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Autonomous Schools and Strategic Pupil Exclusion

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Abstract
This paper studies whether pupil performance gains achieved by autonomous schools – specifically academy schools in England – can be attributed to the strategic exclusion of poorly performing pupils. In England there have been two phases of academy school introduction, the first in the 2000s being a school improvement programme for schools serving disadvantaged pupil populations, the second a mass academisation programme in the 2010s which by contrast enabled better performing schools to become academies. Overall, on average across both programmes, exclusion rates are higher in academy schools. When the two programmes are considered separately, the earlier programme featured a much higher increase in the incidence of permanent exclusion. However, a number of simulated counterfactual experiments based on the statistical estimates show that rather than being used as a strategic means of manipulation to boost measured school performance, the higher rate of exclusion is instead a feature of the rigorously enforced discipline procedures that the pre-2010 academies adopted.

Key words: academies, discipline, exclusion
JEL: I20; I21; I28

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1. Introduction

Autonomous schools - like academy schools in England or charter schools in the US - are able to operate more independently than standard state or public schools. Depending on context, they tend to have more control over many operational issues like staffing, curriculum, budget setting and service provision, plus a range of additional freedoms that they can exercise in running their school. However, at the same time, in most settings the academic performance of their students is assessed in the same way as state/public schools. Therefore, when league tables or other school accountability measures based on student performance are constructed, the rankings of autonomous and non-autonomous schools are typically based on the same measures of performance.

In England the headline figure by which secondary schools have been ranked in the annually published league tables (at least until recently) is the percentage of pupils getting five or more grade A* to C results in the final year of compulsory schooling examinations (taken in year 11 at age 16). These high stakes exams are the General Certificate of Secondary Education (GCSE) exams taken at the end of secondary school. The school-level rankings based on pupils' GCSE performance matter on a number of counts, including for enrolment of new pupils (as they are heavily scrutinised by the parents of potential new enrolees) and for evaluating head teacher and teacher performance (including salaries and potential pay for performance bonuses). They are widely available and highly accessible, for example being published in newspapers and on internet websites.

There is now a quite sizable, and growing literature, about test score manipulation that may occur because of these kinds of incentivisation (see the recent review by Battistin, 2016). However, in the English educational environment, it is difficult to directly manipulate pupils' GCSE performance. Whilst part of the evaluation of the high stakes end of secondary school GCSE

assessments are based on in year teacher assessment, the examinations that comprise the bulk of the scores are completely independent. They are marked externally through graders from a number of examination boards for different GCSE subjects.[1]

One possibility that has received public attention about academy schools is that the greater autonomy with which these schools are able to operate has offered a means to manipulate their results in a different way, namely by differentially excluding poorly performing pupils from their on-roll lists. This could potentially explain some of the positive GCSE gains recorded by some of these academy schools (see Eyles and Machin, forthcoming), rather than there being a genuine improvement in the quality of education provided.

This study empirically assesses this claim using a research design that derives causal estimates of the impact of academy enrolment on permanent exclusion in year 11, the final year of compulsory schooling in England. Permanent exclusion is a novel means of possible school results' manipulation that has not been investigated in the literature to date.[2] The empirical analysis is undertaken for all secondary academies together and separately for the different waves of academy schools that have progressively appeared over time in the English secondary school landscape.

The history of academisation of the English secondary school sector is outlined in more detail later in the paper (on this, see also Eyles aet al., 2016a, or Eyles et al., forthcoming), but this involves a separate comparison for three groups of academy schools that have opened over time: pre-2010 sponsored academies, post-2010 sponsored academies and post-2010 converter academies. The first of these were the pioneering academy schools set up under the Labour government of 1997-2010, which in their pre-academy life were almost all very disadvantaged schools and which, together with a new sponsor, became academy schools between 2003 and 2010.

[1] See Machin, McNally and Ruiz-Valenzuela (2017) for more discussion of grading practices of GCSEs.
[2] Permanent exclusion in England is the same disciplinary action as expulsion in the US' and other education systems.

The second two groups of schools became academies after the UK government changed in the election of May 2010 (to a Conservative/Liberal Democrat coalition) in a government drive towards mass academisation. In this period, two types of academies came about through conversion. Firstly, there was a continuation of the sponsored programme, albeit not featuring such disadvantaged schools as prior to 2010 and which remained relatively small scale. Secondly, new academies known as converter academies appeared. These formed the vast majority of new academy schools, which no longer required a sponsor and which were typically highly performing schools.

The analysis shows that, following academy conversion, academies do exclude more pupils enrolled in year 11. The higher rate of exclusion is bigger among the pre-2010 academies. When one notes that prior research has reported quite sizable test score performance gains for the pre-2010 academies (Eyles and Machin, forthcoming, Eyles et al., 2016a; Eyles et al., 2016b), it then becomes a natural question to ask whether the observed performance improvements can be explained by higher rates of pupil exclusion. Simulations are undertaken in the paper to ask to what extent the gains from pre-2010 academy enrolment can be attributed to permanent exclusion of year 11 pupils. This is done by reinstating the excluded pupils in the excluding school and calculating a counterfactual level of school performance. In doing this, it is also feasible to quantify negative peer effects on other pupils that may have occurred under the counterfactual scenario. The simulation exercise suggests that, for the positive estimated impact of academy enrolment on GCSE test scores in Eyles and Machin (forthcoming) to become insignificant, each excluded pupil would have needed to exert a negative peer effect on the GCSE test scores of all other year 11 pupils in the same school and school-year of more than 30 percent of a standard deviation. This simulated

peer effect appears implausibly large to attribute the performance gains from academy conversion to deliberate strategic exclusion of poorly performing pupils.

Moreover, the academic ability of the excluded children in the autonomous schools is seen to be no worse on average than that of excluded pupils in non-autonomous schools. Thus, whilst pre-2010 academy schools did exclude more, there is no evidence that year 11 pupils excluded from academies were worse performers than year 11 pupils excluded from control schools. Pre-2010 academies also permanently excluded more pupils in year 11 that were still allowed to take their GCSE exams in the school that excluded them, which removes scope for strategic manipulation of GCSE results. Moreover, no systematic association appears between performance gains and changes in permanent exclusion following conversion. The schools that experienced the largest increases in exclusion rates following conversion are not in fact the same schools that experienced the greatest performance gains.

The overall conclusion which follows is that, rather than being used as a strategic means to boost measured school performance, the higher rate of exclusion seems to have been part and parcel of the tough discipline procedures that the pre-2010 academies adopted. An analysis of the distribution of the estimated treatment effects shows that, although a handful of pre-2010 academies experienced large increases in their rates of exclusion following conversion, these schools were not the drivers of the average performance gains recorded by pre-2010 academies. This conclusion is further reinforced by the much smaller impact of academy status on exclusion for the two sets of post-2010 academies. These schools were better performing schools prior to their academy conversion and did not face the kind of behavioural problems and pressure for improvement faced by the pre-2010 academies. This reinforces the overall conclusion that the higher rate of permanent exclusion from the poorly performing schools that first converted to

academies in the 2000s was a consequence of their need to adopt a rigorously enforced disciplinary policy (which may have contributed to performance improvements), rather than a means to artificially massage and so manipulate their overall performance and league table position.

The remainder of the paper is structured as follows. In the next section, the legal framework in relation to school exclusion in England and its connection to school league table performance are described. The data and some aggregate figures on exclusion in secondary schools in England are also presented. Section 3 discusses the relevant related literature, and section 4 presents the research design. Section 5 presents the main results. Section 6 considers possible results manipulation and prior attainment differences for pupils excluded from schools with different levels of autonomy. Finally, section 7 concludes.

2. Pupil Exclusion

Good behaviour in school has frequently been identified as essential to ensure that all pupils can benefit from education (Department for Education, 2015). Indiscipline in school can negatively influence the provision of education in several ways. It can disrupt the agenda of teachers and hinder their ability to provide pupils with a high quality of education; it can divert schools' resources away from their original purposes; and, more generally, it can contribute to make schools an unsafe learning environment for both the school personnel and for the pupils. Indeed, the educational output of pupils is likely to be directly affected by their behaviour in school (Lazear, 2001). In the English setting we study, schools have policies, procedures and staff training in place to promote good discipline and inhibit bad behaviour. Such policies are broadly publicised to pupils and their families, as well as school personnel, to ensure that they all know what standards of

behaviour are expected in school and the array of actions that schools can take to tackle bad behaviour.[3]

One sanction that is available to a school to restore discipline is pupil exclusion. The decision to exclude requires a pupil to leave the school premises, and it is a decision that only head teachers (principals) have the power to take. According to Department for Education (DfE) guidelines, such decision must be "lawful, rational, reasonable, fair and proportionate". However, head teachers are expected to, and often have to, use discretion in taking this decision. In England, a pupil can only be excluded on disciplinary grounds, while it is unlawful to exclude a pupil due to poor academic performance. Possible reasons for exclusion include persistent disruptive behaviour, bullying, theft, physical assault, verbal abuse, threatening behaviour, damage, and drug and alcohol related offences.

In England, the educational setting of focus in this paper, there are three types of exclusions that head teachers can use: lunchtime exclusion, temporary exclusion and permanent exclusion. A lunchtime exclusion consists of an exclusion from the school premises for the duration of the lunchtime period. A temporary exclusion consists of excluding a pupil from the school premises for a set period of time, up to a maximum of 45 school days in a single academic year. This sanction is deemed appropriate in response to breaches of the school's behaviour policy, such as disruptive behaviour, where these are not sufficiently serious to justify a permanent exclusion and smaller sanctions (e.g. detention) are deemed inadequate.

Permanent exclusion is the most serious disciplinary decision that can be taken in response to bad behaviour. It means the pupil has to leave the school premises and s/he will not be allowed

[3] One good example of this is the Student Behaviour Policy 2016-2017 of the Haggerston Community School in the Borough of Hackney (London), that can be found at the following link: https://www.haggerston.hackney.sch.uk/wp-content/uploads/2017/03/Behaviour-Policy-2016-2017.pdf

to return to the school. In practice, this decision is usually the final step in a process for dealing with indiscipline after a wide set of other strategies have been tried without success. In principle, this should count as an acknowledgment by the school that this has exhausted all possible strategies to deal with an undisciplined pupil. This decision should only be taken in response to a serious breach, or persistent breaches, of the school's behaviour policy, and where the presence of the pupil in the school would seriously harm the education or welfare of the pupil, as well as others' in the school.

Following a permanent exclusion, schools and local education authorities have to arrange alternative provision from the sixth day of the exclusion of pupils of compulsory school age. The head teacher must notify the school's governing body, which has to review the exclusion decision.[4] If the governing body upholds the exclusion decision, and if the parents of the excluded pupil dispute the decision, they can also request a review of the exclusion by an Independent Review Panel (IRP). The IRP does not have the authority to reinstate a pupil, but it can direct a governing body to reconsider its decision.[5]

Even though a pupil remains formally enrolled in the excluding school until the end of the exclusion process, permanent exclusion (and the subsequent arrangement of alternative education) is *de facto* one way to modify the list of pupils on-roll and, thus, the list of pupils that will contribute to the school-level aggregate results at the end of the academic year. Moreover, since inclusion in a secondary school's league table results hinges on the census carried out in the January of a pupil's year 11 school attendance, it is possible that schools that operate more autonomously also within the exclusion process might take advantage of this disciplinary sanction.

[4] This is also the case for fixed-period exclusions that would result in a pupil being excluded for more than 15 school days in any one term, or missing a public examination.
[5] Since 2011/12, IRPs have replaced the Independent Appeal Panels that, unlike IRPs, did have the power to reinstate an excluded pupil in school.

Data on Pupil Exclusion

The principal source of data that can be used to study pupil discipline in England is the National Pupil Database (NPD) of the Department for Education. This large administrative dataset contains detailed information on all pupils enrolled in state-maintained schools in England since 2001/02. For the analysis of this paper, data are used from the academic years 2001/02 to 2014/15 inclusive, and include all pupils enrolled in secondary school in English state-maintained schools. For each pupil, information on a set of relevant characteristics is collected every year (e.g. on age, gender, ethnicity, native language, special educational needs status, free-school-meal eligibility, and the school where the pupil is enrolled on the census date). Pupil-level information on schooling outcomes that include the pupils' school test scores at different stages of their schooling trajectory (Key Stage 2 (KS2) exams, which pupils take at the end of primary school, and Key Stage 4 (KS4) exams, the high-stakes GCSE exams that pupils take at the end of secondary school) is also available.

Of particular importance for the current analysis is that the NPD also provides administrative records for a set of behavioural outcomes at the pupil level, namely for school absence and for the different types of exclusion described above. The NPD data on exclusion contains information on the type of exclusion (permanent, temporary, lunchtime), on the reason for exclusion, on the start date of the exclusion, on the school where the pupil was enrolled at the time of the exclusion and, for temporary exclusions, on the duration of the exclusion. The focus of this paper is on the most serious discipline action, permanent exclusion, and in total from the school year 2001/02 to 2014/15 there is detailed information on pupil characteristics, school test scores and permanent exclusion for 10,675,192 pupils enrolled in state-maintained secondary schools in

England. Since every pupil is observed for up to five years in secondary school, this results in 40,471,691 pupil-year observations.

