives on to a school's governing body – a group of parents, teachers and community representatives that provides governance to the school. LEAs typically offer a number of administrative and management functions including training, personnel and financial services. Up until the 2010/11 school year, the majority of primary aged pupils (67%) attended community schools in which LEAs are the statutory employer of school staff, owner of the buildings and the authority that manages student admissions.[9] Most other state primary schools are faith schools (which have greater autonomy from the LEA). Although parents can apply to send their child to any primary school (i.e. there are no strict catchment areas), popular schools are often oversubscribed and places are rationed according to a Schools Admissions Code.[10]

*Academy Schools*

When a school becomes an academy, it is governed outside the LEA and is overseen and funded directly by central government. An academy school is run in many ways like a company, where governors are classed as trustees or directors and the principal/head teacher is the chief executive. Strong financial management and governance at the level of the

[9] For more information about the operation of primary schools and local government prior to 2010, see Gibbons et al. (2011).

[10] The School Admissions code applies to all state-maintained schools and academies. In practice, schools have very little scope to employ differing admission criteria (all schools aside from community schools and a limited amount of religious schools, where the LEA determines the criteria, set their own). All schools have to accept applications unless they are oversubscribed. In the case of oversubscription the criteria that schools can use is limited (distance, presence of sibling at school) and the adopted criteria tends to vary little across schools.

individual academy are very important (National Audit Office, 2012), especially given that oversight is no longer provided by the LEA. Unlike Community Schools (i.e. most state primary schools), academies manage their own admissions. While they still have to adhere to the Schools Admissions Code, they may choose to run their admissions policy differently than in the past. Although academies are required to teach a broad and balanced curriculum, including English, maths, science and religious education, they are not legally required to use the national curriculum. They have the ability to set their own pay and conditions for staff and more freedom in their hiring decisions (e.g. they may hire unqualified teachers).[11] Although academies are supposed to be funded on an equal basis with non-academies, they do get extra funds to cover the services that the LEA provides freely to other state maintained schools[12]; therefore, they have greater freedom on how to use the budget allocation. They also have the responsibility of organising payroll functions, insurance and accountancy functions in-house or by contracting this out. Academies also have the ability to change the length of the school day and the shape of the academic year (through term times).

In the interests of minimising risk following the Academies Act of 2010, the Department of Education adopted a phased approach to the criteria for schools wishing to convert (National Audit Office, 2012), prioritising and buying a green light in the conversion process to better performing schools. A key component of this prioritising decision featured the rating in the reports of the Schools Inspectorate (Ofsted) that visits schools every 3-5 years and rates schools on a four point scale ranging from 'outstanding' to 'unsatisfactory'. At the time, about 20% of schools were rated as 'outstanding' and 50% as 'good'.

The coalition government initially prioritised schools rated as outstanding and fast-tracked their applications for conversion. The first such schools were converted to academy status in September 2010. In November of the same year, this fast track route was extended to

[11] Those with qualified teacher status typically have an undergraduate degree and have completed a one year postgraduate teacher training course.
[12] Schools are given a £25,000 grant to support the conversion process.

all good schools with outstanding features. At the same time, recognising the potential for economies of scale, academies were also encouraged to convert in chains or undertake some post-conversion collaborative arrangement with other schools. This option was made available for any school (irrespective of Ofsted grade) if it joined an academy trust with an outstanding school or an education partner with a strong record of improvement. In April 2011, the criteria was further widened to include schools that were 'performing well', which included consideration of the last three years' exam results, the latest Ofsted inspections, and financial management.

As shown in Figure 1, the initial take-up rate for primary academies in the first possible academic year (2010/11) was modest. This is unsurprising given the unexpected nature of the announcement, with legislation being rapidly passed by receiving royal assent in June 2010 and the fact that schools are likely to take time before making the decision to take on extra responsibilities (especially given the small size of primary schools in England). However, after that, there was a huge rise in the number of primary school academies in England between 2010/11 and 2016/17, with nearly a quarter of the sector being academy schools by 2016/17.

Following the way in which numbers were constructed for Figure 1, schools are said to convert in a given academic school year (September to August) if they are running as an academy by December of that academic year. Thus, for example, a school is classed as converting to academy status in 2014/15 if it converts at some point during the 2014 calendar year.

The number of schools in the sample of converter academies studied in this paper, by year of academy conversion, is given in Table 1. There are a number of reasons for there being some discrepancy in numbers between Figure 1 and Table 1. Firstly, because of the research design that is adopted, schools are only included in the sample if they have students

enrolled in grades 2 and 6 in each academic year between 2006/07 and 2014/15. Secondly, the analysis focusses only on academies that voluntarily convert to academy status (around 30 percent of primary academies are sponsored academies that typically convert to academies as a result of government intervention). Thirdly, schools that participated in the KS2 strike of 2009/10, and who therefore have missing outcome data in that year, are excluded.

One further institutional detail of interest is that, in the post-May 2010 phase of academisation, schools have also been encouraged to convert in a chain or partnership. The Department for Education has stated 'this can enable schools to support one another once they are academies, share resources, experience and ideas. Such an approach is particularly valuable to small primary schools where working together allows economies of scale to be achieved' (Department for Education, 2013). The most prevalent model of collaboration is the multi-academy trust (MAT) wherein all schools within the MAT are governed by one trust and board of directors. MATs perform a role similar to that which would otherwise be played by the LEA in that they hire/fire teachers and are responsible for negotiating every aspect of teacher contracts - the disciplinary process and redundancy pay amongst other things - with the exception of pensions. MATs can also substitute for local educational authorities (LEAs) in that they top-slice funds allocated to schools under their trust and use this to supply central services previously provided by the LEA. In 2016/17, about 80% of primary academies were in a multi-academy trust. In the sample studied here, there are slightly fewer schools in MATS, with around 70% operating under this organisational structure.

## 3. Data and Methodology

*Data*

The National Pupil Database (NPD) is a census of all pupils in the state system in England. NPD includes basic demographic details of pupils – such as ethnicity, free school meal

eligibility (FSM), gender, and whether or not English is their first language. The school attended by pupils can be linked to other school-level information such as the date of conversion to an academy school and the date and grade of Ofsted inspections (which are publicly available data). The data is longitudinal and tracks students as they progress through the state school system.

As discussed in Section 2, the national curriculum in England is organised around Key Stage', the first two undertaken in primary school (in grades 1 to 6) and the second two in secondary school (in grades 7 to 11). Head teachers have a statutory duty to ensure that their teachers comply with all aspects of the Key Stage assessment and reporting arrangements. During primary school, this corresponds to Key Stage 1 and 2 which respectively cover grades 1-2 and 3-6. Local Authorities (and other recognised bodies) are responsible for moderation of schools. Thus, although teachers make their own assessments of students (and therefore could be susceptible to potential bias), there is a process in place to ensure that there is a meaningful assessment that is standardised over all of England. At the end of grade 2 in Key Stage 1, students are given a 'level' (i.e. there is no test score as such). However, following standard practice, National Curriculum levels achieved in Key Stage 1 assessments are transformed into point scores using Department for Education point scales and these scores are used in the empirical work reported on below.[13]

At the end of primary school in grade 6 (or the end of the Key Stage 2 phase of education), pupils take national tests in reading and maths, which are externally set and marked on a scale of 1-100. The final dataset used in this paper contains of multiple cross sections of grade 6 pupils linked to their school, demographic information, and test scores for the academic year 2006/07 to 2014/15. Test scores – both baseline KS1 and the outcome KS2 - are standardised, within the sample, at the grade/year/subject level.

[13] The point score can take on 7 values ranging from 3-27. The main results, given in Table 8, are unchanged when KS1 is removed as a control.

Other data sources are utilised in parts of the empirical analysis. The School Workforce Census is school level data that is available from the 2010/11 school year and provides a snapshot of each maintained school's workforce composition. Also studied is publicly available information on the income and expenditure of maintained schools and academies that is available from the 2009/10 school year. Finally, some results from a survey conducted by the Department for Education regarding the use of academy freedoms are presented (the source of these survey data is Cirin, 2014). This survey, which covers 25% of the 2919 academies that had opened by 1st May 2013, and pertains to the freedoms exercised by schools once they gain academy status.

*Methodology*

The main research question of interest is to identify the effect of academy conversion on pupil achievement in the grade 6 national Key Stage 2 tests taken by pupils at the end of primary school. Administrative data that follows pupils through their school careers is used to estimate the impact of academy enrolment on Key Stage 2 performance. In order to study this, a research design where instrumental variables are combined with difference-in-differences is implemented. In this design, outcomes of individuals in academies are compared with those who attend schools that later become academies, but do so after they sit their KS2 exams.

For a given cohort of year 6 pupils who were legacy enrolled in academy conversions and control schools (future to be academies) a basic estimating equation is the following

$$KS2_{ist} = \alpha_s + \alpha_t + \theta_1 Academy_{ist} + \sum_{j=1}^{J} \pi_{1j} X_{jist} + \varphi_1 KS1_{ist} + v_{1ist} \quad (1)$$

In (1) i denotes pupil, s denotes the legacy enrolment school and t denotes school calendar year. Thus, $\alpha_s$ is a legacy enrolment school fixed effect[14] and $\alpha_t$ is a time effect for the

[14] These are identified by inclusion of grade 6 students who sit exams in treatment schools prior to conversion.

academic year in which the pupil is in grade 6. The vector X is a set of control variables, and the binary Academy variable takes value 1 if pupil i who was legacy enrolled in school s sits their end of primary school KS2 examination in an academy school. Finally, $v_1$ is an error term.

Despite already restricting to the legacy enrolment sample so as to avoid endogeneity concerns it may still be problematic to estimate equation (1) by ordinary least squares because legacy enrolled pupils may leave the school before the end of Key Stage 2. To allow for students to (potentially) sort into schools non-randomly as a result of the school obtaining academy status, an instrument for academy attendance is therefore used. This is whether or not the pupil was already enrolled in the school in the year prior to conversion in grades 2-5. Those for whom this variable takes a value of one are referred to as being intention-to-treat (ITT).

