ISSN 2042-2695

CENTRE *for* ECONOMIC PERFORMANCE

CEP Discussion Paper No 1455

November 2016

Unexpected School Reform: Academisation of Primary Schools in England

Andrew Eyles
Stephen Machin
Sandra McNally

LSE THE LONDON SCHOOL OF ECONOMICS AND POLITICAL SCIENCE

E·S·R·C ECONOMIC & SOCIAL RESEARCH COUNCIL

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 convert to become an academy school on a fast track. In England, schools that become academies have more freedom over many ways in which they operate, including the curriculum, staff pay, the length of the school day and the shape of the academic year. However, the change to allow primary school academisation has been controversial. In this paper, we study the effect for the first primary schools that became academies. While the international literature provides growing evidence on the effects of school autonomy in a variety of contexts, little is known about the effects of autonomy on primary schools (which are typically much smaller than secondary schools) and in contexts where the school is not deemed to be failing or disadvantaged. The key finding is that schools did change their modes of operation after the exogenous policy change, but at the primary phase of schooling, academisation did not lead to improved pupil performance.

Keywords: academies, pupil performance
JEL codes: I20; I21; I28

This paper was produced as part of the Centre's Education and Skills Programme. The Centre for Economic Performance is financed by the Economic and Social Research Council.

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

Andrew Eyles, Centre for Economic Performance, London School of Economics. Stephen Machin, Department of Economics and Centre for Economic Performance, London School of Economics. Sandra McNally, Department of Economics, University of Surrey and Centre for Economic Performance, London School of Economics.

Published by
Centre for Economic Performance
London School of Economics and Political Science
Houghton Street
London WC2A 2AE

1. Introduction

Since 2010, the educational landscape in England has radically altered. Nearly two-thirds of secondary schools and a fifth of primary schools are now academies, which are schools that have been granted considerable operational autonomy by government. As Michael Gove, the Minister then responsible, put it schools have been 'given the freedom and the power to take control of their own destiny'.[1]

Although academies have been around as a school improvement policy for underperforming secondary schools since 2002 (and studied by Eyles and Machin, 2015, and Eyles at al., 2016a, 2016b), the programme was radically altered and expanded following the election of the new government in May 2010. It became a structure to which all schools were invited to aspire. Enabling legislation was rapidly put in place two months after the election of the new government. For the first time, and through a completely unexpected policy change, primary schools were invited to become academies, with better performing schools given priority to convert. The first batch of such schools converted in the school year beginning in September 2010. In this paper, we study the impact of primary school conversion, three to four years after academisation took place.

This policy has been introduced in an international context where publicly-funded, autonomous schools have become a more familiar school improvement policy, most notably through charter schools in the US, free schools in Sweden and secondary academies in England. Evaluations of the former tend to find achievement gains associated with charter status and with the 'injection' of charter school features to public schools.[2] Evaluations of

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

[2] However, there is some controversy within the literature. Recent experimental evaluations of charters in or near particular US cities (Boston and New York) find positive impacts on educational achievement (see Abdulkadiroglu et al. 2011, 2014; Angrist et al. 2013, 2016; Dobbie and Fryer 2011; Hoxby and Murarka 2009). Wider coverage non-experimental evaluations produce more mixed results (Center for Research on Education Outcomes, 2009). On the injection of charter school features to public schools in Houston, and their beneficial effects, see Fryer (2014).

the Swedish free schools find some short-term effects, but no evidence of medium to long-term effects (see Bolhmark and Lindahl, 2015).

The policy studied here is different from many others in the literature in three respects: (1) it involves conversion of existing schools rather than the creation of new schools;[3] (2) it is about the voluntary conversion of more highly performing schools and not the forced conversion of failing schools. The former tend to have lower proportions of children from disadvantaged backgrounds; (3) we focus on effects for young children (aged 11) who attend quite small schools (compared to secondary schools). Although there have been evaluations of elementary schools in the charter school context, this appears to be less often the subject of evaluation than middle and high schools. Furthermore, the policy being studied here was in no way anticipated by schools or parents, which gives us leverage to identify causal effects.[4]

In the English context, academy schools operate outside local authority control, having autonomy to operate in areas such as hiring and pay of teachers, schools admissions (subject to national rules), curriculum (subject to some conditions), and decisions about the length of the school day and term. Many of these areas fall within the sphere of process and personnel decisions which are claimed in some economics of education research to exert 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). However, the context was one in which a couple of

[3] Although this is also the case for Clark (2009) or Eyles and Machin (2015) for English secondary schools and to Abdulkadiroglu et al. (2014) for schools in New Orleans.

[4] The proposal that the opportunity to become academies be extended to primary schools was first raised in the Conservative Manifesto that was published in April 2010 (one month before the election) and implemented soon afterwards. This came as a surprise at the time and, importantly for establishing causality, was well after initial schooling decisions were made by the cohorts considered in this analysis.

hundred (previously 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.

Better performing schools have done well within existing structures (by definition) and small schools may not have the expertise to cope with the increased responsibility that goes along with academy status. For example, they can no longer rely on Local Authorities for finance and accountancy. They need to report company accounts (like private companies). Governors (who are unpaid) are classed as trustees or directors of the company sitting on boards (rather than on a committee). Heads are personally responsible to the governing body for upholding good accounting standards. Thus, the management and reporting requirements of becoming an academy are not to be taken lightly and small schools may not necessarily have the range of expertise within their organisation. Schools are given a £25,000 grant to support the conversion process. However, in recognition of some of the difficulty of taking on 'back office' functions and the potential for economies of scale, there is encouragement and incentives to convert in chains or undertake some post-conversion collaborative arrangement with other schools.

In fact 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, the UK government have 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 government vision is now to encourage, but not make it compulsory, that all schools should 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 (although they are much smaller than secondary schools). 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.

To evaluate the impact of academy conversion, we first select a treatment and control group of schools. The treatment group consists of primary schools that converted to academy status between 2010 and 2012. The control group are those that converted, but after the end of our sample period (2015 and 2016). We show that these treatment and control groups have similar pre-trends in outcome variables. Further, we study performance effects for pupils who were already enrolled in the primary school prior to conversion (and who had completed their first major assessment at age 7) and who were affected by academy conversion for their remaining primary schooling, which finishes at age 11 with national tests in English and maths. This legacy enrolment strategy mirrors that used in Eyles and Machin (2015) in their evaluation of the first underperforming 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. (2014) who study school takeovers in New Orleans, referring to pupils who stay in converting schools as 'grand-fathered' pupils.

The importance of estimating effects for pupils who were already enrolled in the school prior to conversion is that 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, a very strong first stage estimate (of the effect of academy status on the probability of pre-conversion

enrolment) suggests that our effect is estimated for the majority of eligible pupils in the school.

The rest of the paper is structured as follows. In Section 2, we present a brief summary of related studies. In Section 3, we discuss primary education in England and how academies have been introduced. In Section 4, we describe the data and research strategy. In Section 5, we describe our main results and explore mechanisms in Section 6. We conclude in Section 7.

2. Related Literature – a Brief Summary

This paper follows on most directly from Eyles and Machin (2015) who studied the effect of academy conversion on the first schools to become academies in England in the early 2000s.[5] These were about 200 secondary schools, most of which were converted to academies because they were underperforming. As discussed in Eyles et al. (2016a), the characteristics of predecessor schools were very different to schools that became academies from 2010 onwards (from which time there was also a massive expansion in the number of secondary academies, with over a thousand schools converting within a short period). The earlier programme very much focused on disadvantaged inner city schools, serving very deprived communities. However, within a year of the new government, schools that were allowed to convert to academy status were much higher up the national test score distribution. They also had much fewer students eligible to receive free school meals. As discussed below, this change in the characteristics of schools that became academies has to do with government

[5] An earlier English study that looked at the impact of schools changing status is by Clark (2009). He looked at whether students in secondary schools that became grant-maintained (in the late 1980s/early 1990s) performed better after their change in status. Although interpreted as a result of the increase in autonomy, the setting is very different to that considered here.

policy on what schools could be fast-tracked for conversion. Thus, the current paper applies a similar methodology, but in a completely different setting.

The literature on charter schools in the US is relevant to our research because charter schools are also publicly funded schools that have operational autonomy from local and central government. However, most charter schools (like free schools in Sweden) are new schools rather than conversions. Most of the literature on charter schools that use a lottery design find positive effects on attainment for pupils who 'win' a place at a charter school relative to those that do not (e.g. Abdulkadiroglu et al, 2011, Angrist et al., 2010, Angrist et al., 2013, Dobbie and Fryer, 2011, Dobbie and Fryer, 2013 and Hoxby et al. 2009) and on longer run outcomes such as college attendance (Angrist et al., 2016, and Dobbie and Fryer, 2014). An exception is Gleason et al. (2010) who does not find evidence of average effects but does find evidence of improvements for disadvantaged students. However, literature using quasi-experimental designs has not found evidence of positive average results (Betts et al. 2006; Dobbie and Fryer, 2016; CREDO, 2009, 2013).

Differences in the results between types of study might be for methodological reasons – for example, Dobbie and Fryer (2013) show that non-experimental methods lead to downward bias in the context of New York schools. It might also be because of different socio-economic contexts considered by different types of study. Most studies using a lottery design are based on charters serving disadvantaged children in urban areas and must also be based on over-subscribed schools (for the lottery design to work). Many of the studies using a lottery design are based on middle or high schools (exceptions include Dobbie and Fryer (2011, 2013), Hoxby et al. (2009), who also include elementary schools in their analysis). One way in which our study differs from much of this literature is that we are focusing on better-performing primary schools (not mainly serving disadvantaged students) that voluntarily convert to a structure (academies) which allows them more autonomy.