Descriptive Analysis

Table 1 shows the distribution of permanent exclusions for pupils in state-maintained secondary schools in England from 2001/02 to 2014/15. Panel A shows the count and shares of all permanent exclusions in the school year for year 7 through year 11 pupils. Panel B shows the count and shares only for the permanent exclusions that occurred in the months prior to the January census in each school year (i.e. from September to the exact date in mid-January when the January census was conducted every year).

The reason for showing pre-January numbers separately is an institutional feature of the education system that enables possible strategic results manipulation. The January cutoff is used because pupils that are excluded in year 11 after this date may contribute towards the secondary school league table results of the school from which they were excluded because they are allowed to sit the summer exam in the school. Pupils excluded before January are not allowed to do so. Thus, this is a strategic time window during which schools may exclude poorly performing pupils that could bring down the school average.

Table 1 shows that over the whole time period for which we have data 79,868 pupils were permanently excluded from school, corresponding to 0.2 percent of the universe of students. Of these exclusions, 8 percent were pupils excluded in year 7, 19 percent in year 8, 29 percent in year 9, more than 30 percent in year 10, and 12 percent in year 11. Panel B reveals that only 31,853 pupils, or less than 0.08 percent of the total, were permanently excluded in the months of the school year that precede the January census. Interestingly, in each of the years 7-10 the fraction of pupils excluded in the months prior to the January census is well below half the number of pupils excluded

throughout the year. This is not the case in year 11. Almost 80 percent of pupils that were excluded in year 11 between 2001/02 and 2014/15 were excluded prior to the January census. As noted above, this is an important cutoff because inclusion in a mainstream secondary school's league table results crucially hinges on which school year 11 pupils are enrolled in when the January census is conducted. This cutoff is therefore critical to the notion that strategic permanent exclusion might take place.

Table 2 reports estimates of the determinants of behavioural outcomes for year 11 pupils. A series of logistic regressions were estimated and Table 2 reports estimated odds ratios from these (with over/under unity corresponding to a higher/lower outcome likelihood). Complete data is available on 7,336,508 year 11 pupils starting from 2001/02. Estimates of the likelihood of exclusion reported in columns (1), (2) and (3) reveal a consistent profile regarding characteristics of pupils with better or worse discipline outcomes. Pupils most at risk of exclusion in year 11 are from low socio-economic background (as proxied by the free school meals, FSM, variable), are males and are English native speakers. Better prior schooling attainment (Key Stage 2) is also a strong negative predictor of year 11 exclusion.

Columns (4) and (5) report estimates from specifications for unauthorised absence and persistent unauthorised absence in year 11. The former is measured by a continuous variable on the fraction of school sessions missed without authorisation, while the latter is a binary variable that captures whether the pupil missed at least 20 percent of sessions available in the school year.[6] The coefficients in columns (4) and (5) reveal that pupils from low socio-economic background are more likely than others to miss school sessions without authorization, as well as to miss 20 percent or more of the available school sessions. Girls are more likely to be absent from school

[6] This was the Department for Education's definition of "persistent absence" at the beginning of the period studied here.

without authorisation, and English native speakers are more likely to miss school without permission. Prior academic performance is also a negative predictor of the likelihood to miss school without authorization in year 11.

The assertion that schools operating with more autonomy might have been able to take advantage of disciplinary sanctions with an intention to manipulate aggregate school performance has been made by detractors and critics of academy schools in England. Indeed, the notion has received press and media coverage in recent years.[7] The aim of this paper is to rigorously investigate this claim. It does so by offering causal evidence of the impact of academy enrolment on permanent exclusion of year 11 pupils based on rich administrative data, and then evaluating the potential impact this has on school performance differences between more and less autonomous schools.

3. Strategic Manipulation of School Performance

In England, as well as many other countries around the world, the quality of education is measured through standardized testing procedures working through a national curriculum and secondary schools are ranked based on their aggregate test scores. This system appears desirable for a number of reasons. Lack of accountability can induce teachers to shirk (Angrist et al., 2017a), whereas its presence, at least in principle, gives an incentive to schools and teachers to put more effort in the provision of education. Test-scoring procedures also provide summary and uniform performance metrics that can be used to judge schools and potentially take remedial action. Finally, the widespread public availability of school rankings enables parents to compare schools and choose among them.

[7] See the Appendix for a number of selected examples of media coverage in the UK and US.

Moral Hazard Issues

This study contributes to the recent literature on the moral hazard that can arise from test-scoring procedures and on the various forms of cheating, by both administrators and teachers, which may contaminate the reliability and fidelity of standardized test results (see Neal, 2013, and Battistin, 2016). In an early study, Jacob and Levitt (2003) present evidence from Chicago public schools that test score manipulation by unscrupulous administrators and teachers responds strongly to small modifications in incentives, especially in low-achieving classrooms. Jacob (2005) uses data from Chicago public schools to show that score accountability pressures may also induce teachers to *'teach to the exam'*. Neal and Schanzenbach (2010) show that, in order to inflate their assessment scores, teachers may pay less attention to top- and worst-performers in a given grade, focus on pupils close to the relevant performance cutoff, and leave worst-performers behind.

No evidence of test score manipulation is found in Lavy (2009), who shows that a monetary incentive programme for English and math teachers in Israel led to greater teacher effort, higher test taking rates, improved conditional pass rates, and higher mean test scores. Diamond and Persson (2016) estimate the positive and lasting effects of maths grade inflation in the last year prior to high school in Sweden. Dee et al. (2017) document regular test score manipulation in New York's high school exit exams, known as the Regents Examinations, which hurts better students and helps worse students who are at risk of dropping out. Borcan et al. (2017) also find evidence of grade manipulation on high-stakes exams at the end of high school in Romania. Angrist et al. (2017a) find evidence of teachers' cheating in smaller classes in Southern Italy, and they interpret this as a form of cheating that is induced by the lack of accountability of teachers in the Italian mezzogiorno, rather than an excess of it. Battistin and Neri (2017) document some consequences

of score regrading at the end of primary school in England for pupils just below relevant achievement thresholds.

Finally, and of direct relevance to this paper, Machin et al. (2017) look at whether or not English secondary school students just pass or fail their GCSE English Language examinations (they have data on actual marks and so can study differences of one mark either side of the grade C pass cutoff). Their focus is on long term effects of just passing or failing, but part of their analysis highlights that the nature of the examination system (especially its external grading system via exam boards) makes it hard to teachers in schools to directly manipulate grades in these high stakes examinations. Despite this, incentives for teachers and head teachers now play a big role in the English education system where the ranking of schools and their managers by pupil performance has become a central feature of the system. If direct manipulation of exam results is difficult, then this raises the distinct possibility that other means to try to enhance league table positions might be attempted. The one considered in depth in this paper is permanent exclusion of low performers so that they do not get entered into the exams that count towards league table positions.

School Accountability

This paper is therefore most closely related to a literature that investigates how accountability pressures can lead schools to try and shape the pool of pupils that sit exams in a strategic manner. Figlio and Getzler (2002) find that, following the introduction of the Florida Comprehensive Assessment Test in 1996, schools began to label students as disabled and, thus, as unable to contribute to the school's aggregate results, especially in low-income schools and for pupils with low prior academic performance or from low socio-economic background. Figlio (2006) documents that schools in Florida attempt to reshape the pool of test-taking students by widening the gap between the punishment assigned to low-performing pupils and to high-

performing pupils in periods of high-stakes exams for pupils in testing grades. Jacob (2005) shows that, under the accountability regime introduced by the accountability reform in 1996/97 in Chicago public schools, teachers started to retain pupils preventively, they increased special education placements and they substituted away from low-stakes subjects. Cullen and Reback (2006) also find evidence of strategic efforts in Texas schools in the 1990s to manipulate the pool of test-taking students, especially affecting low-performing Hispanic and Black students. Finally, Angrist et al. (2017b) document enrolment manipulation to produce smaller classes in Israel, plausibly reflecting the schools' efforts to appeal to educators and parents, and attract greater public financial support. However, the interest of their paper lies in the effect of class size on school achievement, and they do not find this effect to be affected by enrolment manipulation.

School Autonomy

The content of this paper also relates to an important, growing literature on school autonomy. A number of countries have recently implemented important reforms of their schooling systems to introduce new types of schools that have more autonomy to operate than do regular state/public schools. Such school reforms have taken place in a number of countries where inequality in access to education, as well as concerns with the quality of public education, have featured prominently in the public debate. Attempts to improve pupil performance, reduce educational inequalities and a quest to try to find the optimal educational structure are all factors underpinning the reforms. Over the last 25 years, charter schools have been introduced in the US, free schools in Sweden ('friskolor'), academy schools in England, and 'Tomorrow's Schools' in New Zealand. In the belief that increased autonomy would enable schools to deliver a better service, these new school types were granted greater autonomy in several dimensions in comparison with already existing schools.

Charter schools in the US are especially prevalent in large urban areas, such as Boston, Los Angeles, New York and Philadelphia where many students are poor and non-white. Schools in these areas often record low standardized test scores, high truancy rates, and high dropout rates (Abdulkadiroglu et al., 2016). Although this literature has found mixed evidence on the effects of charter attendance on academic and labour market outcomes (see, e.g., Dobbie and Fryer, 2016), positive effects appear concentrated in urban centres and at schools practicing the *"No Excuses"* model, which stresses behavioural norms and work ethic (Abdulkadiroglu et al., 2011; Dobbie and Fryer, 2011; Angrist et al., 2016; Dobbie and Fryer, 2015 and 2016). Benefits from charter attendance tend to accrue to less-privileged students (e.g., Gleason et al., 2010), and they appear larger in math than in reading test scores (see, e.g., Hoxby et al., 2009; Gleason et al., 2010; Angrist et al., 2010; and Fryer, 2014).

Key contributions to the literature on the English academies studied here are Eyles and Machin (forthcoming), Eyles et al. (2016a) and Eyles et al. (2016b)[8]. These studies focus on the first (i.e., pre-2010) batch of academies, and they study the impact of the academy conversion on pupil intake, pupil performance at the end of secondary school and post-secondary school outcomes. They find that the academy conversion led to an improvement in the quality of pupil intake, as measured by the pupil prior schooling attainment, while enrolment of FMS eligible pupils dropped. Eyles and Machin (forthcoming) and Eyles et al. (2016b) also find that pupils who attended academies performed significantly better in their GCSE exams at the end of secondary school, with greater gains accruing to pupils enrolled in an academy for longer and to pupils from disadvantaged backgrounds (Eyles et al., 2016b). Academies in London experienced the greatest

[8] Machin and Wilson (2008), Machin and Vernoit (2011) and Wilson (2011) first examined the effects of the pre-2010 academies programme on the quality of pupil intake. Machin and Silva (2013) first documented positive performance effects of academy conversion using KS4 test scores. Clark (2009) is also related to this literature as he studied the effect of school conversions to become grant-maintained (GM) schools on pupil performance.

gains, a result that echoes the evidence on urban charters in the US (see, e.g., Angrist et al., 2013). Academy enrolment raises the probability of completing a Bachelor degree by the age of 21, and this effect too appears greater for academies in London (Eyles et al., 2016b). Eyles et al. (2016a) show that academy enrolment raises the chances to remain in school at the end of compulsory schooling, it has longer run effects by raising the probability of entering higher education in the four years after completion of KS4 exams, and it increases the likelihood to enrol in a Bachelor's degree in a non-Russell group university.

In sum, the existing literature suggests positive effects from enrolment in autonomous schools, especially those adopting a "*No Excuses"* approach. However, if autonomous schools do deliver performance improvements, one key research question is whether they do so by improving the education levels of children enrolled in the school, or whether their extra autonomy means that they are able to manipulate measures of school performance. In the English setting we study, one way to do this is to exclude poorly performing students who bring down the school average. The reviewed literature suggests that this is a possibility, and this is exactly what the popular press, and some politicians and educationalists, have argued that academy schools do.[9] This, in fact, is a concern that has also been raised in US media about charter schools.[10] In this paper, we directly study this question, looking at causal estimates of academy status on pupil exclusions and their strategic nature, and whether it is indeed possible that the pupil performance improvements seen in some academy schools can be attributed to this form of strategic exclusion.

[9] Appendix B reports selected examples available online of media coverage.

[10] To our knowledge, there is to date no academic research on expulsions by US charters. The closest research compares exit patterns of students in charters with exit patterns of students in traditional public schools, and fails to detect significant differences between these (see Zimmer and Guarino, 2013; and Furgeson et al., 2014).