Attention is focussed on those pre-enrolled in grades 2 to 5 as grade 5 is the penultimate year of primary education and grade 2 is when the KS1 assessment takes place, thus ensuring that KS1 assessment does not take place in an academy for the ITT pupils.[15] It is important to note that pupils enrolling in the school after conversion are not included in the analysis. To ensure that the control group and treatment group are selected in the same way, a slightly different control group is used for each cohort of academy converters. For those converting in 2010/11 for example, the control group consists of pupils who are in grades 2-5 in 2009/10 at schools that convert between 2011/12 and 2016/17, but are not expected to sit

[15] As schools typically enrol students 3 years prior to grade 2, we could estimate the effects for students pre-enrolled in academies in earlier grades. In our case, we would be able to estimate the effect of one extra year for those in grade 1 in the first cohort of converters. However, doing so would either entail dropping KS1 scores from our estimates or assuming that KS1 performance is unaffected by academy attendance. Given that we do not gain many observations by adding extra pre-enrolled pupils we focus on those pre-enrolled in grade 2-5.

their exams in an academy.[16] Control groups are defined similarly for all schools converting up to and including 2014/15.[17]

Because of these restrictions, the event study on pupil performance has to be limited to a maximum of four years post-conversion, including the year of conversion itself. This is because there are 4 remaining years of primary school after the Key Stage 1 assessment. Thus, pupils affected by conversion in grade 2 of primary school (when KS1 assessments are taken) could have up to four post-conversion years of education in the academy. Similarly children affected by conversion when enrolled in the predecessor school in grade 3 could have up to three conversion years, and so on for children in grades 4 and 5 in the predecessor school.

This setup permits implementation of a research design which estimates of the causal impact of being in an academy. For a given cohort of grade 6 treatment and control pupils this can be operationalised through the following estimation equations:

$$\text{Academy}_{ist} = \alpha_s + \alpha_t + \theta_2 \text{ITT}_i + \sum_{j=1}^{J} \pi_{2j} X_{jist} + \varphi_2 \text{KS1}_{ist} + v_{2ist} \quad (2)$$

$$\text{KS2}_{ist} = \alpha_s + \alpha_t + \theta_3 \text{ITT}_i + \sum_{j=1}^{J} \pi_{3j} X_{jist} + \varphi_3 \text{KS1}_{ist} + v_{3ist} \quad (3)$$

In the first stage, equation (2), estimates of $\theta_2$ show the proportion of the ITT group that stay in the academy and take their KS2 tests there. Equation (3) is the reduced form regression of KS2 on the instrument. A two stage least squares estimate (2SLS) estimate can then be obtained as the ratio of the reduced form coefficient to the first stage coefficient, $\theta_3 / \theta_2$. The main specifications that are estimated are equations (2) and (3) based on pooled data for the five cohorts of academy conversions already described.

[16] For example, in the case of the first cohort this necessitates removing those who, in 2009/10, are grades 2-4 in 2011/12 converters, grades 2-3 in 2012/13 converters and grade 2 in 2013/14 converters.
[17] The exact control group/treatment group structure is given in the appendix

Extending this to an event study framework enables separate estimates for the number of years a pupil is exposed to being in an academy post conversion (up to a maximum of four including conversion year) to be obtained. In this case, there are four instruments for whether a pupil is expected to sit their exams in the year of conversion (those in grade 5 in the year prior to conversion to instrument one year of exposure), the next year (those in grade 4 in the year prior to conversion to instrument two years of exposure) and so on up to the maximum of four years exposure (for those legacy enrolled in grade 2). It should be noted that, because of the data that we have, not all cohorts of converters contribute to the exposure estimates for later years. For instance, we can only identify the effect of four years exposure for those who are pre-enrolled in grade 2 in the first two cohorts of conversions.

*Comparison Schools*

A naive comparison between primary academies and all other state-maintained schools is likely to suffer from significant selection bias, since (as discussed above) conversion to an academy was done on a voluntary basis and better-performing schools were prioritised and actively encouraged to convert.[18] One might expect schools seeking to become academies to have common unobservable characteristics such as having a school ethos more in line with the academy model. To account for this, pupils attending future converters are used as a control group in a difference-in-differences setting. Thus the data structure that is utilised is a balanced panel of schools for the school years 2006/07 to 2014/15 with repeated cross-sections of grade 6 pupils.

*Balancing Tests*

This approach can be legitimised first through covariate balancing tests between treatment and controls in the baseline academic year (2006/07). Second, and probably more importantly, the empirical analysis shows there to be no evidence of differential pre-

[18] In other words, the instrument is only assumed to satisfy the exclusion restriction conditional on pupils being in a well-defined sub-sample of the population.

conversion trends in outcomes between pupils in treatment and control schools. On the former of these, Table 2 shows the extent to which treatment and control groups are balanced at baseline (2006/07) for the full sample of treatment and control schools, and separately for outstanding and non-outstanding schools. In terms of the full sample of all schools, there is a significant difference with respect to KS2 scores prior to the policy, with treatment schools being better performing in maths. The workforce in treatment schools also appears to be both larger and, on average, younger and there are more enrolled pupils in the treatment schools.

The above differences are not so surprising when it is acknowledged that the government prioritised better performing schools for conversion to academy status. For instance, within the sample of schools studied here, over 80% of the first cohort of conversions were deemed outstanding by Ofsted. This proportion declines monotonically to 11% for the 2016/17 cohort of conversions.[19] For this reason it is necessary to look within Ofsted grades (as defined by the latest Ofsted grade awarded prior to 2010/11) when comparing treatment and control schools. When this is done, the schools look much more balanced on observables.[20] In fact, as Table 2 shows, within categories of outstanding and non-outstanding schools, there are few statistically significant differences at baseline between treatment and control schools. In fact, for outstanding schools there are no statistically significant differences (at the 5 percent level) for any baseline characteristics including KS2

[19] The exact proportions for the school years 2010/11 to 2016/17 are 83%, 43%, 20%, 17%, 14%, 14%,and 11%.
[20] In addition to separately considering the relatively large number of pre-policy variables used here, we also adopted a different approach to create a single summary index and perform balancing tests on this. On example comes from regressing the pre-policy 2006/07 KS2 scores on English is first language, gender, ethnicity, free school meals status and Key Stage 1. This new index was balanced across treatment and control schools. For example in the case of the pooled sample, the treatment control difference (standard error) in the summary index was 0.025 (0.017), with a p-value of 0.14. The extent of balance was seen to be much better in outstanding schools where the treatment control difference (standard error) in the summary index was -0.003 (0.033), with a p-value of 0.92. In non-outstanding schools the difference was 0.034 (0.019) with a p-value of 0.078.

and KS1 scores.[21] Thus, regressions are estimated for schools within each Ofsted grade, as well as for the pooled sample.[22]

## 4. Did Primary Academies Change Their Modes of Operation?

Before looking at the effect of primary academies on pupil performance, evidence is presented on whether changes in the mode of operation occurred at primary schools that became academies prior to or during the 2014/15 academic year. Four aspects of this are considered. First, whether primary schools took up the option to exercise the many academy freedoms that became available from increased autonomy. Second, whether patterns of expenditure changed. Third, whether there were changes in workforce composition. Fourth, whether academies altered their pupil intake.

*Use of Academy Freedoms*

There have been various investigations into whether schools actually use their academy freedoms upon conversion (e.g. Academies Commission, 2013; Cirin, 2014). The existing descriptive evidence confirms that they mostly do, but with some degree of variation. The Academies Commission (2013) conclude that take-up of freedoms had been 'piecemeal rather than comprehensive', in part because changes can take time to implement and sometimes require consultation. Surveys of recent converters by Bassett et al. (2012) and Cirin (2014) found financial motives to be important in the decision to convert. In the former study, over 75% of respondents cited it as one of their reasons for converting and two-fifths as their primary reason. Cirin (2014) found that the desire 'to gain greater freedom to use

[21] The closest to significance are school size (in terms of number of pupils) and number of teachers. The next section reports results on whether these changed significantly on becoming an academy, revealing there to be no significant changes.

[22] While there are four possible grades awarded by Ofsted, two groups are considered, outstanding and non-outstanding, the latter comprising good, satisfactory and unsatisfactory. The direct focus on the outstanding group is because they were the ones that were earmarked for the fast track to become an academy. The non-outstanding group are also amalgamated on account of the relatively small number of satisfactory and unsatisfactory schools in the sample. When estimating the pooled regressions over all schools, all variables are interacted with Ofsted grade.

funding as you see fit' was the most commonly cited reason for conversion (cited by 83% of respondents). The vast majority (almost 9 in 10) also moved to procure services themselves.

Importantly, Cirin (2014) breaks down results by primary and secondary status. This shows that the majority of academies do exercise freedoms, but this is more common in secondary than in primary schools. This is shown in Table 3, taken from his survey of 720 academies which were open on 1 May 2013. The numbers in the Table show most schools report a use of academy freedoms, but that the percentage of primary schools making a particular change is smaller than it is for secondary schools. Furthermore, Cirin (2014) reports that almost all schools surveyed made at least one change (702 out of 720), implying that at least 95% of primary converters (262 primary converters were surveyed) exercised at least one freedom, with two-thirds believing that the changes improved attainment.

*Changes in Expenditure Patterns*

Studies cited above on the use of academy freedoms suggest that the financial motive to convert was important. Table 4 shows numbers on income and expenditure before and after conversion in treatment and control schools using administrative data on school income and expenditure. Changes between the 2009/10 and the 2014/15 school years are reported. There are some data issues that need to be highlighted upfront before discussing these numbers. First, the timing of reporting changed after conversion, with academies reporting in the September-August school year as opposed to the April-March financial year.[23] The latter is in line with local authority financial statements and was the practice in schools before they converted to become an academy and in control schools throughout the period of the analysis. Secondly, accounts for schools that do not convert in the period (the control schools) do not include the value of LEA provided services; however, information is available on how much extra income is given to academies to cover the value of these services (the Education

[23] For a small number of schools (35), the accounts cover a period exceeding 12 months. In these cases, it is known how long the accounts cover and numbers for them are weighted accordingly (i.e. proportionately scaling down all items by the fraction 12/period covered).

Services Grant - ESG). To make the numbers comparable this is removed from both the grant income and expenditure for academies in column (2) of Table 4.

Columns (1) and (4) of Table 4 show that per pupil income and expenditure was similar for the treatment and control schools before conversion. For example, as shown in Panel A, total income in all treatment and control schools was £3,974 and £4,156 per pupil respectively. Total expenditure was £3,966 and £4,154 per pupil in treatment and control schools respectively. As shown in Panels B to C of the Table, these pre-conversion numbers are also closely aligned for the comparisons undertaken within the outstanding and non-outstanding groups of schools.