In respect of school conversion rather than creation of a new school, our study is related to the literature on schools that convert to charters, and the introduction of practices used in charters to US public schools. With regard to the former, Abdulkadiroglu et al. (2014) study the effect of conversions to charters in Boston and New Orleans. Being enrolled in a school the year prior to conversion makes a pupil eligible to be 'grandfathered' into the new school. Legacy enrolment is used as an IV for years of enrolment in a charter school and they find evidence of large positive effects on test scores. Fryer (2014) examines the introduction of practices used in charters to public schools in Houston. Although these practices are assigned randomly to schools, students can select out of the treatment sample. To mitigate this concern, Fryer (2014) uses assignment to treatment as an instrumental variable for actual attendance at a charter. He finds evidence for strong positive effects on pupil performance because of mechanisms such as increased use of school time, the use of high dosage tutoring (administered by effective staff), the use of data in informing instruction practices and a culture of high expectations.

Much of this literature focuses on the effects of autonomy (in various guises) in communities that are very disadvantaged. The study setting of this paper offers an opportunity to evaluate whether such autonomy is advantageous in a more prosperous setting, where schools do not have intakes that are disproportionately from disadvantaged backgrounds.

3. Primary Education in England and Academies Policy

In England, children start school 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.[6] Schooling

[6] 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

in England is organised into Key Stages. At the end of Key Stage 1 (age 7), pupils are assessed by their teachers in English, Science and Maths according to national guidelines. At the end of Key Stage (age 11), they undertake national tests in English and Maths.[7] These tests are used to construct Performance Tables for primary schools, which are publicly available. There is essentially 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's main functions in relation to primary schools are in building and maintaining schools, allocating funding, 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. 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 also offer a number of administrative and management functions including training, personnel and financial services. Up until 2010, the majority of pupils (67%) attended 'Community Schools' in which LEAs are also the statutory employer of school staff, owner of the buildings and the authority that manages student admissions. 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, of which proximity to the school is a common criterion.[8]

junior school; in these cases, the proportion of infant school attendees switching to the linked junior school is very high and we treat the two linked schools as though they were one single school.

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

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

Academy schools were first introduced to English education in the early 2000s. In general, these academies were a small number of secondary schools that had been underperforming. When a new government was elected in May 2010, there were 203 academies. Although 'free schools' and education reform were issues raised in the manifesto of the new government prior to their election, there was no mention of large-scale expansion of the academies programme.[9] The Academies Act of 2010 paved the way for schools to convert to academy status in much larger numbers.[10]

When a school becomes an academy, it is governed outside the Local Authority and is overseen and funded directly by central government. A 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 individual academy are very important (National Audit Office, 2012), especially given that oversight is no longer provided by the Local Authority. 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). As funding comes directly from the government rather than through the Local Authority, they have greater freedom on how to use the budget allocation (i.e. nothing is

[9] Free schools are completely new schools that can be set up by interested parties (e.g. parents). There are now about 400 free schools open or approved.

[10] 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. We do not evaluate the effect of academisation on such schools (which are closer to the original New Labour academies, evaluated by Eyles and Machin, 2015).

withheld by the Local Authority to fund central services), though 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, the Department of Education adopted a phased approach to the criteria for schools wishing to convert (National Audit Office, 2012), prioritising better performing schools. A key component of this decision is the report by the Schools Inspectorate (Ofsted) that visits schools every 3-5 years and rates schools on a four point scale ranging from 'outstanding' to 'inadequate'. At the time, about 20% of schools were rated as 'outstanding' and 50% as 'good'. The 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, this was extended to all good schools with outstanding features. There was also an opportunity for any school to convert (irrespective of Ofsted grade) if it joined an academy trust with an excellent school or an education partner with a strong record of improvement. Then in April 2011, the criteria was 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. Figure 1 shows the huge rise in the number of primary school academies in England between 2010 and 2015.

Schools are encouraged to convert in a chain or partnership because '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). There are a variety of models that can be adopted to work collaboratively where the most formal is a multi-academy trust wherein all schools are governed by one trust and board of directors.

An alternative is an umbrella trust where academy schools work together while still retaining a certain level of independence and individuality. The third main model is a collaborative partnership, which is a looser arrangement between schools, with no shared trust or formalised governance structure. In 2013/14, about two-thirds of primary academies were in a multi-academy trust.

4. Data and Methodology

Data

The National Pupil Database (NPD) is a census of all pupils in the state system in England. During the primary phase of education, this accounts for the vast majority of children. The NPD includes basic demographic details of pupils – such as ethnicity, deprivation (measured by whether they are eligible to receive free school meals), gender, and whether or not English is their first language. We also know the school attended and can link this information 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 we can track students as they progress through the school system.

As discussed in Section 2, the national curriculum in England is organised around 'Key Stages', the first two undertaken in primary school (at ages 5-11) and the second two in secondary school (at ages 11 to 16). At the end of Key Stage, head teachers have a statutory duty to ensure that their teachers comply with all aspects of the Key Stage 1 assessment and reporting arrangements. The assessments are in reading, writing, speaking and listening, mathematics and science. Local Authorities (and other recognised bodies) are responsible for moderation of schools. Thus, although teachers make their own assessments of students (and therefore are 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 age 7, students are given a 'level' (i.e. there is no test score as such). However, following standard practice, we

transform National Curriculum levels achieved in Key Stage 1 assessments into point scores using Department for Education point scales and control for these scores in our regressions.

At the end of primary school (or the Key Stage 2 phase of education), pupils take national tests in English and maths, which are externally set and marked. These are the outcome variables used in our analysis.

Methodology

We are interested in evaluating the effect of academy conversion on pupil achievement in national tests taken by pupils at the end of primary school, i.e. Key Stage 2 (KS2) when they are aged 11. A conversion event E(t = c) is defined as occurring in the school year *t* that the school converts to an academy.

The approach we adopt is to compare what happens before and after conversion compared to a set of control schools and so it is important to discuss the dimensions of the research design that enable us to interpret our estimates as causal. Firstly, we consider pupils who are already enrolled in the school and who have completed Teacher Assessments at Key Stage 1 (i.e. age 7) *before* the schools converts as an academy. This ensures that academy conversion is exogenous to the enrolment decision and enables us to control for a measure of prior attainment. Secondly, we limit the event study on pupil performance 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 Year 2 of primary school (when KS1 assessments are taken) could have up to four post-conversion years of education in the academy (i.e. since their full enrolment in the school runs from $E = c\text{-}1$ up to $E = c+3$). Similarly children affected by conversion when enrolled in the predecessor school in Year 3 could have up to three conversion years (to $E = c+2$), and so on for children in Years 4 and 5 in the predecessor school.

Incorporating these features into a research design enables us to estimate the causal impact of being in an academy. We use administrative data that follows pupils through their school careers to estimate the impact on Key Stage 2 performance by means of the following value added equation:

$$KS2_{ist} = \alpha_s + \alpha_t + \theta_1 A_{ist} * I(E \geq t = c) + \sum_{j=1}^{J} \pi_{1j} X_{jist} + \varphi_1 KS1_{ist} + v_{1ist} \quad (1)$$

where X denotes a set of control variables and where we restrict analysis to pupils enrolled in the pre-conversion school. However, not all pupils who end up taking their KS2 test at a school that becomes an academy (i.e. $A_{ist} = 1$) were enrolled at the school pre-conversion. Conversely, some students who were initially enrolled in a school that converted to an academy ($ITT_{ist} = 1$) leave the school and take their KS2 tests elsewhere. Thus ordinary least squares estimates of θ_1 from (1) will not reflect a causal estimate. In the Appendix, we document the structure of the ITT groups, presenting the sample accounting used in our research design.

Denoting the variable indicating treatment by an academy conversion as $Z_{ist} = A_{ist} * I(E \geq t = c)$, the approach we adopt accounts for selection into and out of treatment by using intention to treat status (ITT_{ist}) as an instrument for Z_{ist}, to estimate a local average treatment effect (LATE) in the following set-up:

$$Z_{ist} = \alpha_s + \alpha_t + \theta_2 ITT_{ist} * I(E \geq t = c) + \sum_{j=1}^{J} \pi_{2j} X_{jist} + \varphi_2 KS1_{ist} + v_{2ist} \quad (2)$$

$$KS2_{ist} = \alpha_s + \alpha_t + \theta_3 ITT_{ist} * I(E \geq t = c) + \sum_{j=1}^{J} \pi_{3j} X_{jist} + \varphi_3 KS1_{ist} + v_{3ist} \quad (3)$$

In the first stage, (2), estimates of θ_2 show the proportion of the ITT group that stay in the academy and take KS2 tests there. Equation (3) is the reduced form regression of KS2 on the

instrument. The instrumental variable (IV) estimate is the ratio of the reduced form coefficient to the first stage coefficient, θ_3/θ_2.

Extending this IV setting to the event study framework we are able to estimate separate estimates for the four years from conversion onwards (E = c to c+3) using four instruments for whether a pupil is ITT for event year c, event year c+1 and so on.[11]

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.[12] 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 we use pupils attending future converters - schools that convert in the 2014/15 and 2015/16 academic years - as a control group in a difference-in-differences setting. As we have two waves of treatment (i.e. schools converting in 2010/11 and 2011/12), we use a slightly different sample as a control group for each. In our control group for the first wave, we restrict attention to pupils who were in schools that convert in 2014/15 and 2015/16 but would have been ITT had those schools converted in 2010/2011. We do the same for the second wave (but use 2011/2012 to define ITT) and pool the estimates together. For all pre-conversion years the control groups are identical - they are Year 6 pupils who sit their KS2 assessments in schools that converted to academy status in 2014/15 or 2015/16.