4. Autonomous Schools and Research Design

Autonomous Schools in England

The secondary sector of the English educational system consists of around 3000 schools spread across 152 local education authorities. Up to the late 1990s, most secondary schools in England were community schools that were largely under the control of local educational authorities. In addition to community schools, religious schools (called voluntary controlled or voluntary aided schools) and foundation schools made up the remainder of the state funded secondary sector.[11] These schools existed in the 1990s and they continue to exist today. Community schools differ from other types of schools in a number of ways. For instance, foundation and voluntary aided schools usually have more autonomy than community schools. Some also have selective admission procedures inherited from the past, whereas a selection system based on ability is not permitted to other state-funded schools in England. For community schools, admissions of pupils, as well as hiring and firing of staff, are managed by the local education authority. Finally, no state-funded school in England is allowed to charge fees to their students.

Up until 2002, community schools operating under the control of local authorities constituted the majority of secondary schools in England. Between the late 1990s and the early 2000s a general recognition spread in England that some secondary schools were not delivering a good enough education to their pupils. These schools were usually concentrated in disadvantaged urban areas, and their poor educational standards, coupled with significant discipline problems, generated mounting pressure on public authorities. The proposed solution was to replace the existing school with a new type of state school - the academy school. The first academy schools were introduced by the 1997-2010 Labour government, and they opened their doors in September

[11] During the 1990s, a small number of so-called City Technology Colleges (CTCs) also existed. In some ways these were the pre-cursors of academy schools (see, for example, the discussion in West and Bailey, 2013).

2002. While only a handful of schools were converted to academies in the school year 2002/03, 5,272 academies existed by April 2016. In hindsight, the introduction of academy schools in the English education system not only constituted an important development in the history of education in England, but it also appears to be one of the most radical and encompassing programmes of school reforms experienced in recent years across advanced economies.

Academy schools are independent, non-selective and state-funded schools. They are run outside of the control of the local education authority and, in most cases, they are conversions from an already-existing predecessor school. Prior to the Academies Act of 2010, when key changes were made to the definition of academy status, they were managed by a private team of independent co-sponsors. The sponsors of the academy school delegate management of the school to a mainly self-appointed board of governors that have the responsibility to employ the academy staff, to agree levels of pay and conditions of service, to decide on the policies for career development, discipline, performance management and staffing structure, and to manage school admissions.[12] Unlike other schools, including foundation and voluntary aided schools, academies also have more autonomy in a number of dimensions, such as the freedom to opt out and not follow the national curriculum (except for core subjects) that defines what is studied in English secondary schools.

Up until 2010, the central aim behind the creation of these schools was to turn around failing schools. In order to establish an academy school, it was necessary to identify a sponsor, which could be an individual, a business, a religious body or a university. Together with the school's local authority (LA), the sponsor needed to submit a formal expression of interest to the Secretary of State for Education for the ministerial approval and, upon receipt of approval, the feasibility stage

[12] Although academies have control over their own admission arrangements, they operate under the same admissions code as other state schools. Thus, they cannot select students based on ability and they have to prioritise the most disadvantaged students, i.e., those who are or have been in care and those with learning difficulties.

was the last step before the actual academy conversion of the school.[13] Since the first academies opened in 2002/3, a growing number of schools converted in the following years and by January 2010, 203 academy schools, or four percent of secondary schools, were operating in England.

A major change to the academisation process of the English education system started in May 2010, when the new Conservative/Liberal Democrats coalition government was elected and quickly introduced the Academies Act of 2010. While the pre-2010 Labour government conceived the academies programme as a remedial programme for poorly performing schools, the Academies Act of 2010 brought about a step-change to it in order to bring greater autonomy and competition in the entire school sector. The programme was no longer limited to poorly performing schools, and schools no longer had to sign up a sponsor prior to becoming an academy[14]. Furthermore, primary schools were also allowed to become academies. The new policy regime resulted in a remarkable increase in the number of academies in England; by January 2015, 2,075 secondary schools were academy schools, constituting 61% of secondary schools in England. In line with the new legislation, and unlike the pre-2010 academies, these were generally high performing schools enrolling advantaged pupils, and only few of these (531, including the 203 pre-2010 academies) signed up a sponsor prior to conversion (Eyles et al., 2016a).

Table 3 shows results from permanent exclusion logistic regressions in year 11 by school type. Panel A shows estimates for the likelihood of permanent exclusion throughout year 11, whilst panel B shows the same for pre-January census permanent exclusion. For purpose of comparison, column (1) of panel A reproduces the column (1) of Table 2 results, while column (1) of panel B

[13] The sponsor's role was mainly to contribute management expertise. Until 2007 sponsors were also required to contribute 10 percent to the capital cost of new school buildings, or up to a maximum of two million pounds, but this requirement was relaxed in 2007 and removed in 2010.

[14] Under the new regime, would-be 'converts' only had to debate their intention to convert with parents, staff and pupils, and obtain permission to convert from the foundation or religious body funding the school. The intention to convert then had to be registered at the Department for Education (DfE), and information had to be submitted on school attainment and school finances for the last three years, and on the latest inspection by the school inspectorate (Ofsted).

does likewise for column (2) of Table 2. These are the all school estimates. In the rest of Table 3, estimates are shown for year 11 pupils in the different school groupings: column (2) for pre-2010 academies after conversion, column (3) for all post-2010 academies, column (4) for post-2010 sponsored academies, column (5) for post-2010 converter academies and column (6) for non-academy or future to be academy schools.[15]

The coefficients reported in Table 3 mostly reveal a consistent pattern both across school types and for all year or pre-January census exclusion samples. In particular, pupils from low socio-economic background face a greater risk of exclusion in all types of schools. Males and native English speakers are also more likely to be excluded from all school types. Individuals with inferior prior academic performance are also much more likely to be excluded in year 11 in all types of schools. Overall the message to take away is that pupils with similar characteristics are more likely to be excluded in each of the school types considered in Table 3.

Research Design

The next step is to move on from the descriptive analysis undertaken so far to study the causal effect of school autonomy on pupil permanent exclusion in year 11. The research design used for this is a difference-in-differences approach that exploits the gradual introduction over time of academy schools in England. Due to the important differences between the academies programmes before and after 2010, the two are studied jointly as well as separately, with the post-2010 analysis also distinguishing between sponsored and converter academies.[16]

Table 4 provides a timeline of academy conversions between 2001/02 when there were no academies and 2016/17, for schools with complete data available from the start year. As Table 4

[15] Depending which school year the data covers.
[16] For some empirical evidence on the differences between pre- and post-2010 academies, see Eyles et al. (forthcoming).

shows, the growth of academies began slowly – just three academy schools opened in 2002/03 – rising gradually to 208 schools by 2010/11, and a very rapid growth followed in the period of mass academisation.

As with other work in the area of academy effects on pupil outcomes (notably Eyles and Machin's, forthcoming, analysis of pupil performance effects of academy conversion for the pre-2010 academies), there is a need to compare academies with a set of similar schools. The approach taken here is as in that other work, namely to use schools that will become academies in due course, but after the sample period ends. These have been shown to be well matched on pre-conversion observable characteristics (balancing tests for our samples of schools are given in Appendix Tables B1 to B4, and will be discussed in due course), but will also capture unobservable common reasons for conversion to an academy (like ethos or aspiration to become an academy).

As Table 4 shows, for pre-2010 academies by January 2009, 94 schools had become academies. These form the treatment group for the pre-2010 analysis, and the matched control group is the 114 schools that became academies in the school years 2009/10 and 2010/11. Importantly, all of these were approved for conversion prior to May 2010, i.e., under the Labour government and before the coalition government was elected in 2010.

Table 4 also shows that, for the analysis of the post-2010 academies programme which involved many more academy conversions, there is complete information on 1,474 academies, i.e., on 1,220 converters and on 254 sponsored academies. The research design adopted here defines treatment schools as becoming academies following the 2010 general election and by January 2014. The control group includes schools that became academies after this date. Thus, for the post-2010 sponsored academies programme, there is a treatment group of 157 schools that became sponsored academies by the school year 2013/14, and a control group of 97 schools that became sponsored

academies later. For the post-2010 converter academies, there is a treatment group of 1,116 schools that became converter academies by the school year 2013/14, and a control group of 104 schools that subsequently became converter academies. When the two programmes are analysed together, all the treatment schools from the separate programmes are combined and compared with all the control schools from the different programmes. This results in a treatment group of 1367 treatment schools to be compared with a control group of 315 schools.

The modelling approach of Eyles and Machin (forthcoming) is modified here to look at pupil exclusion as the outcome of interest rather than performance. Since academy conversion can change the 'quality' of pupil intake, an ordinary least squares regression of exclusion on academy status is unlikely to yield a credible estimate of the impact of being enrolled in an academy on exclusion. This problem of composition change in the school is circumvented by focussing attention only on pupils already enrolled in the school prior to conversion. This is the approach taken in Abdulkadiroglu et al. (2016) and Eyles and Machin (forthcoming), where pre-enrolled (in the respective studies referred to as 'grandfathered', or 'legacy-enrolled') pupils in schools that later became charters in the US, or academies in England, are studied.

Let *t* indicate academic year, let s(i, t) indicate the school in which pupil i is enrolled in year t, and let g(i, t) indicate the grade in which pupil i is enrolled in year t (this takes values 7-11, i.e., the secondary school years in England). The year of conversion for school s is denoted by CY_s. Finally, conversion cohorts are sets of academy schools – A_t – that convert in the same academic year t. Academy status is a binary variable and equals 1 for pupils enrolled in an academy at the start of year 11. Academy conversion is defined as:

$$\text{Academy}_{it} = \begin{cases} 1 \text{ if } t \geq CY_{s(i,t)} \\ 0 \text{ if } t < CY_{s(i,t)} \end{cases} \quad (1)$$

The basic difference-in-differences set up for a given conversion cohort of year 11 pupils i in treatment and control schools is:

$$EX_{it} = f(s, t) + \theta_1 Academy_{it} + \sum_{j=1}^{J} \beta_{1j} X_{jit} + \varphi_1 P_{it} + v_{1it} \quad (2)$$

where EX is a binary variable that takes value 1 if a pupil was excluded in year 11 and 0 otherwise, $f(s, t)$ is a general (to be specified) function of school and time, X is a set of pupil level control variables, P is pupil level prior achievement and v_1 is an error term.[17] Each cohort of treatment-comparison estimates can be stacked to obtain average difference-in-differences estimates of the coefficients in equation (2).

For this set up, one can define legacy enrolment status for pupil i in an academy school as an intention to treat binary variable ITT_{it} that equals one for those legacy enrolled in academy schools and 0 for pupils legacy enrolled in control schools:

$$ITT_{it} = \begin{cases} 1 \text{ if } s(i,t-1) \in A_t \text{ and } 11 > g(i,t-1) \geq 7 \\ 0 \text{ if } [s(i,t-1) \in A_t \text{ and } g(i,t-1) = 11] \cup s(i,t-1) \notin A_t \end{cases} \quad (3)$$

Pupils are legacy enrolled (i.e., $ITT_{it} = 1$) if they are enrolled in an academy in the year prior to conversion and are not in their final year of compulsory schooling. This remains fixed through time spent in secondary school. Since pupils spend five years in secondary school, exclusion effects in year 11 for pupils who stay in the school for a maximum of four years post conversion, including the year of conversion itself, can be estimated. This exercise can generate a causal effect for year 11 pupils because they had already previously enrolled in the same (predecessor) school before it converted to become an academy.

[17] The control variables in X are gender, free-school meal eligibility status, ethnicity, whether a pupil is native English speaker and a proxy for pre-determined school-level behaviour in the ITT school where pupil i pre-enrolled. Prior achievement P is measured by the end of primary school Key Stage 2 performance of the pupil.

There is, however, still a problem of estimating equation (2) only for the sample of legacy enrolled pupils. The issue that arises is that not all students initially enrolled in a school that converted to an academy ($ITT_{it} = 1$) remain in the school until the start of year 11 ($Academy_{it} = 1$). Thus, ordinary least squares estimates of θ_1 from (2) estimated on legacy enrolled pupils may not reflect a causal estimate if the decision to defy ITT assignment is non-random. Selection into and out of treatment can however be addressed by using intention to treat status (ITT_{it}) as an instrument for $Academy_{it}$, to estimate a local average treatment effect (LATE) as follows:

$$Academy_{it} = \alpha_s + \alpha_t + \theta_2 ITT_{it} + \sum_{j=1}^{J} \beta_{2j} X_{jit} + \varphi_2 P_{it} + v_{2it} \quad (4)$$

$$EX_{it} = \alpha_s + \alpha_t + \theta_3 ITT_{it} + \sum_{j=1}^{J} \beta_{3j} X_{jit} + \varphi_3 P_{it} + v_{3it} \quad (5)$$

where f(s, t) is specified as the additive function $\alpha_s + \alpha_t$, with α_s being a set of school fixed effects measured at the time of legacy enrolment and α_t a set of time fixed effects. In the first stage (4), estimates of θ_2 show the proportion of the ITT group that stay in the academy and are still enrolled at the beginning of year 11. Equation (5) is the reduced form regression of EX on the instrument. The instrumental variable (IV) local average treatment effect (LATE) estimate is then defined as the ratio of the reduced form coefficient to the first stage coefficient, θ_3 / θ_2.

