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Academies, Charter and Free Schools: Do New School Types Deliver Better Outcomes?

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Abstract

School reforms featuring the introduction of new types of schools have occurred in the education systems of a number of countries. The most well-known of these new school types to be recently introduced are charter schools in the United States, free schools in Sweden and academy schools in England. We review the evidence on the impact of the introduction of these new schools on pupil outcomes and present new evidence for the case of England, whose introduction of academy schools has been one of the most radical changes in the school landscape over the past decade. The analysis of academies, charter and free schools concludes that, in certain settings, they can improve pupil performance.

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1. Introduction

Recently some countries have introduced school reforms involving new types of schools with an aim of improving pupil performance and in a quest to strive for what might be perceived as the optimal school structure. Adopting these reforms has typically occurred where there has been a recognition that schools are not delivering the quality of education that parents and educators would like for their children. It is not surprising that such school reforms have occurred in countries like the US or England, where educational inequalities and the issues of problem schools (often in disadvantaged urban areas) have featured prominently in debates on education quality.

While some countries have gone for school reform, others have stuck with the traditional local – or community – school structure. The education system of Finland, for example, is often championed in this regard as its egalitarian school system seems to have delivered results that place Finnish children right near the top of the academic performance distribution in international test scores. In the Programme for International Student Assessment (PISA) tests, Finland has systematically placed very high in the rankings of participating countries.[1]

Whether school reforms involving new types of schools have been able to raise pupil performance forms the subject matter of this paper. We try to develop a better understanding of why these school reform have taken place and critically appraise what we can learn from the growing literature that tries to evaluate their impact on pupil outcomes.

The notion that giving schools more autonomy and that the decentralised operation of schools has scope to improve performance has been a crucial ingredient of

[1] For example, in reading literacy Finland was 1st out of 32 participating countries in 2000, 1st out of 41 in 2003, 2nd out of 56 in 2006, 3rd out of 65 in 2009 and 6th out of 65 in 2012.

the school reform movement. We consider the extent to which one can harness evidence to support this position. We do this in two ways. First, we review the existing evidence base with an aim of appraising the extent to which there is scope for these new schools to improve pupil performance. Second, we undertake a detailed case study of evidence from one specific set of school reforms – the introduction of academy schools in England.

The rest of the paper is structured as follows. In Section 2 we discuss the introduction of new types of schools and show some cross-country changes in autonomy levels that have occurred as they recently entered the education arena. Section 3 describes research methods used to evaluate the introduction of these new school types, and critically reviews evidence on them. Section 4 presents findings on academy schools in England, with a focus upon the impact of academy conversion on the quality of pupil intake, pupil performance and on medium term post-compulsory schooling outcomes. Section 5 offers some conclusions.

2. Introduction of New Types of Schools

Many types of school reforms can take place if educational authorities or government believe education standards are not as high as they should be. One can think of reforms that take place within existing school structures or reforms that change the type of school where children are educated. Reform within existing school structures can take the form of delegating tasks that were previously the responsibility of national or local education authorities to a school. More radical changes have involved the introduction of new school types, either converting existing schools or creating new ones with

considerably more powers over various aspects of school management. It is these organisational reforms that form the focus of this paper.

School Reform and New School Types in Different Countries

Table 1 shows examples of countries where introduction of a new school type has occurred since 1990. The nine countries in the Table were chosen to reflect what might be broadly thought of as competitor countries with the UK, and which are characterised by different levels and changes in school autonomy between 1990 and today. Three of the nine education systems introduced new school types since 1990. The other six countries did not introduce new school types over the same time period.[2]

The three key changes in school type that are of interest to us in this paper shown in Table 1 are: Sweden's free schools ("friskolor"), the US's charter schools and England's academy schools. The first two of these first appeared on the education landscape in the early 1990s. Academies first appeared in England in the early 2000s. All are schools that are characterised by greater autonomy and independent operation than are standard community schools.

There are two main ways of introducing new school types to an education system. Either brand new schools – start ups – are created or already existing schools are converted to a new school type – takeovers. The three main sets of school reform of the recent past differ in this regard. The majority of US charters and Swedish free schools, although not all, are new start ups while the majority of English academy schools, although again not all, are takeovers of existing, so called predecessor schools. This is an important institutional feature to bear in mind throughout this paper and is of

[2] This is not to say that no types of school reform took place in the other countries, merely that they did not feature the introduction of any new forms of school organisations over this time period.

particular relevance to the modelling approaches adopted to evaluate schools reforms, which are discussed later in the review of the existing work.

Changes in Autonomy and School Reforms

Figure 1 plots an index (standardised to have mean 0 and standard deviation 1) summarizing the level of autonomy held by schools over budgets, teacher hiring and firing and teacher salaries derived from PISA data for the nine countries considered in Table 1 in the years 2000 and 2012.[3] Countries located at around zero have levels of autonomy in line with the OECD average, while those with an index below (above) zero have below (above) average autonomy.

The Figure reveals that most of the countries that did not introduce new school types over the past twenty years show little change in the autonomy index as they lie close to the 45-degree line. These include Germany, France and Spain, who have low levels of autonomy in both years. Others stay near the 45-degree line with medium autonomy levels (Finland) or with high autonomy levels (the Netherlands). Swedish schools started out with a comparatively high level of autonomy in 2000, presumably because the free schools were introduced prior to this, and this remained relatively unchanged by 2012.

In terms of change, the two countries that stand out are England and the United States, where autonomy has increased by over one standard deviation (England) and around half of a standard deviation (United States) over 12 years, moving them from a position around the OECD average to among the systems with the highest autonomy. In the case of the United States, this change is likely to be partly a consequence of the new charter schools, which constituted 8 percent of 19470 secondary schools in 2012. In

[3] See Appendix 1 for a description of the construction of this index.

England, by the end of 2012 the share of academy schools among secondary schools had reached 50 percent. The introduction and sizable expansion of academy schools, which operate in a decentralised manner outside of local authority control, means that a large share of schools now have complete autonomy over personnel and budget decisions that were previously managed for most schools by local education authorities.

The PISA data confirms that de facto autonomy gains in England were mainly driven by the larger powers given to schools over the way they set teacher salaries and the hiring and firing of teachers, as well as over the formulation and allocation of school budgets (see Figures A1-A3 in the Appendix). In the US, the autonomy gains can be explained by similar changes. Finally, it is also interesting to note that it is not the whole of UK education that has featured a significant change in autonomy – these changes have only occurred in England and not in the other nations of the UK (Northern Ireland, Scotland and Wales) as the Rest of the UK data point on the Figure shows. The lack of change in the rest of the UK is consistent with the fact that the big change in the education landscape in this time period in the whole of the UK was the introduction of academy schools.

3. Evidence on the Impact of School Reforms

There is by now a growing and quite sizable literature examining the new types of schools in the US and Sweden.[4] This section reviews the means by which they have been evaluated and appraises what can be learnt from the existing research about them.

[4] The focus is placed upon studies that try to estimate the causal impact of the introduction of a new school type on pupil outcomes. Hence we focus on the literature on US charters and Swedish free schools as there are no studies on this for English academies except Eyles and Machin (2015), which is drawn upon for some of the results reported in Section 4 of this paper. There are reports that consider non-causal estimates on the Labour academies and the full Labour and Coalition academies programmes respectively by the National Audit Office (2010, 2014). There is also some largely descriptive, similarly

Methods of Evaluation

Various methods have been used to isolate the causal effect of school reforms on student outcomes. Naïve comparisons of outcomes for those who attend a reformed school against those who do not will not be likely to reflect the causal effect of attendance. This is true even after controlling for observable characteristics, if unobservable characteristics are correlated with both attendance and the outcome of interest. The methods used to allow for the endogenous nature of school choice often depend upon institutional context - for instance, the way school places are allocated or whether reformed schools are new start ups or conversions of already operating schools.

The research methods used in the literature that studies the impact of the introduction of new school types can be summarised as:

a) Regression based methods

A number of models have been proposed to estimate education production functions. A few of these, particularity in the Swedish context, have been used to estimate the effect of attending a free school on test scores. Unfortunately most non-experimental estimates are likely to be descriptive rather than causal as unobserved variables are likely to confound the estimate of free school attendance.

Two popular models are value added models (where previous test scores are used as control variables) and sibling differences (where differences in outcomes for siblings are regressed upon differences in inputs). The impetus behind the former is that previous test scores act as a sufficient statistic for unobserved inputs into education

non-causal, school-level empirical work in the education field. See, for example, Gorard (2014) or West and Bailey (2013).

production that may be correlated with the type of school attended. This is true only under very stringent, and somewhat arbitrary, parametric assumptions (see Todd and Wolpin, 2003). Sibling estimates can do a better job of controlling for unobservable confounding variables if these variables are constant across siblings as one might postulate for family background. However if differences in child-specific factors, such as tastes for learning or innate ability, drive differences in school choice then the causal effects of free school attendance will not be identified.

An illustrative example of the difficulty of adequately controlling for omitted variables is given in Dobbie and Fryer (2013). They obtain lottery estimates of the effect of charter school attendance on test scores. They combine regression with matching, on the same sample of lottery winners, where winners are matched to students attending public schools. Assuming that their lottery estimates are the true effects the non-experimental specifications appear significantly downwardly biased even after the matching procedure.

The difficulty in controlling for omitted variables means that experimental and quasi-experimental evidence yields more convincing estimates. The institutional context of US charter schools has made such methods more readily applicable.

b) Lottery estimates

Places in US charter schools are allocated via lottery in the case of oversubscription. If lottery places are randomly assigned comparisons of ‘lotteried in’ students with ‘lotteried out’ students can identify the causal effect of charter school attendance on outcomes. Most of the papers argue that, conditional on the specific charter lotteries entered, winning or losing a lottery is exogenous to outcomes such as test scores.

It is worth noting that winning a place at a charter school via lottery and attending a charter school need not be the same. Someone winning a lottery place may choose to attend a public school while those who lose may be offered a place at a later date. If winning a place has strong predictive power for actual attendance a binary variable indicating whether or not a pupil is offered a charter place via lottery can be used as an instrumental variable (IV) for the number of years spent in a charter. The effect identified is then a local average treatment effect (LATE), which is the effect of treatment on compliers – that is those who attend a charter because they win a place via lottery.

One potential problem with lottery estimates is that they rely upon the schools being oversubscribed and thus estimates may not have much external validity. Indeed there is suggestive evidence in Abulkaridoglu et al (2011) that oversubscribed charters in Boston generated test score gains in excess of those generated at non-lottery charters.

c) Instrumental Variables

As aforementioned, one-way to allow for endogenous school choice is to find a variable that influences the school choice decision but has no independent impact on the outcome of interest. Recent papers on US charters have been able to find credible IVs to isolate causal effects. Fryer and Dobbie (2011) note that within the Harlem Children's zone different cohorts of children have differential access to charters due to the opening year of the charter and their starting year of school.[5] Similarly, children within a targeted block zone have access to the schools while those just outside this zone are not.[6] They use a cohort/living in zone interaction as an instrument for the

[5] Cohort here refers to the year a child begins kindergarten.
[6] The Harlem Children's zone is currently 97 blocks. According to Dobbie and Fryer it is those in the original 24 blocks that are targeted by Harlem Children's zone staff to attended charters.

number of years enrolled. The underlying assumption is that while living in the zone and being in a given cohort may not be exogenous to test scores, the interaction between them is. Among other restrictions, this means that the effect of living in the zone on test scores must differ between birth cohorts only through the effects that this has on charter enrolment.

