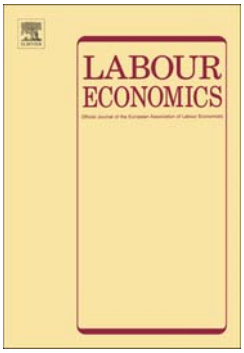


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# School Reforms and Pupil Performance

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Abstract

The relationship between school reforms, specifically those involving the introduction of new school types, and pupil performance is studied. The particular context is the introduction of academy schools in England, but related evidence on Swedish free schools and US charter schools is also presented. The empirical evidence shows a causal positive impact of the conversion of disadvantaged schools to academies on end of school pupil performance and on subsequent probability of degree completion at university. There is heterogeneity in this impact, such that more disadvantaged pupils and those attending London academies experience bigger performance improvements.

JEL Keywords: Academies; School reform; School autonomy; Pupil Performance.
JEL Classifications: I20; I21; I28.

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## 1. Introduction

The emergence of international student tests such as the Organisation for Economic Co-operation and Development's Programme for International Student Assessment (PISA) or the Trends in International Mathematics and Science Study (TIMSS) have placed the importance of equitably and efficiently designed and managed school systems back on top of the policy agenda in many countries.[1] Over the past decade, a number of countries have introduced reforms to their education systems with the aim of achieving better overall outcomes and reducing educational inequalities. These reforms have included introducing higher educational requirements for teachers (e.g. France), delaying tracking of students (e.g. Germany), increasing the length of the school day (e.g. France), and increasing the scope for exercising more autonomy in existing schools (e.g. Portugal) or introducing new school types with more autonomy (e.g. England).

This paper focusses on the last of these school reforms, where new types of schools were introduced into education systems. The particular focus is on the short and medium term impact of a large-scale reform in England that introduced a new school type - the academy school - to the education landscape. Initially, these schools were typically conversions of pre-existing low performing schools. Academy schools have higher levels of autonomy than the previously predominant community schools, that is, schools operating under the remit of a local education authority. The aim of this paper is to understand whether the autonomy gains after conversion led to an improvement in student outcomes.

There are several reasons why granting schools more autonomy over how they allocate resources, the teachers they hire, how they set teacher pay, the teaching methods they use or what speciality subjects they offer in their curriculum might increase school performance and student outcomes. For instance, more autonomy may allow schools to

[1] See Hanushek and Woessmann (2011) for discussion of these international test scores and their widespread use in research.

adapt better to the needs and wishes of the local community and student population. Likewise, greater autonomy over the choice of school leadership may allow governing bodies to select better and more suitable headteachers than a government authority that may be less informed about local circumstances and needs.

However, individual schools and policy makers may have conflicting interests, making transfer of power to schools problematic. For example, the government might have as an objective the reduction in student inequality and segregation by ability. One way to achieve this is to impose ability blind admission procedures. Were schools to be provided with the freedom to set their own admissions policies they would have an incentive to admit only the best students, which could lead to higher levels of inequality. Similarly, allowing autonomous schools to select their own teachers, as opposed to having them assigned via a centralised mechanism, may cause greater selection of the best teachers into more autonomous schools, potentially leading to an increase in inequality.

Analysing the causal effect of increased autonomy can prove difficult because of several potential endogeneity problems. A naive comparison of outcomes between students who attend schools with high levels of autonomy and those who attend schools with low levels of autonomy is likely to be biased if unobservable characteristics are related to both school choice and student performance. Schools with higher autonomy have the scope to exercise freedoms that enable them to attract students from better family backgrounds, or they may attract better teachers because they have different pay schemes and incentives than non-autonomous schools. Likewise, in a setting where new schools with more autonomy are set up, the identification of the causal effect of attendance is made difficult by the absence of pre-treatment outcomes for these schools.[2]

[2] Another factor making the study of more autonomy difficult is that it can mean a wide range of things, for instance having freedoms to hire teachers and set their salaries, determining subjects taught and teaching methods, or freedom over how to allocate budget across teaching and other activities. This makes it hard to pin

Research that is related to our study has been undertaken on two new types of schools that have been introduced in the US and Sweden - charter schools and free schools. Some of this work, especially in the US setting, spends a lot of time and effort in trying to uncover the causal effect of attending a specific school (sometimes using lottery assignment of places when schools are oversubscribed). Some of the empirical approaches used in this work are relevant to what we do in this paper, and we review some of this growing literature below. Another strand of research looks at the effects of school autonomy on student performance using cross-country PISA and TIMSS data. This literature however often fails to deal with the problem of within-country selection into more autonomous schools, and is therefore unable to uncover causal effects of more autonomy on student outcomes (see Hanushek, Link and Woessmann, 2013).[3]

The specifics of the English reform provide us with a setting that allows for the identification of the causal effect of giving schools more autonomy by analysing the effect of academy conversion. The reform meant that existing schools were converted to academy status, hence we can implement a difference–in-differences approach where we use those pupils who attend early academy converters as a treatment group and those who attend schools that later convert to academies as a control group. Additionally, by looking only at students already enrolled in the school before the conversion took place (in the terminology of Abdulkadiroglu et al., 2014, 'grandfathered' or pre-enrolled students) we can remove potential effects that work through changes in pupil composition post conversion, and avoid

down which aspects of autonomy may be conducive to better performance or lower inequality across schools and students. The English reform does not directly allow us to disentangle the separate effects of different aspects of autonomy on student performance, but we present evidence in Section 5 on what type of autonomies were reportedly most used by headteachers of academy schools. Section 2 also presents evidence on US charter schools that shed light on mechanisms that drive school improvement through more autonomy.

[3] An exception is Verschelde et al. (2015) who uncover a significant positive effect of school staff autonomy on student performance by exploiting plausibly exogenous variation in school staff autonomy levels within the same school types using PISA data.

problems associated with selection on unobservable characteristics into schools that have been converted.[4]

Using hypothetical years of exposure for pre-enrolled students at a to-be-converted school as an instrument for actual years of exposure to the converted academy, we find that, on average, attending a more autonomous academy school increases exam outcomes at age 16 by 7.2 percent of a standard deviation per year of exposure. It also increases the likelihood of degree completion within five years of completing compulsory education by 0.7 percentage points, which is equivalent to around a 10 percent rise compared to the average in our sample. More flexible specifications suggest that positive effects increase non-linearly with years of exposure, with those having attended an academy for four years gaining 29 percent of a standard deviation in age 16 exam outcomes, compared to 4 percent of a standard deviation for those with only one year of exposure, and 2.4 percentage points in terms of the likelihood of degree completion, compared to only 0.8 percentage points for those who attended the academy for only one year prior to conversion.

Analysis of heterogeneous effects show that for short term outcomes, the impact of attending an autonomous school are larger for disadvantaged students (students eligible for free school meals), who gain 8.9 percent of a standard deviation, compared to 6.3 percent for those not eligible for free school meals, and this gap holds both for boys and girls. Medium term outcomes do not seem to differ much across subgroups. Interestingly, consistent with various studies that emphasise big improvements in the performance of pupils studying in schools in London over the last ten to fifteen years (Burgess, 2014; Blanden et al., 2015), we find bigger effects of academy conversion for pupils enrolled in schools in the capital. These results are consistent with evidence from US charter schools where effects tend to be stronger for disadvantaged students and students in urban areas (Angrist et al., 2013). We

[4] The analysis therefore focuses on those individuals who were pre-enrolled in the school prior to conversion, but excludes the small number of pupils who join the school after conversion.

also offer some descriptive evidence on mechanisms that could have led to the gains from greater autonomy, which appear to be changes in leadership, changes to the curriculum on offer and changes to the performance management system for teachers and improved collaboration with other schools.

The rest of the paper is organised as follows. Section 2 gives information on the English reform and how this affected school autonomy and discusses the existing evidence on the impact of school reform on student outcomes for the US and Sweden. Section 3 introduces the data used for this study and the empirical strategy used to identify the casual effect of academy introduction on short and medium term outcomes. Section 4 and 5 present the main results and heterogeneous effects, respectively, and section 6 concludes.

## 2. School reform in England and internationally

Until about fifteen years ago, the English education system was marked by a relatively homogenous school landscape, which involved a majority of secondary modern schools or community schools – traditional publicly funded and locally managed schools – who made up 66% of schools in 2002 (Eyles, Hupkau and Machin, 2015). Religious schools (known as voluntary aided and voluntary controlled schools), foundation schools and a very small number of City Technology Colleges made up the remaining 34% of state schools in the country.[5] Column (1) of Table 1 shows the numbers of each of these types of state schools in England in the school year of 2001/2.[6]

Community schools in England operate under the remit of local authorities who are responsible for funding and budgets, determining admission policies, employing school staff, and appointing members to school governing bodies. School governing bodies play a vital

[5] England also has fee paying private schools, which about 7% of students attend (Independent Schools Council, 2015).
[6] The notes to the Table give more detail on the characteristics of the different types of English secondary schools.

role in the decision making for a range of aspects in England. Among other things, they are responsible for budget decisions, appointing and dismissing staff, and holding the headteacher accountable for school performance.