[11] Formally, an individual enrolled in a treatment school in event year c-i and academic year group k is, for instance, intention to treat for c+1 if c-i + (6-k) is equal to c+1, where 6 is the academic year group in which KS2 exams are sat. The binary instrument in equation (4) is equal to 1 only if any one of the four instruments used for the event study equals 1.

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

This approach is legitimised in the empirical findings we describe below where there are no differential pre-conversion trends in the same school years. Thus the data structure we use is a balanced panel of schools for the school years 2006/07 to 2013/14 with repeated cross-sections of Year 6 pupils.

5. Results

Balancing Tests

Table 1a shows the extent to which treatment and control groups are balanced at baseline (2006/07) for the full sample of treatment and control schools. There is a significant difference with respect to KS2 and KS1 scores prior to the policy, with treatment schools being better performing. Treatment schools also have a slightly lower proportion of pupils eligible to receive free school meals. This is not so surprising as the government prioritised better performing schools for conversion to academy status (as discussed above). If we look at the position of schools in the distribution of all schools in 2010, we see that treatment schools were higher up the distribution than control schools. Treatment schools were at the 64^{th} and 68^{th} percentile in KS2 English and maths respectively. Control schools were at the 52^{nd} and 51^{st} percentile in English and maths respectively.

However, treatment and control schools are more balanced when we look within school inspection grades (as defined by Ofsted in 2007-10). In fact, as Table 1b shows, within each category there are no statistically significant differences at baseline between treatment and control schools for any baseline characteristic. Furthermore, they come from similar parts of the KS2 test score distribution in 2010. Thus, we estimate regressions for schools within each Ofsted grade, as well as for the pooled sample.[13]

[13] We put schools with satisfactory and unsatisfactory grades together as there are so few of the latter. When estimating the pooled regressions over all schools, all variables are interacted with Ofsted grade.

Main Results

Table 2 shows estimates of the ITT and 2SLS specifications, including all controls. Coefficients are shown by wave and for the pooled sample. Columns 1-6 show estimates when the treatment is whether the school converts to academy status. Columns 7-12 show estimates for years of exposure. The first stage estimates are 0.91-0.93 in all cases, showing that the vast majority of legacy enrolled pupils stay in the school to take their KS2 exams. The regression estimates show a precisely estimated zero effect. On average, academisation had no impact on pupil performance for up to 3-4 years of exposure. Table 3 shows effects within schools of a particular Ofsted grade (i.e. outstanding; good; and satisfactory/unsatisfactory). However, the coefficients tell a similar story. Pupils did not benefit from attending an academy school irrespective of their predecessor school's initial Ofsted grade.

One might be concerned about the research design being potentially contaminated by differential pre-policy trends. Figure 2 therefore shows an event study for pupils from 4 years prior to academy conversion to 3 years later. Effects are seen to be precisely defined and zero on average for every single year (although confidence intervals widen in c+3). There is no sign of pre-policy trends or a gradual improvement in results post-conversion. Figures 3 and 4 show that this is also true for pupils in schools within each Ofsted grade.

Table 4 shows estimates for 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.

One might also wonder if there is much heterogeneity in the effect of the treatment within groups of schools. We have explored this by estimating a separate treatment effect for

each school within categories of Ofsted grade. The results are illustrated in Figure 6 for schools classified as outstanding, good and satisfactory/unsatisfactory. In each case, the distribution of estimated treatment effects is quite strongly concentrated around zero, especially for 'good' schools, in the middle chart, for which we have the highest number of observations.

Falsification Test

To do a falsification test, we alter the year in which each academy school converted to four years prior to when they actually did. This ensures that there is no overlap between those that are actually treated and those that receive this 'fake' treatment. We then estimate regressions for 2003-2010 rather than 2007-2014 (i.e. the sample used in our main analysis). All the treatment and ITT variables are defined for the 'fake' conversion year so that pupils who attend Wave 1 and Wave 2 treatment schools are considered as 'treated' after 2007 and 2008 respectively. Results are shown in Figure 5 and show that all estimated effects are zero. Thus, this placebo test offers additional evidence of there being no differential trend in treatment and control groups in the pre-policy period.

6. Did Academies Change Their Modes of Operation?

The empirical evidence presented in the previous section makes it clear that we can harness no evidence of pupil improvements in primary schools that became academies. This finding of a zero effect might arise from schools not having changed at all or from schools changing in ways that did not benefit students. In this section, we therefore consider three aspects of possible change. The first is whether primary schools took up the option to exercise the many academy freedoms that became available to them from increased autonomy post-conversion. The second is whether they changed their patterns of expenditure and the third is whether they changed their staffing and management structures.

Use of Academy Freedoms

There have been various largely descriptive investigations into whether schools have used their academy freedoms (e.g. Academies Commission, 2013; Cirin, 2014). The Academies Commission (2013) concluded that take-up of freedoms had been 'piecemeal rather than comprehensive', in part because changes take time to be implemented and in practice require consultation (e.g. changing the length of the school day). Other hypotheses with some anecdotal support included potential risk aversion and insufficient skills of school leaders and that not all schools saw the need to innovate. Surveys of recent converters by Bassett et al. (2012) and Cirin (2014) found that financial motives were 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 funding as you see fit' was the most commonly cited reason for conversion (cited by 83% of respondents).

Importantly for our analysis, Cirin (2014) reports change resulting from the exercise of academy freedoms to be more common in secondary than in primary schools. This is illustrated in Table 5, taken from his survey of 720 academies which were open on 1 May 2013. This shows that for most categories, fewer than half of primary schools had made the changes suggested. In every case, the proportion of primary schools having made a particular change is smaller than the proportion of secondary schools making that change. In some areas (e.g. increasing the length of the school day; changing the length of school terms and hiring teachers without qualified teacher status), there are very few schools reporting having made a change. At the same time, this Table shows that quite a lot of primary academies did make some changes. Furthermore, Cirin (2014) reports that two-thirds of academies believe that the changes they made have improved attainment.

Changes in Patterns of Expenditure

The academy freedom studies suggest that the financial motive to convert was important. In Table 6, we therefore look at income and expenditure before and after conversion for our treatment and control schools using administrative data on school income and expenditure. 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. 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 our analysis. Secondly, academies opening between March and August have the option of filing a return that exceeds 12 months, but is less than 18 months. For some years we have information on whether schools take up this option and find that few do. However, the schools that do take up this option do so in a predictable way: for example, August converters file a 13 month return, July converters file a 14 month return and so on. We are able to use this information to impute expenditure and income data into a 12 month pro rata number, for those schools opening between March and August, when we do not know the true length of the return.[14] Throughout, however, we also show results for the sub-sample that report 12 month finances, with few differences emerging. Finally, as our main focus, we look at expenditure shares to see if academies alter their modes of spending. Considering shares is not affected at all by varying reporting periods as the number of months in the return is the same for total incomes/expenditures and their components.

In columns (1) and (4) of Table 6, we see that the 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 £3,917 and £4,138 per pupil

[14] Further details are given in the Appendix.

respectively. Total expenditure was £3,883 and £4,107 per pupil in treatment and control schools respectively. These pre-conversion numbers are even more closely aligned for the comparisons within the Outstanding, Good and Satisfactory/Unsatisfactory groups of schools, shown in Panels B to D of the Table.

It is evident, however, that the converting primary schools both got more money and spent more money post-conversion. The Table also shows the income and expenditure per pupil after conversion and is converted into a difference-in-difference estimate in the final column of the Table. This shows significant income and expenditure gaps arising after conversion relative to what happened in the control schools at the same time. The differences in total income and expenditure are estimated as £624 and £527 per pupil per year. The increases are very clearly driven by the relative increase in grant income. This is what one would expect since because of academy conversion, schools received their funding directly from the government. Formerly, about 10% of their income would have been top-sliced by the Local Authority for central services. A similar qualitative pattern is shown for schools classified as Outstanding, Good and Satisfactory/Unsatisfactory, but with higher income and expenditure shown for the latter schools most likely reflecting a higher proportion of disadvantaged students in this group.

In Table 7, we show the change in categories of expenditure per pupil before and after conversion. There are two Panels, the upper one for the full sample, and the lower one for the sample of schools that report 12 month accounts. They look very similar and show that the bulk of the relative increase in per pupil expenditure is being used for non-teaching staff and running costs of the school.

Table 8 shows expenditure shares, again for the full sample in the upper Panel and for 12 month return academies only in the lower panel. They are extremely similar and make it very clear that even though primary academies spent more within each category after

conversion (relative to control schools), the relative share spent on 'frontline services' such as teaching staff declined (with a small or negligible change in learning and ICT resources) whereas the relative share spent on administrative costs (i.e. non-teaching staff and other running costs) increased. This is true for schools of all Ofsted categories.

In Figure 7, we plot estimated treatment effects (for each school) against the change in grant funding within those schools. We do this separately for each group of schools classified as Outstanding, Good and Satisfactory/Unsatisfactory. For each group, there is no obvious relationship between the estimated effects of academy conversion (on pupil attainment) and the change in grant funding. Given that the bulk of the financial gain from academy conversion was subsumed into activities formerly undertaken by the Local Education Authority, this is not very surprising.[15]

Changes in Leadership

Eyles and Machin (2015) found change in leadership to be an important mechanism behind the positive effects found for conversion of early secondary Academies. In the upper panel of Table 9, we consider whether this mechanism is at work for primary academies. We estimate a difference-in-difference regression and show the effect of academy conversion on the probability of a change in the head teacher. We estimate these regressions for all schools (column 1) and then within each category of school (Outstanding, Good, Satisfactory/Unsatisfactory). In all cases, the estimates are small and not statistically different from zero.