Angrist et al. (2015), who combine instrumental variables (IV) with matching, make note of the fact that while charters tend to be new schools, many charters in Boston and New Orleans are actually conversions from pre-existing schools. Being enrolled in a school the year prior to conversion (referred to as 'legacy enrolment') makes you eligible to be 'grandfathered' into the new school. They compare outcomes for a sample of legacy enrolees and a matched sample of students from other public schools. Legacy enrolment then can be used as an IV for years of enrolment in a charter school.[7] The IV estimates provide a potentially useful counterpart to results gained via lottery research designs, as they do not focus on a subsample of schools that are oversubscribed.

d) Randomised Controlled Trials

Randomised controlled trials can provide convincing evidence of the impact of charter attendance on outcomes. Rather than randomly allocate students to charter schools, Fryer (2014) randomly allocates charter school practices to public schools in Houston. Students enrolled in the treatment schools prior to treatment (but not in their final grade) or those who are zoned to attend a treatment school then provide intention to treat (ITT) estimates of the effect of charter practices on pupil performance. Because

[7] The paper actually allows for partial violations of the exclusion restriction due to the fact that they have outcomes in the legacy grade. The exclusion restriction as stated in their paper is that potential test score gains form legacy year to outcome year must, in expectation, be the same for legacy and non-legacy enrolees once treatment status (charter attendance) is fixed.

treatment is randomly assigned a straightforward comparison of outcome means should reveal a causal effect. However, while the random nature of assignment prevents non-random selection into treatment, students can select out of the treatment sample and study elsewhere, giving these estimates an ITT interpretation. Assignment to treatment can then be used as an instrumental variable for actual attendance of a charter school to purge any bias from the estimates that could result from non-random selection out of the treatment group.

Evidence on US Charters

America's first charter school opened its doors in 1992 and since then there has been significant growth in the prevalence of charter schools in the US public school system. As of 2013-2014 around 6400 charter schools are in existence serving 2.5 million pupils.[8] Like academies, charter schools enjoy significant autonomy relative to the rest of the public school system and are publicly funded while being privately owned.

Table 3 gives a partial summary of the literature on charter schools.[9] The overall takeaway from the Table is that the evidence on charter effectiveness is mixed with some charter schools seemingly generating large gains on test scores and even improving middle to long-term outcomes for some students. A stylised finding is that studies of charters located in urban areas that adhere to the 'No Excuses' model, which stresses behavioural norms and work ethic, tend to produce large gains when serving less-privileged students. Studies across multiple states (like Gleason et al., 2010) have

[8] Numbers available from - Estimated Number of Public Charter Schools & Students, 2013-2014, National Alliance for Public Charter Schools. Available at <http://www.publiccharters.org/wp-content/uploads/2014/02/New-and-Closed-Report-February-20141.pdf>

[9] Unless mentioned otherwise results on test scores are in standard deviations and refer to the effect per year of charter attendance. Similarly lottery estimates refer to LATE rather than intention to treat estimates.

found less compelling results with negative impacts on standardized state level tests for more privileged pupils who do not qualify for free school meals (FSM). Similarly, studies described in Table 3 have found that urban charters tend to generate greater achievement gains relative to non-urban charters.

For those schools that have generated achievement gains Fryer (2014) highlights the use of increased schooling time (often including Saturday school), the use of effective staff who deliver high-dosage tutoring, the use of data in informing instruction practices and a culture of high expectations as being the key components in delivering gains. Interestingly, Fryer and Dobbie (2013) find that in 39 New York City charters more traditional inputs into education production such as class size and teacher's education do not correlate with effectiveness.

Evidence on Sweden's Free Schools

In the late 1980s and early 1990s the Swedish schooling system underwent rapid and encompassing changes. Alongside a new funding system for schools, in which money previously allocated by central government was given to municipalities, there were also policies aimed at increasing the school choice available to parents. In 1992, a voucher system was introduced into Swedish schools whereby fee-paying private schools had the option to apply for free school status.

Free school status (which was subsequently taken up by almost all of Sweden's private schools) enabled former private schools to attract students whose education would be funded by the student's municipality of residence. While such schools retained freedoms over and above those afforded to publicly owned schools, such as greater freedoms over setting curriculum, they could no longer charge fees or select students on ability.

The reforms also allowed new, privately owned, schools to come into existence funded with public money. This has led to a modest increase in the number of free schools over time in Sweden. Before the reform 89 independent schools served lower secondary pupils. This number grew to 790 by 2013 and now around 16 percent of Sweden's pupils are free school students (West 2014).

Estimates of the return to attending a free school are scarce. The reform meant that, at municipality level, every student attending a free school leads to a reduction in the budget available to the municipality's public schools. Research has thus focused on the impact that free school competition has had on public school performance by relating variation in the share of students attending a free school across municipalities to municipality level outcomes (see, for instance, Bergstrom and Sandstrom, 2005).

Nevertheless some papers do have, often descriptive, evidence of returns to attending a free school in Sweden. Table 4 offers a summary of the existing literature on free schools. The evidence tentatively suggests that while greater growth in free schools at municipality level has had positive effects on municipality level test scores, the return to free schools themselves may well only account for a small part of this. Thus there seems to be less evidence that the free schools programme have produced anything like some of the sizable performance boosts that some of the US charters seem to have delivered.

4. Case Study of Academy Schools in England

In this section of the paper, we describe and offer evidence on a number of features of the English academies programme. As already noted, the vast majority of academies are takeovers of existing schools, and so the most applicable evidence for comparison

with cited in the previous section is the small number of papers on charter takeovers (Abdulkadiroglu et al., 2014) and the injection of charter school practices into US public schools (Fryer, 2014).[10]

The Schooling System in England

The school years in England are divided into four Key Stages: Key Stages 1 and 2 in primary school and Key Stages 3 and 4 in secondary school. Children attend primary school for six years between the ages of 5/6 and 10/11 and take Key Stage 1 tests at the end of year 2 and Key Stage 2 tests when they leave primary school at the end of year 6. They then move to secondary school for five years of compulsory schooling from ages 11/12 to 15/16 (in years 7 to 11). In secondary school, Key Stage 3 assessments take place at the end of year 9 and the school leaving exams - Key Stage 4 - at the end of year 11 (commonly known as the General Certificates of Secondary Education, or GCSEs), which for the time period we study was the last year of compulsory schooling.[11] After this, they choose whether to stay on in education and take Key Stage 5 assessments two years later (in the academic track, these are Advanced Levels, or A levels), after which they can apply to enrol in higher education.

Data

England has rich administrative data on pupils and schools. This has featured in quite a lot of research in the recent past (see Machin and Vignoles, 2005, for a review of earlier work and Cassen, McNally and Vignoles, 2015, for an overview of more recent work). Pupil level data on Key Stage results dates back to the mid-1990s in the

[10] The free schools that have been introduced in England since 2010 are a closer comparison to the majority of US charters and to the Swedish free schools. To our knowledge, there is no quantitative evidence on the impact of free school introduction in England as of yet.

[11] The leaving age in England where individuals must engage in some form of education or training became o 17 in 2013 and 18 in 2015.

National Pupil Database (NPD).[12] From the 2001/2002 school year the Pupil Level Annual Schools Census (PLASC) contains information on all children enrolled in state schools in years 1 through 11, together with a range of demographic characteristics.[13] We use both of these in the analysis that follows, together with school level data from the Annual Schools Census. To identify academy conversions we use data made available by the Department for Education, the UK ministry responsible for schools, in online extracts that give information on all academies that have opened or are in the process of opening.

We also look at post-compulsory schooling outcomes using upper secondary schooling data from the NPD (Key Stage 5), measuring whether or not a student stayed on after compulsory schooling and did their A-Levels, and data on university participation from the Higher Education Statistics Agency (HESA), which tracks all students enrolled in a higher education institution in the United Kingdom.

The Introduction of Academy Schools

In the 1990s and early 2000s there was a widespread recognition that some secondary schools in England were not delivering a good enough education to children attending them. Increasing pressure to do something about these schools, which were typically serving children living in disadvantaged urban areas, led to the introduction of a new type of school – the academy school.

Prior to this, the majority of secondary schools in England were community schools, most of whose operations were carried out under the control of local education authorities. There are around 150 such local authorities in England and just over 3000

[12] The first available year of pupil level data for Key Stages 1 to 4 differs, with the first KS4 data dating back to 1994, KS2 and KS3 to 1996 and KS1 to 1998.

[13] This data has been collected three times per year (January, May and September) and pupils can be traced back to earlier years of the NPD via their unique id. We use the year-on-year January collection because this collection is the most available and consistent through time.

secondary schools. Some other types of state funded schools did and continue to exist – either religious schools (called voluntary controlled or voluntary aided schools) or foundation schools.[14]

Foundation and voluntary aided schools have traditionally had considerably more autonomy than community schools – for example, they have a governing body that employs staff and has the responsibility for the admissions policy. But they are very different from the sponsored academies established during the 2000s. They are often located in privileged neighbourhoods and sometimes have selective admission procedures they inherited from the past, which newly established academies could not adopt.[15]

By contrast, admissions for community schools are run by the local education authority, as well as the hiring and firing of staff. No state funded school in England is allowed to charge any fees to their students.[16] This is in contrast to many other types of highly autonomous, privately managed and largely publicly funded schools in Europe (like *colegios concertados* in Spain or *ecoles privés sous contrat* in France), who usually charge small fees to their students. As Table 2 shows, in January 2002, the year before the first academies were introduced, the majority – around two thirds of secondary schools – were community schools that largely operated under the remit of the local authority.

[14] Foundation schools used to be called grant maintained schools. In the 1990s, they in part decentralised from local authority control if a majority of parents had voted in a ballot to do so (see Clark, 2009). Prior to the emergence of academies, there were also a small number of city technology college (CTCs) which, in many respects (though not poor pupil performance), were the pre-cursors of academy schools – see Whitty, Edwards and Gewirtz (1993). Almost all of the small number of existing CTCs (there were 14 in the 2001/2 school year) converted to become academies in the 2000s.

[15] Selection on ability was banned in the year 1997/98 in England for all state funded schools. Only schools that at the time had such arrangements in place continued to be allowed to select pupils on ability. The remaining selective secondary schools – there were about 200 of them as of August 2015 (Department for Education, 2015) – used to be or continue to be grammar schools.

[16] Around 7 percent of secondary school age children attend fee paying private schools in the independent sector.