It is evident, however, that converting primary schools both received more money and spent more money post-conversion, even once the extra money given for LEA provided services is accounted for. The Table also shows the income and expenditure per pupil after conversion and a difference-in-difference estimate in the final column. This shows significant income and expenditure gaps arising after conversion relative to what happened in the control schools. The differences in total income and expenditure are estimated as £296 and £522 per pupil per year. The increases are clearly driven by the relative increase in grant income. A similar qualitative pattern is shown for schools classified as outstanding and non-outstanding, but with higher income and expenditure shown for the latter schools, most likely reflecting a higher proportion of disadvantaged students in this group.

Table 5 shows the change in categories of expenditure per pupil before and after conversion.[24] There are three Panels, which differ according to assumptions made about which services the academies procure post-conversion given that they are no longer provided for them by the local authority. The numbers in the upper Panel A are changes inclusive of the extra money delegated to them. The numbers in the middle Panel B subtract an equal

[24] The detailed expenditure categories that have been aggregated to the four categories in Table 5 are reported in Appendix Table A1.

share of the ESG money from each category of expenditure. Finally, those in the lower Panel C remove all of the extra ESG money from the expenditure on non-staff related running costs.

In each case it is very clear that, even though primary academies spent more on teaching staff, non-teaching staff, and other running costs after conversion (relative to control schools), the increase was greater for administrative costs (i.e. non-teaching staff and other running costs). This is true for schools in all Ofsted categories. Because the amount of money earmarked for services previously provided by LEAs from expenditure is removed, these shifts cannot be attributed solely to the mechanical shift caused by the school having to take on more administrative tasks post-conversion. It seems that the primary academies studied in this paper did receive more income, but that they spent it disproportionately on day-to-day running operations rather than on 'frontline services' such as teaching staff.

*Changes in Workforce*

Table 6 reports evidence on changes in the composition of the school workforce between 2010/11 and 2014/15 for schools that became academies in that period relative to schools that became academies in 2015/16 and 2016/17. Changes are shown for all schools and stratified by Ofsted rating. The Table reports difference-in-differences estimates for the total number of teachers employed, the pupil/teacher ratio, the mean teacher salary, the proportion of teachers who are in the leadership group or whether the school changes its head teacher.

In general, the results reported in the Table show little evidence of workforce changes resulting from academisation. The one exception is head teacher turnover. For the full sample, there is a statistically significant 6.3 percentage point reduction in head teacher turnover in primaries that became academies. When broken down by Ofsted category, this occurs only in non-outstanding schools, which are 7.2 percentage points less likely to take on

a new head teacher. This stands in direct contrast to the finding of Eyles and Machin (2015) who found that the vast majority of the first phase of academy conversions in the 2000s were characterised by new head teachers coming into academies and therefore that changes in managerial structure were a key feature of academy conversion that facilitated increased autonomy. This mechanism appears to be completely absent in the case of primary schools.

*Changes in Intake*

Alongside performance effects we look at whether pupil composition changed once a school gained academy status. As data is available prior to 2010/11, the analysis considers year-on-year changes between 2007/08 and 2014/15 in the characteristics of those entering the earliest grade in which the schools enrol pupils. We also include observations of pupils over this period that enter schools which become academies after the sample ends (in 2015/16 and 2016/17). The three outcomes considered are the fraction of the pupil intake who are eligible for free school meals, the fraction with English as a native language, and the total size of the entry year intake (in logs). In each case, school and year effects are included. The results, presented in Table 7, show no evidence that schools alter their intake along these dimensions.[25]

*Summary*

Taken together, the findings show that primary schools changed some aspects of their operations after becoming academies. In particular, most primary academies began to use freedoms made available to them as a consequence of conversion. They also received more income and altered how their expenditure was allocated across functions. With regard to the latter, the observed spending changes mainly affected administrative functioning and day-to-day operations, because of the removal of such provision from the local authority. At the

[25] This contrasts with the findings on the first batch of secondary school academies reported in Eyles and Machin (2015), where intake changed significantly.

same time, there was not much change in the school personnel or in composition of the pupil intake.

## 5. Pupil Performance Results

This section reports the results on pupil performance, starting with the main baseline set of results showing the causal impact of academy conversion on pupil performance. Then, in the light of the previous section's results showing that most, but not all, primary schools altered their modes of operation post-conversion, heterogeneous estimates along a number of dimensions are reported.

*Main Results*

Table 8 shows estimates of the 2SLS specifications studying the impact of academisation on pupil performance in reading and maths in tests at the end of primary school. Separate coefficients are shown for each subject, both for the pooled sample and by whether the predecessor school's Ofsted grade was outstanding or not. Columns (1) to (3) show estimates when the treatment is whether the school converts to academy status. Columns (4) to (6) show estimates for years of exposure.

As the vast majority of legacy enrolled pupils stay in the school to take their KS2 exams - first stage estimates range from 0.92 to 0.95 - only 2SLS estimates are presented. In all cases, there is no evidence of any performance boost from academisation. The estimates are small in magnitude, sometimes negative, and almost all statistically insignificant. In terms of magnitude, the largest positive estimate is 0.02σ (with standard error 0.03) for reading in outstanding schools as reported in specification (2) of the Table. All of the other 2SLS estimates are lower than this, and nine of the twelve maths and reading estimates (including all six for maths) have negative signs. When considering the average of reading and maths scores it seems that primary age pupils did not benefit from attending an academy school in

terms of their performance at the end of primary school. [26] As results prove similar whether reading or math marks are used as the outcome of interest, only results based on average points are shown for the rest of the paper.[27]

One might be concerned about the research design being potentially contaminated by differential pre-policy trends.[28] Figure 2 therefore shows estimates from an event study, for the pooled sample, for pupils attending academies four years prior to academy conversion to three years after. The effects of being in an academy remain numerically small and insignificant (as the c to c+3 coefficients all overlap with the zero line on the Figure). Moreover, there is no sign of pre-policy trends, nor any gradual improvement in results post-conversion.

Table 9 also further generalises the Table 8 baseline results by reporting estimates for legacy enrolled pupils by discrete years of exposure, ranging from one to a maximum of four. Again, there is neither any sign of a positive effect nor any suggestion that benefits might be increasing with years of exposure. If anything, the opposite is the case, as the absolute values of the negative coefficients mostly get larger with more years of exposure.

*Heterogeneity*

While there is no evidence of performance effects on average, nor in the event study and years of exposure analysis, it may still be the case that academisation has scope to benefit some subsets of students and not others. It is also possible that certain school characteristics may be associated with differential academy effects on pupil performance.

Table 10 therefore shows results from investigation of whether the estimated 2SLS effect size differs in several ways: i) with whether the pupil is eligible for free school meals

[26] We also looked at mobility between grades 2 and 6 and how it differs between treatment and control schools. Using pupil mobility as an outcome variable revealed there to be no differential transfer between the two sets of schools. Running this on all schools, and within the Ofsted groupings, detected no significant differences.
[27] Results for Tables 8 to 10 for maths and reading considered separately are available in the appendix.
[28] A second concern flagged by referees was the potential for spillovers between treated and control schools. To deal with this we re-estimated our main regressions, but removed control schools that were within 3km of any treated school. Our results were unaffected by this change.

or not; ii) with an indicator for whether the school is in an urban area or not (given that the charter school literature finds positive effects to be concentrated amongst urban schools); iii) whether it differs with pre-conversion school size (as larger schools may be more adept at managing their extra freedoms); and iv) whether the school joins a multi-academy trust (MAT).

The results reported in the Table do little to alter the prior analysis. First, there is little evidence that the effect of academy attendance differs depending on whether one is eligible for free school meals or attends an urban academy. Panel C of Table 10, shows that the same can be said for pupils attending schools of differing sizes. Although performance effects appear to decline with school size, none of these interactions reach statistical significance.

The final aspect of heterogeneity considered – whether or not pupils attend an academy that becomes part of a (MAT) or not – does uncover some differences. The most noteworthy is that some of the estimates for not being in a MAT are significantly negative. This is the case for all schools where there is a 0.06σ (0.02) fall – closer investigation shows that this is confined to the non-outstanding schools. This is consistent with the hypothesis that conversion in stand-alone (non-MAT) schools, which are not able to benefit from the economies of scale that a MAT can bring, may have actually proven detrimental to pupils enrolled in previously non-outstanding academies. However, this result should be taken with caution. About 60% of the primary academies considered here are part of a MAT, but it should be acknowledged that whether or not a school is able to join a trust is endogenous to KS2 performance; results showing performance drops could be due to negative selection to the category of non-MAT academy schools (evident only for non-outstanding schools).

## 6. Conclusion

The English government has radically restructured its school system under an assumption that academisation delivers benefits to schools and students. This paper reports results from investigations studying the unexpected policy change that occurred in 2010 that enabled (and encouraged) primary schools to become academies. It looks at the first primary schools that have become academies in England (between 2010/11 and 2014/15) and finds no evidence of pupil performance improvement resulting from conversion.

How should an overall zero effect be interpreted in the light of some evidence showing positive effects of autonomy in other contexts? One reason is that schools that converted were already doing well within the system and simply did not require additional autonomy in order to thrive and therefore did not make substantive changes. Indeed the limited changes that are seen – increasing expenditure on non-instructional tasks – do not correspond to the kinds of changes, such as effective discipline and higher quality teaching that have been found to increase test scores in other contexts such as charter schools (Fryer, 2014).

In existing research, much of the positive effects of autonomous schools have been shown for disadvantaged students and not so much for advantaged students. While there was scope to improve achievement within these schools, it may be that changes introduced as a result of school autonomy simply do not benefit such students at the margin. However, given the survey evidence reported above and the research into how additional income was used by schools, it would appear that many of these schools did not make changes that affect 'frontline services' (as opposed to administrative roles).

Another possible reason is that effects are estimated in the short run. It may be that the programme will bear fruit once more schools convert and facilitate greater economies of scale by entering into or deepening collaborative arrangements with each other. In the heterogeneity analysis, we found some evidence of variation by whether or not schools are in

a multi-academy trust. Although we do not take the effect to be causal due to the endogenous decision to join a MAT, it is still a worrying finding that performance dips for the non-outstanding primary schools (around 40 percent of converters) that do not join a multi-academy trust.

Finally, one of the key models for some successful urban charters in the US and some secondary schools in England[29] – an effective discipline approach for academies and the No Excuses model of charters – is of less relevance to the age range of children enrolled in English primary schools than for secondary age children (since behavioural problems that may lead pupils to be suspended or excluded from school are much more prevalent in the latter).[30] In the light of all these factors, it is not surprising that there has been no overall effect on pupil performance.