In the early 2000s, an increasing preoccupation with inequality in student outcomes across schools, and in particular a widespread recognition of the poor performance of some inner city schools in deprived neighbourhoods, led the government to introduce a new type of school - the academy school. Academy schools are removed from local authority control and receive their funding directly from the central government. The possibility for academy conversion during the 2000's – the 'initial programme' - was restricted to very low performing schools. After a change in government in 2010, the possibility of conversion was made available to all types of school rather than just secondary schools. In addition, rather than the conversions mostly being low performing schools like those we study in this paper, many high performing schools also took up the opportunity to become an academy.[7]

Column (2) of Table 1 shows that by the 2008/09 school year, there were 133 academies open and operating. The reform was the start of what was to become one of the most wide-ranging shifts in school organisation witnessed across Western Europe and the United States in recent history. Indeed, by 2015 community schools represented less than 20% of all secondary schools in England, and academy schools accounted for 61%. Eyles, Hupkau and Machin (2015) and Eyles, Machin and Silva (2015) discuss various aspects of this mass academisation of English education (although not in terms of evaluating their impact on pupil performance, which it is still too early to do). This paper studies the performance effects of conversion for schools obtaining academy status under the initial programme up to 2008/09.

[7] See Eyles, Machin, Silva (2015) for a discussion of the later academies, which are not subject of this paper, in part since some of the outcomes we study have not yet had time to occur yet for these more recent conversions.

The first academies required the signing up of a sponsor, who would provide part of the initial capital cost for the school building, a condition that was later relaxed (National Audit Office, 2010). The sponsor can be a business, a charity, a philanthropist, a university or a religious organisation. Academies are, like all other publicly funded schools, accountable to the central education authority, and are managed by a headteacher who is appointed by the governing body. They are subject to the same inspection regime as other state funded schools and students have to take exams in the same three compulsory subjects at age 16 – English, Maths and Science. While academy schools are free to set their own admission policies, they are constrained by the same admissions code as all other state funded schools, which implies that they cannot select students based on ability[8] and that they have to give priority to the most disadvantaged students, that is, students who are or have been in care and those with learning difficulties.

English academy schools have some similarities to privately managed schools operating in other countries. Table A1 in the Appendix gives examples of the different forms of state funded but autonomously managed schools operating in a number of European countries and the United States. However, despite sharing similarities with academies, some details, particularly with respect to funding and fee charging, vary across countries and would make an evaluation of the effect of more autonomy difficult as it may be confounded by different levels of financial resources available for certain schools. For instance, Spain's *Colegios Concertados*, and Germany's state-funded independent schools can charge fees, but they do not receive the same level of government funding that other state-funded schools receive. In contrast to England's academies, France's private schools under government

[8] Up to 2007/08 schools with specialist subjects (i.e. sports, visual and performing arts) were allowed to select up to 10% of their students based on aptitude. A limited form of selection on ability is permitted, so called banding. Banding arrangements have to ensure that the intake represents the range of a school's applicants' abilities (up to 2006) or is representative of the national ability range or the local authority (from 2006). See Department for Education and Skills (2003), Department for Education and Skills (2006) and Department for Education (2014a).

contract (*établissements sous contrat*) can select pupils based on ability. Unlike England's academy schools, most privately managed but largely publicly funded schools around Europe have existed for decades and any evaluation of their introduction is constrained by historical data availability. Despite their recent introduction, the share of secondary school pupils in England attending academy schools had already reached 44% by 2014, only being exceeded by the Netherlands, where over 60% of secondary school pupils in 2012 attended a private school.

Figure 1 shows a ranking of average autonomy levels amongst schools in Western Europe and the US using an index derived from the 2012 PISA survey. England ranks highest according to this index, which takes higher values as greater resource allocation responsibilities fall into the remit of the school. The other nations of the UK did not introduce any academy schools, but maintained the community school centred school system. As can be seen in the Figure, their autonomy levels are around the OECD average and much lower than for the English school system.

For England and the Rest of the UK, Table 2 uses PISA 2012 data to further probe into these autonomy differences. The Table shows the percentage of schools where only the principal or teacher, both the principal or teacher and the regional or national education authority or school governing bodies, or only the regional or national authority have responsibility for the autonomy tasks. The first thing to note is that in all aspects of school management England has a higher share of schools for which the principal or teachers only are responsible. When we divide the sample into private schools, privately managed and publicly funded[9] – the group academy schools fall into - and public schools – the group community schools fall into - we see that within England and the rest of the UK there are

[9] A school is defined as privately managed and publicly funded when it is recorded in PISA as a private school and received at least 50% of its funding from the government. This definition is consistent with the one used in OECD (2012).

significant differences. For instance, among private schools 85% have complete autonomy over teacher hiring, while this percentage is 60% for privately managed and publicly funded schools and only 46% for state schools. Overall, privately managed publicly funded schools in England are more similar to private schools, in terms of their powers, than to public schools. In the rest of the UK, privately managed and publicly funded schools tend to delegate most responsibilities to their governing bodies, or responsibilities are held jointly by schools and education authorities.

*Existing evidence of the effects of the introduction of more autonomous schools*

England is not the only country to have introduced new types of more autonomous schools with the explicit aims of innovating their education system and increasing pupil performance. Both the United States, with the introduction of charter schools in 1992, and Sweden, which introduced free schools as part of a wider educational reform in the early 1990s, initiated new school programmes. While these have been pursued on a smaller scale than the academies programme - around 6% of state educated pupils in the US attended charters in 2013 (National Centre for Education Statistics, 2015) and about 14% attended free schools in Sweden in 2012[10] - they share some similarities. In each case, the schools are privately run but rely on public funds to operate; furthermore, these school types enjoy greater operational autonomy than other publicly funded schools.

The extent to which, within Sweden and the US, attendance at these school types has aided pupil performance in academic tests and, more importantly, led to positive longer-term outcomes, is the subject of a growing literature. The overall nature of the programmes, as well as the methods of allocating places in charters and free schools respectively, has shaped both the phrasing of research questions and the way in which researchers have estimated performance effects for these schools.

[10] Own calculation from PISA 2012 data.

In the Swedish context, the large growth in the publicly funded but independent *friskola* came as a result of educational reforms, particularly the introduction of a voucher system, enacted in the early 1990s, which were primarily aimed at increasing competition amongst schools at municipality level (see Bjorklund et al., 2005, for a detailed review of the various reforms around this period). The voucher system enabled students to attend newly established schools funded by their home municipality. Both public and free schools in Sweden receive the same level of per pupil funding, so overall funds going to state schools fall as the share of students choosing to attend free schools rises. It was hoped that this competition for students would drive up educational standards amongst incumbent schools.

The research on these Swedish independent schools has focused on the competitive effects of the voucher system. For example, Bohlmark and Lindahl (2015) relate the share of free school pupils at municipality level to municipality level test scores.[11] As noted by the authors, who find positive effects of the voucher reform, any overall performance effect found at this aggregated level is a mixture of the direct effect of increased free school attendance and the indirect effect that works through changes in state school performance that result from increased competition. The emphasis on the competition related effect of the reform means that little direct evidence exists of the gains from attending a free school. The small, and mainly positive effects that have been found, are elaborated upon in Table 3.

In contrast to the above, the research on charter schools has focused on the direct impact of charter school attendance on both test scores and, in a limited number of cases, further outcomes such as college enrolment. The use of admission lotteries to allocate places in oversubscribed charters has been used extensively as a natural experiment to isolate the casual effects of charter attendance (Abdulkadirolgu et al., 2011) on these outcomes. In a similar vein, the random assignment of charter school practices into pre-existing public

[11] Rather than assume that free school entrance is random across municipalities, researchers typically include municipality level fixed effects as well as time varying controls.

schools has enabled researchers to isolate specific mechanisms by which charter schools increase performance in standardized tests (Fryer, 2014). While the majority of papers focus on the effect of newly built charters, Abdulkadiroglu et al. (2014) use pre-enrolment in schools that later convert to charters, as well as matching, to study the effect of charter takeover on test scores and find positive effects; features of this identification strategy are detailed extensively in Section 3 in relation to academy school takeovers in England.