Academy schools are a new type of state funded school that operates outside of local authority control. In almost all cases, they are conversions from an already operating predecessor school. Like foundation and voluntary aided schools, they have a governing body that employs staff and has the responsibility for school admissions.[17] But they also have more autonomy in a number of dimensions through additional freedoms they are able to exercise. This includes opting out of and not being obliged to follow the national curriculum (apart from for core subjects) that defines what is taught in English secondary schools, and a host of other freedoms – for more details, see the discussion in Department for Education (2014) and our discussion on the use of academy freedoms that we offer later in the paper.

In terms of the processes to set up an academy school, their introduction required the signing up of a sponsor, or team of independent co-sponsors, who appoints and delegates the management of the school to a board of governors with responsibility for employing all school staff, agreeing levels of pay and conditions of service and deciding on the policies for staffing structure, career development, discipline and performance management. The role of the sponsor, which could be an individual, business, religious body or a university, was mainly to contribute management expertise. In the early days, sponsors were required to contribute 10 percent to the capital cost of new school buildings, up to a maximum of two million pounds, the rest being complemented by a government grant. The condition however was relaxed in 2007, and eventually eliminated in 2010, because it was considered a barrier to

[17] Whilst academies are responsible for their own admission arrangements, they operate under the same admissions code as other state schools. For instance, in the case of oversubscription, priority first has to be given to children in care or who have been in care (see for instance Department for Education, 2003, and 2006). De facto, academies implement the same criteria for allocating places in case of oversubscription to those used by community schools, giving priority to siblings, then typically using distance criteria based on living close to the school or (in a few cases) using a lottery (see Noden, West and Hind, 2014).

recruiting sufficient numbers of sponsors and because, de facto, most sponsors never actually paid the full contribution (National Audit Office, 2010).

The first three academies opened in the school year 2002/3 and there was a gradual conversion of schools to academies in subsequent years so that, as shown in Figure 1 and Table 2, there were 203 academy schools – or 4 percent of secondary schools - up and running by January 2010. A change of government came with the election of May 2010 as a Conservative/Liberal Democrats coalition government replaced the Labour party, which oversaw the initial academies programme. Following the change of government, and the appointment of a very keen school reformer (Michael Gove) as Secretary of State for Education, there was a very rapid introduction of legislation via the Academies Act of 2010, which resulted in a huge expansion of the academies programme.[18] By January 2015 many more conversions had taken place, so that the majority of secondary schools (2075 or 61 percent) were operating as academies.[19]

It is important to note that, in both an institutional sense and in terms of which kind of schools converted to academies, the pre-Academies Act and post-Academies act conversions are different. The 203 schools that converted prior to January 2010 were on the whole very poorly performing schools attended by disadvantaged pupils. Moreover they all had to sign up a sponsor prior to becoming an academy. Both of these things changed for the mass academisation that occurred after the Academies Act.

[18] The Act was passed very quickly, receiving Royal Assent and being put into operation in July 2010. Whilst the pre-2010 period only permitted secondary schools to become academies, the Act enabled primary schools to become academies and also ushered in free schools similar to the Swedish free schools, which were start up schools typically set up by parent or community groups. By January 2015 there were 119 secondary free schools.

[19] Strictly speaking, 1893 were secondary academies and a further 182 were new types of schools also classified by the Department of Education under the academies definition (these 182 were made up of 119 free schools, 30 university technical colleges and 33 studio schools).

First, schools no longer had to sign up a sponsor and, as Figure 2 shows, the majority did not – of the 2075 academies in operation by 2015 only about a quarter (531) were sponsored academies (and these 531 include the 203 sponsored academies which converted pre-2010). Second, many of the post-2010 academies were quite the opposite of what went before. The post 2010 academies are, on average, high performing schools serving advantaged pupils.[20]

This is shown in Figure 4. In the labour economics literature (notably Juhn, Murphy and Pierce, 1993), wage differentials between groups of workers (e.g. split by race or gender) are sometimes illustrated by positioning the average wage of one group of workers at a percentile point in another group's wage distribution. We apply this technique to our setting in Figure 4, which shows the percentile point of the average academy set up in a given school year in the non-academy distribution of school performance and disadvantage in the year prior to conversion.

The first chart in Figure 4 shows this exercise for Key Stage 4 performance in the year before conversion. It is very clear that pre-2010 schools that converted to academies had on average low-level KS4 performance in their predecessor state. For example, the average points score in the year before conversion of the first three converters in the school year 2002/2003 was located at the 4th percentile of the KS4 points score distribution of all other (non-academy) secondary schools. For all conversions to sponsored academies before 2010, the average converter was at the 15th percentile of the distribution.

[20] The school accountability regime that operates for English schools is called the Office for Standards in Education which (roughly every four years) inspects schools and rates them in a range from outstanding to inadequate. Post-2010 non-academy schools rated outstanding were actively encouraged by the Department for Education to become academy converters.

This changes after 2010. There is a transition year in 2010 in the Figure made up of pre- and post-Academies Act academies, but after that the mean percentile jumps up massively. Including the transition year, average point score of new academies rises to the 50th percentile. If calculated just on 2011 onwards it rises to the 56th percentile. Thus, the key feature that pre-2010 academies were previously badly performing schools is actually reversed post-2010 as the average academy comes from an above median KS4 predecessor school.[21]

The second chart of Figure 4 conducts the same exercise for a measure of pupil disadvantage, the proportion of pupils eligible for free school meals (FSM), in the year prior to conversion. The Figure shows that the pre-2010 Labour academies were, on average, very high up the FSM distribution – the average converter was at the 88th percentile of the non-academy distribution prior to conversion. For the post-2010 Coalition academies, the mean was located much further down the non-academy distribution at the 57th percentile (including the 2010 transition year).

Therefore, in terms of what kinds of schools and pupil populations were involved in academy conversions, it is clear that there was a turnaround from the initial programme to the subsequent mass academisation. The initial model of trying to turn around badly performing schools serving disadvantaged pupils became one of a school reform catering for better performing schools serving advantaged pupils.

Research Designs and Results For Evaluating The Initial Programme

We now consider the pre-2010 academies programme in more detail, showing evidence of the impact of conversion on the quality of pupil intake, pupil performance

[21] Eyles, Machin and Silva (2015) show difference-in-difference estimates of the impact of predecessor school KS4 (measured in the year before conversion) on the probability of conversion. The sign of the estimated coefficient switches from significantly negative pre-2010 to significantly positive post-2010.

and post-compulsory schooling outcomes. Given that these academy conversions took place from a selected group of badly performing schools, with disadvantaged pupil populations, it is important to implement a coherent research strategy that ensures estimates of impact are not biased by this. We therefore first discuss the research designs that we implement to do so, followed by the results.

a) Research Design – Quality of Pupil Intake

We measure quality of pupil intake in terms of the end of primary school Key Stage 2 test scores and free school meal eligibility of children enrolling in secondary school in year 7. The research question of interest is whether conversion to an academy changed the quality of intake compared to predecessor schools.

To study this we have considered whether KS2 and FSM levels of pupils enrolling in a treatment group of academy conversions in year 7 improved relative to pupils enrolling in a comparable set of control schools. Because the treatment group is selected it is necessary to find a control group that is matched on pre-conversion date characteristics (and pre-trends). Eyles and Machin (2015) study the initial academies programme in the school years 2001/2 to 2008/9 defining a treatment group of 106 schools that gained academy status in those years. Their control group of 114 schools is schools that also become academies, but after the sample period ends. The balancing tests they report show that the treatment and control group to be well balanced with no statistically significant differences between baseline school performance (KS4 and KS2) and pupil characteristics (proportions male, FSM, white and special educational needs). As the control schools become academies after the sample period this also means they are likely to be balanced on the common set of unobservables (like having an 'ethos' for academisation) that may lie behind academy conversion.

With this comparison it is possible to study whether the quality of the pupil intake alters when a school converts to an academy from difference-in-difference regression estimates of changes in pupil intake, measured by age 11 test scores (KS2 results) and FSM status of year 7 enrollers, before and after conversion for pupils attending schools that do and do not convert in the sample period. The following equation specified for pupil i in school s in year t enables estimation of the differences-in-differences coefficient δ:

$$Y_{ist} = \alpha_s + \alpha_t + \delta A_{ist} * I(E \geq t = c) + \sum_{j=1}^{J} \lambda_{1j} X_{jist} + u_{1ist} \quad (1)$$

In (1) Y is the outcome of interest (KS2 scores or FSM status), A is a dummy variable equal to 1 if the secondary school s attended by pupil i in year t in the entry year of secondary school is in the treatment group (i.e. will become or is an academy in the sample period) and equals 0 if the school is in the comparison group (schools that do not convert to an academy in the sample period, but convert after the sample period ends). Defining E as an event year, the dummy variable indicator I(E ≥ t = c) takes a value 1 if the pupil enrols post-conversion year c and X denotes a set of control variables. Finally, α_s are school fixed effects, α_t are year effects and u_1 is an error term.

The specification in (1) imposes an average post-conversion effect across all post-conversion years. A more flexible specification estimates separate treatment effects for pre- and post-conversion years, in an event study setting, as follows:

$$Y_{ist} = \alpha_s + \alpha_t + \sum_{e=c-4}^{e=c+3} \delta_e A_{ist} * I(E=e) + \sum_{j=1}^{J} \lambda_{2j} X_{jist} + u_{2ist} \quad (2)$$

We report event study estimates of four pre-conversion δ's (from E = c-4 to c-1) and four conversion year and post-conversion δ's (from E = c to c+3).

b) Results – Quality of Pupil Intake

Estimates of equation (1) and (2) are shown in Table 5. Columns (1) and (2) show the estimates for KS2, and columns (3) and (4) for FSM status. Considering first the average effect of academy conversion on KS2 scores, the specification in column (1) shows that the quality of pupil intake rises significantly, increasing by 0.074 of a standard deviation (σ) a year relative to the control schools in the year of conversion and the three subsequent years of operation as an academy school. The event study specification in column (2) shows that this average of 0.074σ featured an increase in the actual conversion year (0.058σ) followed by a significant pick up in the post-conversion years (of 0.083σ in c+1, 0.142σ in c+2 and 0.115σ in c+3). The statistically insignificant pre-academy conversion coefficients (from c-4 to c-1) also reveal that there were not different pre-conversion year trends in the treatment and control schools.

The FSM status results confirm the pattern. The column (3) specification shows a significant reduction, of the order of 2.2 percentage points, in the number of FSM eligible children enrolling after academy conversion. The increase (in absolute terms) in the post-conversion years is also seen for this outcome, as shown in column (4) where the estimated effect goes from -0.022 in conversion year c, -0.028 in c+1, -0.043 in c+2 to -0.051 in c+3. Thus, for both measures, the analysis reveals an improvement in the ability composition of pupils in terms of their prior academic achievement entering schools after they become academies, at least in their first years of operation.

c) Research Design – Pupil Performance and Post-Compulsory School Outcomes

The results of Table 5 show that the intake quality of pupils enrolling in academy schools rose compared to the pupils enrolling in the schools in their predecessor state. Thus the pupil composition altered, and this has ramifications for

studying the impact of academies on pupil performance. It is therefore important to implement a research design that is not contaminated by changing pupil composition.