Descriptive Difference-in-Differences

Table 5 shows the results from a descriptive difference-in-differences exercise comparing year 11 permanent exclusion three years before and after academy conversion in treatment group schools and control group schools. These are shown separately for pooled cohorts of pre-2010 academies and post-2010 academies, and their respective comparison schools. Results are also presented separately for post-2010 sponsored academies and post-2010 converter academies in the

third and fourth panels of Table 5. Thus, for example, for schools that became academies in 2005/06, the pre-period rate of permanent exclusion in year 11 was calculated in the school years 2002/03 to 2004/05, and the post-period rate of permanent exclusion in year 11 was calculated in the school years 2005/6 to 2007/8. These rates were compared with the exclusion rates in the same school years of control group schools.

Considering first pre-2010 academies in panel A of Table 5, column (1) reveals that 0.18 percent of pupils enrolled in year 11 in pre-2010 academies were permanently excluded in the three years prior to conversion, and that this rose sharply to 0.35 percent in the three years after academy conversion. There is much less change in control schools. By taking the difference between these 'pre' and 'post' exclusion rates, and then differencing across these yields the difference-in-differences (DiD) estimate shown in column (3). This is sizable at 0.16 percent for the pre-2010 academies, showing that they did engage in more permanent exclusions after becoming academy schools. This statistically significant increase represents a discrete increase in the three post-conversion years.

The analysis of the exclusion rates for the post-2010 sponsored academies, displayed in panel B of Table 5, does not exhibit the same pattern. Post-2010 academies display higher rates of permanent exclusion in year 11 both before and after conversion in comparison with control schools. The unconditional DiD comparison for these schools reveals that their permanent exclusion rates in year 11 increased by 0.007 percentage points compared to control schools. However, the DiD estimate for post-2010 academies is not significantly different from zero.

The analysis of the exclusion rates for the post-2010 sponsored academies shows that these schools, unlike their control schools, excluded slightly more pupils after they became academies. However, also in this case the DiD estimate is not significantly different from zero. Post-2010

converter academies display higher rates of permanent exclusion in year 11 compared to control schools both before and after academy conversion. However, the unconditional DiD comparison between treatment and control post-2010 converters does not indicate any significant difference in the exclusion rates in year 11 between these two groups, as both experienced a similar decline over time in the rates of exclusion in year 11.

Scope for Strategic Manipulation

The earlier discussion of the numbers in Table 1 revealed that almost 80 percent of pupils that were permanently excluded in year 11 from 2001/02 to 2014/15 were excluded earlier in the school year than the January census. This does not reflect seasonality in bad behaviour of pupils, given that in each of the years 7-10 the fraction of pupils excluded in the months prior to the census was well below half the number of pupils excluded throughout the year. Moreover, as discussed in section 2, the January census is an important cutoff for the purpose of inclusion in a mainstream secondary school's league table.

The very different year 11 within-school year profile of permanent exclusions raises the question as to whether the pre-January higher rates are observed because schools may be trying to manipulate their end of final year exam results. This is even more pertinent for the pre-2010 academies whose permanent exclusion rates went up after they became academy schools. In the data being analysed in this study, it is possible to study this as we are able to track in which schools permanently excluded pupils sit their GCSE exams.

Table 6 shows the fraction of pupils that sit exams in the excluding school by period of permanent exclusion within year 11.[18] Columns (1) and (2) show results for all schools, and columns (3) and (4) for the matched sample of treatment and control schools. Two interesting facts

[18] Tables A1 and A2 in the Appendix show the fraction of pupils that sit GCSE exams in the excluding school by month of permanent exclusion in year 11.

emerge. First, Table 6 reconfirms that the vast majority of year 11 permanent exclusions - roughly 80 percent – occur before the January census. Since this is unlikely to reflect seasonality in bad behaviour, this is suggestive that schools may be trying to shape the pool of KS4 takers by excluding more pupils at the beginning of year 11.

The second striking result of Table 6 also concerns the hypothesis that schools may use permanent exclusion in year 11 in a strategic manner. The results in Table 6 very strongly show that pupils who are permanently excluded before the January census are very unlikely to sit their summer exams at the end of year 11 in the school that excluded them. While almost 70 percent of pupils that are excluded after the January census are allowed, in spite of the exclusion, to sit exams in the excluding school, the comparable figure for pupils excluded prior to the January census was around 10 percent.[19] Thus, schools may well use permanent exclusion at the beginning of year 11 in a strategic way to manipulate results. For the remainder of the paper, pupils excluded in year 11 prior to the January census are referred to as *strategically excluded pupils*.

Table 7 shows descriptive DiD results in the same way as the earlier Table 5 did for any permanent exclusion, now placing the focus on this measure of strategic permanent exclusion. The analysis of pre-2010 academies, shown in panel A of Table 7, reveals a positive and statistically significant increase in strategic permanent exclusion taking place after academisation relative to matched future to be academy control schools. By contrast, the analysis of post-2010 academies fails to detect an overall increase in year 11 strategic permanent exclusions. The DiD estimate for post-2010 converters appears positive and significant, but also much smaller than the DiD estimate for pre-2010 academies and not robust to the inclusion of post-2010 sponsored academies in the

[19] It is important to notice that, although some of the excluded pupils are able to sit exams in the excluding school, almost all have to leave the school premises, and only if they are reinstated, they are subsequently allowed to return to the school.

analysis. This descriptive evidence suggests that academies, especially those that opened prior to 2010, may have started to exclude more following conversion, and they may have done so in a strategic manner in order to manipulate the pool of KS4 takers at the end of year 11.

5. Statistical Results

This section turns to the main statistical estimates that look at the causal effect of academies on permanent exclusion in year 11 and that subject the descriptive findings to more stringent statistical testing through a research design specification that accounts for endogenous sorting of pupils across schools. The point of departure is to show that the choice of control schools is justified in terms of pre-academisation balancing tests, followed by reporting of results from the main statistical analysis.

Balancing Tests

Tables B1 to B4 in the Appendix report balancing tests comparing pupil and school characteristics for pre-2010 academies, post-2010 academies, post-2010 sponsored academies and post-2010 converter academies to their respective sets of control schools. The tests were calculated just before the beginning of the pre-2010 and post-2010 programmes, in 2001/02 as reported in Table B1 for the pre-2010 academies, and for 2009/10 as shown in Tables B2, B3 and B4 for post-2010 academies. In both cases, these dates precede any academy conversion under each academies programme that is studied.

For pre-2010 academies, the comparison between treatment and control schools shows them to be balanced almost in all of the dimensions considered. The one exception is percent white, which is lower in pre-2010 academy schools (most likely reflecting the well-known fact that more such schools were located in urban, inner city locations). Nevertheless, the comparisons as a whole

clearly justify the definition of the control group for the analysis. The picture is not quite so strong for the post-2010 academies, although it is important to note that all the variables considered in the balancing tests are included as covariates in the reported statistical analysis. Post-2010 academies seem to enroll fewer free school meal eligible pupils compared to their set of control schools. When post-2010 sponsored and converter academies are analysed separately, the presence of free school meal eligible pupils appears well balanced between treatment and control schools but the prior achievement Key Stage 2 score appears respectively higher for converter academies (as shown in Table 4.C) and lower for sponsored academies (as shown in Table 3.C). The remaining pupil and school characteristics prove to be well balanced at baseline.

Initial Estimates

Table 8 shows the first set of estimates of the causal effect of academy enrolment on year 11 permanent exclusion. The Table shows results for pre-2010 sponsored academies, post-2010 sponsored academies and post-2010 converter academies when these are compared together with their respective control groups. Results are displayed for both any permanent exclusion in year 11 and for strategic permanent exclusion in year 11, i.e., that occurred prior to the January census. For each dependent variable, it first shows OLS estimates of equation (1), then ITT estimates and finally IV estimates. While the OLS estimates do not account for endogenous sorting of pupils across schools, the ITT estimates are based on the legacy enrolment decision that was made prior to the academy conversion, and the IV estimates instrument enrolment in an academy in year 11 with the ITT legacy enrolment status. Thus, the ITT and IV estimates circumvent the issue of endogenous sorting of pupils across schools and correct for the potential bias generated by it. Overall, the results indicate that academies started to permanently exclude more in year 11 following conversion. This conclusion holds true whether we assess the effect of academy

enrolment on any permanent exclusion in year 11 or on strategic permanent exclusion in year 11. This conclusion confirms the argument put forward by some popular press in England on the increase in exclusion rates in English academies.

Table 9 shows estimates of the effect of academy enrolment on the likelihood to experience any permanent exclusion in year 11 for pre-2010 academies, post-2010 academies, and separately for post-2010 sponsored and post-2010 converter academies. For each academy grouping, it still shows OLS estimates of equation (1), then ITT estimates and finally IV estimates. The results for pre-2010 academies confirm the conclusions from the unconditional DiD estimates of the previous section, as they show that the likelihood of a pupil experiencing permanent exclusion in year 11 increased in these schools following conversion. In contrast with the conclusions from the unconditional DiD estimates of the previous section, a significant increase appears visible also in post-2010 academies: although the estimated impact of enrolment in a post-2010 sponsored academy is not statistically significant, the coefficient associated to these schools, to post-2010 converters and to post-2010 academies overall are very similar.[20] Although the estimated impact of enrolment in a post-2010 academy is much smaller than the estimated effect for the pre-2010 academies, this effect is statistically significant at all conventional levels.

This conclusion that there was differential exclusion of year 11 pupils in both pre-2010 and post-2010 academies is further corroborated by Figures 1 and 2, which display event study IV estimates of the impact of academy conversion on permanent exclusion in year 11 for pre-2010 and post-2010 academies respectively. The event study generates separate estimates of academy impact years since conversion, but also allows examination of possible pre-conversion differences between treatment and controls. There are two notable features in Figure 1. First, there seem to be

[20] We could not reject the null hypothesis of equality of the estimated IV coefficients for the different groups of post-2010 academies in Table 9.

parallel pre-conversion trends between pre-2010 academies treatment and control schools. Second, there is a steep and persistent increase in the likelihood of a pupil experiencing permanent exclusion in year 11 starting from the year of conversion. Figure 2 also reveals parallel pre-conversion trends between post-2010 academies treatment and control schools, though the estimated effect is smaller in this case.

Since the potential use of permanent exclusion for strategic manipulation of GCSE results is of central interest to this study, Table 10 shows the estimates of the causal effect of academy enrolment on year 11 strategic permanent exclusion separately for each group of academies. Using the same Table structure as the previous Table 9 for any permanent exclusion, Table 10 shows the OLS, ITT and IV estimates by academy group. The results in Table 10 are consistent with those in Table 9, as they also show that academies started to exclude more after conversion.

Following academy conversion, pupils enrolled in pre-2010 academies became more likely to be strategically excluded. The OLS coefficient in column (1) shows that, among pupils in pre-2010 academies, academy conversion increased the probability of strategic exclusion by 0.260 percentage points. The ITT coefficient in column (2) indicates that the academy conversion increased the likelihood for legacy-enrolled pupils to get strategically excluded in year 11 by 0.262 percentage points. Column (3) shows the IV estimate of the effect of academy enrolment on strategic permanent exclusion. Since, as suggested by the reported first stage coefficient of 0.929, not many pupils switch school, the ITT and IV coefficients are similar. The IV estimate indicates that the academy conversion in pre-2010 academies led to a 0.282 percentage points increase in the probability that a pupil enrolled in a pre-2010 academy was strategically excluded. Table 10 also shows results for the post-2010 academies, where an increase in the incidence of strategic permanent exclusion following academy conversion could also be detected. Also in this case, very

similar coefficients were estimated for post-2010 sponsored and post-2010 converter academies, with the estimated effect of academy enrolment being smaller than for the pre-2010 counterpart but nonetheless positive and significant.

Discussion

The results of Table 10 are inconclusive on whether school autonomy led academy schools in England to employ permanent exclusion as a tool to bolster their aggregate test performance. On the one hand, the fact that there is an effect for both pre-2010 and post-2010 academies suggests that more autonomous schools did use discipline sanctions to manipulate their results. On the other hand, the effect appears much larger for pre-2010 academies. As we already discussed, these were poorly performing schools prior to academy conversion, they had serious discipline problems and they recorded very poor aggregate academic results.[21] They became academies precisely as part of a remedial programme by the 1997-2010 Labour government. In contrast, post-2010 academies were much better schools prior to the academy conversion. Therefore, it is very unlikely that these schools faced the same extent of discipline problems. The fact that we find a greater increase in permanent exclusion in year 11 for pre-2010 academies compared to the result for post-2010 academies suggests that the increase in exclusion rates observed in Tables 9 and 10 may simply reflect the discipline problems that academies faced, with the pre-2010 academies facing more serious discipline problems than the post-2010 academies.