A somewhat stylised finding of the literature on charter schools is that charters are able to generate sizeable test score gains in English and Math, and that overall gains often mask considerable heterogeneity. Gains often appear to be strongest in urban areas and for those who are disadvantaged. A more detailed review of the literature on charter schools and free schools can be found in Eyles, Hupkau and Machin (2015). Table 3 of this paper offers a brief summary of that longer review.

## 3. Data and empirical strategy

The focus of the empirical work in this paper is on conversions of existing state schools in the English secondary school sector into academies. In England, at the time of our study, pupils attended secondary schools from years 7 through 11 of their compulsory education when aged 11 to 16. Whilst attending secondary school, students are assessed in Key Stage 3 in year 9 and Key Stage 4 in year 11, the last year of compulsory education. The Key Stage 4 exams they take then are known as the General Certificate of Secondary Education (GCSE) school leaving exams. Prior to attending secondary schools most children (except in the minority of local authorities where middle schools are present) make the transition from

primary to secondary school having completed their Key Stage 2 assessments in the final year of primary school, year 6, at age 11.[12]

*Data*

We use rich administrative data that records schooling outcomes of individual pupils through all their Key Stage assessments up to the end of compulsory education at age 16. For children who stay on in post-compulsory education we also observe their Key Stage 5 (KS5) performance (known as Advanced or A levels) at the age of 18. In addition, we have matched higher education administrative data that records all students enrolled in a higher education institution in the United Kingdom. We obtain demographic characteristics from the School Census, which contains information on age, gender, ethnicity, special educational needs status and whether or not the pupil is eligible for free school meals.

We study two outcomes. The first is Key Stage 4 (KS4) performance, defined as the standardised total point score achieved at age 16 for the best 8 results in exams the student took at the end of compulsory school.[13] The second is a post-compulsory schooling outcome, degree completion, defined as having completed a Bachelor degree at one of the UK's higher education institutions by age 21, five years after completing compulsory education.

*Empirical strategy*

We estimate the effect of academy attendance on KS4 test scores and degree completion using repeated cross sections of quasi-natural experiments from events of academy conversion that took place in different school years. The events we study are described in Table 4. The Table shows a gradual introduction of academy schools, with the

[12] The other assessment in the Key Stage sequence, Key Stage 1, is taken earlier in primary school at age 7 at the end of year 2.

[13] The results reported in this paper are invariant to using alternative Key Stage 4 measures, such as total uncapped scores and/or based on alternative scales. See Appendix A of Eyles and Machin (2015) for more detailed discussion of the issues to do with measuring KS4 performance of pupils.

first three opening in the 2002/3 school year, and then in the subsequent school years as follows: 2003/04 - 9; 2004/05 - 5; 2005/06 - 10; 2006/07 - 20; 2007/08 - 36; 2008/09 - 50.

The conversions we analyse in this paper are for state schools for which we have full data before and after they become an academy. Therefore, we are not able to study the 12 newly set up schools (where there was no predecessor school), nor the 5 private schools on which we do not have data. We also do not consider conversions from City Technology Colleges (CTCs) as they were already highly autonomous schools that were performing well. In fact, many commentators have identified CTCs as the precursors of academies (see West and Bailey, 2013).[14] This leaves us with the following numbers per year, as shown in the Table: 2002/3 – 3; 2003/4 – 6; 2004/5 – 2; 2005/6 – 7; 2006/7 – 14; 2007/8 – 25; 2008/9 – 37.

To illustrate the empirical approach we adopt, note that in the first year of conversions (2002/3), three schools became academies. We only consider children who were enrolled in the academy before conversion, which in this case will be children in year 7-10 in the school year 2001/2. Because they (and their parents or carers) have already made the enrolment decision to attend the school pre-conversion, the conversion is exogenous to them. By focussing on individuals who have already made their enrolment decision, this is the approach referred to as studying legacy enrolments or 'grandfathered' children – defining pupils who stay in a converting school as 'grand-fathered' pupils - as exploited in, for example, Abdulkadiroglu et al. (2014), who study school takeovers in New Orleans.

Thus children in year 10 in 2001/2 will sit their Key Stage 4 exams in 2002/3, getting one year exposure to the academy; children in year 9 in 2001/2 will sit their exams in

[14] Almost all CTCs took up the opportunity to become academies when it arose with the introduction of academy schools. There were only 15 CTCs before the introduction of academy schools and 12 of them converted in the school years we consider in this paper. They were highly autonomous schools already, being able to not fully follow the national curriculum, to run their own admissions, and not being maintained by the local authority. One can argue that the autonomy gains they experienced from academy conversion were negligible, unlike for the state maintained schools that converted who we study in this paper.

2003/4, gaining two years exposure and so on until the last year group of legacy enrolled/grandfathered children (year 7 pupils in the pre-conversion year) will get four years exposure taking their Key Stage 4 exams in school year 2005/6. Anyone enrolling from the year of conversion 2002/3 onwards are knowingly enrolling in the academy school and conversion is not exogenous to them.[15]

In their event study based analysis of academies, Eyles and Machin (2015) use pupils enrolled in schools that become academies after the sample period ends as the control group. They show that treatment and control pupils are well balanced on observable pre-treatment (i.e. academy conversion) characteristics. For the empirical approach we adopt here, we can further refine the definition of control pupils. Consider again the example of the 2002/3 conversions. The last of the grandfathered children sit their exams in 2005/6, so a valid set of control schools would be those that convert in the year following that, 2006/7. We can thus match pupils in the same school years as the grandfathered children in treatment schools to those in these control schools. Over and above this, the use of students enrolled in future academy schools as a control group allows us to control for school level unobservables such as ethos for change or being 'academy friendly'; in addition, as academy schools are typically struggling before conversion it allows us to compare outcomes for pupils who choose to attend similar, low performing schools.[16]

The above example refers to one set of conversion events taking place in the 2002/3 school year. We then have analogous events in subsequent school years, where we can adopt the same approach of studying education outcomes for children enrolled in the school before it becomes an academy. Thus we can define rolling cohorts of grandfathered children and

[15] Eyles and Machin (2015) and Eyles, Machin and Silva (2015) show, for the academy conversions prior to 2010 studied in this paper, that the quality of pupil intake (as measured the Key Stage 2 performance of year 7 enrolments before and after conversion) did indeed increase, thus significantly changing the pupil composition.

[16] See Eyles and Machin (2015) who show that academy conversions by 2008/9 and the future control group conversions they consider were very much concentrated in poorly performing schools. As already noted, there is one exception, which is the conversions from City Technology Colleges, who we do not include in our analysis.

matched children in control schools for conversions that take place across the 2002/3 through 2008/9 school years.

For each cross section of academy converters, we use pupils who attend schools that convert 4 years after the treatment group as a control group. Overall we study pupils in 303 treatment and control schools.[17] To make the control group consistent with the treatment we focus on those who are 'intention to treat' in the control schools (i.e. they do not get treated because the school does not convert, but would have done under the alternative scenario that it did convert to become an academy). Thus, intention to treat is defined to match with respect to the year of conversion of treatment schools. For instance, our first experimental cross-section compares outcomes for year 7-10 students, who in 2002 were enrolled in a school that converts in 2003 with year 7-10 students, who in 2002 were enrolled in a school that converts in 2007.[18]

For each of these cohorts of conversion, we identify year 7-10 pupils who are enrolled in a school that converts to academy status in the next academic year. Expected exposure to academy status, for a pupil in academic year group t, is then defined as 11-t. We refer to pupils with non-zero expected years of exposure as the intention to treat group.[19] As an example, pupils in year group 10 in the year prior to conversion have one year of potential exposure while pupils in year 7 one-year prior to conversion have 4 years of potential exposure. We then use potential years of exposure as an instrument for actual years of exposure.

We also include in our analysis those pupils who sit their exams in the year prior to academy conversion, enabling a before/after conversion comparison to be made. It also

[17] Note therefore that the 2006/7, 2007/8 and 2008/9 treatment schools are also control schools for earlier converting cohorts. Owing to the rotation between control and treatment through time, they are therefore effectively double counted in Panel A of Table 4.

[18] If the soon to be converted state schools used as control schools already start to adjust to some degree before conversion, then this will lead to a downward bias in the estimates.