Eyles and Machin (2015) study KS4 performance effects for pupils who were already enrolled in the school prior to conversion and are then affected by academy conversion in a subsequent year of their secondary schooling. Since the initial enrolment decision was made for the pre-conversion school, academy conversion should be exogenous to these students, and can be set up as in terms of an intention to treat (ITT) empirical exercise, from which we can obtain a causal estimate of a local average treatment effect (LATE). The ITT group is all pupils enrolled in the predecessor school who either do or do not take their year 11 KS4 exams in the school. The approach is similar to that taken in Abdulkadiroglu et al. (2014), who study school takeovers in New Orleans, referring to pupils who stay in a converting school as 'grand-fathered' pupils.

As we are interested in the causal impact of academy conversion on KS4 results we can first operationalise our empirical analysis by means of the following value added equation:

$$KS4_{ist} = \alpha_s + \alpha_t + \theta_1 A_{ist} * I(E \geq t = c) + \sum_{j=1}^{J} \pi_{1j} X_{jist} + \varphi_1 KS2_{ist} + v_{1ist} \qquad (3)$$

In (3) estimates of the θ_1 coefficient is analogous to the KS2/FSM difference-in-difference set up above, but because we now restrict to pupils enrolled in the pre-conversion school there is one subtle difference. This is that not all pupils who end up taking their KS4 exam at a school that becomes an academy ($A_{ist} = 1$) were enrolled in the school pre-conversion. Conversely, not all students initially enrolled in a school that converted to an academy ($ITT_{ist} = 1$) remain in the school to take their KS4 exams.

Thus, ordinary least squares estimates of θ_1 from (3) will not reflect a causal estimate. To account for selection into and out of treatment we use enrolment in a to be converted school (ITT_{ist}) as an instrument for A_{ist}, to estimate a LATE as follows:

$$A_{ist}*I(E \geq t = c) = \alpha_s + \alpha_t + \theta_2 ITT_{ist}*I(E \geq t = c) + \sum_{j=1}^{J} \pi_{2j} X_{jist} + \varphi_2 KS2_{ist} + v_{2ist} \quad (4)$$

$$KS4_{ist} = \alpha_s + \alpha_t + \theta_3 ITT_{ist}*I(E \geq t = c) + \sum_{j=1}^{J} \pi_{3j} X_{jist} + \varphi_3 KS2_{ist} + v_{3ist} \quad (5)$$

In the first stage in (4) the estimates of θ_2 show the proportion of the ITT group that stay in the academy and take KS4 exams there. These are the 'grandfathered' pupils that remain in the school. Equation (5) is the reduced form regression of KS4 results 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).

d) Results – Pupil Performance

Estimates of the impact of academy conversion on Key Stage 4 pupil performance are shown in Table 6. The Table shows two sets of OLS, ITT and IV estimates of the impact of academy conversion on end of secondary school KS4 pupil performance in value added specifications that net out end of primary school K2 pupil performance. Columns (1) to (3) show the average estimates as per equations (3)-(5). In columns (4) to (6) the estimates extending this to the event study setting are presented.

Looking at the estimates reported in columns (1)-(3) of Table 6 shows that KS4 pupil performance improved significantly in the academy conversions relative to the control schools. The interpretation of the ITT estimate in column (2) of a significant

0.073σ improvement is that KS4 went up by 0.073σ more for children enrolled in a pre-conversion school as compared to children enrolled in control schools in the same school years. The IV estimate in column (3) corrects for the fact that not all ITT children sat their KS4 examinations in the school (in fact 93.2 percent did as the highly significant first stage at the bottom of the Table shows) and this rises to 0.079σ.

The event study estimates in columns (4)-(6) show that effects are more sizable as the academy has been in place longer and where children therefore get more years attendance at the academy. The IV estimates of column (6) show an insignificant 0.037σ effect in the year of conversion that rises to 0.184σ at three years post conversion (c+3). The event study specifications reveal no differences in the pre-conversion time periods between pupils in treatment and control group schools.

These 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. Whilst the first stage shows the size of the complier group to be very high at 93.2 percent, we did further probe this by looking at estimates where we interacted the intention to treat dummy in the first stage regression (equation (4) for the column (3) specification in Table 6) with indicators for being white, being eligible for free school meals, and the pupil's standardised KS2 score. We find that white individuals, those who are ineligible for free school meals and have higher KS2 scores are more likely to comply. However,

[22] See Angrist and Imbens (1994). The conditions are intuitively reasonable in our case. We 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 must be random across individuals, and unrelated to, for instance, ability. While we cannot directly test whether this assumption holds, balancing tests show that individuals pre-enrolled in an academy and those in control schools who are not are observationally similar (see Eyles and Machin, 2015).

the magnitudes of the differences are small (compared to the average of 93.2 percent) with differential compliance not being very marked.[23]

In the IV research design setting that we implemented, we find that being affected by academy conversion had a positive impact on end of compulsory school pupil performance in the initial academies programme. 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. Next we ask whether it had any longer lasting impact on post-compulsory schooling outcomes.

e) Research Design –Post-Compulsory School Outcomes

The final set of outcomes we consider for the initial academies programme are those related to post-compulsory schooling. We are interested in whether the school performance effects we have identified translate into improved educational outcomes post-treatment. We have four outcomes to study here:

- staying on in education at the end of compulsory schooling (age 16 in the time period we study) and doing A-levels;
- entering higher education in the four years after KS4 completion (this includes starting a bachelor's degree but also foundation degrees, which are sometimes an alternative to A Levels for entry to bachelor degree programmes);
- being enrolled for a bachelor's degree at a Russell Group university (the elite UK universities) four years after KS4 completion;

[23] The word 'comply' is used loosely here since in IV terminology compliers are those who only attend an academy if they are pre-enrolled. We use 'comply' to refer to those pupils who receive treatment given that they are intention to treat. For those who were pre-enrolled, being white increases the probability of compliance by 2 percentage points while free school meal eligibility decreases the probability of compliance by 0.7 percentage points and a one standard deviation increase in KS2 performance increases probability of compliance by a 0.6 percentage points.

- being enrolled for a bachelor's degree at a non-Russell Group university four years after KS4 completion.

We first study the causal impact of academy conversion on these medium term outcomes using the same approach as in the KS4 analysis. We then additionally control for KS4 outcomes to net out the treatment effect on end of secondary school pupil performance. We do this to see whether any impact on the medium term outcomes is driven mainly by KS4 improvements, or whether the KS4 boost that pupils exposed to academy conversion received propels them to a higher trajectory over and above the effect on their age 16 outcomes.

f) Results – Post-Compulsory School Outcomes

Estimates of the impact of academy conversion on the four post-compulsory outcomes are shown in Table 7. The instrumental variable estimates in columns (3) and (6) show that having attended an academy increases the likelihood of staying on after compulsory schooling age by 1.7 percentage points on average, and it increases the likelihood of entering university by 2.8 percentage points. When looking at Bachelor Degrees only, results differ by the type of university attended. As the bottom panel of Table 7 shows, students who attended an academy were a statistically insignificant 0.2 percentage points more likely to enrol in a Bachelors Degree at an elite university (Russell Group institution) than the control group, compared to an average of 2 percent in the sample. Enrolling in Bachelor Degree at a non-Russell Group university is a significant 2.5 percentage points higher, relative to an average of 15 percent in the sample.

Table 8 shows the event study estimates for the four post-compulsory outcomes in the odd numbered columns (1), (3), (5) and (7). These show that the effects of

academy conversion on students' medium term outcomes increase with the amount of years an academy has been in operation. While for those students who took their KS4 in an academy one year post-conversion there was only a 1.4 percentage points increase in the likelihood of staying on, taking exams four years post-conversion (which for those of the group enrolled pre-conversion who stayed in the school to KS4 corresponds to four years exposure) resulted in a 4.9 percentage points higher rate of staying on. The effect on attending university are even stronger: the one year (t) effect of academy conversion increased the likelihood of going to higher education by 1.7 percentage points, while the four years (c+3) effect is an increase of 8.8 percentage points, a large and highly significant effect. Similar patterns hold for the additional two outcomes, enrolling in a Bachelors Degree at an elite university and non-elite university. However, here the effects are still small for the elite universities as the positive impact is concentrated in getting admitted to a non-elite BA degree.

Finally, we consider whether these medium term effects are attributable to the KS4 impact of academy conversion in the even numbered columns (2), (4), (6) and (8) of Table 8. These show that when controlling for KS4 outcomes, the effect of academy conversion on staying on post-compulsory education is (not surprisingly) largely wiped out, suggesting that students who did stay on did so largely because of their improved outcomes at age 16. However, and importantly, there remains an effect above and beyond what is driven by improved KS4 outcomes on the probability of attending university: students with potential to be exposed to four years of schooling at an academy (the c+3 estimates) were a sizable 6.2 percentage points more likely to attend university, suggesting that academy conversion had an effect on students' aspirations or that academies were better at motivating students to apply for university.

Research Designs and Results For Evaluating Mass Academisation

The mass academisation that followed the election of the new government in May 2010 is still a relatively recent phenomenon and it is not possible to yet study impacts on pupil performance for pupils in these more recent conversions. But we can study whether the quality of pupil intake was altered in the post-2010 academies as compared to the pre-2010 initial programme conversions. We adopt a similar treatment-control design to that followed earlier, with academy converters being compared to schools that will subsequently become academies but do not in the time period considered. For the post-2010 academies, we are also able to distinguish between sponsored and converter academies, the former being more like the initial programme academies, all of which were sponsored.

Table 9 reports results from carrying out this exercise from Eyles, Machin and Silva (2015) studying KS2 point scores amongst year 7 enrolments in treatment versus control schools before and after academy conversion. They are therefore analogous to the initial programme academy results in Table 5 that we discussed earlier except timing issues now mean we can only study what happened in the three years including and subsequent to conversion (i.e. from c to c+2, as compared to c to c+3 in Table 5).

Columns (1) and (2) of Table 9 respectively show the academy conversion impact on the level of prior attainment of new enrolments (year 7 KS2) for pre-2010 and post-2010 academies. The column (1) estimate of 0.031σ is consistent with the earlier results and shows a small, but significant improvement in the quality of pupil intake for the initial programme academy conversions. However, as column (2) shows, for the post-2010 mass academisation conversions, the effect is small and essentially zero. As we showed in Figure 4, the prior to conversion level of attainment and the

composition of students at the post-2010 coalition academies were much more favourable than for the initial programme academies, suggesting that their intake was already higher achieving and that there was less scope for improvement.

This post-2010 effect pools sponsored and converter academies. When they are split out and allowed to have separate coefficients, as shown in columns (3) of Table 9, it is interesting that a positive and significant intake effect of very similar magnitude (= 0.046σ) to the pre-2010 academies estimate (= 0.031σ) emerges for the post-2010 sponsored academies. On the other hand, the intake effect is insignificant and tiny in magnitude for the converters. Thus it seems that the post-2010 sponsored academies are having similar intake quality effects to the initial programme academies, but that the pure converters (who are not sponsored) are not.