One might argue that if academisation has no average effect on pupil performance, this could still be a reasonable public policy if there are other reasons for why this might be beneficial – for example, if school leaders can more easily make changes that might benefit students (or their parents) and staff. However, the process of restructuring individual schools has been shown to be financially costly and restructuring on a system wide basis would likely prove to be too costly in the long run if it fails to generate gains for students in terms of test scores. Furthermore, risks are also posed by an increasing number of schools becoming academies.[31] For example, they are no longer regularly monitored at the local level. Problems might not therefore come to light unless they are flagged up by an Ofsted inspection, which are not regular events. There are potential negative spill-overs on other schools if opting out

[29] A well-known, and highly publicised, example of the latter is Hackney's Haggerston School in London which is a secondary school has fully utilised an effective discipline and good behaviour approach in its successful rise up the KS4 achievement distribution, despite having a relatively disadvantaged pupil intake.

[30] For instance, exclusions and fixed term suspensions are extremely rare in the age range that we study. In English schools, 3.88% of pupils received a fixed period exclusion in 2014/15, and 0.07% were permanently excluded. For primary schools the numbers were much lower - 1.1% of pupils received a fixed term exclusion and 0.02% were permanently excluded in the school year 2014/15 (Department for Education, 2016).

[31] See Ladd and Fiske (2016).

of Local Authority control undermines services that the Local Authority is able to provide to other schools in the same geographic area (e.g. child psychologists to support children with special needs in many schools). Studying the operational aspects of academies, and the institutional structures in which they function, is an important subject for future research.

**References**

Abdulkadiroglu A., J. Angrist, S. Dynarski, T. Kane and P. Pathak (2011) Accountability and Flexibility in Public Schools: Evidence From Boston's Charters and Pilots, Quarterly Journal of Economics, 126, 699-748.

Abdulkadiroglu A., J. Angrist, P. Hull and P. Pathak (2016) Charters without Lotteries: Testing Takeovers in New Orleans and Boston, American Economic Review, 106, 1878-1920.

Academies Commission (2013) Unleashing Greatness: Getting the base from an academised system.
http://www.educationengland.org.uk/documents/pdfs/2013-academies-commission.pdf

Angrist J., S. Cohodes, S. Dynarski, P. Pathak and C. Walters (2016) Stand and Deliver: Effects of Boston's Charter High Schools on College Preparation, Entry and Choice, Journal of Labor Economics, 34, 275-318.

Angrist J., S. Dynarski, T. Kane, P. Pathak and C. Walters (2010) Inputs and Impacts in Charter Schools. KIPP Lynn, American Economic Review, 100, 239-43.

Angrist J., P. Pathak and C. Walters (2013) Explaining Charter School Effectiveness, American Economic Journal: Applied Economics, 5, 1-27.

Bassett, D., G. Lyon, W. Tanner, B. Watkin (2012) Plan A+ Unleashing the Potential of Academies. London: Reform and Schools Network.
http://www.reform.uk/publication/plan-a-unleashing-the-potential-of-academies/

Bertoni, M., S. Gibbons and O. Silva (2017) What's in a Name? Expectations, Heuristics and Choice During a Period of Radical School Reform, Centre for Economic Performance Discussion Paper 1477.

Betts J., L. Rice, A. Zau, Y. Tang and C. Koedel (2006) Does School Choice Work? Effects on Student Integration and Achievement, Public Policy Institute of California.

Bolhmark A. and M. Lindahl (2015) Independent Schools and Long-run Educational Outcomes: Evidence from Sweden's Large-scale Voucher Reform, Economica, 82, 508–551.

Clark, D. (2009) The Performance and Competitive Effects of School Autonomy, Journal of Political Economy, 117, 745-83.

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

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

Cirin, R. (2014) Do Academies Use Their Autonomy? Department for Education Research Report.
https://www.gov.uk/government/uploads/system/uploads/attachment_data/file/401455/RR366_-_research_report_academy_autonomy.pdf

Department for Education (2013) Academies Annual Report. Academic Year: 2011/12. London: The Stationary Office.

Department for Education (2016) Permanent and Fixed Period Exclusions in England: 2014 to 2015, National Statistics, SFR 26/2016,
https://www.gov.uk/government/uploads/system/uploads/attachment_data/file/539704/SFR_26_2016_text.pdf

Dobbie W. and R. Fryer (2011) Are High Quality Schools Enough to Close the Achievement Gap? Evidence From a Social Experiment in Harlem, American Economic Journal: Applied Economics, 3, 158–87.

Dobbie W. and R. Fryer (2013) Getting Beneath the Veil of Effective Schools: Evidence from New York City, American Economic Journal: Applied Economics, 5, 58–75.

Dobbie W. and R. Fryer (2014) The Medium Term Impacts of High Achieving Charter Schools, Journal of Political Economy, 5, 985–1037.

Dobbie W. and R. Fryer (2016) Charter Schools and Labor Market Outcomes, National Bureau of Economic Research Working Paper 22502.

Epple D ., R. Romano and R. Zimmer (2016) Charter Schools: A Survey of Research on Their Characteristics and Effectiveness, in Hanushek, E., Machin, S. and L. Woessmann (eds.) Handbook of the Economics of Education, Volume 5, North Holland.

Eyles, A., C. Hupkau and S. Machin (2016a) Academies, Charter and Free Schools: Do New School Types Deliver Better Outcomes?, Economic Policy, 31, 453-501.

Eyles, A., C. Hupkau and S. Machin (2016b) School Reforms and Pupil Performance, Labour Economics, 41, 9-16.

Eyles, A., and S. Machin (2015) The Introduction of Academy Schools to England's Education, CEP Discussion Paper. No. 1368.

Fryer, R. (2014) Injecting Charter School Best Practices into Traditional Public Schools: Evidence From Field Experiments, Quarterly Journal of Economics, 129, 1355-1407.

Gibbons, S., S. McNally and M. Viarengo (2011) Does Additional Spending Help Urban Schools? An Evaluation Using Boundary Discontinuities. SERC Discussion Paper No. 90; CEE Discussion Paper No. 128. London School of Economics.

Gleason P., M. Clark, C. Clark Tuttle, E. Dwyer and M. Silverberg (2010) The Evaluation of Charter School Impacts (NCES 2010-4029), U.S. Department of Education, National Center for Education Statistics. Washington, DC: U.S. Government Printing Office.

Hanushek E. and L. Woessmann (2011) The Economics of International Differences in Educational Achievement, in Hanushek E., S. Machin and L. Woessmann (eds.) Handbook of the Economics of Education, Volume 3, Amsterdam: Elsevier.

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

Hoxby C., S. Murarka and J. Kang (2009) How New York City's Charter Schools Affect Achievement, New York City Charter Schools Evaluation Project

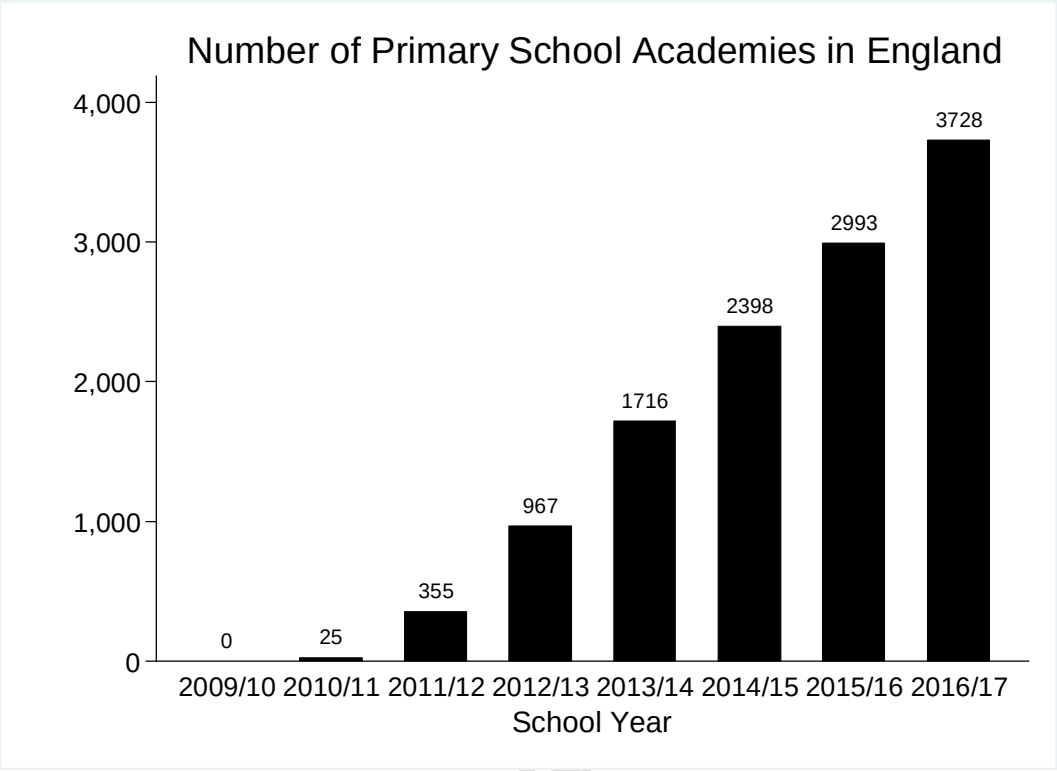
Ladd H. and E. Fiske (2016) England Confronts the Limits of School Autonomy, Working Paper 232 National Center for the Study of Privatization in Education Teachers College, Columbia University

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

Roberts N. and P. Bolton (2017) School Funding in England. Current System and Proposals for 'Fairer School Funding, House of Commons Briefing Paper Number 06702