Changes in Teaching Staff

The lower panel of Table 9 shows difference-in-difference estimates of staff turnover; turnover is measured as being the number of the previous year's staff who are no

[15] In contrast, Holmlund et al (2010) and Gibbons et al. (2011) find positive effects from general increases in pupil expenditure in primary schools in England.

longer at the school weighted by the total number of staff in the previous year. The estimates in Table 9 measure turnover amongst fixed term and permanent staff, although the results are statistically indistinguishable when part-time staff are included. Similarly to head teacher turnover, there is no evidence of primary academies changing their staff differentially from non-academies upon conversion.

Summary

Taken together, these findings suggest that primary schools did change some aspects of their operation and expenditure after becoming academies even though there was not much change in the school personnel. However, they did not change radically post-conversion in any way likely to directly improve pupil performance. Although extra resources were made available to them, the changes they actually made were mainly to affect their administrative functioning and day to day operations.

7. Conclusion

The English government has radically restructured its school system under an assumption that academisation delivers benefits to schools and students. We study the totally unexpected change in direction that occurred in 2010 when policy was changed to let primary schools become academies. We consider the first primary schools to become academies in England (between 2010 and 2012) and find no evidence of pupil performance improvements resulting from conversion.

How should we interpret a zero effect in the light of 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. Another reason may be that these schools are not serving disadvantaged communities and therefore not making changes that will make a

difference to average or higher-performing students. 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 (i.e. on average they were in the 64th-68th percentile of national test scores prior to conversion), 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 our 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). Finally, one of the key models for some successful urban charters in the US and some secondary schools in England[16] – an effective discipline approach for academies and the No Excuses model of charters – is simply not relevant to the age range of children enrolled in English primary schools. In the light of all these factors, it is not surprising that there has been no effect on pupil performance.

One might argue that if academisation has no 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 schools in England in this way seems excessively costly if there are no gains for students. Furthermore, risks are also posed by an increasing number of schools becoming academies. 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 of Local Authority control undermines services that the Local Authority is able to provide to other schools in the same geographic

[16] Probably the best known of the latter is Hackney's Haggerston School which 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.

area (e.g. child psychologists to support children with special needs in many schools). On the other hand, it might be the case that academisation has beneficial effects on certain groups of students within these schools, or that effects are positive if schools work together (e.g. in the context of multi-academy trusts). These are some of the issues that need to be investigated in 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 (2014) Charters Without Lotteries: Testing Takeovers in New Orleans and Boston, American Economic Review, forthcoming.

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/

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.

Dobbie W. and R. Fryer (2016) Charter Schools and Labor Market Outcomes. Working Paper. Harvard University.

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, 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, 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.

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.

Holmlund, H., S. McNally and M. Viarengo. (2010). Does Money Matter for Schools? Economics of Education Review. 29, 1154-1164.