Table 10 shows the same exercise for FSM eligibility. The proportion FSM falls in both Labour and Coalition academies following conversion, though in terms of magnitudes by twice as much in the former. On this metric, in contrast to the KS2 results, a comparison of columns (1) and (3) of the Table shows the Labour sponsored academies are however not the same as the Coalition sponsored academies, suggesting this to be one dimension along which they seem to be less comparable.

As already noted, it is too early to evaluate end of school KS4 effects for the post-2010 academies since they have not been in place long enough to carry out the same kind of ITT research design that we discussed above for the initial programme academies. But it is interesting that the two sorts of new academies are different in their effect on changing pupil composition which may suggest scope for performance effects to be different when they can be evaluated once the cohorts of pupils enrolled pre-conversion complete their five years of compulsory secondary schooling.

The Use of Academy Freedoms

The final issue we consider is which of the different freedoms that academy conversion generates for schools may be related to the findings reported above. To do so we draw upon survey responses from a Department for Education (2014) survey of headteachers on the use of academy freedoms. The Department for Education sent a questionnaire to all 2919 academies open in May 2013, obtaining 720 responses. Table 11 reports responses on what uses of academy freedoms are put into action from questions asking about which changes academies have made, on whether the main reason for change was in an aim to improve attainment, and on what the most important changes have been.

The first column 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 is aimed to be an overall school improvement programme. A number of operational aspects resulting from increased autonomy feature prominently; for example, those linked to procurement, cost savings and capital expenditure. Changes in management structures, via reconstituting governing bodies and changing the school leadership, are also prominent. Similarly, collaboration with other schools and changing pay structures for teachers are often implemented. Curriculum change is also high up the list. Other changes that academies have made are in common with features of the 'no excuses' charters in the US, like lengthening the school day and changing the lengths of school terms. Overall, the high percentage that report changes related to autonomy freedoms is striking and, returning back to our earlier discussion of autonomy levels rising in English schools, is evidence that the academies programme is likely to have contributed to this increase.

When one considers (in the last two columns of Table 11) which of the reported changes headteachers state are linked to improved attainment, and when one considers the most important change they claim to have made, it is evident that the 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 key factors that are likely to lie behind the pupil intake and pupil performance improvements identified in our empirical analysis.

5. Conclusions

The focus in this paper has been on whether the introduction of new school types to different educational settings has scope to improve pupil outcomes. The answer to that question from our analysis of academy schools in England, free schools in Sweden and charter schools in the United States is that, in certain settings, they can. To establish this, we review evidence on the impact of the introduction of these new schools on pupil outcomes and present new evidence for the case of secondary schools in England.

Academy schools in England have become a major widespread and encompassing school reform. Evidence on the impact of academy introduction highlights two key episodes of academy school reform. The first is a gradual introduction of academies to secondary school education in England where disadvantaged pupils enrolled in poorly performing schools were the focus of attention. The second is a mass academisation where the number of schools converting to academies accelerated very rapidly, extended to other aspects of schooling (most

notably the primary sector and the introduction of new free schools) and where the target was no longer turning around bad performers.

There is evidence that the initial programme altered the quality of pupil intake (i.e. increased demand for places at the newly converted academies), boosted end of compulsory school exam performance and had a positive impact on medium term post-compulsory schooling outcomes. These improvements are in line with the findings from US schools where the best performing charter schools produced improvements in pupil performance and seem to generate more promising outcomes than the Swedish experiment on free schools.

However, whilst it is still too early to properly evaluate the impact of the subsequent mass academisation on pupil performance, the positive KS2 intake effects are not present, on average, in the schools featuring in this mass rollout. There is one exception to this: for a minority of the schools in the newer programme, who function like the initial batch of academies in that they have a sponsor involved in the school reform and the post-reform running of the school, we found positive effects on KS2 intake quality. This is not seen in the converter academies that are rather different in nature from the sponsored academies. It will therefore be interesting, and a future research project of high policy relevance, to see in due course whether or not the benefits that the initial academies programme generated translate over to either the new sponsored or converter academies. Similarly, in the longer run an evaluation of whether the positive initial and medium term educational impacts of the initial academies translate into better labour market outcomes will be an important future research undertaking.

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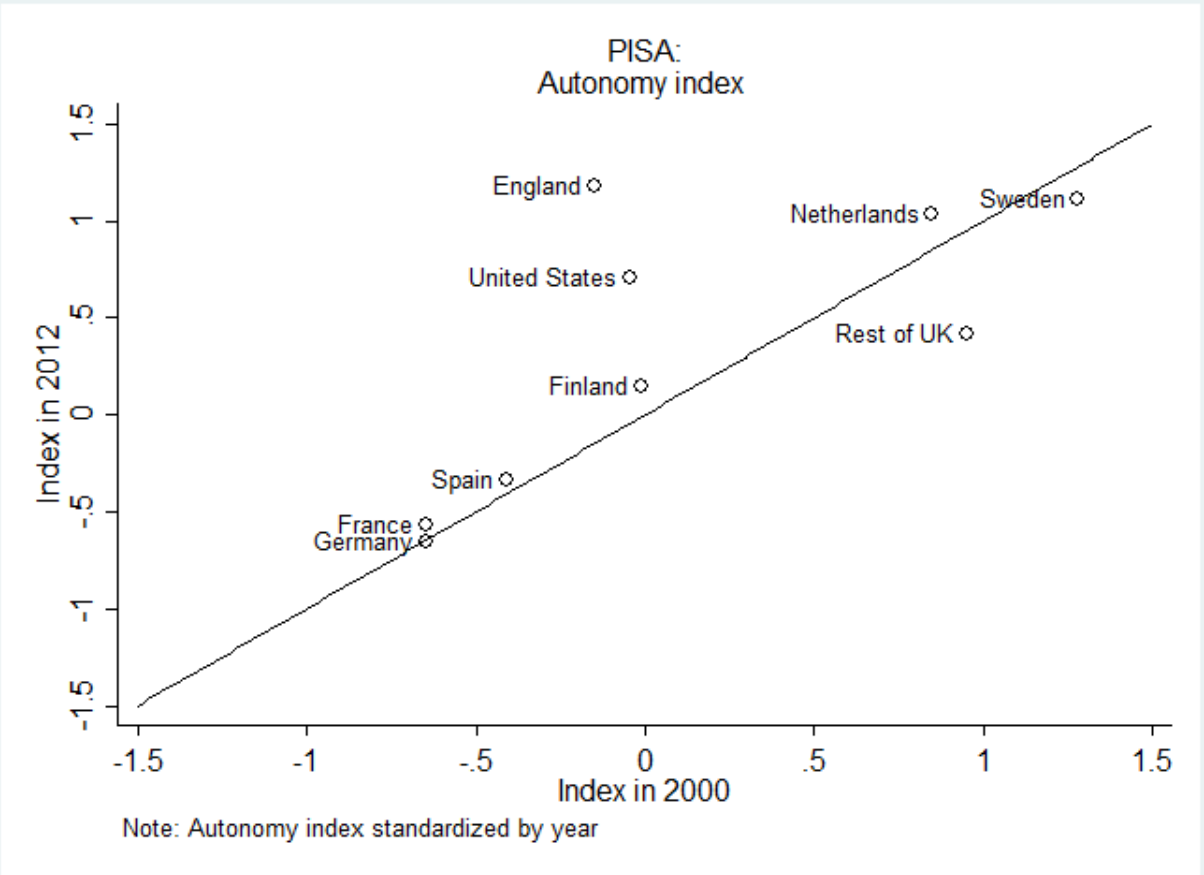
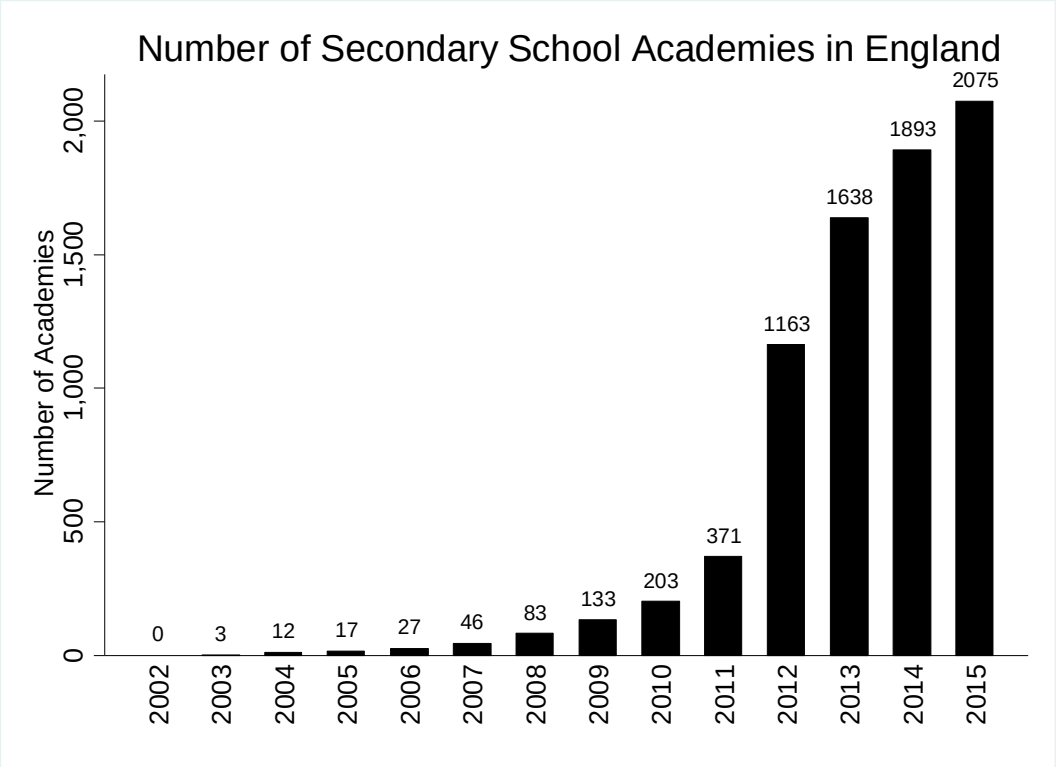
Figure 1: Autonomy Levels From PISA Data in Nine Selected Countries, 2000 to 2012



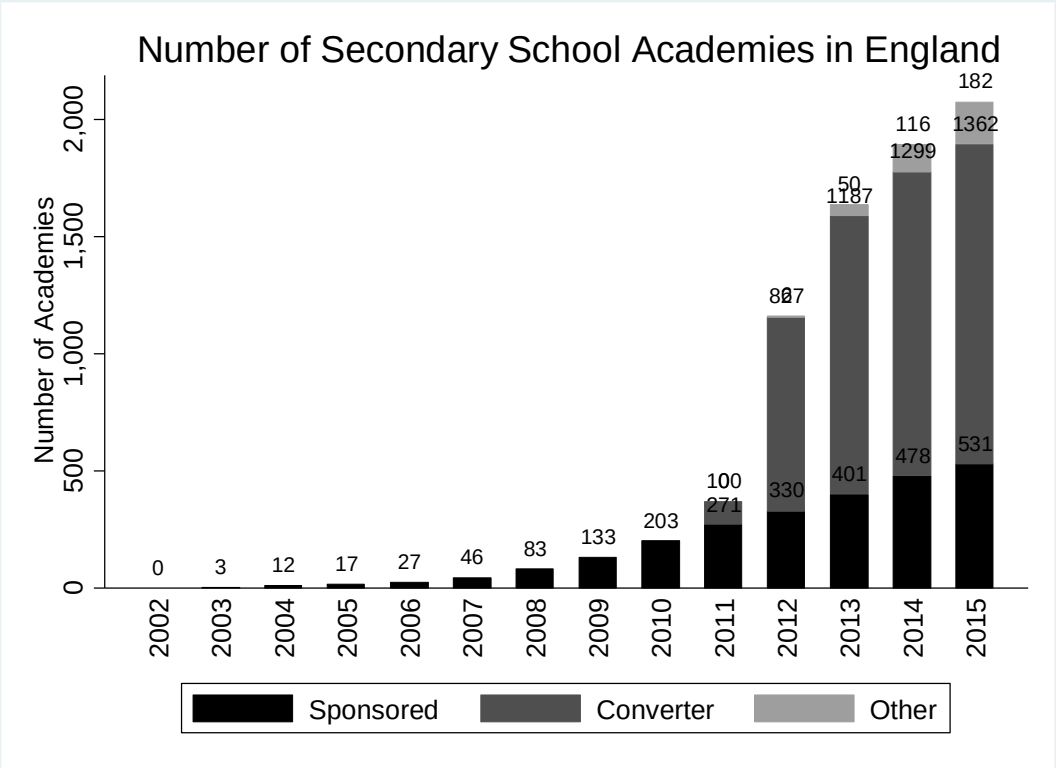
Notes: Own calculations from PISA 2000 and 2012.