Moreover, one may be worried that by restricting the analysis to permanent exclusion in year 11 and not looking at permanent exclusion in prior school years, this rules out an important

[21] As an example for one of the early pre-2010 academies in our sample, the London Evening Standard in 2003 reported that "Supply teacher Kate Gibbs described a nightmare two weeks during which she was confronted by a group of 16-year-old boys and thrown across a desk after she tried to stop a violent struggle. She was also regularly sworn at by pupils and unfairly accused of racism, a ploy used by rowdy students to duck responsibility for disruptive behaviour. And she was shocked by the levels of illiteracy among pupils and by the despair of teachers who appeared to have "given up"." The full article is available at the following link: https://www.standard.co.uk/news/super-school-fails-after-6-months-6351882.html

channel through which academies may attempt to shape the pool of KS4 takers, as academies may possibly exclude more also before a pupil reaches year 11. Looking at pre-2010 academies, for which we find larger effects, the comparison of the OLS coefficient in column (1) with the ITT and IV coefficients in columns (2) and (3) is informative, as it reveals these coefficients to be very similar. The OLS appears slightly smaller than the ITT and IV coefficients, but all the estimated coefficients are positive, significant at all conventional levels and of similar magnitudes. The smaller size of the OLS coefficient suggests that some pre-enrolled badly behaved pupils may have left the academy after conversion and prior to the start of year 11. However, the degree of similarity between the OLS, ITT and IV coefficients clearly suggests that at best this could only involve a small number of pupils, and that most of the strategic exclusion that these schools implement takes place in year 11. In turn, this does suggest also that permanent exclusion may have been used by these schools to shape their pool of GCSE test-takers. It is also important to notice that the first stage coefficient in column (3) indicates that only roughly seven percent of pre-enrolled pupils moved to a different school by the time they were in their last year of secondary school.

Finally, there is the issue that permanent exclusion is a rare event involving very few pupils. From an econometric standpoint, exclusion operates in the tail of the distribution of pupils and raises issues that a linear probability model may predict outside the 0-1 interval. However, as both the endogenous variable of interest and the instrumental variable are binary variables, the comparison is *de facto* one looking at averages of treated and controls in the probability that strategic exclusion occurs after academy conversion, holding constant all other covariates in our specification. There would be a concern linked to rarity of the outcome variable if a linear probability model were used to estimate the marginal effect of a continuous treatment. In this case, moving along a linear or a non-linear function in the tails may generate different results.

6. Possible Results Manipulation and Exclusion by Prior Achievement

The key question that emerges from the empirical work presented so far is whether the higher exclusion propensities in academies occurred strategically in attempts to manipulate overall test results. As mentioned earlier, Eyles and Machin (forthcoming) and Eyles et al. (2016b) reported evidence of a positive causal impact of academy enrolment on GCSE achievement of pupils in pre-2010 academies. In this paper, the interest is whether this can be attributed to higher rates of strategic permanent exclusion. As noted earlier, this is exactly what some popular press, some politicians and some educationalists have argued.

Table 11 shows estimates that replicate the findings of Eyles and Machin (forthcoming) on the effect of academy attendance on end of secondary school pupil performance. The outcome of interest in Table 11 results is now the (standardised) test score of pupils in the KS4 high-stakes exams that pupils sit at the end of year 11.[22] In the same way as in Eyles and Machin (forthcoming), academy (and control school) enrolment status is based on the January census. Thus, strategically excluded pupils are not in the excluding school, and rather appear in whatever new school they attend following exclusion.[23]

The results in columns (1) to (3) of Table 11 are very much in line with those of Eyles and Machin (forthcoming). According to the IV specification in column (3), pupils enrolled in pre-2010 academies scored 0.104 of a standard deviation (0.104σ) higher than pupils enrolled in counterfactual schools. Analogous results for post-2010 academies are also reported, and here there

[22] The data description part of the Appendix describes the points system and measurement of the KS4 test score in detail.
[23] Recall that, for the analysis of the exclusion impact of academy enrolment, pupil attendance was defined in terms of the school at the beginning of year 11.

are opposite conclusions, as no performance improvement could be detected for pupils enrolled in post-2010 academies[24].

The results in Table 11 suggest quite clearly that pre-2010 academies recorded a steep performance improvement following conversion, whereas post-2010 academies did not record an average performance improvement following conversion. The question of whether strategic exclusion in year 11 may explain the performance improvement of pre-2010 academies is considered directly in this section of the paper, where four complementary pieces of evidence on the question are presented. The first is a simulation exercise that constructs a counterfactual outcome looking at what school performance would have been had the excluded pupils not been excluded during year 11. The second asks whether excluded pupils differed in terms of prior achievement in autonomous and non-autonomous schools. The third asks whether autonomous schools excluded more year 11 pupils enrolled in the school in the January census, for whom there is not scope for strategic exclusion as they are permitted to sit their GCSE exams in the excluding school. The fourth asks whether the schools that experienced the greatest increase in exclusion rates following conversion are the ones that also experienced the greatest school performance gains. It turns out that none of these are in line with the notion of differential strategic results manipulation by autonomous schools. Rather, they appear to be symptomatic of genuine discipline issues faced by the pre-2010 academy schools.

Possible Results Manipulation

To define a counterfactual scenario that enables computation of what the performance effect of academisation would have been had strategic exclusion not taken place, a simulation exercise

[24] We do not present separate results for post-2010 sponsored and post-2010 converter academies in Table 11 because we retrieved very similar results for permanent exclusion for both groups of post-2010 academies as shown in Section 5.

was undertaken to reinstate strategically excluded pupils back into the excluding schools. There are several ways in which this can be done to recompute what school performance would have been had pupils not been excluded. Results are shown in Table 12, for the pre-2010 academies where there is evidence of differential strategic exclusion.

The first column of Table 12 reproduces the 0.104σ performance effect from the KS4 regression reported in Table 11. Column (2) shows what happens to this estimated effect when strategically excluded pupils are reinstated in the excluding school. In doing so, the excluded pupil is given the lowest GCSE test score (zero) in both treatment and control schools, and then the estimated academy effect is reconstructed.[25] As column (2) shows, and as would be expected because academies exclude more pupils, the estimated performance improvement falls under this counterfactual, dropping down ever so slightly to 0.103σ. The drop is tiny and reflects that the low rate of permanent exclusion cannot in and of itself impact substantively on the overall KS4 impact of academy attendance.

Of course, the counterfactual in (2) increases the number of pupils to higher than it was in reality for the GCSE exams. There are two issues to do with this. First, year 11 size is bigger. Second, if the excluded pupils had stayed in the school, and their discipline problems prevailed, they may have had a negative effect on other pupils. To consider the first of these, in column (3), to maintain the actual size of the year 11 cohort, an equal number of pupils were dropped from the counterfactual calculation undertaken in column (2). To maximise the performance impact, an equal number of top KS4 performers within each school and school year was eliminated, and then

[25] Zero is the worst possible KS4 outcome, and this KS4 outcome is assigned to excluded pupils to simulate the hypothetical worst-case scenario that may have induced a head teacher to exclude strategically a poorly performing pupil. However, we also looked at a range of other possibilities. One is to reallocate the pupils to the lowest score of all other pupils taking GCSEs in the school. A second (albeit endogenous) possibility is to use their actual GCSE test scores in the new school where they sit their exams. Using these, in place of zero, made little difference in practice.

the academy effect recomputed. To be clear, for example if five pupils were strategically excluded from school *s* in school year *t*, they were reinstated and the top five KS4 performers in the same school and school year were removed. When this is done, as column (3) shows, a further small drop, to 0.102σ, is seen.

Thus reinstatement of permanently excluded pupils alone barely affects the conclusion that the pre-2010 academies improved end of secondary school performance. But what of peer effects? The remainder of Table 12 considers this possibility, by producing counterfactual academy effects on pupil performance for increasingly larger negative peer effects that could have occurred had excluded pupils not been excluded from treatment and control schools. The counterfactuals are constructed with the intention of being able to answer the question, how big would negative peer effects have to be to eradicate the performance gains that pre-2010 academies generated?

The remaining columns of Table 12 (columns (4) through (12)) make it very clear that negative peer effects resulting from the small number of permanently excluded pupils would have to be implausibly large to cancel out the performance gains. Moving from column (4) which assumes a 5% of a standard deviation negative peer effect through to column (12) which assumes a huge 50% of a standard deviation negative peer effect does gradually lower the estimated academy impact on KS4. But, for a ten percent significance level cutoff, the effect only becomes insignificantly different from zero when column (9), with an assumed 35% negative peer effect, is reached. And even there the estimated academy effect is still 0.063σ, as compared to the starting point of 0.104σ in column (1). Thus negative peer effects would have to be implausibly large for strategic permanent exclusion to explain away the GCSE performance gains made by pre-2010 academies.

Prior Achievement Differences

The second issue would be if academies demonstrably excluded worse performers. Thus there would be a bigger differential performance gain accruing to the autonomous schools. One reason why this seems unlikely is that the same result on a need for implausibly large negative peer effects shown in Table 12 for treatment and control schools emerges if the control schools are dropped from the analysis.[26] Columns (1) and (2) in Table 13 also present further evidence in line with this, presenting evidence in two different ways that there do not appear to be differences in the prior achievement levels of permanently excluded pupils in treatment and control schools.

In Column (1) of the Table, an interaction between academy attendance and prior achievement measured by end of primary school Key Stage 2 performance of pupils is added to the IV LATE model of the probability of strategic permanent exclusion that was presented earlier. The interaction term is not statistically different from zero, suggesting that the causal LATE impact of academy attendance on exclusion does not systematically differ with prior achievement.

In Column (2) of the Table only the excluded pupils in treatment and control schools are considered, and the reported estimate considers whether KS2 test scores differ between those excluded from academies and control schools. As the estimate shows, there is no significant difference between the prior schooling attainment of pupils excluded in year 11 in academies and the prior schooling attainment of pupils excluded in control schools.

Non-Strategic Permanent Exclusion in Year 11

The third analysis of this section considers whether pre-2010 academies also excluded more pupils in year 11 that were permitted to sit exams in the school that excluded them because their date of exclusion post-dated the January census. Of course, such permanent exclusions are outside

[26] The column (1) estimate in this case was 0.084σ, and as with Table 12, similarly implausibly large negative peer effects would need to operate to offset this performance effect.

the strategic time window for pupils' exam results to not appear in the published school league tables. For this empirical exercise, the dependent variable now takes the value one for pupils who were permanently excluded in year 11 after the January census and zero otherwise. Absence of significance in these estimates would be consistent with the notion that pre-2010 academies only excluded more pupils in the strategic window - i.e. those that could be removed from their on-roll lists of GCSE test-takers - pointing towards the use of permanent exclusion in year 11 as a means for strategic manipulation of aggregate school results. However, the result of this exercise is displayed in Column (3) of Table 13 and, as the estimate shows, this is not what is found. There is still an academy differential in permanent exclusion post January census. Pupils enrolled in pre-2010 academies were more likely than pupils in counterfactual schools to experience permanent exclusion in year 11 also after the January census of their year 11's year and, thus, regardless of whether they contributed or not to the school aggregate results.

School-Specific KS4 Gains and School-Specific Strategic Permanent Exclusion in Year 11

The fourth part of this section investigates whether the pre-2010 academies that experienced the largest increase in strategic exclusion are also the ones that experienced the greatest school performance gains, as measured by the Key-Stage 4 test scores of their on-roll list of pupils. To this end, school-specific IV estimates of the effect of academy attendance on strategic exclusion in year 11 and on KS4 outcomes were retrieved and plotted against each other. Figure 3 shows the result of this exercise for pre-2010 academies, and three notable features appear.

First, while almost the entirety of pre-2010 academies recorded greater strategic exclusion rates after conversion, only a handful of schools increased their strategic exclusion rates by more than 1 percent. These may be, in fact, the schools that attracted media coverage and then catalysed public attention. Second, there is a weak positive (and statistically insignificant) correlation

between increases in strategic exclusion rates in year 11 and KS4 gains after conversion. Third, the schools that experienced the largest increases in exclusion rates do not seem to drive the average KS4 gains of pre-2010 academies, as these are not the schools that experienced the greatest KS4 gains after conversion.