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

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

Figure 1: Number of Primary School Academies in England

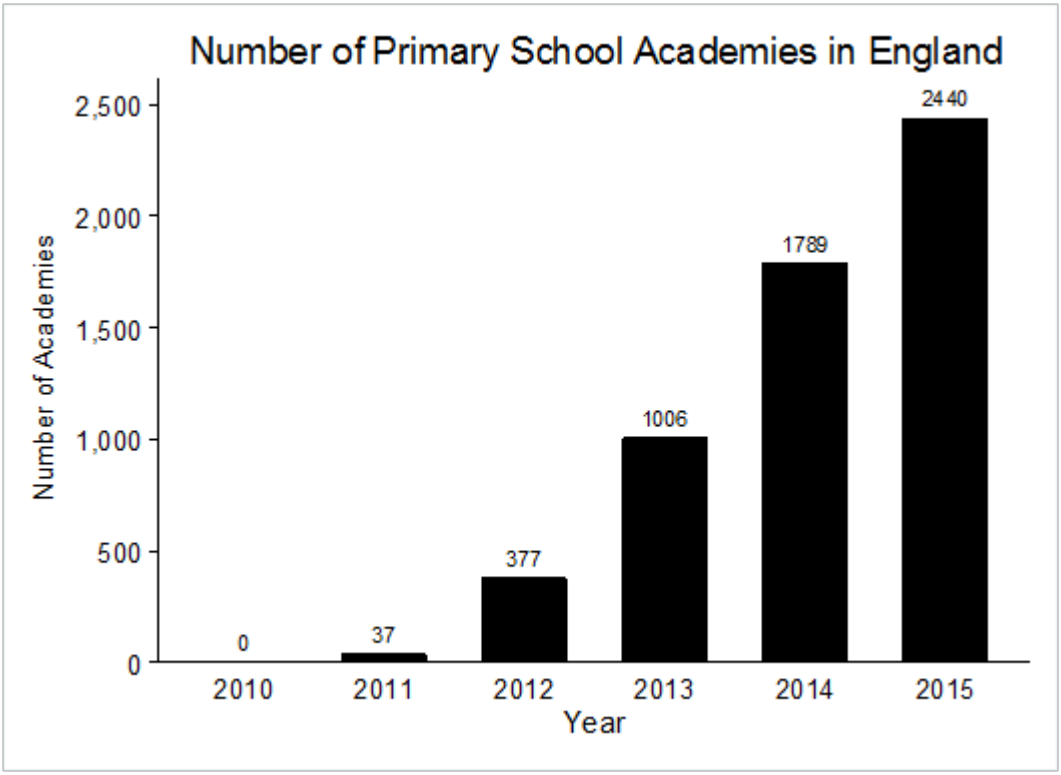

Figure 2: Event Study Estimates, Pre- and Post-Academy Conversion

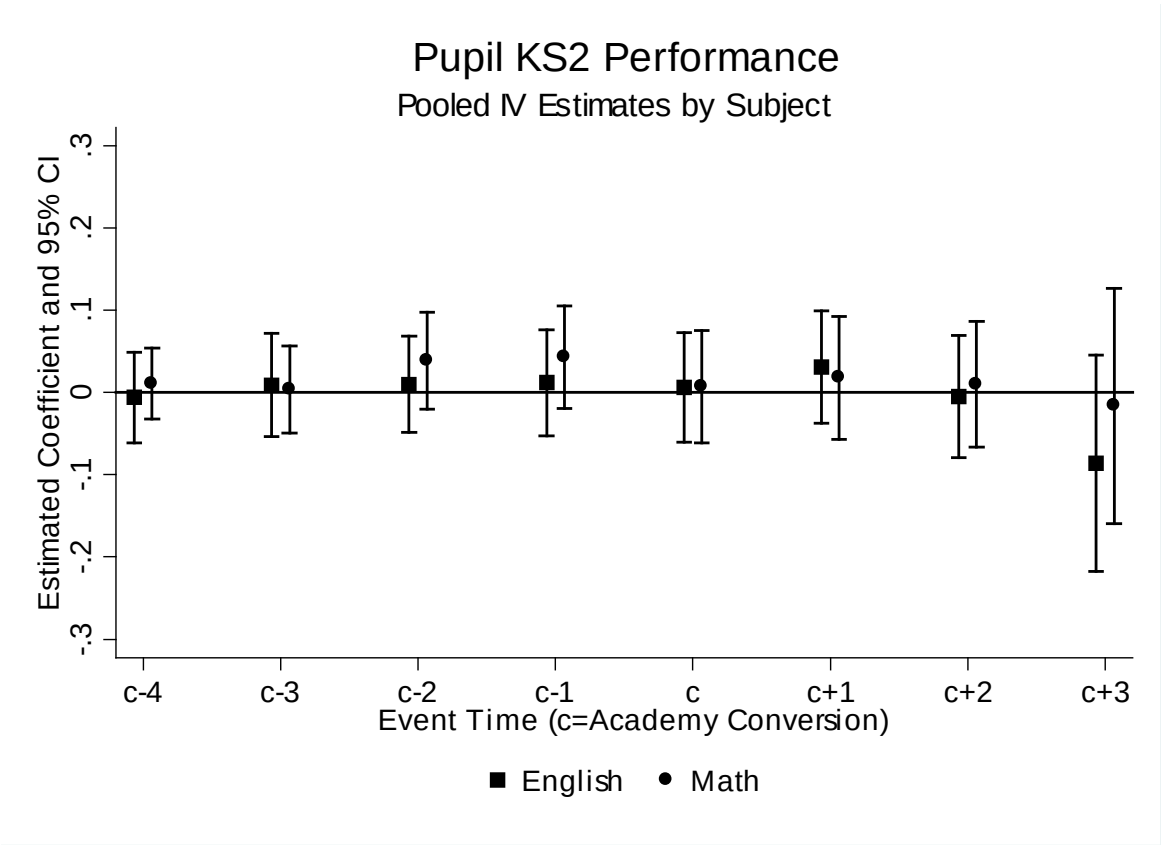

Figure 3: Event Study Estimates by (Pre-Intervention) Ofsted Grade, Maths

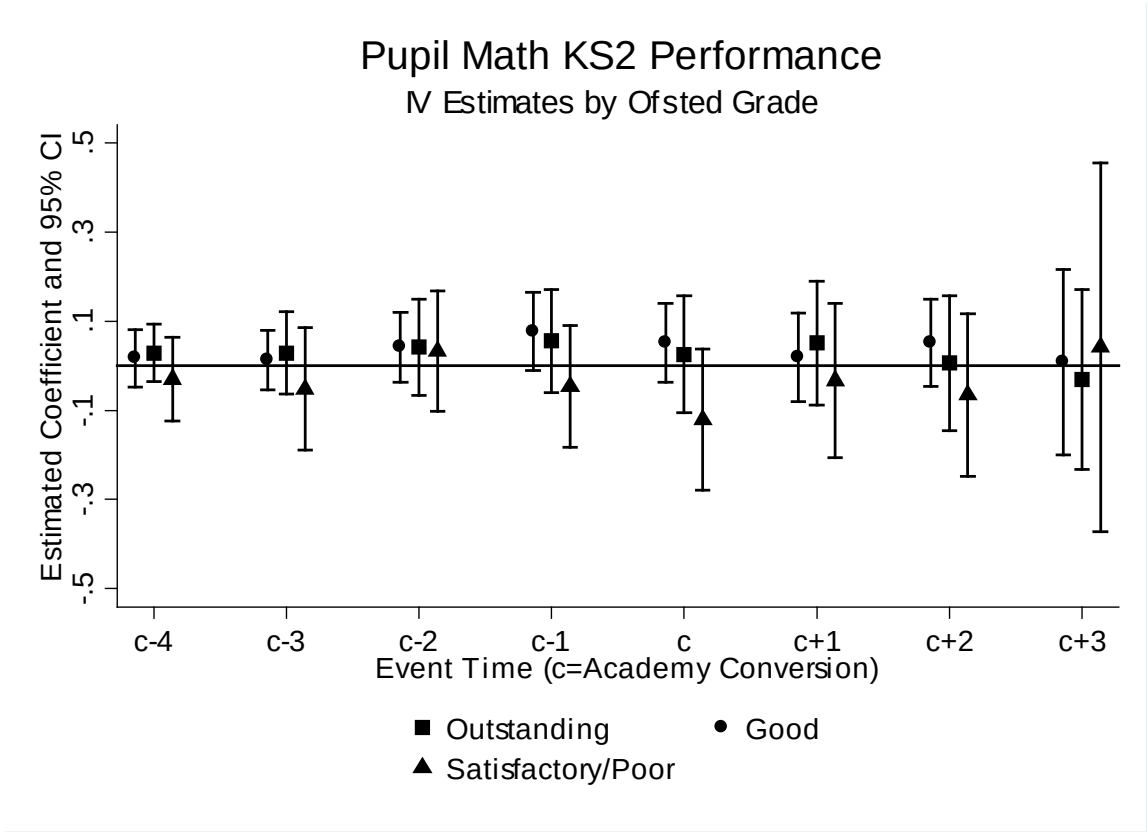

Figure 4: Event Study Estimates by (Pre-Intervention) Ofsted Grade, English

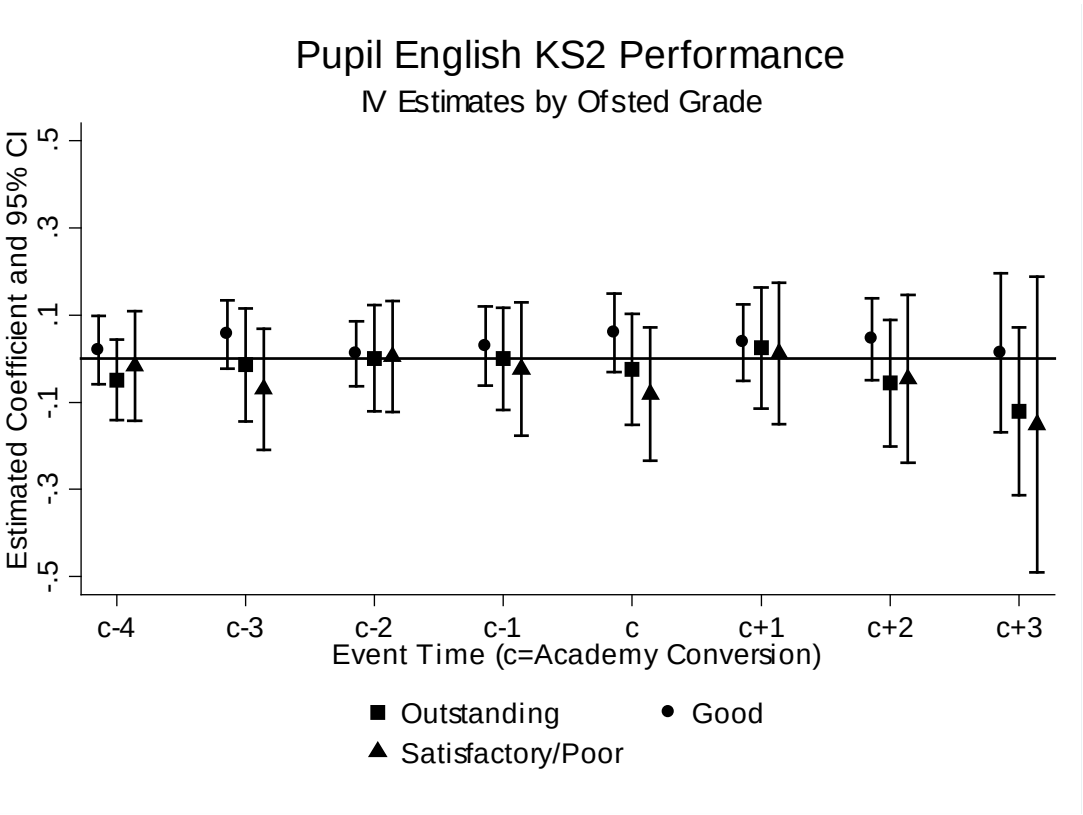

Figure 5: Falsification Test

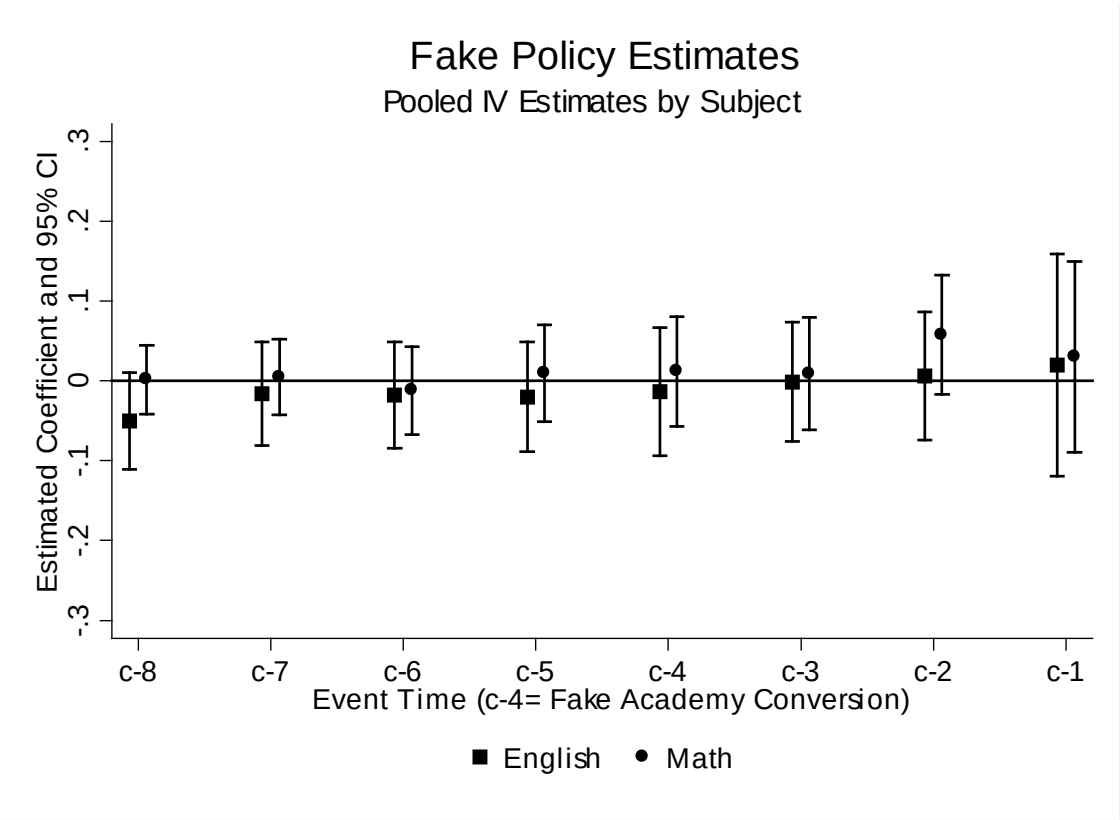

Figure 6: Heterogeneous Treatment Effects

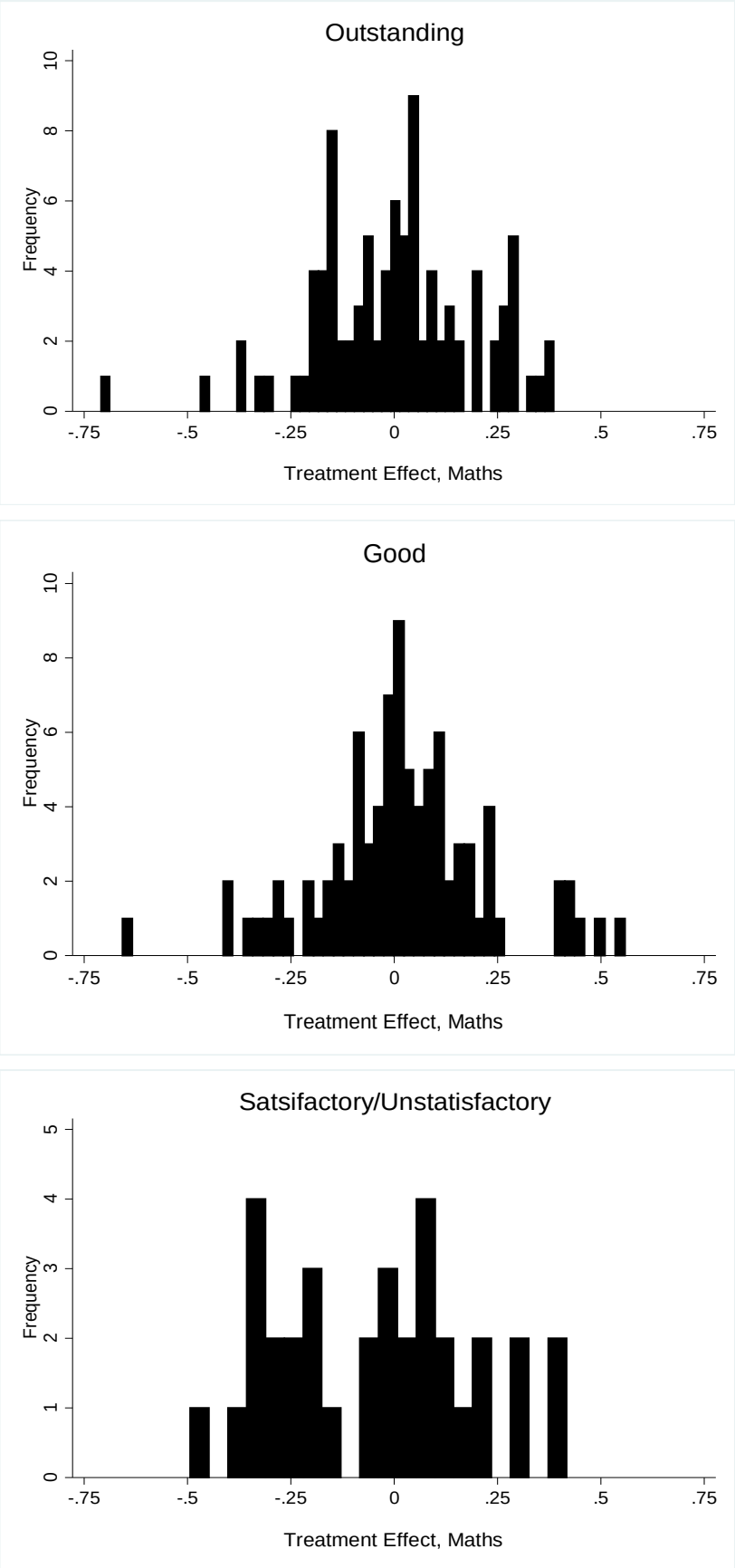

Figure 7: Estimated Treatment Effects and Changes in Grant Income

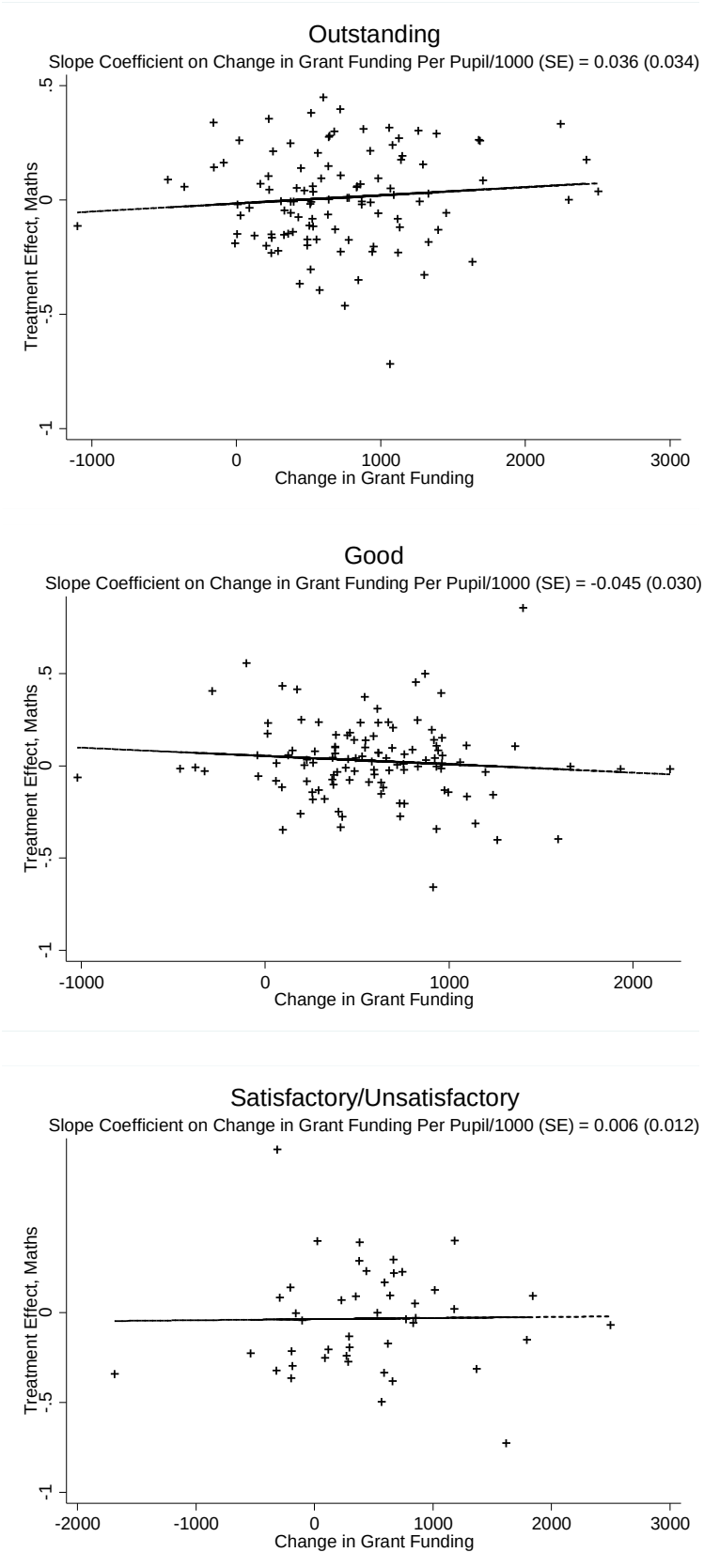

Table 1a: Baseline Characteristics: Pooled Sample and by Treatment Wave

	All treatment and control schools			Wave 1 treatment v comparison			Wave 2 treatment v comparison		
	Treatment	Control	p-value	Treatment	Control	p-value	Treatment	Control	p-value
Number of schools	270	395		46	395		224	395	
English is first language	0.92	0.93	0.543	0.91	0.93	0.361	0.93	0.93	0.763
White British	0.83	0.85	0.347	0.83	0.85	0.577	0.83	0.85	0.401
Eligible to receive free school meals	0.10	0.12	0.049	0.13	0.12	0.644	0.09	0.12	0.013
Male	0.51	0.51	0.577	0.51	0.51	0.727	0.51	0.51	0.608
KS2 English	0.07	-0.06	0.000	-0.04	-0.06	0.698	0.10	-0.06	0.000
KS2 Maths	0.09	-0.08	0.000	0.03	-0.08	0.144	0.10	-0.08	0.000
KS1 English	0.61	-0.05	0.000	-0.07	-0.05	0.823	0.09	-0.05	0.000