Figure 2: Academy Secondary Schools in England, 2002 to 2015



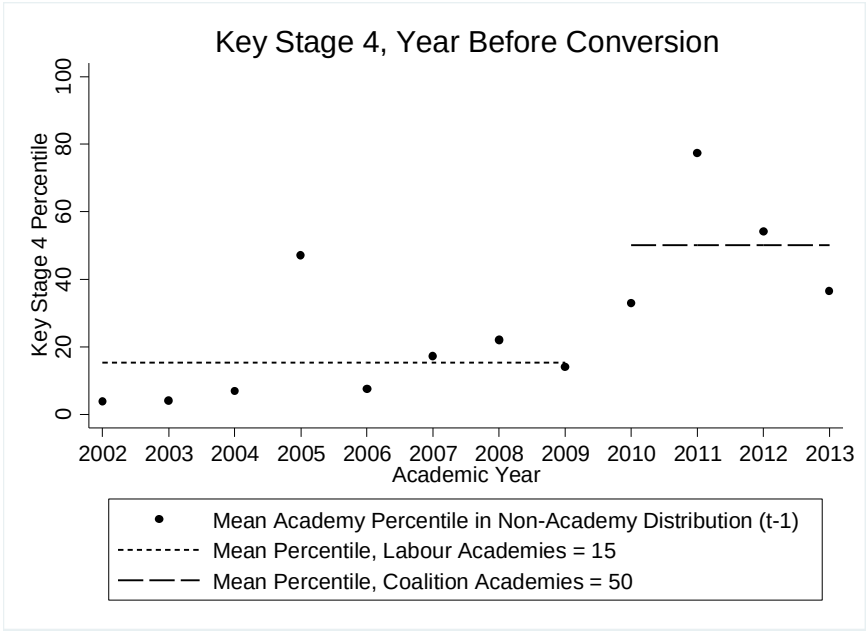
Notes: Source - Department for Education.

Figure 3: Sponsored and Convertor Academy Secondary Schools in England, 2002 to 2015



Notes: Source - Department for Education. Other comprises free schools, university technical colleges and studio schools.

Figure 4: Academy Conversions in the Initial Academies Programme and the Post-2010 Mass Academisation



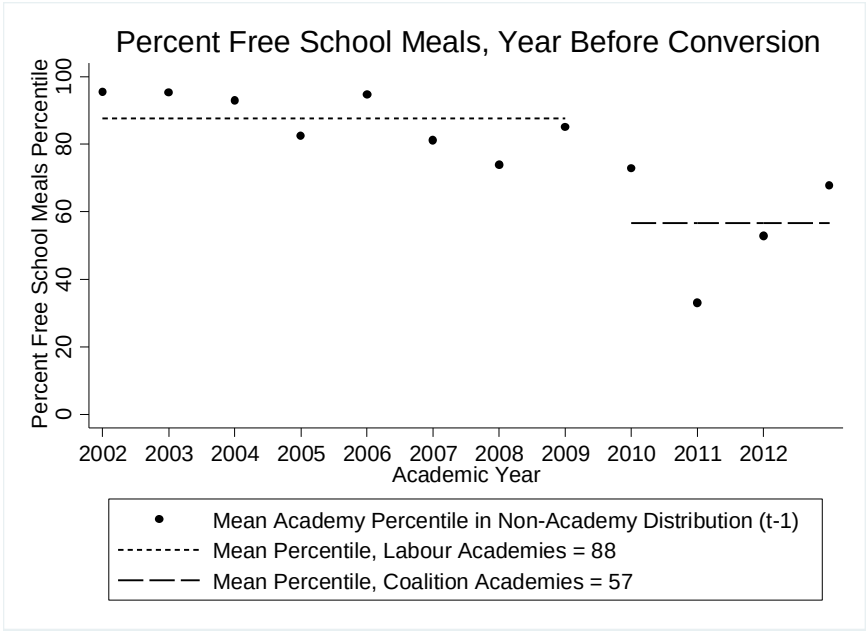


Table 1: Introduction of New School Types Since 1990 in Nine Countries

Country	New school types introduced since 1990	Description if new school type introduced	Autonomy level before	Autonomy level after
England	Yes	Academy schools, first introduced in 2002 Free schools, first introduced in 2010.	Medium	High
Finland	No		Medium	Medium
France	No		Low	Low
Germany	No		Low	Low
Netherlands	No		High	High
Rest of UK	No		Medium	Medium
Spain	No			
Sweden	Yes	Free schools (‘friskolor’), first introduced in 1992	Low	High
United States	Yes	Charter schools, first introduced in 1992	Low	High

Table 2: Number (Percent) of Secondary Schools in England, 2002, 2010 and 2015

	Number (Percent) of Secondary Schools by Type		
	2002	2010	2015
Academy	0 (0.0)	203 (4.0)	2075 (61.4)
Community	2278 (65.6)	2017 (59.9)	657 (19.4)
Other Types	1193 (34.4)	1161 (36.1)	649 (19.2)
Total	3471	3361	3381

Notes: Numbers refer to January of each year. Source – Department for Education.

Table 3: Evidence on US Charter Schools

Study	Type	Data	Results	Comments
Betts et al (2006)	Regression using student fixed effects.	47,403-62,795 student observation in San Diego focusing on 16 charter schools.	Use Stanford 9 test of math and reading achievement as outcomes. -0.08 in reading and 0.06 math for middle school students. Losses of -0.18 math elementary school students.	
Hoxby Murarka and Kang (2009)	Lottery.	70'560 3rd to 8th grade and 3771 9th-12th grade students in test taking grades in NYC between 01/02 and 07/08	For grades 3-8 students' 0.12 gain in math and 0.09 in English language arts (ELA) in statewide tests. For grades 9-12 0.19 in math and 0.18 in English regents examinations.	They detect no differential effects between subgroups (Hispanic and Black). All 9-12 grade outcomes include baseline scores as a control.
Angrist, Dynarski, Kane, Pathak and Walters (2010)	Lottery.	Lottery cohorts 2005-2008 in KIPP middle school in Lynn, Massachusetts. 856 observations.	Test scores from Massachusetts Comprehensive Assessment System (MCAS). 0.346 in math and 0.120 in ELA.	Those 1sd below the mean with respect to baseline test scores gain an additional 0.06 per year relative to those at the mean.
Gleason et al (2010)	Lottery.	36 charters from 15 states. 2141 observations.	No significant differences in reading or math achievement. Estimates are treatment on the treated.	These average results mask significant heterogeneity in treatment effects. For instance, those eligible for free school meals seem to improve their math achievement (in their second year) substantially as a result of attending a charter (by 0.17) while those ineligible lose 0.14 in each year they are in the charter.
Abdulkadiroglu et al 2011	Lottery.	5 middle schools and 3 high schools in Boston. Sample sizes vary between 1401 and 3258 pupils depending upon the specification.	For MCAS test scores 0.359 gains per year in math and 0.198 in ELA for middle school. 0.364 math and 0.265 ELA for high school pupils.	All include baseline test scores as a control.
Fryer and Dobbie (2011)	Lottery estimates and IV using living within original	Elementary and middle Promise Academy charter schools in Harlem (NYC) Children's Zone. 748-1449 observations	Statewide exams. 0.229 for math in middle school using lottery estimates and 0.206 using the	

	Harlem Children's zone (HCZ) interacted with kindergarten cohort as an instrument.	for lottery estimates. 34148-41029 for IV (includes those living up to 800m out of original 24 block zone).	IV. Imprecisely measured and insignificant (at the 5% level) gains in math in elementary school using lottery estimates. The only significant gains in ELA come from IV estimates for elementary school students.	
Fryer and Dobbie (2013)	Lottery (for 29 schools) and matching for a larger sample with 10 additional undersubscribed schools.	29 lottery schools and 10 schools without admissions lotteries in NYC. 15'439 students in elementary school lotteries and 16'340 in middle school lotteries.	Statewide exams. 0.113 in math and 0.058 in ELA for elementary school pupils. 0.126 in math and 0.048 in ELA in middle school.	Matching results are not reported. Comparing matching regressions (on the lottery school subsample) with lottery estimates show that significant downward bias remains even after matching.
Angrist, Pathak and Walters (2013)	Lottery.	17 middle and 6 high schools in Massachusetts. Sample sizes between 16285-16543 for middle school students and 4103-4150 for high school students.	MCAS scores. No significant differences for high school students in non-urban areas. Losses off -0.123 in math and -0.144 in English in non-urban middle schools. Gains of 0.321 in math and 0.146 in ELA in urban middle school. Similar gains of 0.339 in math and 0.264 in ELA in urban high schools.	
Angrist, Cohodes et al (2013)	Lottery.	6 Boston high schools. Between 1382 and 2957 depending upon the outcomes reported.	Mid-term outcomes. Improvements of 0.4 in math SAT scores and 0.3 in overall SAT scores. 0.17pp more likely to enrol in 4 year college than no college and 0.06pp less likely to enrol in 2 year college.	Interestingly, pupils with low baseline test scores drive results on college enrolment. The paper contains results on many more outcomes.
Dobbie and Fryer (2014)	Lottery.	Promise academies in HCZ. 205-599 observations depending upon outcome.	Males are 7pp less likely to be incarcerated while females are 15pp less likely to have a child in their teens. 28pp more likely to enrol in college immediately after graduating from high school.	These specifications control for baseline scores. Also find positive results on a number of tests suggesting that positive effects in other studies are not due to grade inflation at charters on state administered exams (or 'teaching to the test').