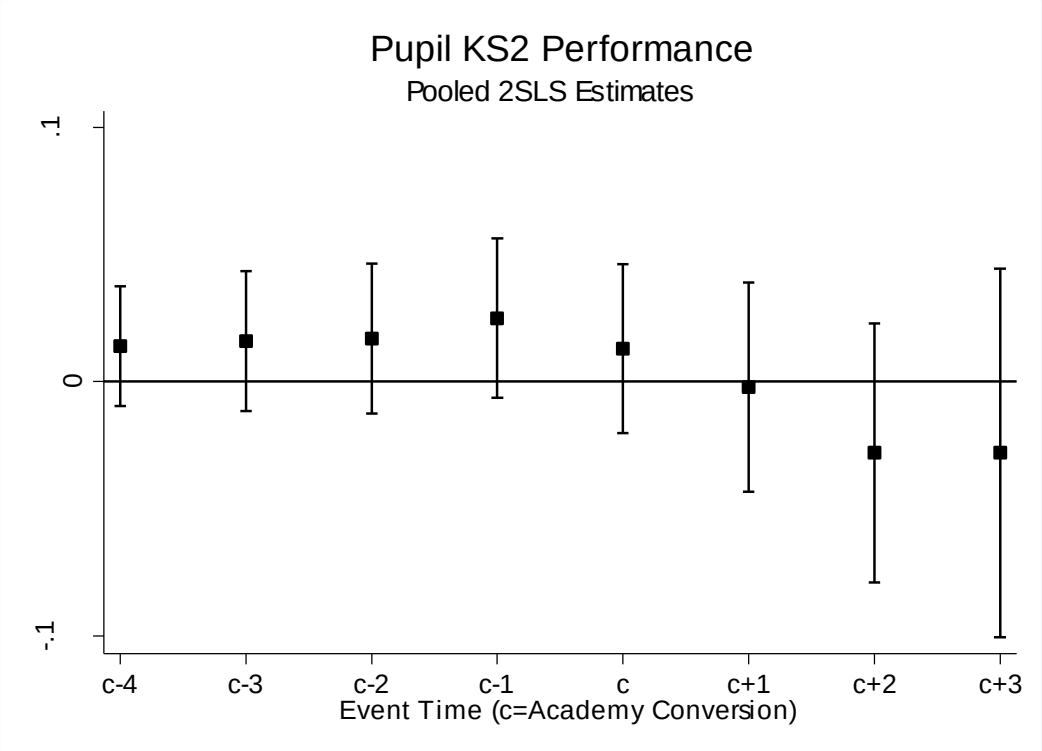
Steinberg M. (2014) Does Greater Autonomy Improve School Performance? Evidence from a Regression Discontinuity Analysis in Chicago. Education Finance and Policy, 9(1), 1-35.

**Figure 1: Number of Primary School Academies in England**



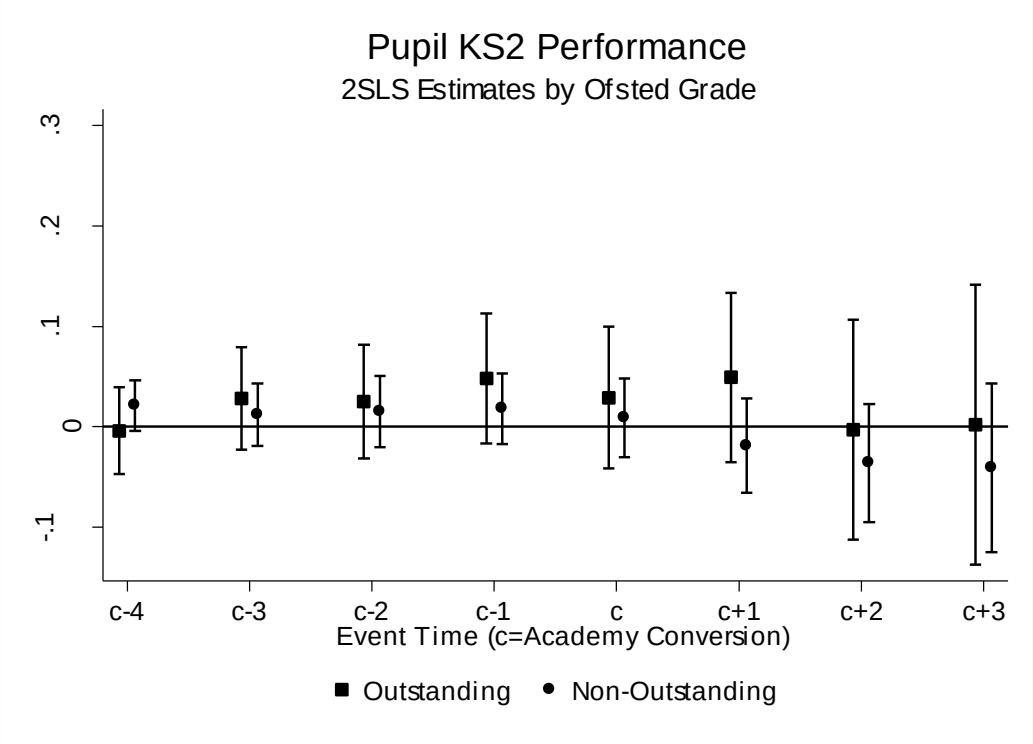

Notes: The Figure shows the number of primary/ middle deemed primary schools open in each school year as academies, and/or free schools. Source data Edubase, available at http://www.education.gov.uk/edubase/home.xhtml

**Figure 2: Event Study Estimates, Pre- and Post-Academy Conversion**




Notes: c refers to academy conversion year. KS2 performance is measured by standardised average point score. The coefficients come from the same 2SLS estimates as reported in column (1) of Table 8, but with dummies for the number of years before or after conversion the exam is sat in an academy. The four post conversion dummies (c to c+3) are instrumented for with four ITT/ITT grade interactions. A joint test for the significant of the pre-conversion dummies gives a chi square statistic of 0.71 (p-value = 0.59).

**Figure 3: Event Study Estimates by (Pre-Intervention) Ofsted Grade**




Notes: c refers to academy conversion year. KS2 performance is measured by the standardised average point score. The coefficients come from the same 2SLS estimates as reported in columns (2) and (3) of Table 8, but with dummies for the number of years before or after conversion the exam is sat in an academy. The four post conversion dummies (c to c+3) are instrumented for with four ITT/ITT grade interactions. A joint test for the significant of the pre-conversion dummies gives a chi square statistic of 1.26 (p-value = 0.28) in the case of outstanding schools and 0.79 (p-value = 0.60) in the case of non-outstanding.

**Table 1: Number of New Primary Converter Academies in the Study Sample**

| | Academic Year | | | | | | |
|---|---|---|---|---|---|---|---|
| | 2010/11 | 2011/12 | 2012/13 | 2013/14 | 2014/15 | 2015/16 | 2016/17 |
| Pooled | 12 | 174 | 252 | 234 | 243 | 210 | 309 |
| Outstanding | 10 | 74 | 51 | 40 | 35 | 30 | 35 |
| Non-Outstanding | 2 | 100 | 201 | 194 | 208 | 180 | 274 |

Notes: In order to implement the research design, only schools in the sample that have students in grades 2 and grades 6 in each academic year between 2006/07 and 2014/15 are included. The main discrepancy between the numbers in this Table and the total number of primary academies given in Figure 1 arise because of: a) the removal of infant and junior schools (the latter do not to enrol children in grade 2, while the former do not do so in grade 6) and b) because Figure 1 also includes sponsored academies (comprising around 30 percent of primary academies) and a small number of free schools (139 by 2016/17), which are not studied in this paper.

**Table 2: Baseline Characteristics: Pooled Sample and by Ofsted Grade**

| | All Schools | | | Outstanding Schools | | | Non-Outstanding Schools | | |
|---|---|---|---|---|---|---|---|---|---|
| | Treatment | Control | Treatment – Control p-value | Treatment | Control | Treatment – Control p-value | Treatment | Control | Treatment – Control p-value |
| English is first language | 0.924 | 0.926 | 0.858 | 0.908 | 0.911 | 0.808 | 0.929 | 0.929 | 0.964 |
| White British | 0.877 | 0.881 | 0.609 | 0.855 | 0.854 | 0.989 | 0.883 | 0.886 | 0.543 |
| Eligible to receive free school meals | 0.126 | 0.133 | 0.336 | 0.108 | 0.109 | 0.889 | 0.131 | 0.138 | 0.309 |
| Male | 0.512 | 0.509 | 0.073 | 0.509 | 0.506 | 0.279 | 0.512 | 0.509 | 0.141 |
| KS2 reading | 0.049 | 0.003 | 0.418 | 0.298 | 0.280 | 0.647 | -0.025 | -0.048 | 0.496 |
| KS2 maths | 0.060 | -0.010 | 0.032 | 0.334 | 0.296 | 0.300 | -0.021 | -0.066 | 0.060 |
| KS1 reading | 0.030 | 0.003 | 0.721 | 0.186 | 0.173 | 0.732 | -0.016 | -0.028 | 0.817 |
| KS1 maths | 0.024 | -0.002 | 0.727 | 0.172 | 0.163 | 0.826 | -0.02 | -0.032 | 0.777 |
| Number of teachers | 14.958 | 14.294 | 0.020 | 16.443 | 15.419 | 0.087 | 14.516 | 14.09 | 0.093 |
| Proportion unqualified teachers | 0.033 | 0.030 | 0.484 | 0.039 | 0.036 | 0.587 | 0.031 | 0.029 | 0.624 |
| Number of pupils | 278.709 | 264.890 | 0.018 | 313.448 | 289.825 | 0.052 | 268.362 | 260.357 | 0.109 |
| Pupil teacher ratio | 18.555 | 18.322 | 0.231 | 19.179 | 18.843 | 0.402 | 18.369 | 18.228 | 0.372 |
| Mean teacher age | 41.077 | 41.483 | 0.034 | 40.276 | 40.413 | 0.630 | 41.322 | 41.683 | 0.032 |
| Mean teacher salary | 32412 | 32545 | 0.034 | 32568 | 32545 | 0.863 | 32364 | 32545 | 0.046 |
| Number of schools | 1434 | | | 275 | | | 1159 | | |

Notes: All variables are measured in the school year 2006/07. All KS1 and KS2 scores are standardized to have mean zero and standard deviation of 1 (within the year and overall sample). Ofsted grades are measured prior to the policy. Since Ofsted inspect schools every 3-5 years (see Section 3), the grades here are the most recent grade between 2006/07 and 2009/10 (i.e. prior to the policy change).