Taken together, the results of Tables 12 and 13 and Figure 3 appear inconsistent with the notion that pre-2010 academies may have started to exclude more to strategically manipulate their aggregate school performance results. The greater exclusion rates in academies after conversion reflect the *"No Excuses"* approach and its emphasis on discipline and good behaviour in school that some pre-2010 academies adopted, but they did not constitute an attempt to manipulate the school's aggregate results.[27] Sizable negative peer effects need to be assumed for the performance improvements of pre-2010 academies to become insignificant, and prior schooling achievement appears to be unrelated to the effect of academy enrolment on permanent exclusion in year 11. Unlike some previous studies that found the use of sanctions and other means of shaping the pool of test-taking pupils to be concentrated among poor-performing pupils (e.g., Figlio and Getzler, 2002, and Figlio, 2006), these results suggest that pre-2010 academies started to exclude more following conversion, but this did not relate with pupils' prior academic performance. Moreover, the greater likelihood to experience permanent exclusion in year 11 in pre-2010 academies also extends to pupils who contributed to the school aggregate results in spite of the exclusion. Finally, although some schools did experience sizable increases in strategic exclusion in year 11 following conversion, these are not the schools that recorded the greatest performance gains among pre-2010

[27] An example of the tough discipline policies of the pre-2010 academies is the Student Behaviour Policy 2015 of the Mossbourne Community Academy in the Borough of Hackney (London). This was one of the first pre-2010 academies to open in England and its Student Behaviour Policy 2015 is accessible here: http://www.mca.mossbourne.org/wp-content/uploads/sites/3/2015/08/WA-4-3-Behaviour-inc-sixth-form-H-A-Agreement.pdf

academies. In contrast, performance gains appear unrelated to increases in exclusion for pre-2010 academies.

7. Conclusion

A number of countries have recently reformed their schooling systems by introducing new types of schools that have more autonomy to operate than do regular state/public schools. The most well-known, and studied, are charter schools in the US and academy schools in England. A recent literature has documented significant gains from enrolment into some of these types of schools, especially in urban settings (e.g., Abdulkadiroglu et al., 2016; and Eyles and Machin, forthcoming). If autonomous schools do deliver performance improvements, one key research question is whether they do so by improving the education levels of children enrolled in the school, or whether their extra autonomy means that they are better able to manipulate measures of school performance. In the English setting we study, one way to do this is to permanently exclude poorly performing students who bring down the school average.

This question is directly studied in this paper, which contributes to the growing literature on possible test score manipulation by presenting results on a novel possible means of results manipulation to the existing work. It presents causal estimates of academy status on pupil exclusions and their strategic nature, and asks whether it is indeed possible that the achievement gains seen in some academy schools can be attributed to this form of strategic exclusion. Whilst it is the case that pre-2010 academies did exclude more pupils enrolled in the final year of secondary school, it is not possible to uncover evidence that this differential exclusion facilitated manipulation of overall school performance.

The analysis of the post-2010 academies also reveals that these schools started to exclude more following conversion. However, the estimated effect here is much smaller than for the pre-2010 academies, and this is most likely due to the very different nature of the academies programme after 2010. In fact, post-2010 academies were much better schools prior to the academy conversion, thus it is plausible that they did not face the kind of behavioural problems and pressure for improvement faced by the pre-2010 academies.

In conclusion, as some popular (albeit non-evidence based) public debate has sometimes argued, academy schools in England do indeed show higher rates of pupil permanent exclusion than otherwise comparable non-academy schools. However, the focus on the initial batch of academy schools in England, which were introduced as a remedial school improvement programme in the 2000s and that recorded steep performance improvements following conversion, reveals that exclusion was not a means of facilitating better outcomes for autonomous schools in the published league tables. Rather it seems to be part and parcel of the disciplinary behaviour procedures that these schools implement (i.e., an integral part of the "No Excuses" culture some academies operate). This finding is corroborated by the fact that we see much smaller gaps in permanent exclusion between the second (post-2010) conversions to academies, which were not disadvantaged schools with the behaviour problems that characterised the initial pre-2010 phase of academisation of the English secondary school sector.

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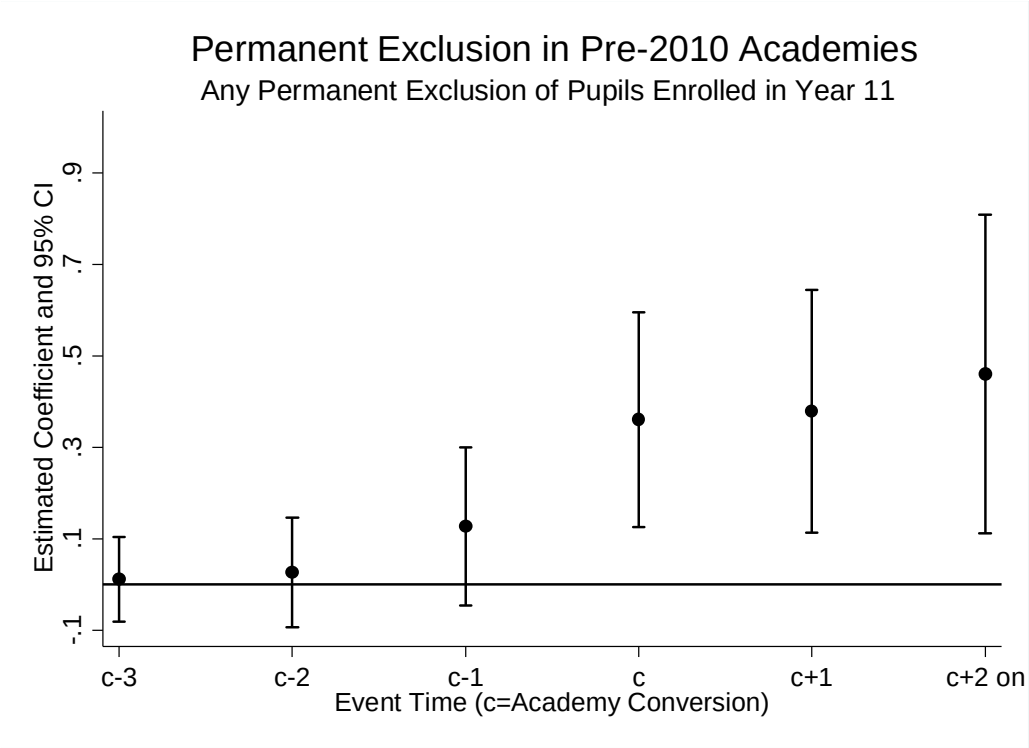
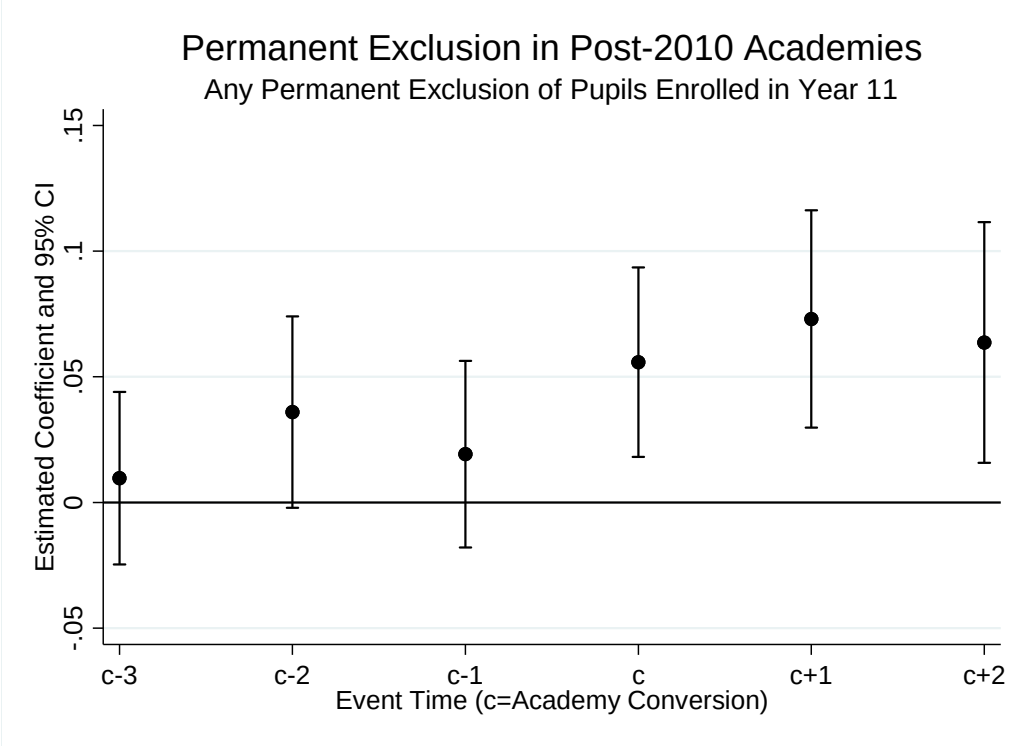
Figure 1. IV Estimates of Impact on Permanent Exclusion in Year 11 in Pre-2010 Academies



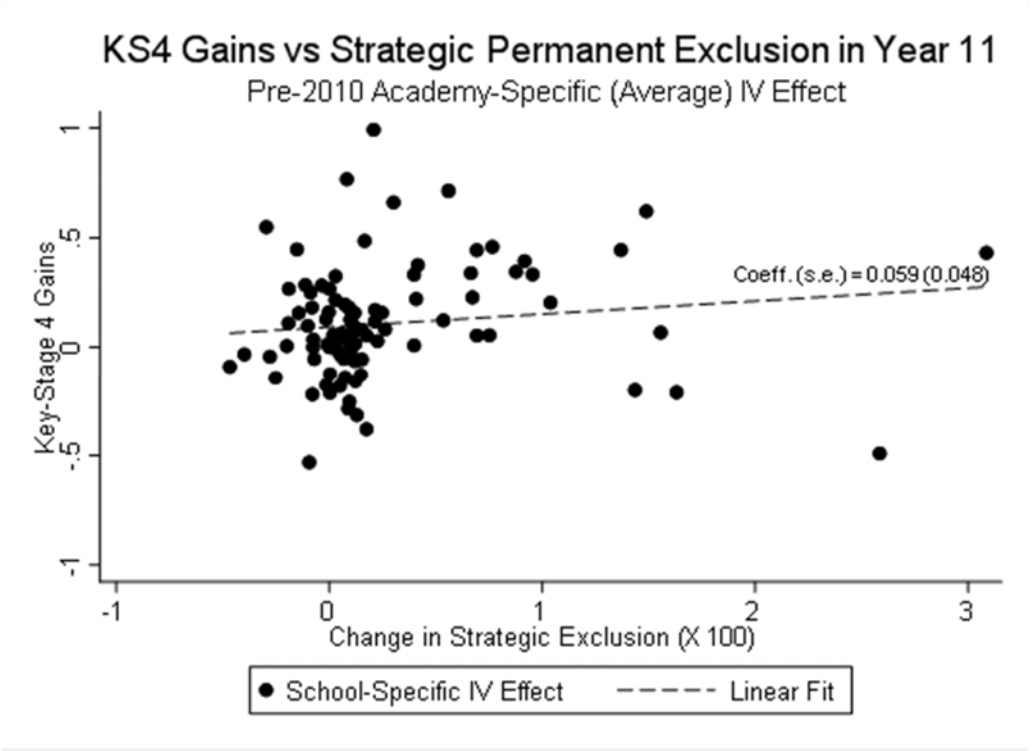
Notes: Figure 1 shows event-study IV estimates of the causal effect of academy enrolment on year 11 permanent exclusion for Pre-2010 academies. c indicates year of academy conversion and 95 percent confidence intervals are reported together with point estimates. Robust standard errors were clustered at the school level. Control variables included are dummies for whether the pupils are eligible for free school meals, whether they are male, native English speakers, the pupils' ethnicity group and a proxy for pre-determined school-level behaviour in the ITT school where the pupils pre-enrolled, entered together with KS2 test scores and a dummy variable for pupils for whom KS2 data is unavailable.

Figure 2. IV Estimates of Impact on Permanent Exclusion in Year 11 in Post-2010 Academies



Notes: Figure 2 shows event-study IV estimates of the causal effect of academy enrolment on year 11 permanent exclusion for Post-2010 academies. c indicates year of academy conversion and 95 percent confidence intervals are reported together with point estimates. Robust standard errors were clustered at the school level. Control variables included are dummies for whether the pupils are eligible for free school meals, whether they are male, native English speakers, the pupils' ethnicity group and a proxy for pre-determined school-level behaviour in the ITT school where the pupils pre-enrolled, entered together with KS2 test scores and a dummy variable for pupils for whom KS2 data is unavailable.

Figure 3. School-Specific IV Estimates of Impact on Key-Stage 4 Test Scores against School-Specific IV Estimates of Impact on Strategic Permanent Exclusion in Year 11 in Pre-2010 Academies



Notes: Figure 3 shows school-specific IV estimates of the causal effect of academy enrolment on year 11 strategic permanent exclusion plotted against school-specific IV estimates of the causal effect of academy enrolment on Key-Stage 4 test score for Pre-2010 academies. A linear function fitting the distribution of school-specific IV estimates is also shown in Figure 3. For each Pre-2010 academy in the analysis, the horizontal axis shows the IV estimate of the causal effect of academy enrolment on year 11 strategic permanent exclusion, whereas the vertical axis shows school-specific IV estimates of the causal effect of academy enrolment on Key-Stage 4 test score.