KS1 Maths	0.05	-0.04	0.002	-0.07	-0.04	0.642	0.07	-0.04	0.000

Table 1b: Treatment v Control by (Pre-Intervention) Ofsted Grade

	Outstanding schools			Good schools			Satisfactory/unsatisfactory schools		
	Treatment	Control	p-value	Treatment	Control	p-value	Treatment	Control	p-value
Number of schools	108	51		115	188		47	156	
English is first language	0.91	0.91	0.865	0.94	0.94	0.875	0.94	0.93	0.766
White British	0.81	0.83	0.667	0.85	0.86	0.670	0.84	0.84	0.994
Eligible to receive free school meals	0.09	0.10	0.795	0.09	0.10	0.328	0.14	0.14	0.821
Male	0.51	0.49	0.070	0.51	0.51	0.688	0.49	0.51	0.205
KS2 English	0.25	0.24	0.807	0.01	0.01	0.971	-0.26	-0.26	0.954
KS2 Maths	0.28	0.23	0.426	0.04	0.03	0.649	-0.33	-0.30	0.720
KS1 English	0.13	0.06	0.197	0.04	0.00	0.299	-0.09	-0.15	0.376
KS1 Maths	0.10	0.02	0.147	0.06	0.02	0.288	-0.14	-0.13	0.910
Position in dist. of all schools (percentile)									
English	75th	79th		60th	65th		39th	38th	
Maths	72th	73th		58th	57th		38th	36th	

Notes: All variables are measured in the school year 2006/07. There are very few unsatisfactory schools in the sample (4 in the treatment and 18 in the controls). Hence we pool satisfactory and unsatisfactory schools together. 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 2007 and 2010. For all schools, the position in the percentile distribution in 2010 for the treatment group is 64th and 68th for English and Maths respectively. For the control group, this is 52th and 51st respectively. Treatment and control are more evenly matched within Ofsted grade.