**Table 3: Percentage Using Freedoms Since Becoming an Academy: Primary and Secondary Schools**

| | Secondary Schools | Primary Schools |
|---|---|---|
| Changed your pattern of capital expenditure | 63 | 54 |
| Introduced savings in back-office functions | 62 | 54 |
| Changed the performance management system for teachers | 63 | 49 |
| Changed the curriculum you offer | 60 | 49 |
| Changed school leadership | 51 | 43 |
| Introduced or increased revenue-generating activities | 41 | 28 |
| Hired teachers without qualified teacher status (ATS) | 23 | 8 |
| Sought to attach pupils from a different geographical area | 14 | 5 |
| Increased the length of the school day | 10 | 5 |
| Changed the length of school terms | 6 | 2 |
| Number of schools | 360 | 334 |

Source: Cirin (2014). Online survey of 720 academies that were open on 1 May 2013.

## Table 4: Changes in School Income per Pupil and Expenditure per Pupil Before and After Academy Conversion

| | Treatment Schools | | | Control Schools | | | Treatment – Control |
|---|---|---|---|---|---|---|---|
| | Before | After | Change | Before | After | Change | Difference-in-Difference |
| | (1) | (2) | (3) = (2) – (1) | (4) | (5) | (6) = (5) – (4) | (7) = (3) – (6) |
| **A. All Schools (843 Treatment, 466 Control)** | | | | | | | |
| Total income | 3974 | 4997 | 1023 (26) | 4156 | 4883 | 727 (41) | 296 (48) |
| Grant income | 3810 | 4771 | 961 (24) | 4019 | 4704 | 685 (40) | 276 (47) |
| Other income | 164 | 226 | 63 (10) | 137 | 179 | 42 (7) | 20 (12) |
| Total expenditure | 3966 | 5121 | 1155 (33) | 4154 | 4788 | 633 (43) | 522 (54) |
| **B. Outstanding (200 Treatment, 59 Control)** | | | | | | | |
| Total income | 3755 | 4807 | 1052 (47) | 3851 | 4819 | 967 (199) | 85 (203) |
| Grant income | 3580 | 4559 | 979 (43) | 3706 | 4598 | 892 (199) | 88 (202) |
| Other income | 175 | 248 | 73 (23) | 145 | 221 | 76 (27) | -2 (35) |
| Total expenditure | 3754 | 4890 | 1135 (59) | 3834 | 4754 | 920 (201) | 215 (208) |
| **C. Non-Outstanding (643 Treatment, 407 Control)** | | | | | | | |
| Total income | 4042 | 5056 | 1014 (30) | 4200 | 4892 | 692 (37) | 322 (48) |
| Grant income | 3882 | 4837 | 955 (29) | 4064 | 4719 | 655 (36) | 300 (46) |
| Other income | 160 | 220 | 59 (11) | 136 | 173 | 37 ( 6) | 22 (13) |
| Total expenditure | 4032 | 5193 | 1161 (39) | 4201 | 4792 | 592 (39) | 569 (55) |

Notes: The sources for expenditure data are publicly available consistent financial reporting records for all state-maintained schools and academies financial benchmarking data for academy schools. The former are available at https://www.compare-school-performance.service.gov.uk/ and the latter can be accessed at https://www.gov.uk/government/collections/statistics-local-authority-school-finance-data. For academies opening in April to August of the school year, incomes and expenditures in the first full year of conversion are appropriately scaled. In columns (3), (6) and (7), long changes are considered between 2009/10 (Before) and 2014/15 (After). Standard errors in parentheses.

## Table 5: Changes in Expenditure Category per Pupil Before and After Academy Conversion

| | All Schools | | Outstanding | | Non-Outstanding | |
|---|---|---|---|---|---|---|
| | Pre-Change Mean | Difference-in-Difference | Pre-Change Mean | Difference-in-Difference | Pre-Change Mean | Difference-in-Difference |
| | (1) | (2) | (3) | (4) | (5) | (6) |
| **A. Includes ESG** | | | | | | |
| Total teaching staff | 2063 | 58 (27) | 1969 | -43 (104) | 2086 | 65 (27) |
| Total non-teaching staff | 1236 | 168 (24) | 1113 | 10 (66) | 1266 | 197 (26) |
| Learning and ICT resources | 212 | -8 (8) | 216 | -3 (22) | 211 | -10 (9) |
| Other running costs | 523 | 305 (22) | 475 | 251 (47) | 535 | 318 (25) |
| **B. ESG Equally Deducted** | | | | | | |
| Total teaching staff | 2063 | 32 (27) | 1969 | -75 (104) | 2086 | 40 (27) |
| Total non-teaching staff | 1236 | 148 (24) | 1113 | -12 (66) | 1266 | 178 (26) |
| Learning and ICT resources | 212 | -12 (8) | 216 | -8 (22) | 211 | -13 (9) |
| Other running costs | 523 | 294 (22) | 475 | 239 (47) | 535 | 308 (25) |
| **C. ESG Deducted From Other Running Costs** | | | | | | |
| Total teaching staff | 2063 | 58 (27) | 1969 | -43 (104) | 2086 | 65 (27) |
| Total non-teaching staff | 1236 | 168 (24) | 1113 | 10 (66) | 1266 | 197 (26) |
| Learning and ICT resources | 212 | -8 (8) | 216 | -3 (22) | 211 | -10 (9) |
| Other running costs | 523 | 245 (22) | 475 | 181 (49) | 535 | 262 (26) |
| Number of treatment schools | 843 | | 200 | | 643 | |
| Number of control schools | 466 | | 59 | | 407 | |

Notes: As for Table 4. The top panel includes extra money given to academies to cover services previously provided by the LEA. The middle panel removes this expenditure equally from each expenditure category. The bottom panel removes all the extra money from the other running costs category.

**Table 6: Changes in Workforce, School-Level Difference-in-Differences Estimates**

| | All Schools | Outstanding | Non-Outstanding |
|---|---|---|---|
| | (1) | (2) | (3) |
| Log(Number of teachers) | 0.007 (0.012) | 0.015 (0.025) | 0.007 (0.014) |
| Log(Pupil teacher ratio) | -0.018 (0.011) | -0.007 (0.023) | -0.016 (0.012) |
| Log(Mean teacher salary) | -0.003 (0.007) | 0.001 (0.013) | -0.006 (0.008) |
| Proportion of teachers in leadership group | 0.003 (0.005) | 0.005 (0.010) | 0.003 (0.005) |
| Change in head teacher | -0.063 (0.028) | -0.002 (0.070) | -0.072 (0.031) |

Notes: Based on data from the schools' workforce census for the academic years 2010/11 and 2014/15. All variables are long changes between these two academic years. The subsample is the sample of schools who are observed in each of the two years. We exclude schools converting in 2010/11 as we do not observe a pre-treatment observation. Robust standard errors in parentheses are reported in each case. The sample sizes for the first three rows (Number of teachers; Pupil teacher ratio; proportion of teachers in leadership group) are 1326, 254, 1072 for all schools, outstanding schools, and non-outstanding schools respectively. For the head teacher regression the sample sizes are 1327, 257 and 1070 for all schools, outstanding schools, and non-outstanding schools respectively. Baseline means are: 15.321 teachers; 21.565 pupils per teacher; £36446 average salary; 0.173 of teachers are in the leadership group 0.445 of schools change head teacher over the course of the five years.

**Table 7: Changes in Pupil Intake**

| | All Schools | | | Outstanding | | | Not-Outstanding | | |
|---|---|---|---|---|---|---|---|---|---|
| | FSM | English Language | Log (No of Pupils) | FSM | English Language | Log (No of Pupils) | FSM | English Language | Log (No of Pupils) |
| Academy | 0.000 | -0.007 | 0.007 | 0.002 | 0.004 | 0.009 | -0.001 | -0.011 | 0.006 |
| | (0.003) | (0.009) | (0.006) | (0.004) | (0.019) | (0.012) | (0.003) | (0.011) | (0.007) |
| School fixed effects | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Year dummies | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Sample size | 467386 | 466635 | 12906 | 102565 | 102459 | 2475 | 364821 | 364176 | 10431 |
| Number of schools | 1434 | 1434 | 1434 | 275 | 275 | 275 | 1159 | 1159 | 1159 |

Notes: Variables refer to the pupils entering the lowest grade in the school in each year. Each cell is a coefficient estimated from a separate regression. Standard errors (in parentheses) are clustered at school level

**Table 8: The Effect of Treatment on KS2 Test Scores (measured at age 11)**

| | 2SLS (Incidence) | | | 2SLS (Years of Exposure) | | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| | Pooled | Outstanding | Non-Outstanding | Pooled | Outstanding | Non- Outstanding |
| Maths | -0.021 | -0.002 | -0.027 | -0.012 | -0.004 | -0.016 |
| | (0.014) | (0.030) | (0.016) | (0.007) | (0.014) | (0.008) |
| Reading | 0.001 | 0.020 | -0.005 | -0.004 | 0.005 | -0.007 |
| | (0.013) | (0.026) | (0.014) | (0.006) | (0.012) | (0.007) |
| Average Point Score | -0.013 | 0.008 | -0.021 | -0.010 | -0.001 | -0.014 |
| | (0.014) | (0.029) | (0.016) | (0.007) | (0.014) | (0.008) |
| School fixed effects | Yes | Yes | Yes | Yes | Yes | Yes |
| Year dummies | Yes | Yes | Yes | Yes | Yes | Yes |
| Sample size | 1636948 | 296675 | 1340273 | 1636948 | 296675 | 1340273 |
| Number of schools | 1434 | 275 | 1159 | 1434 | 275 | 1159 |
| First stage | 0.937 | 0.950 | 0.932 | 0.928 | 0.942 | 0.922 |
| | (0.002) | (0.002) | (0.002) | (0.002) | (0.003) | (0.003) |

Notes: Each cell is a coefficient estimated from a separate regression. Full controls are included (for gender, ethnicity, speaks English as first language, eligible for free schools meals, prior attainment (Key Stage 1). Standard errors (in parentheses) are clustered at school level.