[19] The exposure variable takes values 0-4. Maximum exposure is achieved by those who are enrolled as year 7 students in a school that converts in the following year and who stay in that school until year 11.

enables us to compare baseline pre-conversion pupil performance in matched treatment and control schools. It turns out that pupils enrolled are well balanced in terms of pre-conversion year Key Stage 4 performance – as shown in Panel B of Table 4.[20]

In formal terms, we are interested in the causal impact of exposure to academy status on pupil performance. This is modelled in a difference-in-differences specification which allows for exposure effects. For pupil i who is pre-enrolled in school s in year t, the basic regression setup for studying the impact of academy conversion across the pooled treatment-control cohorts c takes the form:

$$Y_{istc} = \alpha_s + \alpha_t + \alpha_c + \beta_1 X_{istc} + \delta_1 A_{istc} * I(t \geq e) * Exposure_{istc} + \varepsilon_{1ist} \quad (1)$$

where Y is pupil performance, the α's are fixed effects (for school s, time t and cohort c respectively), X is a row vector of covariates including pupil-level control variables and, in some cases, a standardised Key Stage 2 score, and $\varepsilon_1$ is an error term. Treatment by academy conversion is defined by the post academy conversion indicator $A_{istc} * I(t \geq e)$, where A denotes a school that becomes an academy in a given year and the indicator function $I(t \geq e)$ denotes years after the conversion event year e. As the effect of academy conversion is allowed to vary with years of exposure (Exposure), the main parameter of interest - $\delta_1$ in (1) - is a difference-in-difference estimator with continuous treatment. As well as allowing the treatment to vary linearly with years of exposure, we also consider a model with a discrete functional form where the treatment effect differs by the number of years the pupil attends the academy since conversion.

The potential problem with only considering ordinary least squares estimates of equation (1) is that the population of pupils who sit their exams in the school that converts

[20] The research design adopted here means the balancing tests can only be looked at in the year prior to conversion. Of course, the pre-conversion trajectories might be different – this is studied in a different, less stringent on data, research design reported on in our companion papers (Eyles and Machin, 2015; Eyles, Hupkau and Machin, 2016) with the pre-conversion trends looking similar for treatment and control schools.

may not be the same as the pupils who were enrolled pre-conversion. Some pupils of that group may leave the school and sit their exams elsewhere. This generates the possibility of a threat to our research design because actual treatment status may be non-random: if the worst of the legacy enrolled pupils decided to leave schools once they gained academy status then our estimates will be upward biased. To circumvent this source of possible bias we generate an intention to treat (ITT) estimate and use intention to treat status as an instrument for actual treatment in an instrumental variable (IV) setting. The ITT, or reduced form equation is:

$$Y_{istc} = \alpha_s + \alpha_t + \alpha_c + \beta_2 X_{istc} + \delta_2 A_{istc} * I(t \geq e) * \text{ITT Exposure}_{istc} + \varepsilon_{2istc} \quad (2)$$

where ITT Exposure is the expected years of exposure when viewed from the pre-academy conversion year c for the already enrolled pupils. It measures the hypothetical number of years in an academy that they would be exposed to were they to stay on to sit their Key Stage 4 examinations there. The first stage regression that predicts treatment exposure is

$$\text{Exposure}_{istc} = \alpha_s + \alpha_t + \alpha_c + \beta_3 X_{istc} + \delta_3 A_{istc} * I(t \geq e) * \text{ITT Exposure}_{istc} + \varepsilon_{3istc} \quad (3)$$

The IV estimator is then obtained using the prediction of Exposure from (3), which can be defined as $\widehat{\text{Exposure}}$, in place of the direct Exposure variable in (1) as follows:

$$Y_{istc} = \alpha_s + \alpha_t + \alpha_c + \beta_4 X_{istc} + \delta_4 A_{istc} * I(t \geq e) * \widehat{\text{Exposure}}_{istc} + \varepsilon_{4istc} \quad (4)$$

so that the IV estimator $\delta_4$ equals the ratio of the reduced form coefficients $\delta_2/\delta_3$.

The main identifying assumption is that pre-enrolment in a school, which converts in year t, as opposed to t+4, is orthogonal to test scores. The fact that schools that convert over the 2003-2013 period are observationally similar prior to conversion (see Eyles and Machin 2015), and that enrolment decisions were made without knowledge that the school would subsequently gain academy status, suggests that this assumption is met. Under this assumption we estimate a local average treatment effect (LATE) of academy exposure that

identifies the causal effect of exposure to academy status for those who attend an academy, but do so only because they happened to enrol in a school that later gained academy status before they left compulsory schooling.

Further interest lies in how the effect of academy attendance differs across subgroups of the population of students. In our empirical work below, we explicitly study heterogeneous effects, looking at differences by free school meal status and gender. We also consider separate estimates for academy conversions in London compared to those outside the capital. A reason for doing this is recent work that emphasises very considerable improvements in London schools that have happened over the last fifteen to twenty years. This work does not consider causal effects regarding different education policies, often being very descriptive in their mode of study (see, for example, Blanden et al., 2015 or Burgess, 2014).

## 4. Main results

*First Stages*

Estimates of the first stage regressions (equation (3) above) are reported in Table 5, for two functional forms of ITT years of exposure. In columns (1) and (2), which differ on whether or not pupil exam results at age 11 (their Key Stage 2 results) are controlled for, the ITT years of exposure is a continuous measure, ranging from zero in control schools up to a maximum of four for pupils who were pre-enrolled in a to-be-academy in their year 7. In columns (3) and (4), ITT years of exposure is represented by four dummy variables for one, two, three and four years respectively.

All of the first stage estimates in the Table show a strong relationship between actual years of exposure to being educated in an academy and the expected measure defined in the pre-conversion year t = (e-1). The estimated coefficients in columns (1) and (2) show for the

continuous measure that 92.3 percent of the theoretical expected years of exposure were completed by the pre-enrolled students. The same pattern, but as one would expect with a drop off for longer durations, emerges for the dummy variable representation of ITT exposure, as is shown in columns (3) and (4) of the Table. For one year of exposure, 98.5 percent of ITT predicted years were completed, and this drops to 94.9 percent for two years, 91.1 percent for three years and 87.5 percent for the maximum four years.

These first stages therefore show that a very large fraction of pupils sit their KS4 exams in the school in which they were pre-enrolled. If a dummy variable for pre-enrolled status alone is instead used as the ITT variable to predict actual years of exposure (as in Abdulkadiroglu et al., 2014) it attracts a highly significant estimated coefficient of 1.603 (with an associated standard error of 0.046). This suggests, for the sample we study, an average of 1.6 years of exposure to being educated in an academy for the pre-enrolled pupils.[21]

*Key Stage 4 Results*

The impact of academy conversion on end of secondary school pupil performance, as measured by KS4 points score, is considered in Table 6. The Table shows six sets of estimates. Columns (1) to (3) do not include Key Stage 2 performance, while columns (4) to (6) show estimates from the value added specifications where Key Stage 2 performance is one of the independent variables. For each of these, the three specifications reported are the ordinary least squares (OLS) estimates (equation (1) above), the intention to treat (ITT) estimates (equation (2) above) and the instrumental variable (IV) estimates (equation (4) above). For the latter, the first stages are shown in Table 5 (as discussed above).

[21] Because of the cross-cohort rolling conversion dates, the percentages of pupils in each year of post-academy conversion treatment are bigger for the smaller years of exposure. More specifically, 53.0 percent have one year of exposure, 26.9 percent have two years exposure, 13.8 percent have three years exposure and 6.4 percent have the maximum four years.

The results show that KS4 performance improved significantly for pupils in the academy conversions relative to pupils in the control schools. The OLS estimate in column (1) points to a significant 0.099σ improvement per year of exposure in KS4 scores for children enrolled in a pre-conversion school as compared to children enrolled in control schools in the same school years. The ITT exposure estimate is a little smaller at 0.075σ as shown in column (2), and the IV estimate in column (3) lies between the two at 0.081σ. The value added specifications produce results that temper the magnitudes a little, as shown in columns (4) to (6), but there remains a significant improvement in KS4 performance. The IV estimate in the last column shows an improvement of 0.072σ for one year of exposure. This translates into a sizable 0.288σ for legacy-enrolled pupils who receive four years of secondary schooling in an academy.

This significant raising of KS4 outcomes for pupils already enrolled in the highly disadvantaged schools that subsequently became academies suggests that the academy conversion raised their performance relative to the counterfactual of no conversion. The IV estimates have the interpretation of local average treatment effects (LATE).[22] The estimated effects are local to those who were induced to attend an academy only because they were enrolled prior to conversion, meaning that these individuals would not have attended an academy had they not been pre-enrolled. Given the high compliance rates in the first stages shown in Table 5, the LATE appears to be relevant for many pupils.