Table 2: The Effect of Treatment on KS2 Test Scores (measured at age 11)

	ITT (Incidence)			2SLS (Incidence)			ITT (Exposure)			2SLS (Exposure)		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	2011	2012	Pooled	2011	2012	Pooled	2011	2012	Pooled	2011	2012	Pooled
Maths	-0.050	0.001	-0.011	-0.057	0.001	-0.012	-0.021	0.004	-0.005	-0.023	0.004	-0.005
	(0.053)	(0.022)	(0.023)	(0.058)	(0.023)	(0.025)	(0.019)	(0.009)	(0.010)	(0.021)	(0.010)	(0.011)
English	-0.048	0.018	0.000	-0.053	0.020	0.000	-0.023	0.008	-0.004	-0.025	0.008	-0.004
	(0.053)	(0.021)	(0.022)	(0.058)	(0.022)	(0.024)	(0.017)	(0.009)	(0.009)	(0.018)	(0.010)	(0.010)
School fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year dummies	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Sample size	126459	187481	313940	126459	187481	313940	126459	187481	313940	126459	187481	313940
Number of schools	441	619	665	441	619	665	441	619	665	441	619	665
First stage	0.915	0.937	0.932				0.910	0.937	0.928			
coefficient	(0.007)	(0.003)	(0.003)				(0.008)	(0.003)	(0.004)			

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, primary school). Standard errors are clustered at school level.

Table 3: The Effect of Treatment by (Pre-Intervention) OfSted Grade

	Outstanding				Good				Satisfactory/Unsatisfactory			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	ITT Incidence	2SLS Incidence	ITT Exposure	2SLS Exposure	ITT Incidence	2SLS Incidence	ITT Exposure	2SLS Exposure	ITT Incidence	2SLS Incidence	ITT Exposure	2SLS Exposure
Maths	-0.014	-0.015	-0.009	-0.010	0.008	0.008	0.002	0.002	-0.042	-0.047	-0.007	-0.008
	(0.046)	(0.049)	(0.019)	(0.020)	(0.028)	(0.030)	(0.012)	(0.013)	(0.053)	(0.058)	(0.023)	(0.025)
English	-0.014	-0.015	-0.011	-0.012	0.020	0.021	0.006	0.006	-0.018	-0.020	-0.004	-0.006
	(0.044)	(0.049)	(0.017)	(0.018)	(0.027)	(0.029)	(0.011)	(0.012)	(0.049)	(0.054)	(0.009)	(0.023)
School fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year dummies	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Sample size			73631				143156				97153	
Number of schools			159				303				203	
First stage	0.942		0.938		0.937		0.934		0.906		0.898	
coefficient	(0.004)		(0.005)		(0.004)		(0.005)		(0.007)		(0.010)	

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, primary school). Standard errors are clustered at school level.

Table 4: Effects by Year of Exposure

	All schools		Outstanding		Good		Satisfactory/Unsatisfactory	
	Maths	English	Maths	English	Maths	English	Maths	English
One year of exposure	-0.015	0.000	-0.010	-0.011	0.021	0.035	-0.010	-0.057
	(0.024)	(0.024)	(0.041)	(0.042)	(0.032)	(0.033)	(0.057)	(0.056)
Two years of exposure	-0.004	0.026	0.015	0.039	-0.013	0.010	-0.007	0.039
	(0.027)	(0.025)	(0.050)	(0.049)	(0.038)	(0.036)	(0.059)	(0.051)
Three years of exposure	-0.012	-0.011	-0.031	-0.042	0.017	0.018	-0.037	-0.020
	(0.030)	(0.028)	(0.059)	(0.055)	(0.038)	(0.035)	(0.070)	(0.066)
Four years of exposure	-0.042	-0.094	-0.073	-0.106	-0.033	-0.022	0.075	-0.121
	(0.069)	(0.063)	(0.088)	(0.083)	(0.099)	(0.084)	(0.207)	(0.165)
School fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year dummies	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Sample size	313940		73631		143156		97153	
Number of schools	665		159		303		203	
First stage coefficient on IIT	0.968		0.972		0.952		0.972	
x one year of exposure	(0.002)		(0.003)		(0.006)		(0.003)	
First stage coefficient on IIT	0.931		0.945		0.905		0.934	
x two years of exposure	(0.004)		(0.004)		(0.008)		(0.006)	
First stage coefficient on IIT	0.907		0.924		0.871		0.912	
x three years of exposure	(0.004)		(0.005)		(0.013)		(0.006)	
First stage coefficient on IIT	0.852		0.873		0.800		0.829	
x four years of exposure	(0.011)		(0.013)		(0.013)		(0.025)	

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

Table 5: Use of 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 6: 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 (256 Treatment, 393 Control)							
Total Income	3917	4738	822 (105)	4138	4335	197 (20)	624 (107)
Grant Income	3732	4478	746 (104)	3987	4164	177 (19)	569 (106)
Other Income	185	261	76 (17)	151	171	21 (4)	56 (18)
Total Expenditure	3883	4575	693 (102)	4107	4273	165 (20)	527 (104)
B. Outstanding (106 Treatment, 50 Control)							
Total Income	3792	4550	759 (68)	3887	4181	293 (80)	465 (104)
Grant Income	3594	4236	642 (56)	3744	4009	265 (81)	377 (98)
Other Income	197	314	117 (36)	143	171	28 (9)	89 (37)
Total Expenditure	3769	4423	654 (76)	3820	4061	241 (39)	413 (85)
C. Good (110 Treatment, 187 Control)							
Total Income	3893	4632	738 (74)	3974	4185	211 (19)	528 (76)
Grant Income	3709	4407	698 (70)	3821	4013	193 (18)	505 (72)
Other Income	185	225	40 (18)	153	171	18 (6)	22 (18)
Total Expenditure	3849	4472	623 (79)	3952	4136	184 (22)	439 (82)
D. Satisfactory/Unsatisfactory (40 Treatment, 156 Control)							
Total Income	4314	5532	1218 (624)	4417	4567	150 (38)	1068 (616)
Grant Income	4161	5314	1153 (625)	4267	4397	130 (35)	1023 (617)
Other Income	153	218	65 (30)	150	170	20 (8)	45 (30)
Total Expenditure	4277	5262	985 (591)	4387	4506	119 (41)	866 (584)

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.

Table 7: Changes in Expenditure per Pupil Before and After Academy Conversion

Schools Including Those With Adjustment For > 12 Month Accounts in First Full Conversion Year

	All Schools		Outstanding		Good		Satisfactory/Unsatisfactory	
	Pre-Change Mean	Difference-in-Difference	Pre-Change Mean	Difference-in-Difference	Pre-Change Mean	Difference-in-Difference	Pre-Change Mean	Difference-in-Difference
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Total teaching staff	2854	142 (70)	2674	95 (56)	2789	57 (58)	3049	367 (393)
Total non-teaching staff	455	167 (18)	407	149 (24)	435	180 (24)	512	159 (68)
Learning and ICT resources	218	21 (17)	231	-18 (34)	210	-4 (17)	222	113 (66)
Other running costs	525	196 (26)	481	187 (43)	495	207 (27)	592	227 (106)
Number of treatment schools	256		106		110		40	
Number of control schools	393		50		187		156	

Schools With 12 Month Accounts in First Full Conversion Year

	All Schools		Outstanding		Good		Satisfactory/Unsatisfactory	
	Pre-Change Mean	Difference-in-Difference	Pre-Change Mean	Difference-in-Difference	Pre-Change Mean	Difference-in-Difference	Pre-Change Mean	Difference-in-Difference
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Total teaching staff	2873	173 (123)	2679	120 (57)	2797	44 (38)	3058	505 (681)
Total non-teaching staff	460	158 (26)	412	160 (32)	434	162 (30)	514	125 (108)
Learning and ICT resources	218	0 (23)	238	-81 (33)	208	2 (22)	222	103 (101)
Other running costs	531	219 (40)	483	145 (56)	500	230 (30)	592	379 (174)
Number of treatment schools	125		47		56		22	
Number of control schools	393		50		187		156	

Table 8: Changes in Expenditure per Pupil Shares Before and After Academy Conversion

Schools Including Those With Adjustment For > 12 Month Accounts in First Full Conversion Year

	All Schools		Outstanding		Good		Satisfactory/Unsatisfactory	
	Pre-Change Mean	Difference-in-Difference	Pre-Change Mean	Difference-in-Difference	Pre-Change Mean	Difference-in-Difference	Pre-Change Mean	Difference-in-Difference
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Total teaching staff	0.708	-0.056 (0.007)	0.709	-0.046 (0.01)	0.713	-0.060 (0.009)	0.701	-0.066 (0.024)
Total non-teaching staff	0.111	0.025 (0.003)	0.107	0.023 (0.004)	0.109	0.029 (0.005)	0.115	0.016 (0.01)
Learning and ICT resources	0.054	0.000 (0.004)	0.060	-0.007 (0.006)	0.053	-0.003 (0.006)	0.050	0.011 (0.008)
Other running costs	0.128	0.031 (0.005)	0.125	0.030 (0.007)	0.125	0.035 (0.006)	0.133	0.039 (0.018)