Abdulkadiroglu et al 2014	Instrumental variables. Uses 'grandfathering' instrument described in the previous section and matching to construct a suitable control group.	UP academy charter in Boston and 9 schools in New Orleans. 5625 observations in New Orleans schools and 1543-1549 in Boston depending upon outcome.	State level test for middle grades. Improvements of 0.212 in math and 0.143 in ELA in New Orleans schools. 0.321 in math and 0.394 in ELA in UP academy.	The New Orleans estimates are contaminated somewhat by the fact that matched control pupils can attend non-takeover charters and not just ordinary public schools. Introducing charter attendance at a non-takeover school as a further endogenous variable and using grandfather/demographic interactions as instruments gives gains roughly twice the size.
Fryer (2014)	Randomised control trial.	8 pairs of matched schools (treatment and control) in Houston. 6628 observations.	State level test. 0.112 gain in math and 0.034 in elementary school. 0.146 in math with no gains in reading in middle/high schools.	Further results are presented for Chicago and Denver. While the Denver schools generate similar gains the Chicago schools have very small <0.06 gains.

Table 4: Evidence on Sweden's Free Schools

Study	Type	Data	Results	Comments
Ahlin (2003)	Value Added.	1998 test scores for 9th grade. 6107-6397 depending upon outcomes.	0.318 percentile rank improvement in Swedish and 5.012 percentile rank improvement in ½ of the math exam (math is composed of two papers). These are nationwide tests.	Controls for mother's education, immigrant status and various municipality characteristics
Björklund et al (2005)	Regression.	9th grade performance 1998-2001. 385054 observations at student level.	For nationwide tests 4.86 percentiles rank improvement in math. 6.69 percentiles rank improvement in English. 5.15 percentiles rank improvement in Swedish	Regressions control for gender, immigrant status (including recent previous 5 year status), whether your parents are foreign and variables pertaining to parental education. Both municipality fixed effects and time varying municipality covariates are included.
Bohlmark and Lindahl (2007)	Sibling estimates.	1988-2003 9th grade scores. 213,612 without sibling fixed effects and 210,733 with single child families removed.	Percentile rank in national test scores (averaged across subjects). 1.22 improvement as a result of private school attendance.	The estimated effect is 4.04 without sibling fixed effects suggesting upward bias in some of the estimates reported here.
Bohlmark and Lindahl (2015)	Municipality level regressions using municipality fixed effects.	9th grade performance 1988-2009 observations at municipality level. 284 observations.	Higher grades for those attending private school explain 20–30% of the increase in grades at municipality level associated with rises in the share of private school students. A 5 pp increase in the share of private school students is associated with 2–2.5% of a standard deviation increase in outcomes such as average English and math grades, the fraction of students attending university by age 22 and the fraction taking the academic track in upper-secondary school.	

Table 5: Changes in the Quality of Intake (Key Stage 2 and Free School Meals in Year 7), in the Initial Academies Programme, 2001/2 to 2008/9

	Key Stage 2 (Standardised)		Free School Meals	
	(1)	(2)	(3)	(4)
Academy x Post-Conversion (E = c to c+3)	0.074 (0.023)		-0.022 (0.009)	
Academy x (E = c-4)		-0.007 (0.013)		0.002 (0.007)
Academy x (E = c-3)		-0.002 (0.017)		-0.003 (0.011)
Academy x (E = c-2)		0.001 (0.017)		0.001 (0.009)
Academy x (E = c-1)		0.019 (0.018)		-0.019 (0.009)
Academy x (E = c)		0.058 (0.024)		-0.022 (0.010)
Academy x (E = c+1)		0.083 (0.029)		-0.028 (0.014)
Academy x (E = c+2)		0.142 (0.044)		-0.043 (0.016)
Academy x (E = c+3)		0.115 (0.053)		-0.051 (0.021)
School Fixed Effects	Yes	Yes	Yes	Yes
Year Dummies	Yes	Yes	Yes	Yes
R-Squared	0.087	0.087	0.089	0.089
Sample Size	353077	353077	353077	353077
Number of Treatment and Control Schools	220	220	220	220

Notes: E denotes event year and c is the year of conversion. Robust standard errors (clustered at the school level) are reported in parentheses.

Table 6: Pupil Performance Treatment Effects (Key Stage 4 in Year 11), in the Initial Academies Programme, 2001/2 to 2008/9

	Key Stage 4 Points Score (Standardised)					
	OLS	ITT	IV	OLS	ITT	IV
	(1)	(2)	(3)	(4)	(5)	(6)
Academy x Post-Conversion (E = c to c+3)	0.080 (0.027)	0.073 (0.025)	0.079 (0.027)			
Academy x (E = c-4)				-0.016 (0.018)	-0.025 (0.018)	-0.015 (0.019)
Academy x (E = c-3)				0.002 (0.024)	-0.010 (0.022)	0.004 (0.024)
Academy x (E = c-2)				0.023 (0.028)	0.009 (0.026)	0.025 (0.028)
Academy x (E = c-1)				0.023 (0.034)	0.008 (0.032)	0.025 (0.035)
Academy x (E = c)				0.029 (0.038)	0.022 (0.034)	0.037 (0.039)
Academy x (E = c+1)				0.146 (0.050)	0.128 (0.043)	0.155 (0.051)
Academy x (E = c+2)				0.219 (0.071)	0.172 (0.061)	0.207 (0.074)
Academy x (E = c+3)				0.192 (0.080)	0.148 (0.074)	0.184 (0.090)
KS2 Standardised Test Score	0.523 (0.005)	0.523 (0.005)	0.523 (0.005)	0.523 (0.005)	0.523 (0.005)	0.523 (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
R-Squared	0.482	0.482	0.482	0.482	0.482	0.482
Sample Size	362424	362424	362424	362424	362424	362424
Number of Treatment and Control Schools	220	220	220	220	220	220
First Stage Coefficient on ITT			0.932 (0.007)			
First Stage F-Test			3973			
First Stage Coefficient on ITT x (E = c)						0.952 (0.004)
First Stage F-Test						33071
First Stage Coefficient on ITT x (E = c+1)						0.905 (0.007)
First Stage F-Test						4034
First Stage Coefficient on ITT x (E = c+2)						0.885 (0.011)
First Stage F-Test						1733
First Stage Coefficient on ITT x (E = c+3)						0.875 (0.015)
First Stage F-Test						619

Notes: From Eyles and Machin (2015). E denotes event year and c is the year of conversion. Robust standard errors (clustered at the school level) are reported in parentheses. Control variables included are the same as from the Table 5 regressions, although in specifications including KS2 test scores we now additionally include a separate intercept for pupils for whom KS2 data is unavailable. For children who move out of treatment or control schools to take their KS4, school fixed effects (1715) for the school they move to are also included.

Table 7: Post-Compulsory School Outcome Treatment Effects, Initial Academies Programme

	Stayed on After KS4			Entered Higher Education		
	OLS	ITT	IV	OLS	ITT	IV
	(1)	(2)	(3)	(4)	(5)	(6)
Academy x Post-Conversion (E = c to c+3)	0.017 (0.007)	0.016 (0.007)	0.017 (0.007)	0.026 (0.006)	0.026 (0.005)	0.028 (0.006)
KS2 Standardised Test Score	0.148 (0.002)	0.148 (0.002)	0.148 (0.002)	0.097 (0.002)	0.097 (0.002)	0.097 (0.002)
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
R-Squared	0.230	0.230	0.230	0.176	0.176	0.176
Sample Size	324697	324697	324697	324697	324697	324697
Number of Treatment and Control Schools	220	220	220	220	220	220
First Stage Coefficient on ITT x (E = c to c+3)			0.924 (0.008)			0.924 (0.008)
First Stage F-Test			3374			3374

	Started BA Degree at Russell Group University Four Years After KS4			Started BA Degree at Non-Russell Group University Four Years After KS4		
	OLS	ITT	IV	OLS	ITT	IV
	(7)	(8)	(9)	(10)	(11)	(12)
Academy x Post-Conversion (E = c to c+3)	0.002 (0.002)	0.002 (0.002)	0.002 (0.002)	0.024 (0.005)	0.023 (0.004)	0.025 (0.005)
KS2 Standardised Test Score	0.017 (0.001)	0.017 (0.001)	0.017 (0.001)	0.076 (0.002)	0.076 (0.002)	0.076 (0.002)
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
R-Squared	0.050	0.050	0.050	0.138	0.138	0.138
Sample Size	324697	324697	324697	324697	324697	324697
Number of Treatment and Control Schools	220	220	220	220	220	220
First Stage Coefficient on ITT x (E = c to c+3)			0.924 (0.008)			0.924 (0.008)
First Stage F-Test			3374			3374

Notes: E denotes event year and c is the year of conversion. Robust standard errors (clustered at the school level) are reported in parentheses. Control variables included are the same as from the Table 5 regressions, although in specifications including KS2 test scores we now additionally include a separate intercept for pupils for whom KS2 data is unavailable. For children who move out of treatment or control schools to take their KS4, school fixed effects (1714) for the school they move to are also included. The means of the dependent variables are: (1)-(3) 0.33; (4)-(6) 0.26; (7)-(9) 0.02; (10)-(12) 0.15.

Table 8: Post-Compulsory School Outcome Treatment Effects, Event Study Estimates, in the Initial Academies Programme

	Stayed on After KS4		Entered Higher Education		Started BA Degree at Russell Group University Four Years After KS4		Started BA Degree at Non-Russell Group University Four Years After KS4	
	IV	IV	IV	IV	IV	IV	IV	IV
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Academy x (E = c-4)	0.003 (0.007)	0.014 (0.008)	-0.002 (0.005)	0.001 (0.005)	-0.001 (0.002)	-0.001 (0.002)	0.001 (0.005)	0.003 (0.005)
Academy x (E = c-3)	0.001 (0.007)	0.004 (0.008)	-0.006 (0.006)	-0.006 (0.007)	-0.000 (0.003)	-0.000 (0.003)	-0.005 (0.005)	-0.005 (0.006)
Academy x (E = c-2)	0.000 (0.008)	-0.004 (0.010)	-0.001 (0.007)	-0.003 (0.008)	0.002 (0.003)	0.002 (0.003)	0.000 (0.005)	-0.002 (0.006)
Academy x (E = c-1)	0.007 (0.009)	0.002 (0.011)	0.012 (0.009)	0.009 (0.009)	0.005 (0.003)	0.004 (0.003)	0.007 (0.006)	0.005 (0.007)
Academy x (E = c)	0.014 (0.010)	0.006 (0.010)	0.017 (0.009)	0.013 (0.010)	0.003 (0.003)	0.002 (0.003)	0.017 (0.007)	0.014 (0.008)
Academy x (E = c+1)	0.024 (0.013)	-0.012 (0.011)	0.043 (0.010)	0.022 (0.012)	0.003 (0.003)	0.000 (0.003)	0.040 (0.009)	0.023 (0.009)
Academy x (E = c+2)	0.048 (0.017)	-0.001 (0.015)	0.067 (0.012)	0.039 (0.015)	0.016 (0.006)	0.012 (0.006)	0.048 (0.011)	0.025 (0.011)
Academy x (E = c+3)	0.049 (0.024)	0.005 (0.019)	0.088 (0.017)	0.062 (0.024)	0.013 (0.005)	0.010 (0.006)	0.068 (0.015)	0.048 (0.017)
KS2 Standardised Test Score	0.148 (0.002)	0.019 (0.002)	0.097 (0.002)	0.022 (0.002)	0.017 (0.001)	0.006 (0.001)	0.076 (0.002)	0.016 (0.002)
KS4 Standardised Test Score		0.252 (0.004)		0.146 (0.003)		0.021 (0.001)		0.116 (0.002)
School Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Control Variables	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year Dummies	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R-Squared	0.230	0.378	0.176	0.247	0.062	0.062	0.138	0.191
Sample Size	324697	324697	324697	324697	324697	324697	324697	324697
Number of Treatment and Control Schools	220	220	220	220	220	220	220	220
	Same First Stage for Specifications (1), (3), (5), (7):				Same First Stage for Specifications (2), (4), (6), (8):			
First Stage Coefficient on ITT x (E = c)	0.959 (0.003)				0.959 (0.003)			
First Stage F-Test	12028				11664			
First Stage Coefficient on ITT x (E = c+1)	0.922 (0.007)				0.922 (0.007)			
First Stage F-Test	1102				1217			
First Stage Coefficient on ITT x (E = c+2)	0.904 (0.011)				0.903 (0.011)			
First Stage F-Test	438				536			
First Stage Coefficient on ITT x (E = c+3)	0.892 (0.015)				0.892 (0.015)			
First Stage F-Test	473				454			