*Degree Completion*

Using the same structure as Table 6, Table 7 shows estimates of the causal impact of academy conversion for an educational outcome measured five years after KS4 - namely,

[22] See Angrist and Imbens (1994). The conditions are intuitively reasonable in the context we study. They require that those individuals who do not receive treatment, despite being pre-enrolled in an academy, would still not have received treatment if they had not been pre-enrolled. We also require that being pre-enrolled is random across individuals, and unrelated to, for instance, ability. The balancing tests shown above in Table 4 indicate that on average individuals pre-enrolled in a school that would subsequently become an academy did not differ in their KS4 performance compared to pupils enrolled in the control schools.

whether individuals in our sample complete a Bachelor degree at university. Only 7 percent in the sample of both treatment and control schools do, which is a long way below the national average (of around 20 percent for the measure we look at in the years we study[23]), again showing that the pupils we are studying are highly disadvantaged.

The results show that being exposed to academy conversion significantly raises the probability of degree completion. The IV estimates in the final column show a 0.7 percentage point improvement for a one-year increase in exposure (which multiplies up to 2.8 percentage points for four years exposure) that is statistically significant. Thus it seems there are longer lasting effects for those children who benefitted from attending an academy, with the probability of getting a university degree rising by about 10 percent, relative to the mean of the dependent variable, with a one-year increase in years of exposure.

*Functional Form for Exposure*

Next we consider estimates where, rather than imposing the linear functional form for exposure to education in an academy school, we consider a dummy variable representation. The results are shown for both outcome variables in Table 8. Whilst the estimated coefficients do rise monotonically across the four dummy variables for all specifications, closer inspection of the separate estimates shows the linear continuous measure does not fully represent the pattern of the estimates. In particular, the effects for one year of exposure are quite small relative to two and above years. Indeed, it is two or more years of exposure that translates into sizable effects for both outcomes.

[23] Over the period of analysis, on average about 21% of students in England entered university at age 18. Combined with an average non-completion probability of just under 10%, we estimate that about 20% of students complete a first degree with three years duration (Bachelor degree) within 5 years of leaving compulsory education, that is, by the age of 21. This measure of degree completion is somewhat strict, as it requires a straight path from lower secondary school to upper secondary school and immediate enrolment into a degree at age 18. Disadvantaged students often do not follow such a straight path into university and tend to take longer to complete university entry requirements. Furthermore, some students in the UK take a gap year between school and university. We are forced to adopt this strict definition of degree completion because the last cohort of KS4 exam takers (in school year 2008/9) used in our analysis is only observed in higher education data up to the year 2013/14, which is 5 years after age 16 exams. As more recent data becomes available in due course a relaxation of the degree completion definition to within 6 years can be adopted.

## 5. Heterogeneous effects and additional tests

*Variations by Free School Meals and Gender*

In Table 9 we show estimates where years of exposure to an academy education in secondary school are allowed to vary with two pupil characteristics - free school meal eligibility status (FSM) and gender. The Table shows estimates from IV value added specifications for the continuous treatment intensity variable. Results proved qualitatively the same if the dummy variable representation was used, but were much more cluttered for presentational purposes, and the basic story emerges more clearly for the continuous measure.[24]

The column (1) specification shows that the impact of academy conversion on KS4 results is larger in magnitude for disadvantaged pupils. For those eligible for free school meals (FSM), there is a 0.089σ effect for an additional year of exposure as compared to a 0.063σ effect for non-FSM eligible pupils. There is a less marked gender related difference in effects, as shown in column (2), where magnitudes are nearly the same at 0.072σ for male pupils and 0.071σ for female pupils. Consideration of both pupil characteristics together, in the four-way breakdown shown in column (3), reveals that it is FSM rather than gender that is more important in terms of heterogeneous effects.

Evidence of heterogeneity in the estimated effects is less clear for the degree completion outcome measure. As the estimates reported in columns (4) to (6) show, the estimates of the impact of years of exposure to being educated in an academy secondary school are similar when broken down by FSM or gender or by both. This is probably suggestive of some fade out of the initially bigger effects for FSM pupils at KS4. Pupils

[24] The full dummy variable results are available on request from the authors.

treated by academy conversion are more likely to graduate with a degree, but with little variation by pupil disadvantage.

*London and Non-London Academies*

In Table 10 we consider estimates that further break down the four-way heterogeneity grouping for London and Non-London schools. Overall, there were 41 schools in London and 262 outside London. There is an interesting pattern of considerably larger effects for pupils attending London academies. Effects of academy attendance on KS4 results and degree completion for pupils in London schools are often as much as twice as big as for pupils in schools outside of the capital. For instance, increasing academy exposure by one year increases KS4 exam results by about 0.10σ for disadvantaged boys and by about 0.09σ for disadvantaged girls in London, compared to 0.062σ and 0.063σ for disadvantaged boys and girls, respectively, attending an academy outside London. A one-year increase in exposure to an academy based in London increases the likelihood of degree completion by 1 percentage point for both boys and girls in the FSM eligible group. This is highly suggestive that the academy conversions of the 2002-2008 time period played a role in the performance improvements documented for London over this same period elsewhere (Burgess, 2014; Blanden et al., 2015; Wyness, 2011). Our results are consistent with estimates for US charter schools that find that effects of charter school attendance were stronger in urban areas and for disadvantaged students (Angrist et al., 2013).

*Use of Academy Freedoms*

It is interesting to consider which of the additional freedoms academy status brings lie behind the positive effects on student outcomes. Table 11 shows the use of academy freedoms from survey responses to a Department for Education (2014b) survey of head teachers. In total, there are responses from 148 sponsored academies, comprising 23 of the

academies we study in this paper, alongside responses from 125 academies that opened after our sample period ends.[25]

The first column of the Table reports the percentage of responding academies that have made changes. The very fact that a large number of changes have been implemented emphasises that academy conversion appears to be an overall school improvement programme, and one that affords a large number of new ways to run a school with increased autonomy for those who convert. The three most prominent changes, amongst the 23 converters in our sample, were 'changed school leadership', 'procured services that were previously provided by the local authority' and 'changed the curriculum you offer'. Over 75 percent of the schools said they made these changes pursuant to gaining the new academy freedoms. This ranking is broadly consistent with that of the 148 sponsored academies overall.

When asked what the most important change was, two answers dominate - 'changed school leadership' (56 percent) and 'changed the curriculum you offer' (26 percent). Furthermore, both of these were reported to be linked to improved outcomes (in 73 and 77 percent of cases respectively). Other changes that were notably linked to improved outcomes were 'Increased the length of the school day' (63 percent) and 'Collaborated with other schools in more formalised partnerships' (45 percent).

When one considers the most important change schools claim to have made, it is evident that more operational, day-to-day running type changes are less important. The key responses here are those concerning school leadership changes, changes in the curriculum and creating formalised partnerships with other schools. These are all factors that enhanced

[25] For comparability with our sample we only consider responses to the survey for sponsored academies, as all of the academies we study in this paper are sponsored academies. Prior to the 2010 Academies Act having a sponsor was a requirement for setting up an academy (see also Section 2).

the ability to operate in an autonomous manner, and are likely to lie behind the pupil performance improvements from academy conversion identified in our empirical analysis.

The importance of headteacher changes at the time of conversion is studied in the empirical analysis reported on in Eyles and Machin (2015) who show headteacher turnover in the year of conversion to be very prevalent. In fact turnover of the headteacher is over 60 percentage points higher in treatment as compared to control schools. In line with the suggestion in Table 11 that academies made use of their ability to hire teachers without qualified teacher status to supplement their existing teaching stock, there is also some evidence of modest positive changes in the number of teaching staff around conversion. Thus the idea that leadership matters is reaffirmed as part of the mechanisms behind school improvements from the initial academies programme.

## 6. Conclusion

The focus of this paper is whether school reforms, specifically those involving the introduction of new, more autonomous, school types improve pupil outcomes. The main school reforms of this nature that have taken place in the past twenty years or so have been the introduction of free schools in Sweden, charter schools in America and academy schools in England. We discuss how each has impacted on performance, but the main empirical application of the paper is on the causal impact of academy school attendance on pupil outcomes in England.

We study the academy introductions that took place in English secondary schools between the 2002/3 and 2008/9 school years. For the most part, these were conversions of already existing disadvantaged schools to academies, which were able to operate with much more autonomy than in their predecessor state after conversion. By studying pupils already enrolled in schools prior to conversion, and comparing them to earlier cohorts enrolled in the

school and to a matched set of similar control schools, we study the impact on end of compulsory school performance and on degree completion within five years after the compulsory school leaving age. We find significant improvements in both outcomes, suggesting that the academies programme, which was targeted at badly performing schools, significantly improved pupil performance. Our estimates are quite sizable, at around 29 percent of a standard deviation improvement for legacy-enrolled pupils who receive four years exposure, but not as big as the largest estimates in the charter school studies. Moreover, like the charter findings of more beneficial effects for disadvantaged children in urban areas, we uncover parallel evidence of bigger effects for disadvantaged pupils and for those in London schools. Finally, we offer evidence that the increased autonomy available to academies, particularly with respect to improved management and curriculum flexibility, were important factors enabling the performance improvements for pupils attending academy schools in the time period we study.