Schools With 12 Month Accounts in First Full Conversion Year

	All Schools		Outstanding		Good		Satisfactory/Unsatisfactory	
	Pre-Change Mean	Difference-in-Difference	Pre-Change Mean	Difference-in-Difference	Pre-Change Mean	Difference-in-Difference	Pre-Change Mean	Difference-in-Difference
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Total teaching staff	0.708	-0.047 (0.007)	0.707	-0.025 (0.011)	0.713	-0.057 (0.007)	0.702	-0.066 (0.028)
Total non-teaching staff	0.111	0.021 (0.004)	0.107	0.024 (0.005)	0.109	0.025 (0.006)	0.115	0.001 (0.012)
Learning and ICT resources	0.053	-0.008 (0.003)	0.062	-0.022 (0.006)	0.052	-0.006 (0.005)	0.050	0.007 (0.01)
Other running costs	0.128	0.034 (0.006)	0.124	0.022 (0.009)	0.126	0.038 (0.006)	0.133	0.058 (0.022)

Notes: The categories are only fully available for schools with a minimum number of observations in each (minor) cell. The above table only uses schools for which all categories are non-missing. Total teaching staff includes: teachers, educational support staff and supply teachers. Total non-teaching staff includes administrative and clerical staff (the major category); administrative supply; bought-in professional services; indirect employee expenses, development and training, staff related insurance. Other running costs include premises, energy, rates, insurance, and bought in professional services (curriculum)

Table 9: Change in Head Teachers and Teacher Turnover

	Pr[Change in Head Teacher]			
	All Schools	Outstanding	Good	Satisfactory/Unsatisfactory
	(1)	(2)	(3)	(4)
Academy x Post-Conversion (E = c to c+3)	0.026	0.034	0.008	-0.020
	(0.026)	(0.048)	(0.038)	(0.070)
School fixed effects	Yes	Yes	Yes	Yes
Year dummies	Yes	Yes	Yes	Yes
Sample size	2815	725	1285	805
Number of schools	563	145	257	161
	Teacher Turnover			
Academy x Post-Conversion (E = c to c+3)	-0.018	-0.012	-0.026	0.006
	(0.009)	(0.014)	(0.015)	(0.019)
School fixed effects	Yes	Yes	Yes	Yes
Year dummies	Yes	Yes	Yes	Yes
Sample size	3150	745	1430	975
Number of schools	630	149	286	195

Notes: Based on data from the schools' workforce census for the academic years 2009/10-2013/14. The subsample is the sample of schools who are observed in each of the years and report having a head-teacher in each year. School level regressions where E denotes event year and c is the year of conversion. Robust standard errors (clustered at the school level) are reported in parentheses.

Appendix

This Appendix contains information on the way in which the sample of pupils and schools are selected for our analysis of primary academies and on issues related to the school income and expenditure data analysed in the paper.

1). Sample Accounting Structure

Table A1 describes the structure of the Intention to Treat (ITT) groups in both waves of academy conversions that we study. For Wave 1, our ITT pupils are those enrolled in the predecessor school in academic years 2-5 in 2009/10. For Wave 2, the ITT pupils are those enrolled in the predecessor school in academic years 3-5 in 2010/11.

The first column of each table gives the number of ITT pupils in treatment and control schools. The next set of numbers give the number of ITT pupils who remain in the treatment/control group. For instance, in the Wave 1 table, those who are pre-enrolled as Year 2 students in 2009/10 are expected to sit their KS2 tests in 2013/2014. Of the 2098 treatment students in the sample in 2009/10, 1824 are observed in the school four years later while 11483 of the control group students (out of 13402) remain in the control group four years later. Those who are pre-enrolled in later year groups have lower rates of attrition due to having less time to drop out prior to Year 6.

In all the ITT regressions our outcome of interest is a dummy for being Intention to Treat interacted with a dummy for being pre-enrolled in a treatment school in the appropriate year group.

Table A1: Sample Accounting Structure

Wave 1

Year group in 2009/10		2009/10	2010/11	2011/12	2012/13	2013/14
2	Treatment	2098				1824
	Control	13402				11483
3	Treatment	2108			1912	
	Control	13028			11609	
4	Treatment	2115		1973		
	Control	13055		12176		
5	Treatment	2154	2077			
	Control	13460	13026			

Wave 2

Year group in 2010/11		2010/11	2011/12	2012/13	2013/14
3	Treatment	9629			8827
	Control	13486			1211
4	Treatment	9407		8841	
	Control	13099		12188	
5	Treatment	9553	9288		
	Control	13134	12736		

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[17]) 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.[18] [19]

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); as well as this, schools that gain academy status within an academic year are not expected to file a return for that year.

To compare changes in spending before and after conversion we use two years of data for each treated school, which are as close together as possible, but fall either side of the conversion date. Given the nature of when reports are filed, this entails a comparison of the 2009/10 and 2011/12 reports for schools converting in the 2010/2011 academic year and a comparison of the 2010/11 and 2012/13 reports for schools converting in the 2011/2012 academic year. In both cases, we differences out differences in expenditure for the control schools in the same years and pool together these two separate estimates to give the results in tables 6-8.

In order to correct the data for academies that convert between March and August (and so have the option to file a return of up to 18 months), we utilise data that is available for the 2013/14 year that indicates the length of returns filed for academies converting between March and August 2013. From these data we calculate the percentage of primaries converting in March who file an 18 month return, the percentage of primaries converting in April who file a 17 month return etc. down to the percentage of those converting in August who file a 13 month return.[20] We then apply the following weight to schools that convert in month X where X lies between 3 and 8.

$$1\text{-}\mathrm{Frac}_x + \left(\frac{9+\mathrm{month}_x}{18}\right) * \mathrm{Frac}_x$$

where Frac_x is the fraction of schools converting in month x that file a non 12-month return in year 2013/14.

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

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

[19] 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.

[20] It should be noted that all schools filing a return exceeding 12 months do so in a predictable way i.e. they file a return that runs September to September plus any expenditures incurred between the month of conversion and September.

We also experimented with using the following weights for those converting between March and August:

$$\left(\frac{9+\text{month}_x}{18}\right)*1(\text{expenditure}_{s,x}\leq\text{Frac}_x)$$

$$\left(\frac{9+\text{month}_x}{18}\right)*1(\text{expenditure}_{s,x}>1\text{-Frac}_x)$$

Where $\text{expenditure}_{s,x}$ is the percentile of the expenditure distribution at which school s, converting in month x, lies and 1() is the indicator function. Using either of these adjustments gives very similar results from using the uniform adjustment reported in the Tables.

From Tables 6 to 8 in the main body of the paper, it can be seen that using this adjustment leads to very little differences from estimates using the subsample of academies that file a 12 month return.

In Table 8 in the main body of the paper, we also break down expenditure into four broad subcategories. Table A2 shows a breakdown of the expenditures that are in each category.[21]

Table A2: Expenditure Categories

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

[21] 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>

CENTRE FOR ECONOMIC PERFORMANCE
Recent Discussion Papers

1454	Rabah Arezki Thiemo Fetzer Frank Pisch	On the Comparative Advantage of U.S. Manufacturing: Evidence from the Shale Gas Revolution
1453	Randolph Bruno Nauro Campos Saul Estrin Meng Tian	Foreign Direct Investment and the Relationship Between the United Kingdom and the European Union
1452	Stephen J. Redding Esteban Rossi-Hansberg	Quantitative Spatial Economics
1451	Elias Einiö	The Loss of Production Work: Evidence from Quasi-Experimental Identification of Labour Demand Functions
1450	Marcus Biermann	Trade and the Size Distribution of Firms: Evidence from the German Empire
1449	Alessandro Gavazza Simon Mongey Giovanni L. Violante	Aggregate Recruiting Intensity
1448	Emmanuel Amissah Spiros Bougheas Fabrice Defever Rod Falvey	Financial System Architecture and the Patterns of International Trade
1447	Christian Fons-Rosen Vincenzo Scrutinio Katalin Szemeredi	Colocation and Knowledge Diffusion: Evidence from Million Dollar Plants
1446	Grace Lordan Jörn-Steffen Pischke	Does Rosie Like Riveting? Male and Female Occupational Choices
1445	Stephen J. Redding David E. Weinstein	A Unified Approach to Estimating Demand and Welfare
1444	Anna Valero John Van Reenen	The Economic Impact of Universities: Evidence from Across the Globe

1443	Marta De Philippis	STEM Graduates and Secondary School Curriculum: Does Early Exposure to Science Matter?
1442	Thierry Mayer Marc J. Melitz Gianmarco I.P. Ottaviano	Product Mix and Firm Productivity Responses to Trade Competition
1441	Paul Dolan Georgios Kavetsos Christian Krekel Dimitris Mavridis Robert Metcalfe Claudia Senik Stefan Szymanski Nicolas R. Ziebarth	The Host with the Most? The Effects of the Olympic Games on Happiness
1440	Jörn-Steffen Pischke	Wage Flexibility and Employment Fluctuations: Evidence from the Housing Sector
1439	Brian Bell John Van Reenen	CEO Pay and the Rise of Relative Performance Contracts: A Question of Governance
1438	Fadi Hassan Paolo Lucchino	Powering Education
1437	Evangelia Leda Pateli	Local and Sectoral Import Spillovers in Sweden
1436	Laura Kudrna Georgios Kavetsos Chloe Foy Paul Dolan	Without My Medal on My Mind: Counterfactual Thinking and Other Determinants of Athlete Emotions
1435	Andrew B. Bernard Andreas Moxnes Yukiko U. Saito	Production Networks, Geography and Firm Performance

The Centre for Economic Performance Publications Unit
Tel 020 7955 7673 Fax 020 7404 0612
Email info@cep.lse.ac.uk Web site http://cep.lse.ac.uk