Table 1. Pupils Permanently Excluded From Secondary School, School Years 2001/02 to 2014/15

	Year 7 to 11	Year 7	Year 8	Year 9	Year 10	Year 11
A. Any Permanent Exclusion						
Number	79868	6356	14835	23042	25898	9737
Share of Year 7 to 11	1.00	0.08	0.19	0.29	0.32	0.12
Percent Rate	0.197	0.079	0.184	0.283	0.315	0.121
B. Pre-January Census Permanent Exclusion						
Number	31853	999	4834	8124	10072	7554
Share of Year 7 to 11	1.00	0.03	0.15	0.26	0.32	0.24
Percent Rate	0.079	0.012	0.060	0.100	0.122	0.094

Notes: Table 1 shows the distribution of permanent exclusions across pupils in state-maintained secondary schools in England from 2001/02 to 2014/15. The total number of pupils that were permanently excluded in year 11 from 2001/02 to 2014/15 was calculated using the NPD data. Table 1 reports both the number of pupils that were permanently excluded at any time in the school year and the number of pupils that were excluded in the time window from the beginning of the school year to the January Census. The NPD data was also used to calculate the total number of pupils enrolled in state-maintained schools in England both at the aggregate and separately for each school year. The share of year 7 to 11 in each year shows the share of pupils excluded in a given school year out of the total number of permanently excluded pupils in the school years 7 to 11. For each school year, the Percent Rate shows the percent rate of pupils permanently excluded out of the total of the pupils enrolled in a given school year.

Table 2. Descriptive Determinants of Discipline Outcomes in Year 11

	Any Permanent Exclusion	Pre-January Census Permanent Exclusion	Temporary Exclusion	Fraction of Unauthorised Absence	Persistent Unauthorised Absence
	(1)	(2)	(3)	(4)	(5)
FSM Eligible	2.133***	2.235***	1.853***	1.966***	3.233***
	(0.053)	(0.062)	(0.009)	(0.005)	(0.021)
Male	3.769***	3.593***	2.500***	0.987***	0.896***
	(0.099)	(0.105)	(0.011)	(0.002)	(0.005)
English Native Speaker	2.018***	2.193***	1.863***	1.088***	2.347***
	(0.084)	(0.104)	(0.016)	(0.004)	(0.032)
KS2 Test Score	0.651***	0.639***	0.636***	0.690***	0.570***
	(0.006)	(0.007)	(0.001)	(0.001)	(0.002)
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes
Number of Pupils	7336508	7336508	5219629	5219629	5219629
Number of Schools	3008	3008	2949	2949	2949

Notes: Table 2 reports estimates of the determinants of behavioural outcomes for year 11 pupils. Odds ratios are reported in all columns. Robust standard errors are reported in parentheses. * indicates significance at 10 percent, ** indicates significance at 5 percent, *** indicates significance at 1 percent. Control variables included are dummies for whether the pupils are eligible for free school meals, whether they are male, native English speakers, the pupils' ethnicity group, entered together with KS2 test scores and a dummy variable for pupils for whom KS2 data is unavailable.

Table 3. Descriptive Determinants of Permanent Exclusion in Year 11 by School Type

	All Schools	Pre-2010 Academies	Post-2010 Academies	Post-2010 Sponsored Academies	Post-2010 Converter Academies	Future To Be Academies
	(1)	(2)	(3)	(4)	(5)	(6)
A. Any Permanent Exclusion						
FSM Eligible	2.133***	1.671***	2.365***	1.720***	2.464***	2.118***
	(0.053)	(0.166)	(0.201)	(0.330)	(0.233)	(0.057)
Male	3.769***	3.215***	3.967***	4.264***	3.904***	3.780***
	(0.099)	(0.358)	(0.354)	(0.981)	(0.378)	(0.107)
Native English Speaker	2.018***	2.161***	1.717***	1.698	1.803***	2.048***
	(0.084)	(0.345)	(0.244)	(0.553)	(0.287)	(0.093)
KS2 Test Score	0.651***	0.780***	0.592***	0.720***	0.580***	0.654***
	(0.006)	(0.037)	(0.019)	(0.062)	(0.021)	(0.007)
B. Pre-January Census Permanent Exclusion						
FSM Eligible	2.235***	1.743***	2.495***	1.633**	2.657***	2.217***
	(0.062)	(0.188)	(0.229)	(0.340)	(0.271)	(0.068)
Male	3.593***	3.556***	3.805***	4.212***	3.725***	3.565***
	(0.105)	(0.444)	(0.366)	(1.041)	(0.389)	(0.113)
Native English Speaker	2.193***	2.501***	1.630***	1.656	1.703***	2.252***
	(0.104)	(0.436)	(0.249)	(0.589)	(0.291)	(0.118)
KS2 Test Score	0.639***	0.794***	0.577***	0.700***	0.566***	0.641***
	(0.007)	(0.042)	(0.020)	(0.065)	(0.022)	(0.008)
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Number of Pupils	7336508	229453	839466	76256	763210	6267589
Number of Schools	3008	208	1375	205	1170	3008

Notes: Table 3 reports estimates of the determinants of permanent exclusion in year 11 by school type. In Panel A, the dependent variable is permanent exclusion at any time during year 11. In Panel B, the dependent variable is permanent exclusion before the January Census during year 11. Odds ratios are reported in all columns. Robust standard errors are reported in parentheses. * indicates significance at 10 percent, ** indicates significance at 5 percent, *** indicates significance at 1 percent. Control variables included are dummies for whether the pupils are eligible for free school meals, whether they are male, native English speakers, the pupils' ethnicity group, entered together with KS2 test scores and a dummy variable for pupils for whom KS2 data is unavailable.

Table 4. Academy Conversions By School Year

		All Schools With Full Data (Pre- and Post-Academy Conversion)									
		All	2002/03	2003/04	2004/05	2005/06	2006/07	2007/08	2008/09	2009/10	2010/11
A. Pre-2010 Academies	Treatment Academies, Convert in Labour Years	94	3	6	2	7	14	25	37	0	0
	Control To Be Academies, Convert in 2009/10 to 2010/11	114	0	0	0	0	0	0	0	58	56
		All			2010/11	2011/12	2012/13	2013/14	2014/15	2015/16	2016/17
B. Post-2010 Sponsored Academies	Treatment Academies, Convert in 2010/11 to 2013/14	157			0	30	58	69	0	0	0
	Control To Be Academies, Convert in 2014/15 to 2016/17	97			0	0	0	0	48	44	5
		All			2010/11	2011/12	2012/13	2013/14	2014/15	2015/16	2016/17
C. Post-2010 Converter Academies	Treatment Academies, Convert in 2010/11 to 2013/14	1116			64	640	303	109	0	0	0
	Control To Be Academies, Convert in 2014/15 to 2016/17	104			0	0	0	0	54	42	8

Notes: Table 4 shows the timeline of academy conversion of the secondary schools in England that were included in our analysis. The timeline is presented separately for pre-2010 academies, post-2010 sponsored academies and post-2010 converter academies.

Source is https://www.gov.uk/government/uploads/system/uploads/attachment_data/file/175360/academies_annual_report_2010-11.pdf.

Table 5. Difference-in-Differences Before and After Academy Conversion, Any Permanent Exclusion in Year 11

	Any Permanent Exclusion in Year 11 (X 100)			
	Academies	Future To Be Academies	Academies - Future To Be Academies	Unconditional DiD Estimate
	(1)	(2)	(3) = (1) – (2)	(4)
A. Pre-2010 Academies				
Pre-Period	0.184	0.208	-0.024	DiD =
Post-Period	0.348	0.208	0.140	0.164**
Post - Pre	0.164	0.000		(0.064)
Number of Schools	94	114	208	
B. Post-2010 Academies				
Pre-Period	0.092	0.082	0.010	DiD =
Post-Period	0.087	0.070	0.017	0.007
Post - Pre	-0.005	-0.012		(0.012)
Number of Schools	1273	201	1474	
C. Post-2010 Sponsored Academies				
Pre-Period	0.137	0.113	0.024	DiD =
Post-Period	0.144	0.105	0.039	0.015
Post - Pre	0.007	-0.008		(0.036)
Number of Schools	157	97	254	
D. Post-2010 Converter Academies				
Pre-Period	0.086	0.062	0.024	DiD =
Post-Period	0.082	0.050	0.032	0.008
Post - Pre	-0.004	-0.012		(0.011)
Number of Schools	1116	104	1220	

Notes: Table 5 shows the results of an unconditional difference-in-differences exercise comparing year 11 permanent exclusion three years before and after academy conversion in treatment group schools and control group schools. Panel A shows the result of this exercise for Pre-2010 academies. Panel B shows the result of this exercise for Post-2010 academies. Panel C shows the result of this exercise for Post-2010 sponsored academies and Panel D shows the result of this exercise for Post-2010 converter academies. Robust standard errors clustered at the school level are reported in parentheses. * indicates significance at 10 percent, ** indicates significance at 5 percent, *** indicates significance at 1 percent.

Table 6. Fraction of Pupils Sitting Exams in Excluding Schools

	Academies and Non-Academies		Academies and Future To Be Academies	
	(1)	(2)	(3)	(4)
	Number of Year 11 Permanent Exclusions	Share Sitting Exam in Excluding School	Number of Year 11 Permanent Exclusions	Share Sitting Exam in Excluding School
A. Pre-2010 Academies				
All	6276	0.245	713	0.231
Pre-January Census	4771	0.110	536	0.084
Post-January Census	1505	0.671	177	0.678
B. Post-2010 Academies				
All	5564	0.219	2524	0.227
Pre-January Census	4442	0.102	2013	0.105
Post-January Census	1122	0.682	511	0.710
C. Post-2010 Sponsored Academies				
All	5274	0.220	591	0.208
Pre-January Census	4189	0.100	470	0.106
Post-January Census	1085	0.682	121	0.603
D. Post-2010 Converter Academies				
All	5363	0.218	1933	0.233
Pre-January Census	4289	0.102	1543	0.104
Post-January Census	1074	0.684	390	0.741

Notes: Table 6 shows the fraction of permanently excluded pupils that sit exams in the excluding school by period of permanent exclusion in year 11. The total number of pupils that were permanently excluded in year 11 from 2001/2 to 2014/15 was calculated using the NPD data. Table 6 reports the number of pupils that were permanently excluded at any time in the school year, the number of pupils that were excluded in the time window from the beginning of the school year to the January Census, and the number of pupils that were excluded after the January Census. Since the NPD data reports the exact school premises where the GCSE exams were sat by each pupil, the NPD data was also used to calculate the share of pupils that were permanently excluded and that sat the GCSE exams in the excluding school.

Table 7. Difference-in-Differences Before and After Academy Conversion, Strategic Permanent Exclusion in Year 11

	Strategic Permanent Exclusion in Year 11 (X 100)			
	Academies	Future To Be Academies	Academies - Future To Be Academies	Unconditional DiD Estimate
	(1)	(2)	(3) = (1) – (2)	(4)
A. Pre-2010 Academies				
Pre-Period	0.157	0.154	0.003	DiD =
Post-Period	0.272	0.167	0.105	0.102*
Post - Pre	0.115	0.013		(0.059)
Number of Schools	94	114	208	
B. Post-2010 Academies				
Pre-Period	0.073	0.069	0.004	DiD =
Post-Period	0.071	0.054	0.017	0.013
Post - Pre	-0.002	-0.015		(0.010)
Number of Schools	1273	201	1474	
C. Post-2010 Sponsored Academies				
Pre-Period	0.113	0.091	0.022	DiD =
Post-Period	0.118	0.083	0.035	0.013
Post - Pre	0.005	-0.008		(0.031)
Number of Schools	157	97	254	
D. Post-2010 Converter Academies				
Pre-Period	0.068	0.055	0.013	DiD =
Post-Period	0.067	0.037	0.030	0.017*
Post - Pre	-0.001	-0.018		(0.009)
Number of Schools	1116	104	1220	

Notes: Table 7 shows the results of an unconditional difference-in-differences exercise comparing year 11 strategic permanent exclusion three years before and after academy conversion in treatment group schools and control group schools. Panel A shows the result of this exercise for Pre-2010 academies. Panel B shows the result of this exercise for Post-2010 academies. Panel C shows the result of this exercise for Post-2010 sponsored academies and Panel D shows the result of this exercise for Post-2010 converter academies. Robust standard errors clustered at the school level are reported in parentheses. * indicates significance at 10 percent, ** indicates significance at 5 percent, *** indicates significance at 1 percent.