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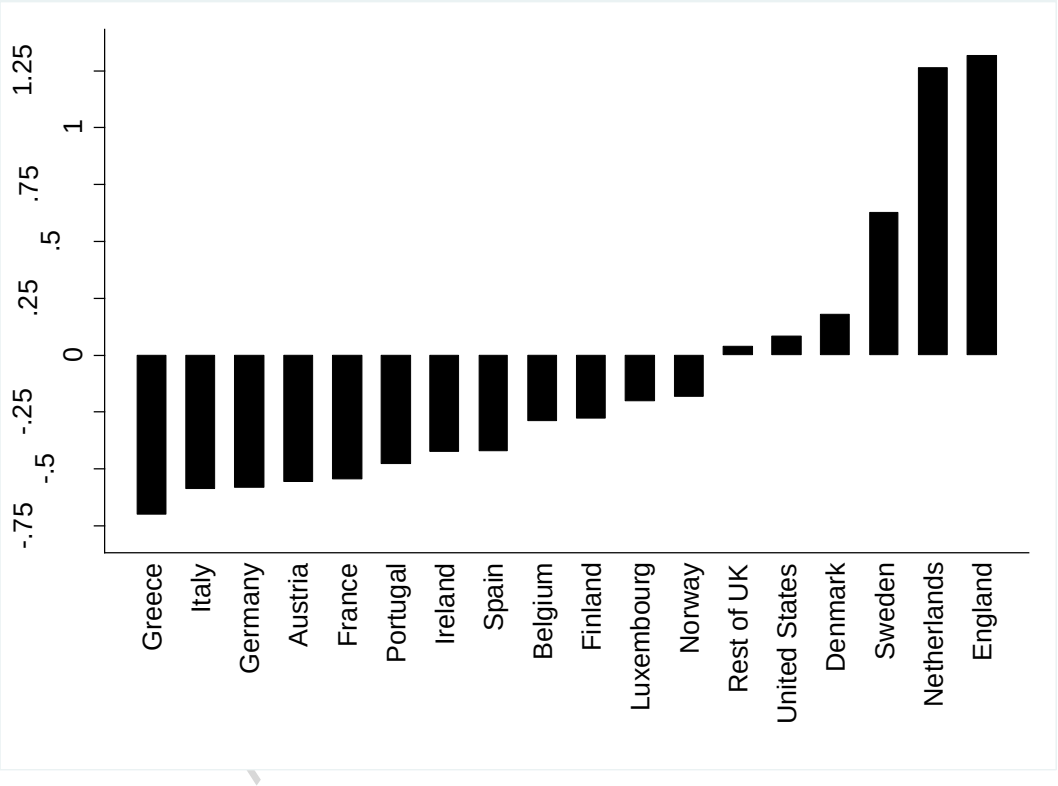
**Figure 1: PISA Autonomy Levels over Resource Allocation in Europe and the US**




Notes: Index (standardised to have mean zero and standard deviation one across all 34 OECD countries in PISA 2012) based upon six dimensions of school autonomy: teacher hiring; teacher firing; establishing teachers' starting salaries; determining teachers' salary increases; formulating the school budget; deciding on budget allocations in schools. The index is calculated from information on all six autonomy tasks on the percentage of students in schools who say that only 'principals and teachers' or both 'principals and teachers' and the 'regional/national education authority' has/have considerable responsibility as compared to only the 'regional/national education authority'. These numbers are the average index reported in Figure IV.4.2 of OECD (2013) but where additionally the United Kingdom is broken out into England and the Rest of the UK.

**Table 1 - Number (Percent) of State Secondary Schools in England, 2001/02 and 2008/09**

| | Number (Percent) of State Secondary Schools by Type | | | |
|---|---|---|---|---|
| | 2001/2 | | 2008/9 | |
| | (1) | | (2) | |
| | Number | Percent | Number | Percent |
| Academy | 0 | 0.0 | 133 | 4.0 |
| City technology college | 15 | 0.4 | 3 | 0.1 |
| Voluntary aided | 549 | 15.8 | 537 | 16.0 |
| Foundation | 501 | 14.4 | 560 | 16.7 |
| Voluntary controlled | 129 | 3.7 | 111 | 3.3 |
| Community | 2278 | 65.6 | 2017 | 59.9 |

Notes: Source – School Census. Includes middle schools. Excludes special schools. From Tables 2.1 and 2.2 and Table 2a in http://webarchive.nationalarchives.gov.uk/20120504203418/http://education.gov.uk/rsgateway/DB/VOL/v000359/dfes_schools_final.pdf

Brief descriptions of the secondary school types are:

Academy schools (prior to 2010/11): all ability independent from local authority specialist schools, which do not charge fees, and are not maintained by the local authority; established by sponsors from business, faith, HE institutions or voluntary groups, working in partnership with central government.

City Technology Colleges: all ability independent from local authority schools, which do not charge fees, and are not maintained by the local education authority. Their curriculum has a particular focus on science and technology education (see West and Bailey, 2013). They were established by sponsors from business, faith or voluntary groups.

Voluntary-aided schools are maintained by the local authority. The foundation (generally religious) appoints most of the governing body. The governing body is responsible for admissions and employing the school staff

Foundation (formerly grant-maintained) schools are maintained by the local authority. The governing body is responsible for admissions and employing the school staff.

Voluntary-controlled schools are maintained by the local authority. These are mostly religious schools where the local authority continues to be the admission authority.

Community schools are maintained by the local authority. The local authority is responsible for admissions and employing the school staff.

**Table 2: Autonomy Comparisons By School Type, PISA 2012, England and Rest of UK**

| | Teacher hiring | | | Teacher firing | | | Teacher starting salaries | | | Teacher salary increases | | | Budget formulation | | | Budget allocation | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | 1 | 2 | 3 | 1 | 2 | 3 | 1 | 2 | 3 | 1 | 2 | 3 | 1 | 2 | 3 | 1 | 2 | 3 |
| *England – all schools* | 55 | 45 | 0 | 26 | 73 | 1 | 36 | 53 | 11 | 22 | 72 | 6 | 13 | 84 | 3 | 47 | 53 | 0 |
| Private schools | 85 | 15 | 0 | 54 | 46 | 0 | 53 | 47 | 0 | 27 | 73 | 0 | 16 | 84 | 0 | 67 | 33 | 0 |
| Priv. managed/publ. funded | 60 | 40 | 0 | 35 | 63 | 1 | 41 | 47 | 12 | 28 | 69 | 3 | 19 | 80 | 1 | 52 | 48 | 0 |
| State schools | 46 | 54 | 0 | 12 | 87 | 1 | 29 | 59 | 12 | 15 | 75 | 10 | 7 | 87 | 6 | 39 | 61 | 0 |
| *Rest of UK – all schools* | 35 | 64 | 1 | 7 | 59 | 34 | 2 | 34 | 64 | 3 | 41 | 56 | 8 | 53 | 39 | 56 | 43 | 1 |
| Private schools | 82 | 18 | 0 | 28 | 72 | 0 | 17 | 83 | 0 | 10 | 73 | 17 | 3 | 97 | 0 | 68 | 32 | 0 |
| Priv. managed/publ. funded | 0 | 100 | 0 | 0 | 100 | 0 | 0 | 57 | 43 | 0 | 100 | 0 | 0 | 73 | 27 | 0 | 100 | 0 |
| State schools | 34 | 65 | 1 | 6 | 58 | 36 | 2 | 32 | 67 | 3 | 39 | 59 | 9 | 51 | 40 | 56 | 42 | 1 |

Notes: Percentage of students in schools where – 1. Only ‘principals and teachers’; 2. Both ‘principals and teachers’ and the ‘regional/national education authority’ or school governing bodies 3. Only ‘regional/national education authority’ has/have considerable responsibility over the six autonomy tasks.