Notes: As for Table 7.

Table 9: Changes in the Quality of Intake (Key Stage 2 in Year 7), in the Initial Academies Programme and the Post-2010 Mass Academisation

	Key Stage 2 (Standardised)		
	Labour Years (Before May 2010)	Coalition Years (May 2010 Onwards)	
	Sponsored Academies	Sponsored and Converter Academies	
	(1)	(2)	(3)
Academy x Post-Conversion (E = c to c+2)	0.031 (0.016)	0.009 (0.005)	
Sponsored Academy x Post-Conversion (E = c to c+2)			0.046 (0.012)
Converter Academy x Post-Conversion (E = c to c+2)			0.003 (0.006)
School Fixed Effects	Yes	Yes	Yes
Year Dummies	Yes	Yes	Yes
R-Squared	0.083	0.163	0.163
Sample Size	324841	1793003	1793003
Number of Treatment and Control Schools	308	1450	1450

Notes: From Eyles, Machin and Silva (2015). E denotes event year and c is the year of conversion. Sample in (1): observations up to 2010 for academies opened between 2003 and 2010 in the time window [c-6, c+2] around opening date c (treated group) and observations up to 2010 for academies that will open in 2011, 2012 and 2013 in the time window [c-6, c-1] around opening date c (control group). Sample in (2), (3) and (4): observations up to 2013 for academies opened in 2011, 2012 and 2013 in the time window [c-6, c+2] around opening date c (treated group) and observations up to 2013 for academies that will open in 2014, 2015 and 2016 in the time window [c-6, c-1] around opening date c (control group). Robust standard errors clustered at the school level are reported in parentheses.

Table 10: Changes in the Quality of Intake (FSM eligibility in Year 7), in the Initial Academies Programme and the Post-2010 Mass Academisation

	Free School Meal Eligibility		
	Labour Years (Before May 2010)	Coalition Years (May 2010 Onwards)	
	Sponsored Academies	Sponsored and Converter Academies	
	(1)	(2)	(3)
Academy x Post-Conversion (E = c to c+2)	-0.013 (0.006)	-0.005 (0.002)	
Sponsored Academy x Post-Conversion (E = c to c+2)			0.013 (0.005)
Converter Academy x Post-Conversion (E = c to c+2)			-0.009 (0.002)
School Fixed Effects	Yes	Yes	Yes
Year Dummies	Yes	Yes	Yes
R-Squared	0.083	0.106	0.106
Sample Size	340414	1925391	1925391
Number of Treatment and Control Schools	308	1450	1450

Notes: As for Table 9.

Table 11: Mechanisms - Use of Academy Freedoms in 720 Academies, May 2013

	Percent Making Change	Percent Making Change Linked to Improved Attainment	Percent Making Change Say Most Important Change
Procured services that were previously provided by the LA	87	20	6
Collaborated with other schools in more formalised partnerships	60	45	15
Changed your pattern of capital expenditure	59	21	3
Introduced savings in back-office functions	58	14	0
Reconstituted your governing body	58	16	1
Changed the performance management system for teachers	56	36	7
Changed the curriculum you offer	55	57	29
Added non-teaching positions	48	35	8
Changed school leadership	47	55	31
Introduced or increased revenue-generating activities	35	20	4
Increased the number of pupils on roll	33	11	1
Changed staff pay structures	24	13	2
Changed your admission criteria	22	8	1
Hired teachers without qualified teacher status (QTS)	16	15	3
Sought to attract pupils from a different geographical area	10	9	1
Increased the length of the school day	8	54	11
Changed the length of school terms	4	16	3
Reduced the number of pupils on roll	2	17	8

Notes: Taken from Department for Education (2014).

Appendix

Derivation of autonomy indices from PISA data

The autonomy index is derived from questions about school responsibilities asked to in all PISA waves. In the questionnaire principals were asked whether the responsibility for 12 different categories of aspects of school management lay within the school or whether an external school board or a national authority were responsible for the task at question. When principals responded that either they or generally the school was responsible for a task, and not a local or national body, we declared this school as autonomous for this particular task. The final index was derived using six items of the questionnaire: Responsibility for hiring and firing of teachers, setting teacher's starting salaries and their increases, and budget formulation and allocation. For each school we added all categories that lay within the school's responsibility and standardized the measure and matched this to student data. The index for a country was derived by computed the weighted average of the index across all students in the survey. The possible answers school autonomy questions changed slightly between 2000 and 2012. In 2000, head teachers were asked who had the main responsibility (over certain aspects of school management) and the options to select from were: 1) Not a school responsibility, 2) Appointed or elected board, 3) Principal, 4) Department head and 5) Teachers. We classified schools as non-autonomous in one of the categories if they answered that the task was not a school responsibility or if they stated that an appointed or elected board was responsible. In 2012, the option 'not a school responsibility' and 'department head' were no longer available, and two new options, 'regional or local education authority' and 'national education authority' had been added. For this year a school was classified as non-autonomous in an aspect of school management if it answered that the responsibility lay with one of the two aforementioned authorities.

Figure A1: Autonomy Levels Over Teacher Hiring and Firing From PISA Data in Nine Selected Countries, 2000 to 2012

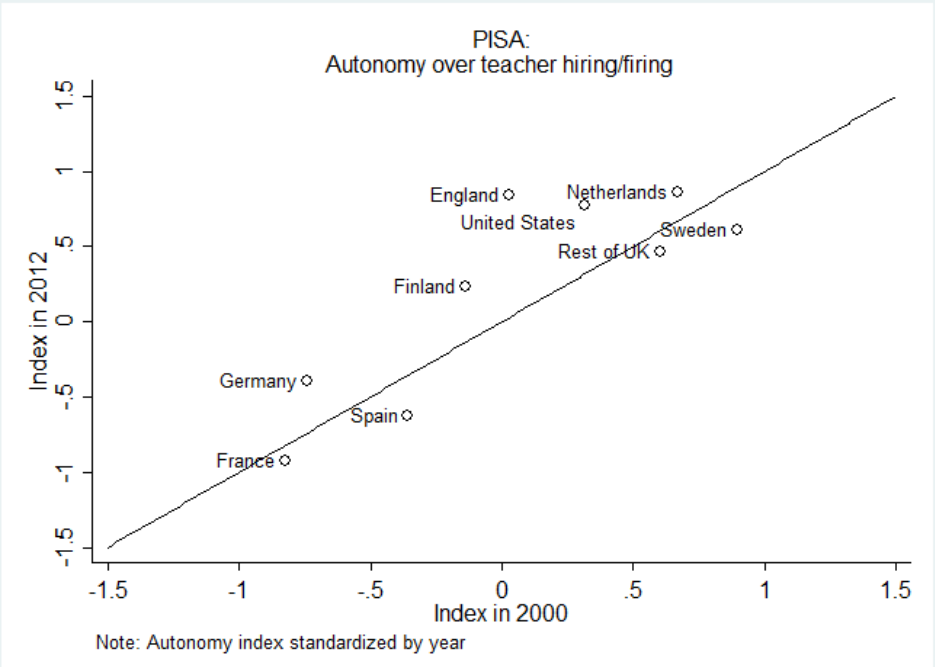


Figure A2: Autonomy Levels Over Teacher Salaries From PISA Data in Nine Selected Countries, 2000 to 2012

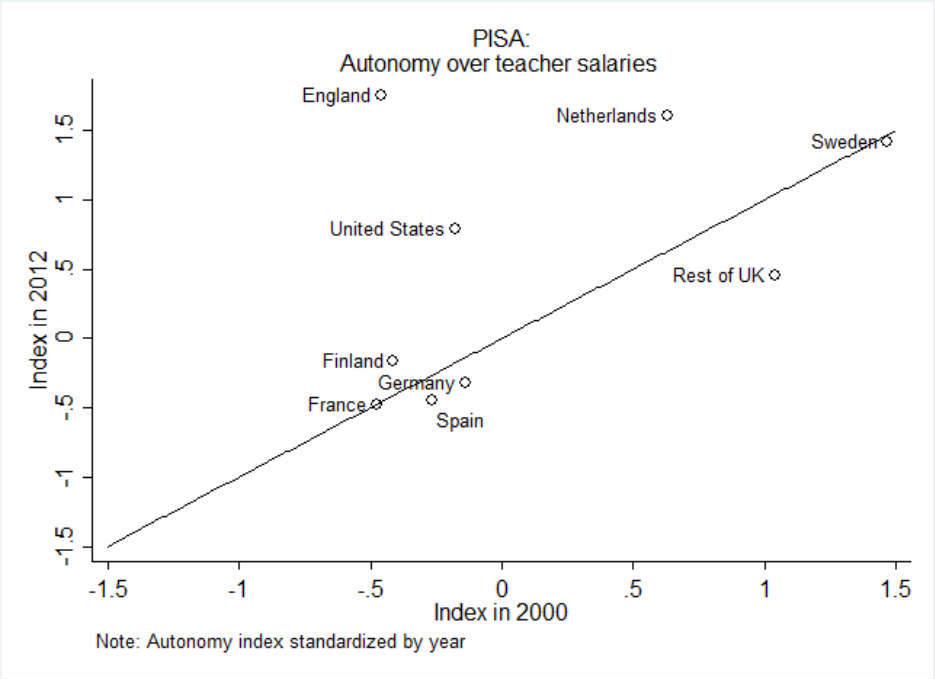


Figure A3: Autonomy Levels Over Budget Allocation and Formulation From PISA Data in Nine Selected Countries, 2000 to 2012