## Table 9: Effects by Year of Exposure

| | All Schools | Outstanding | Non-Outstanding |
|---|---|---|---|
| | Average Point Score | Average Point Score | Average Point Score |
| One year of exposure | 0.001 (0.012) | 0.011 (0.024) | -0.001 (0.014) |
| Two years of exposure | -0.015 (0.016) | 0.030 (0.030) | -0.030 (0.019) |
| Three years of exposure | -0.042 (0.022) | -0.024 (0.045) | -0.049 (0.025) |
| Four years of exposure | -0.042 (0.033) | -0.020 (0.060) | -0.053 (0.039) |
| School fixed effects | Yes | Yes | Yes |
| Year dummies | Yes | Yes | Yes |
| Sample size | 1636948 | 296675 | 1340273 |
| Number of schools | 1434 | 275 | 1159 |
| First stage coefficient on IIT x one year of exposure | 0.963 (0.001) | 0.974 (0.002) | 0.960 (0.002) |
| First stage coefficient on IIT x two years of exposure | 0.931 (0.002) | 0.946 (0.003) | 0.926 (0.003) |
| First stage coefficient on IIT x three years of exposure | 0.902 (0.004) | 0.927 (0.005) | 0.891 (0.005) |
| First stage coefficient on IIT x four years of exposure | 0.869 (0.007) | 0.889 (0.008) | 0.857 (0.011) |

Notes: 2SLS estimates reported. Full controls are included (for gender, ethnicity, speaks English as first language, eligible for free schools meals, prior attainment, primary school). Standard errors (in parentheses) are clustered at school level.

## Table 10: Heterogeneity

| | All Schools | Outstanding | Non-Outstanding |
|---|---|---|---|
| | Average Point Score | Average Point Score | Average Point Score |
| **A. Free school meal eligibility** | | | |
| Yes | 0.008 (0.021) | 0.025 (0.045) | 0.004 (0.024) |
| No | -0.017 (0.014) | 0.006 (0.029) | -0.025 (0.015) |
| **B. Urban** | | | |
| Yes | -0.012 (0.015) | 0.019 (0.031) | -0.023 (0.018) |
| No | -0.021 (0.019) | -0.037 (0.038) | -0.013 (0.021) |
| **C. Baseline school size** | | | |
| Treatment | 0.108 (0.103) | 0.278 (0.166) | 0.065 (0.128) |
| Treatment*Baseline school size | -0.021 (0.018) | -0.046 (0.029) | -0.015 (0.023) |
| **D. Multi academy trust** | | | |
| Yes | 0.030 (0.016) | 0.032 (0.033) | 0.031 (0.019) |
| No | -0.057 (0.017) | -0.013 (0.034) | -0.075 (0.020) |

Notes: 2SLS estimates comparable to columns (1), (2) and (3) of Table 8, but with mutually exclusive interactions included for panels A, B, and to D. Panel C reports estimates that interact treatment status with baseline (2006/07) school size in logs. In terms of free school meal status, 14% of pupils in the treated schools are eligible, 12% of pupils in outstanding treated schools are eligible, and 15% of pupils in non-outstanding treated. For all treated schools in the sample 71% are in urban areas and 57% are in multi-academy trusts. The same numbers for outstanding and non-outstanding schools are: urban 72%/71%; multi-academy trusts 50%/59% respectively.

# Appendix

This Appendix contains information on the way in which the sample of pupils and schools were selected for the analysis of primary academies, issues related to the school income and expenditure data analysed in the paper, and provides the main estimates in Tables 9-10 of the paper by subject.

*1). Sample Accounting Structure*

Here we describe the structure of the Intention to Treat (ITT) groups in each wave of academy conversions that are studied. Because we combine difference in differences with instrumental variables, we use a different group of control schools for each cohort of academy conversion. Below we detail the treatment and control samples for each of the five treated cohorts of academy conversions.

For the first cohort of conversions, the ITT sample consists of those in grades 2-5 in 2009/10 in 2010/11 converters. Alongside this we have pupils in grade 6 in 2010/11 converters in the years 2006/07 to 2009/10. As controls we include the following: those in grade 6 in the years 2006/07 to 2009/10 who sit their exams in schools that go on to convert to academies between 2011/12 and 2016/17; those in grades 2-5 in 2009/10 in 2016/17 converters; those in grades 2-5 in 2009/10 in 2015/16 converters; those in grades 2-5 in 2009/10 in 2014/15 converters; those in grades 3-5 in 2009/10 in 2013/14 converters; those in grades 4-5 in 2009/10 in 2012/13 converters; those in grade 5 in 2009/10 in 2011/12 converters.

For the second cohort of conversions, the ITT sample consists of those in grades 2-5 in 2010/11 in 2011/12 converters. Alongside this we have pupils in grade 6 in 2011/12 converters in the years 2006/07 to 2010/11. As controls we include the following: those in grade 6 in the years 2006/07 to 2010/11 who sit their exams in schools that go on to convert to academies between 2012/13 and 2016/17; those in grades 2-5 in 2010/11 in 2016/17 converters; those in grades 2-5 in 2010/11 in 2015/16 converters; those in grades 3-5 in 2010/11 in 2014/15 converters; those in grades 4-5 in 2010/11 in 2013/14 converters; those in grade 5 in 2010/11 in 2012/13.

For the third cohort of conversions, the ITT sample consists of those in grades 3-5 in 2011/12 in 2012/13 converters. Alongside this we have pupils in grade 6 in 2012/13 converters in the years 2006/07 to 2011/12. As controls we include the following: those in grade 6 in the years 2006/07 to 2011/12 who sit their exams in schools that go on to convert to academies between 2013/14 and 2016/17; those in grades 3-5 in 2011/12 in 2016/17 converters; those in grades 3-5 in 2011/12 in 2015/16 converters; those in grades 4-5 in 2011/12 in 2014/15 converters; those in grade 5 in 2011/12 in 2013/14.

For the fourth cohort of conversions, the ITT sample consists of those in grades 4-5 in 2012/13 in 2013/14 converters. Alongside this we have pupils in grade 6 in 2013/14 converters in the years 2006/07 to 2012/13. As controls we include the following: those in grade 6 in the years 2006/07 to 2012/13 who sit their exams in schools that go on to convert to academies between 2014/15 and 2016/17; those in grades 4-5 in 2012/13 in 2016/17 converters; those in grades 4-5 in 2012/13 in 2015/16 converters; those in grade 5 in 2012/13 in 2014/15 converters.

For the fifth and final cohort of conversions, the ITT sample consists of those in grades 5 in 2013/14 in 2014/15 converters. Alongside this we have pupils in grade 6 in 2014/15 converters in the years 2006/07 to 2013/14. As controls we include the following: those in grade 6 in the years 2006/07 to 2013/14 who sit their exams in schools that go on to convert to academies between 2015/16 and 2016/17; those in grades 5 in 2013/14 in 2016/17 converters; those in grades 5 in 2013/14 in 2015/16 converters.

*2). Income and Expenditure Data Sources*

The income and expenditure data come from two sources; first, data on income and expenditure for academy schools is from the publicly available (at the Department for Education website[32]) benchmark accounts returns, required by the Department for Education, for all academy schools; second, data for maintained schools comes from consistent financial reports, which are also made publicly available, as part of the school performance tables.[33] [34]

While maintained schools and academies are both required to submit financial returns, so as to allow the public to benchmark schools spending against each other, the data collected is slightly different for academies and state schools. In particular, state schools file a return for the standard financial year (April to March) while academies file a return covering the academic year (September to August). Exemptions are also available for academies in terms of both the length of the return and whether or not a return must be filed. When schools convert between March and August of a given year they have the option to file a return that exceeds 12 months (but is less than 18 months). In the very small number of cases where this is done the data are weighted to be made comparable with a 12 month return (i.e. proportionally weighting by 12 divided by the number of months for which the return is filed).

Table A1 shows a breakdown of the expenditures that are in each category.[35]

[32] See <https://www.gov.uk/government/statistics/income-and-expenditure-in-academies-in-england-2014-to-2015> for 2014/15 data.

[33] <https://www.compare-school-performance.service.gov.uk/>

[34] While the data are publicly available some variables are suppressed; for instance, teaching staff costs are suppressed for small schools for confidentiality reasons (it is also necessary to suppress other costs at random so as to make it impossible to impute teaching costs from total expenditure). We would like to thank Andrew Mellon and Robert Drake at the Department for Education for providing us with unsuppressed data for both academies and maintained schools.

[35] A detailed discussion of these categories is available at: https://www.gov.uk/government/uploads/system/uploads/attachment_data/file/423098/CFR_guidance_FINAL_150415.pdf

**Table A1: Expenditure Categories**

| Total Teaching Staff | Total Non-Teaching Staff | Learning and ICT Resources | Other Running Costs |
|---|---|---|---|
| Teaching Staff<br>Supply teaching staff<br>Supply teacher insurance<br>Agency supply teaching staff<br>(minus) Receipts from supply teacher insurance claims<br>Education support staff | Cost of other staff<br>Indirect employee expenses<br>Development and training<br>Staff related insurance<br>Administrative and clerical staff<br>Administrative supply<br>Bought in professional services such as auditor costs | Learning resources (not ICT equipment)<br>ICT learning resources | Premises staff<br>Building maintenance and improvement<br>Grounds maintenance and improvement<br>Cleaning and caretaking<br>Water and sewerage<br>Other occupation costs<br>Catering staff<br>Catering supplies<br>(minus) Income from catering<br>Energy<br>Bought in professional services – curriculum<br>Rates<br>Exam fees<br>Other insurance premiums<br>Special facilities |

## Table A2: Effects by Year of Exposure, by Subject

| | All Schools | | Outstanding | | Non-Outstanding | |
|---|---|---|---|---|---|---|
| | Maths | Reading | Maths | Reading | Maths | Reading |
| One year of exposure | -0.005 | 0.012 | 0.001 | 0.021 | -0.007 | 0.009 |
| | (0.012) | (0.011) | (0.022) | (0.022) | (0.014) | (0.012) |
| Two years of exposure | -0.027 | 0.006 | 0.018 | 0.041 | -0.043 | -0.006 |
| | (0.017) | (0.014) | (0.032) | (0.026) | (0.019) | (0.017) |
| Three years of exposure | -0.045 | -0.029 | -0.026 | -0.013 | -0.052 | -0.034 |
| | (0.023) | (0.019) | (0.045) | (0.039) | (0.026) | (0.022) |
| Four years of exposure | -0.037 | -0.026 | -0.034 | 0.005 | -0.036 | -0.044 |
| | (0.033) | (0.030) | (0.058) | (0.056) | (0.04) | (0.034) |
| School fixed effects | Yes | Yes | Yes | Yes | Yes | Yes |
| Year dummies | Yes | Yes | Yes | Yes | Yes | Yes |
| Sample size | 1636948 | | 296675 | | 1340273 | |
| Number of schools | 1434 | | 275 | | 1159 | |
| First stage coefficient on IIT x one year of exposure | 0.963 | | 0.974 | | 0.960 | |
| | (0.001) | | (0.002) | | (0.002) | |
| First stage coefficient on IIT x two years of exposure | 0.931 | | 0.946 | | 0.926 | |
| | (0.002) | | (0.003) | | (0.003) | |
| First stage coefficient on IIT x three years of exposure | 0.902 | | 0.927 | | 0.891 | |
| | (0.004) | | (0.005) | | (0.005) | |
| First stage coefficient on IIT x four years of exposure | 0.869 | | 0.889 | | 0.857 | |
| | (0.007) | | (0.008) | | (0.011) | |