## Table 3: Summary of Related Studies on New School Types

| |
|---|
| **Evidence on Sweden's Free Schools** |
| Of the 4 papers[a] discussed in Eyles, Machin and Hupkau (2015) 3 find positive and significant effects of free school attendance on percentile ranks in 9th grade test scores ([1], [2], [3]). The largest effect is a 6.69 percentile rank improvement in English [2] while the smallest is a 0.318 percentile rank improvement in Swedish [1]. The other paper [4] finds that some of the municipality level increases in performance can be attributed directly to greater number of students attending free schools. |
| **Evidence on US Charter Schools** |
| Of the 12 papers[b] discussed in Eyles, Machin and Hupkau (2015) all but 2 of the papers ([1], [4]) which focus on, typically state level, test scores find no positive effect on average, although one of these [4] does find a positive effect within the subsample of free school meal eligible pupils. A single paper [8] finds small negative effects of charter attendance in non-urban areas while finding positive effects in urban settings. The remaining papers find positive effects that, in all but one case [11], are higher in math than in English. The estimates for math performance lie between 0.1 and 0.35 while those in English lie between 0.05 and 0.2. The sole paper [11] that finds improvements in English test scores over and above math ones finds a 0.3 increase for math and a 0.4 increase for English. Two of the papers ([9], [10]) explore the effects of charter attendance on further outcomes such as college enrolment and incarceration finding that charter school attendance increases the probability of the former while decreasing the latter. The values are non-trivial: charter attendance decreases a male attendees probability of incarceration by 7 percentage points while increasing the probability of college enrolment, for males and females, by between 0.17 and 0.28 percentage points |

Notes: a – the 4 papers are: [1] – Ahlin (2003); [2] Bjorklund et al. (2005); [3] Bohlmark and Lindahl (2007); [4] Bohlmark and Lindahl (2015); b – the 12 papers are: [1] – Betts et al. (2006); [2] Hoxby et al. (2009); [3] Angrist et al. (2010); [4] Gleason et al. (2010); [5] Abdulkadiroglu et al. (2011); [6] Dobbie and Fryer (2011); [7] Dobbie and Fryer (2013); [8] Angrist et al. (2013); [9] Angrist et al. (2016); [10] Dobbie and Fryer (2014); [11] Abdulkadiroglu et al. (2014); [12] Fryer (2014).

## Table 4: Number of Academy Conversions by Year and Balancing Tests

| | 2002/3 | 2003/4 | 2004/5 | 2005/6 | 2006/7 | 2007/8 | 2008/9 | All |
|---|---|---|---|---|---|---|---|---|
| **A. Treatment and Control Schools** | | | | | | | | |
| Number of Conversions | 3 | 9 | 5 | 10 | 20 | 36 | 50 | 133 |
| Number of Conversions With Full Data | 3 | 6 | 2 | 7 | 14 | 25 | 37 | 94 |
| Number of Control Schools Who Convert Four Years Later | 14 | 25 | 37 | 58 | 56 | 41 | 54 | 285 |
| | | 2002/3 to 2005/6 | | | 2006/7 to 2008/9 | | | All |
| **B. Balancing Tests** | | | | | | | | |
| Key Stage 4 For Pupils Enrolled One Year Before Conversion | | -0.016 | | | 0.017 | | | 0.011 |
| | | (0.046) | | | (0.037) | | | (0.030) |
| Degree Completion For Pupils Enrolled One Year Before Conversion | | -0.004 | | | 0.003 | | | 0.002 |
| | | (0.005) | | | (0.005) | | | (0.004) |
| Number of Pupils | | 27384 | | | 42948 | | | 70332 |
| Number of Schools | | 152 | | | 227 | | | 303 |

Notes: Source for upper panel is <https://www.gov.uk/government/uploads/system/uploads/attachment_data/file/175360/academies_annual_report_2010-11.pdf>. Balancing tests in lower panel are treatment-control differences reported for pooled 2002/3 to 2005/6 and 2006/7 to 2008/9 cohorts (pooled for number of school sample size reasons) and across all cohorts. The tests are carried out on matched treatment-control pupil-level data in the year before academy conversion and include the following controls: dummies for whether the pupil is male, the pupil's ethnicity group, whether they are eligible for free school meals and whether they have special educational needs, plus KS2 test scores and a dummy variable for pupils for whom KS2 data is unavailable. Standard errors clustered at school level reported in parentheses.

## Table 5: First Stages

| | Exposure | | | |
|---|---|---|---|---|
| | OLS | OLS | OLS | OLS |
| | (1) | (2) | (3) | (4) |
| ITT Exposure | 0.923 | 0.923 | | |
| | (0.006) | (0.006) | | |
| One Year of ITT Exposure | | | 0.985 | 0.985 |
| | | | (0.001) | (0.001) |
| Two Years of ITT Exposure | | | 0.949 | 0.949 |
| | | | (0.003) | (0.003) |
| Three Years of ITT Exposure | | | 0.911 | 0.911 |
| | | | (0.006) | (0.006) |
| Four Years of ITT Exposure | | | 0.875 | 0.875 |
| | | | (0.013) | (0.013) |
| Key Stage 2 | No | Yes | No | Yes |
| School Fixed Effects | Yes | Yes | Yes | Yes |
| Control Variables | Yes | Yes | Yes | Yes |
| Year Dummies | Yes | Yes | Yes | Yes |
| Sample Size | 255638 | 255638 | 255638 | 255638 |
| Number of Schools | 303 | 303 | 303 | 303 |

Notes: Standard errors (clustered at the school level) are reported in parentheses. Control variables are dummies for whether the pupil is male, the pupil's ethnicity group, whether they are eligible for free school meals and whether they have special educational needs.

## Table 6: Key Stage 4

| | Standardised Key Stage 4 Points Score | | | | | |
|---|---|---|---|---|---|---|
| | OLS | ITT | IV | OLS | ITT | IV |
| | (1) | (2) | (3) | (4) | (5) | (6) |
| Exposure | 0.099 | | 0.081 | 0.088 | | 0.072 |
| | (0.015) | | (0.017) | (0.015) | | (0.016) |
| ITT Exposure | | 0.075 | | | 0.066 | |
| | | (0.016) | | | (0.015) | |
| Key Stage 2 | | | | 0.506 | 0.506 | 0.506 |
| | | | | (0.005) | (0.005) | (0.005) |
| School Fixed Effects | Yes | Yes | Yes | Yes | Yes | Yes |
| Control Variables | Yes | Yes | Yes | Yes | Yes | Yes |
| Year Dummies | Yes | Yes | Yes | Yes | Yes | Yes |
| Sample Size | 255638 | 255638 | 255638 | 255638 | 255638 | 255638 |
| Number of Schools | 303 | 303 | 303 | 303 | 303 | 303 |

Notes: Standard errors (clustered at the school level) are reported in parentheses. Control variables are dummies for whether the pupil is male, the pupil's ethnicity group, whether they are eligible for free school meals and whether they have special educational needs. In specifications including KS2 test scores a dummy variable for pupils for whom KS2 data is unavailable is additionally included.

## Table 7: Degree Completion

| | Degree Completion | | | | | |
|---|---|---|---|---|---|---|
| | OLS | ITT | IV | OLS | ITT | IV |
| | (1) | (2) | (3) | (4) | (5) | (6) |
| Exposure | 0.009 | | 0.008 | 0.007 | | 0.007 |
| | (0.002) | | (0.002) | (0.002) | | (0.002) |
| ITT Exposure | | 0.007 | | | 0.006 | |
| | | (0.002) | | | (0.002) | |
| Key Stage 2 | | | | 0.052 | 0.052 | 0.052 |
| | | | | (0.001) | (0.001) | (0.001) |
| School Fixed Effects | Yes | Yes | Yes | Yes | Yes | Yes |
| Control Variables | Yes | Yes | Yes | Yes | Yes | Yes |
| Year Dummies | Yes | Yes | Yes | Yes | Yes | Yes |
| Mean of Dependent Variable | 0.070 | 0.070 | 0.070 | 0.070 | 0.070 | 0.070 |
| Sample Size | 255638 | 255638 | 255638 | 255638 | 255638 | 255638 |
| Number of Schools | 303 | 303 | 303 | 303 | 303 | 303 |

Notes: Standard errors (clustered at the school level) are reported in parentheses. Control variables are dummies for whether the pupil is male, the pupil's ethnicity group, whether they are eligible for free school meals and whether they have special educational needs. In specifications including KS2 test scores a dummy variable for pupils for whom KS2 data is unavailable is additionally included.