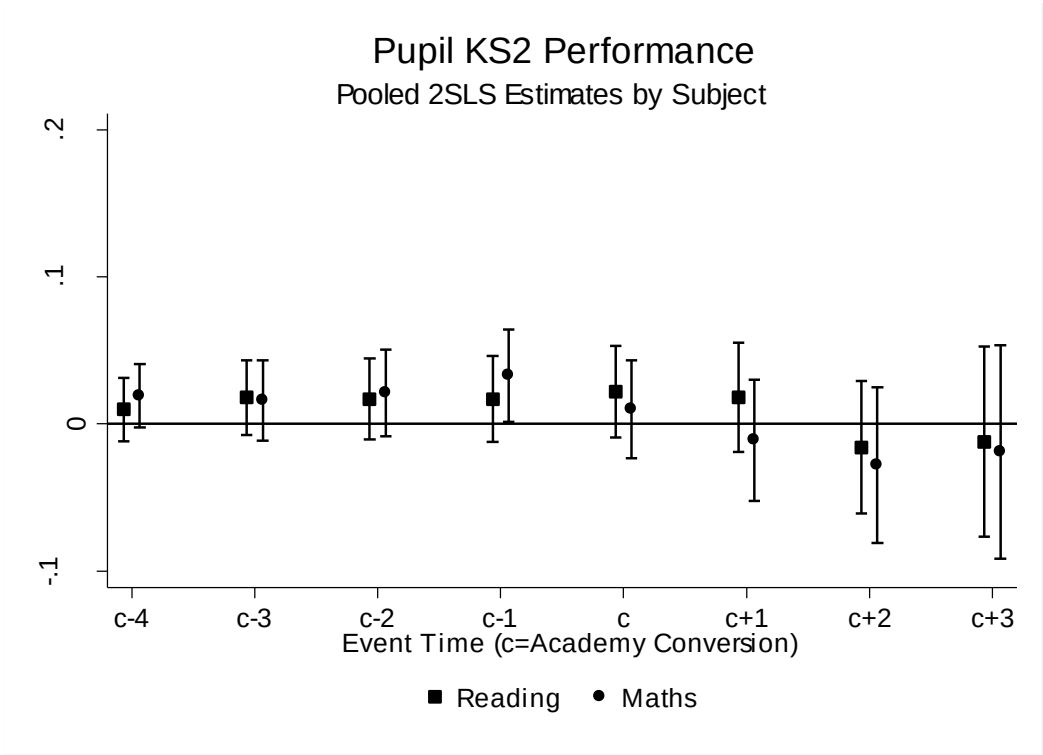
Notes: 2SLS estimates reported. Full controls are included (for gender, ethnicity, speaks English as first language, eligible for free schools meals, prior attainment, primary school). Standard errors (in parentheses) are clustered at school level.

## Table A3: Heterogeneous Effects, by Subject

| | All Schools | | Outstanding | | Non-Outstanding | |
|---|---|---|---|---|---|---|
| | Maths | Reading | Maths | Reading | Maths | Reading |
| **A. Free school meal eligibility** | | | | | | |
| Yes | -0.012 | 0.024 | 0.008 | 0.031 | -0.018 | 0.022 |
| | (0.021) | (0.019) | (0.047) | (0.040) | (0.024) | (0.022) |
| No | -0.022 | -0.003 | -0.003 | 0.018 | -0.029 | -0.010 |
| | (0.014) | (0.012) | (0.029) | (0.025) | (0.016) | (0.014) |
| **B. Urban** | | | | | | |
| Yes | -0.021 | 0.003 | 0.005 | 0.031 | -0.030 | -0.007 |
| | (0.016) | (0.013) | (0.032) | (0.027) | (0.018) | (0.015) |
| No | -0.012 | -0.006 | -0.031 | -0.029 | -0.015 | 0.004 |
| | (0.019) | (0.017) | (0.039) | (0.035) | (0.022) | (0.020) |
| **C. Baseline School Size** | | | | | | |
| Treatment | 0.166 | 0.060 | 0.382 | 0.159 | 0.103 | 0.041 |
| | (0.104) | (0.093) | (0.165) | (0.17) | (0.129) | (0.111) |
| Treatment*Baseline school size | -0.032 | -0.01 | -0.065 | -0.024 | -0.023 | -0.008 |
| | (0.018) | (0.016) | (0.029) | (0.03) | (0.023) | (0.020) |
| **D. Multi academy trust** | | | | | | |
| Yes | 0.023 | 0.035 | 0.018 | 0.046 | 0.026 | 0.032 |
| | (0.017) | (0.014) | (0.034) | (0.030) | (0.019) | (0.016) |
| No | -0.064 | -0.033 | -0.020 | -0.004 | -0.082 | -0.044 |
| | (0.017) | (0.016) | (0.034) | (0.031) | (0.020) | (0.018) |

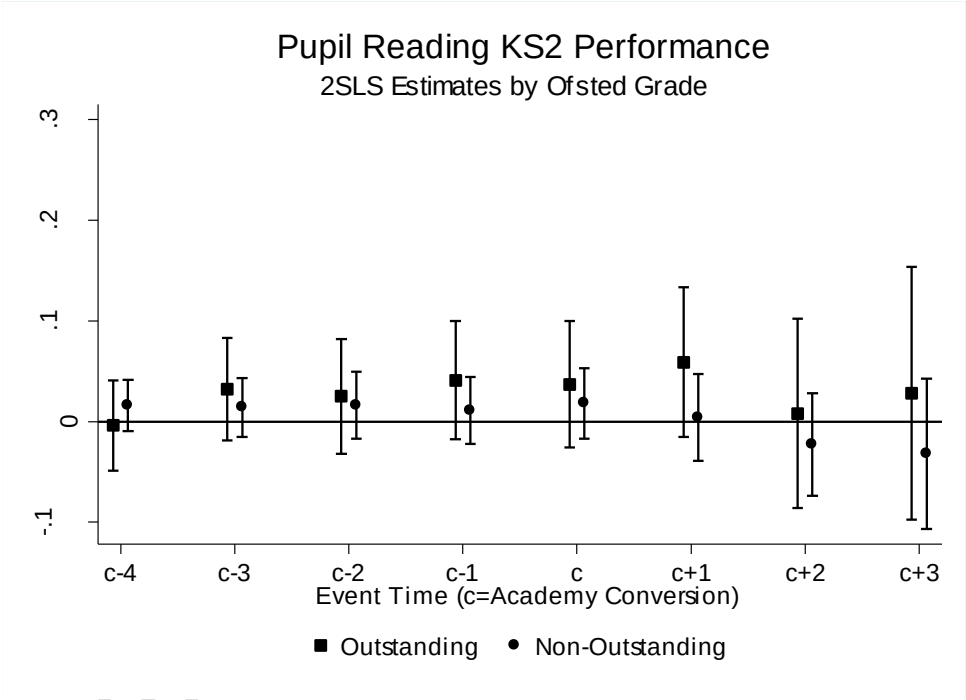
Notes: 2SLS estimates comparable to columns (1), (2) and (3) of Table 9, but with mutually exclusive interactions included for panels A, B and D. Panel C reports estimates that interact treatment status with baseline (2006/07) school size (in logs). In terms of free school meal status, 14% of pupils in the treated schools are eligible, 12% of pupils in outstanding treated schools are eligible, and 15% of pupils in non-outstanding treated. For all treated schools in the sample 71% are in urban areas and 57% are in multi-academy trusts. The same numbers for outstanding and non-outstanding schools are: urban 72%/71%; multi-academy trusts 50%/59% respectively

**Figure A1**



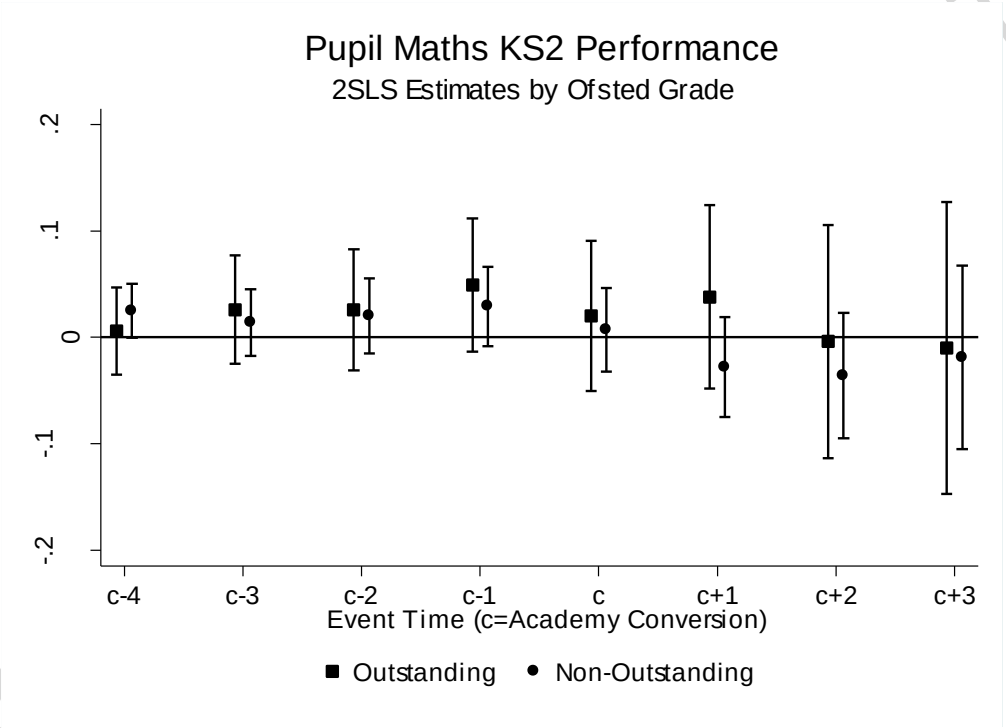

Notes: as for figure 2.

**Figure A2**




Notes: as for figure 3.

**Figure A3**




Notes: as for figure 3.