## Table 8: Functional Form For Years of Exposure

| | Standardised Key Stage 4 Points Score | | | Degree Completion | | |
|---|---|---|---|---|---|---|
| | OLS | ITT | IV | OLS | ITT | IV |
| | (1) | (2) | (3) | (4) | (5) | (6) |
| One Year of Exposure | 0.043 | | 0.038 | 0.007 | | 0.008 |
| | (0.023) | | (0.024) | (0.003) | | (0.004) |
| Two Years of Exposure | 0.204 | | 0.177 | 0.014 | | 0.014 |
| | (0.035) | | (0.037) | (0.005) | | (0.005) |
| Three Years of Exposure | 0.284 | | 0.226 | 0.025 | | 0.022 |
| | (0.059) | | (0.061) | (0.006) | | (0.006) |
| Four Years of Exposure | 0.288 | | 0.235 | 0.027 | | 0.024 |
| | (0.063) | | (0.071) | (0.009) | | (0.010) |
| One Year of ITT Exposure | | 0.038 | | | 0.008 | |
| | | (0.024) | | | (0.004) | |
| Two Years of ITT Exposure | | 0.168 | | | 0.013 | |
| | | (0.036) | | | (0.005) | |
| Three Years of ITT Exposure | | 0.209 | | | 0.021 | |
| | | (0.056) | | | (0.006) | |
| Four Years of ITT Exposure | | 0.214 | | | 0.022 | |
| | | (0.062) | | | (0.008) | |
| Key Stage 2 | 0.506 | 0.506 | 0.506 | 0.052 | 0.052 | 0.052 |
| | (0.005) | (0.005) | (0.005) | (0.001) | (0.001) | (0.001) |
| School Fixed Effects | Yes | Yes | Yes | Yes | Yes | Yes |
| Control Variables | Yes | Yes | Yes | Yes | Yes | Yes |
| Year Dummies | Yes | Yes | Yes | Yes | Yes | Yes |
| Sample Size | 255638 | 255638 | 255638 | 255638 | 255638 | 255638 |
| Number of Schools | 303 | 303 | 303 | 303 | 303 | 303 |

Notes: As for Tables 6 and 7.

**Table 9: Heterogeneity - Free School Meals and Gender**

| | Standardised Key Stage 4 Points Score | | | Degree Completion | | |
|---|---|---|---|---|---|---|
| | IV | IV | IV | IV | IV | IV |
| | (1) | (2) | (3) | (4) | (5) | (6) |
| Exposure X Free School Meals | 0.089 | | | 0.006 | | |
| | (0.018) | | | (0.002) | | |
| Exposure X Not Free School Meals | 0.063 | | | 0.007 | | |
| | (0.016) | | | (0.002) | | |
| Exposure X Male | | 0.072 | | | 0.006 | |
| | | (0.017) | | | (0.002) | |
| Exposure X Female | | 0.071 | | | 0.007 | |
| | | (0.016) | | | (0.002) | |
| Exposure X Free School Meals, Male | | | 0.091 | | | 0.007 |
| | | | (0.018) | | | (0.002) |
| Exposure X Free School Meals, Female | | | 0.087 | | | 0.006 |
| | | | (0.020) | | | (0.003) |
| Exposure X Not Free School Meals, Male | | | 0.064 | | | 0.006 |
| | | | (0.017) | | | (0.002) |
| Exposure X Not Free School Meals, Female | | | 0.063 | | | 0.009 |
| | | | (0.016) | | | (0.003) |
| Key Stage 2 | Yes | Yes | Yes | Yes | Yes | Yes |
| School Fixed Effects | Yes | Yes | Yes | Yes | Yes | Yes |
| Control Variables | Yes | Yes | Yes | Yes | Yes | Yes |
| Year Dummies | Yes | Yes | Yes | Yes | Yes | Yes |
| Sample Size | 255638 | 255638 | 255638 | 255638 | 255638 | 255638 |
| Number of Schools | 303 | 303 | 303 | 303 | 303 | 303 |

Notes: As for Tables 6 and 7.

## Table 10: Additional Heterogeneity - London and Non-London Schools

| | Standardised Key Stage 4 Points Score | | Degree Completion | |
|---|---|---|---|---|
| | London | Non-London | London | Non-London |
| | IV | IV | IV | IV |
| | (1) | (2) | (3) | (4) |
| Exposure X Free School Meals, Male | 0.103 | 0.062 | 0.010 | 0.005 |
| | (0.024) | (0.023) | (0.004) | (0.003) |
| Exposure X Free School Meals, Female | 0.088 | 0.063 | 0.010 | -0.001 |
| | (0.025) | (0.026) | (0.004) | (0.002) |
| Exposure X Not Free School Meals, Male | 0.089 | 0.059 | 0.008 | 0.005 |
| | (0.026) | (0.022) | (0.004) | (0.002) |
| Exposure X Not Free School Meals, Female | 0.083 | 0.059 | 0.007 | 0.008 |
| | (0.024) | (0.021) | (0.005) | (0.003) |
| Key Stage 2 | Yes | Yes | Yes | Yes |
| School Fixed Effects | Yes | Yes | Yes | Yes |
| Control Variables | Yes | Yes | Yes | Yes |
| Year Dummies | Yes | Yes | Yes | Yes |
| Sample Size | 34998 | 220640 | 34998 | 220640 |
| Number of Schools | 41 | 262 | 41 | 262 |

Notes: As for Tables 6 and 7.

**Table 11: Department of Education Survey Responses on Academy Freedoms**

| | 23 Labour Academies | | 148 Sponsored Academies Including the 23 Labour Academies | |
|---|---|---|---|---|
| | Percent Making Change | | Percent Say Most Important Change | Percent Making Change Say Linked to Improved Attainment |
| Changed school leadership | 87 | 72 | 56 | 73 |
| Procured services that were previously provided by the LA | 78 | 83 | 5 | 17 |
| Changed the curriculum you offer | 74 | 61 | 26 | 77 |
| Changed the performance management system for teachers | 74 | 70 | 3 | 39 |
| Collaborated with other schools in more formalised partnerships | 70 | 68 | 8 | 45 |
| Introduced savings in back-office functions | 70 | 55 | 0 | 12 |
| Added non-teaching positions | 70 | 50 | 3 | 31 |
| Reconstituted your governing body | 65 | 76 | 0 | 26 |
| Changed your pattern of capital expenditure | 65 | 54 | 1 | 19 |
| Increased the number of pupils on roll | 61 | 41 | 0 | 12 |
| Hired teachers without qualified teacher status (QTS) | 48 | 24 | 0 | 14 |
| Introduced or increased revenue-generating activities | 48 | 34 | 0 | 8 |
| Changed your admission criteria | 43 | 20 | 0 | 7 |
| Increased the length of the school day | 39 | 18 | 0 | 63 |
| Changed staff pay structures | 30 | 24 | 0 | 9 |
| Sought to attract pupils from a different geographical area | 13 | 12 | 0 | 11 |
| Changed the length of school terms | 9 | 6 | 0 | 22 |
| Reduced the number of pupils on roll | 4 | 3 | 0 | 0 |

Notes: Taken from Department for Education (2014b).

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# Appendix

## Table A1: Examples of Privately Managed, Government Dependent Schools in Europe and the United States

| Country/school type | Can set admission policy | Funding through | Fee Charging | Subject to inspection | Teacher hiring | Exist since | Funding parity | Share of students in secondary school (latest figures) |
|---|---|---|---|---|---|---|---|---|
| England: *academy school* | Yes, but subject to rules[a, c] | Central government | × | ✓ | ✓ | early 2000's | equal state funding as public schools | 48% (2014) |
| France: *établissement sous contrat* | Yes, but subject to rules[b] | Central government | ✓ | ✓ | ✓ | around 1960 | equal state funding as public schools | 22% (2012) |
| Germany: *Schule in freier Trägerschaft* | Yes | Local government (State and school district) | ✓ | ✓ | ✓ | around 1920 | less state funding as public schools (teacher salaries and part of capital cost are born by school) | 11% (2014) |
| Italy: *Scuole paritaria* | Yes | Central government | ✓ | ×[f] | ✓ | early 2000's | equal treatment as public schools with respect to funding | 3% (2012) |
| Netherlands: *Private schools* | No[d] | Central government | × | ✓ | ✓ | 1917 | equal state funding as public schools | 66% (2012) |
| Spain: *Colegios Concertados* | Yes | Local government (autonomous region) | ✓ | ✓ | ✓ | around 1985 | less state funding than public schools | 26% (2014) – figure includes primary schools |
| Sweden: *friskolor* | No (first-come, first-served) | Local government | × | ✓ | ✓ | 1992 | equal state funding as public schools | 14% (2012) |
| United States: *charter schools* | No (first-come, first-served)[e] | Local government (State and school district) | × | ✓ | ✓ | early 1990's | often receive less state funding than public schools | 6.3% (2013) |

Notes: a - Academy schools follow the same admissions code as all other state funded schools. Contrary to community schools however, whose admission arrangements are set by the local authority, academies are their own admissions authority. b - Students cannot be refused admission based on their religious beliefs or race. c - Admission policies cannot be selective. d - No school can refuse an applicant unless full. When oversubscribed a catchment area criterion is used as a tie break. e - If a charter school is oversubscribed, a random lottery should be used to assign places. f - Inspections exists theoretically but are rarely carried out.