Table 8. Estimates of Academy Impact on Permanent Exclusion in Year 11

	Any Permanent Exclusion in Year 11 (X 100)			Strategic Permanent Exclusion in Year 11 (X 100)		
Academies vs Control Schools						
	OLS	ITT	IV	OLS	ITT	IV
	(1)	(2)	(3)	(4)	(5)	(6)
Academy	0.053***	0.088***	0.091***	0.051***	0.080***	0.083***
	(0.017)	(0.017)	(0.018)	(0.015)	(0.016)	(0.016)
Control Variables	Yes	Yes	Yes	Yes	Yes	Yes
School Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Number of Observations	4677340	4677340	4677340	4677340	4677340	4677340
Number of Schools	1682	1682	1682	1682	1682	1682
First Stage Coefficient on ITT			0.965***			0.965***
			(0.001)			(0.001)

Notes: Table 8 shows estimates of the causal effect of academy enrolment on year 11 permanent exclusion and on year 11 strategic permanent exclusion for all academies. Robust standard errors (clustered at the school level) are reported in parentheses. Control variables included are dummies for whether the pupils are eligible for free school meals, whether they are male, native English speakers, the pupils' ethnicity group and a proxy for pre-determined school-level behaviour in the ITT school where the pupils pre-enrolled, entered together with KS2 test scores and a dummy variable for pupils for whom KS2 data is unavailable. * indicates significance at 10 percent, ** indicates significance at 5 percent, *** indicates significance at 1 percent.

Table 9. Estimates of Academy Impact on Any Permanent Exclusion in Year 11

	Any Permanent Exclusion in Year 11 (X 100)											
Academies vs Control Schools	Pre-2010 Academies			Post-2010 Academies			Post-2010 Sponsored Academies			Post-2010 Converter Academies		
	OLS	ITT	IV	OLS	ITT	IV	OLS	ITT	IV	OLS	ITT	IV
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Academy	0.303***	0.319***	0.343***	0.015	0.051***	0.052***	0.038	0.059	0.062	0.011	0.050***	0.051***
	(0.084)	(0.084)	(0.090)	(0.014)	(0.014)	(0.014)	(0.038)	(0.038)	(0.040)	(0.014)	(0.014)	(0.014)
Control Variables	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
School Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Number of Observations	1304990	1304990	1304990	3372350	3372350	3372350	746328	746328	746328	2625995	2625995	2625995
Number of Schools	208	208	208	1474	1474	1474	254	254	254	1220	1220	1220
First Stage Coefficient on ITT			0.929***			0.971***			0.946***			0.978***
			(0.004)			(0.001)			(0.246)			(0.065)

Notes: Table 9 shows estimates of the causal effect of academy enrolment on year 11 permanent exclusion separately by group of academies. Robust standard errors (clustered at the school level) are reported in parentheses. Control variables included are dummies for whether the pupils are eligible for free school meals, whether they are male, native English speakers, the pupils' ethnicity group and a proxy for pre-determined school-level behaviour in the ITT school where the pupils pre-enrolled, entered together with KS2 test scores and a dummy variable for pupils for whom KS2 data is unavailable. * indicates significance at 10 percent, ** indicates significance at 5 percent, *** indicates significance at 1 percent.

Table 10. Estimates of Academy Impact on Strategic Permanent Exclusion in Year 11

	Strategic Permanent Exclusion in Year 11 (X 100)											
	Pre-2010 Academies			Post-2010 Academies			Post-2010 Sponsored Academies			Post-2010 Converter Academies		
	OLS	ITT	IV	OLS	ITT	IV	OLS	ITT	IV	OLS	ITT	IV
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Academy	0.260***	0.262***	0.282***	0.020*	0.051***	0.052***	0.034	0.051	0.054	0.018	0.052***	0.053***
	(0.081)	(0.081)	(0.087)	(0.012)	(0.012)	(0.012)	(0.035)	(0.035)	(0.037)	(0.012)	(0.012)	(0.012)
Control Variables	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
School Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Number of Observations	1304990	1304990	1304990	3372350	3372350	3372350	746328	746328	746328	2625995	2625995	2625995
Number of Schools	208	208	208	1474	1474	1474	254	254	254	1220	1220	1220
First Stage Coefficient on ITT			0.929***			0.971***			0.946***			0.978***
			(0.004)			(0.001)			(0.246)			(0.065)

Notes: Table 10 shows estimates of the causal effect of academy enrolment on year 11 strategic permanent exclusion separately by group of academies. Robust standard errors (clustered at the school level) are reported in parentheses. Control variables included are dummies for whether the pupils are eligible for free school meals, whether they are male, native English speakers, the pupils' ethnicity group and a proxy for pre-determined school-level behaviour in the ITT school where the pupils pre-enrolled, entered together with KS2 test scores and a dummy variable for pupils for whom KS2 data is unavailable. * indicates significance at 10 percent, ** indicates significance at 5 percent, *** indicates significance at 1 percent.

Table 11. Estimates of Academy Impact on Key Stage 4 Test Scores

	Key Stage 4 Standardised Test Scores, End of Secondary School					
	Pre-2010 Academies			Post-2010 Academies		
	OLS	ITT	IV	OLS	ITT	IV
	(1)	(2)	(3)	(4)	(5)	(6)
Academy	0.145***	0.097***	0.104***	-0.008	-0.004	-0.004
	(0.028)	(0.028)	(0.030)	(0.013)	(0.013)	(0.014)
Control Variables	Yes	Yes	Yes	Yes	Yes	Yes
School Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Number of Observations	1304990	1304990	1304990	3372350	3372350	3372350
Number of Schools	208	208	208	1474	1474	1474
First Stage Coefficient on ITT			0.927***			0.970***
			(0.004)			(0.001)

Notes: Table 11 shows estimates of the causal effect of academy enrolment on standardised Key-Stage 4 test score separately by group of academies. Robust standard errors (clustered at the school level) are reported in parentheses. Control variables included are dummies for whether the pupils are eligible for free school meals, whether they are male, native English speakers, the pupils' ethnicity group, entered together with KS2 test scores and a dummy variable for pupils for whom KS2 data is unavailable. * indicates significance at 10 percent, ** indicates significance at 5 percent, *** indicates significance at 1 percent.

Table 12. Simulation Estimates of Academy Impact on Key Stage 4 Test Scores for Pre-2010 Academies

	Pre-2010 Academy Impact on Key Stage 4 Standardised Test Scores											
Pre-2010 Academies	Original IV	Only Reinstating Excluded Pupils with Worst Possible KS4 Score	(2) + Remove Top KS4 Performers	(3) + 5% Negative Peer Effect	(3) + 10% Negative Peer Effect	(3) + 20% Negative Peer Effect	(3) + 25% Negative Peer Effect	(3) + 30% Negative Peer Effect	(3) + 35% Negative Peer Effect	(3) + 40% Negative Peer Effect	(3) + 45% Negative Peer Effect	(3) + 50% Negative Peer Effect
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(l2)
Academy	0.104***	0.103***	0.102***	0.097***	0.091***	0.080**	0.074**	0.069*	0.063	0.058	0.052	0.046
	(0.030)	(0.030)	(0.030)	(0.030)	(0.031)	(0.034)	(0.036)	(0.039)	(0.042)	(0.044)	(0.048)	(0.051)
Control Variables	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
School Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Table 12 shows simulation IV estimates of the causal effect of academy enrolment on standardised Key Stage 4 test score for Pre-2010 academies. Column (1) reports the result in column (3) of Table 11 for purpose of comparison. Column (2) shows the IV estimate of the academy impact on KS4 outcome when strategically excluded pupils are reinstated in the excluding school with the worst possible KS4 outcome. Column (3) shows the IV estimate of the academy impact on KS4 outcome when, in addition to the exercise in column (2), for a given number of excluded pupils in school *s* in year *t*, an equivalent number of Top KS4 performers is removed from the analysis. Columns (4) to (12) show the IV estimates of the academy impact on KS4 outcome when, in addition to the exercise in column (3), an increasingly negative peer effect of each excluded pupil on all other pupils in school *s* and year *t* is assumed. Robust standard errors (clustered at the school level) are reported in parentheses. Control variables included are dummies for whether the pupils are eligible for free school meals, whether they are male, native English speakers, the pupils' ethnicity group, entered together with KS2 test scores and a dummy variable for pupils for whom KS2 data is unavailable. * indicates significance at 10 percent, ** indicates significance at 5 percent, *** indicates significance at 1 percent.

Table 13. Further Empirical Tests for Pre-2010 Academies

	Strategic Permanent Exclusion in Year 11 (X 100)	Key Stage 2 Standardised Test Scores, End of Primary School, Strategic Permanent Excluded	Non-Strategic Permanent Exclusion in Year 11 (X 100)
		Pre-2010 Academies	
	IV	OLS	IV
	(1)	(2)	(3)
Academy	0.283***	0.003	0.061**
	(0.086)	(0.258)	(0.030)
Academy x Key-Stage 2	0.016		
	(0.034)		
Control Variables	Yes	Yes	Yes
School Fixed Effects	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes
Number of Observations	1304990	2023	1304990
Number of Schools	208	156	208
First Stage Coefficient on ITT	0.929***		0.929***
	(0.004)		(0.004)
First Stage Coefficient on ITT x Key Stage 2	-0.008***		
	(0.002)		

Notes: Column (1) of Table 13 shows IV estimates of the causal effect of academy enrolment interacted with prior Key-Stage 2 test score on year 11 strategic permanent exclusion for pre-2010 academies. Column (2) of Table 13 shows OLS estimates of the causal effect of academy enrolment on prior Key-Stage 2 test score for pupils that were strategically excluded in year 11 in pre-2010 academies. Column (3) of Table 13 shows estimates of the causal effect of academy enrolment on year 11 non-strategic permanent exclusion for pre-2010 academies. Robust standard errors (clustered at the school level) are reported in parentheses. In Columns (1) and (3), control variables included are dummies for whether the pupils are eligible for free school meals, whether they are male, native English speakers, the pupils' ethnicity group and a proxy for pre-determined school-level behaviour in the ITT school where the pupils pre-enrolled, entered together with KS2 test scores and a dummy variable for pupils for whom KS2 data is unavailable. In Column (2), control variables included are dummies for whether the pupils are eligible for free school meals, whether they are male, native English speakers and the pupils' ethnicity group. * indicates significance at 10 percent, ** indicates significance at 5 percent, *** indicates significance at 1 percent.

Appendix

Data Description

1. Data on Academy Schools

For the analysis of the pre-2010 academies, we detected all schools that acquired academy status over the school years 2002/03 to 2010/11. For the analysis of the post-2010 academies, we detected all schools that acquired academy status over the school years 2010/11 to 2016/17. In order to do this, we used Department for Education (DfE) extracts that contain information on all academies that have converted or that are undergoing the process of academy conversion. The DfE extracts contain information on the opening date of the academy, its URN (a unique identifier for the school that enables us to identify it in several governmental data sources such as the National Pupil Database and the Pupil Level Annual Census data), the URN number of the predecessor school, the LAEstab number of the academy (an additional unique identifier that combines local authority specific and school specific numbers) and the LAEstab number of the predecessor school.

For the analysis of pre-2010 academies, we used performance tables data from the Department for Education (DfE) to match in predecessor school types. In order to have a balanced panel of schools with KS4 results since 2002, we focused on academies that have some kind of predecessor school open since at least 1997 on. We identified 244 schools that became academies from the first 3 academy openings in 2002/03 to those that became academies by September 2010 (the beginning of the academic school year). For the analysis of Post-2010 academies, we used the information available online from the DfE's Edubase to identify post-2010 academies that had a predecessor open since at least 2002 (this is a Department For Education database of all open and closed maintained schools in England; see http://www.education.gov.uk/edubase/home.xhtml). For the analysis of both pre- and post-2010 academies, in order for our sample to be balanced for intake, we included in our analysis only academies that enrol pupils in year 7, i.e., at the beginning of secondary school. In all cases, we did not include in our analysis brand new academy schools as they have no predecessor school. Likewise, we did not include in our analysis previously independent schools, since pupils in these schools do not have exam information at KS4.

For the analysis of pre-2010 academies, the final sample contains 106 (of which we use the 94 who were not CTCs in their predecessor state) treatment schools (those that opened as academies prior to, or in, September 2008) and 114 control schools with observations ranging over the years 2001/02-2008/09. None of our control schools become academies during these sample years, but all converted by September 2010. For the analysis of post-2010 academies, the final sample contains 254 post-2010 sponsored academies and 1220 post-2010 converter academies. For the study of post-2010 sponsored academies, we define a sample of 157 treatment schools that opened by December 2013 (i.e., by the academic year 2013/14), and a group of 97 control schools that opened after December 2013 but were either open or in the pipeline for opening by the spring 2016. Similarly for the study of post-2010 converter academies, we define a sample of 1116 treatment schools that opened by December 2013 (i.e., by the academic year 2013/14), and a group of 104 control schools that opened after December 2013 but were either open or in the pipeline for opening by the spring 2016.

2. Pupil Level Data

We use data from PLASC (pupil level annual schools census) and the NPD (national pupil database) from the school years 2001/2 to 2013/14. The NPD reports data on all key stage 2 (KS2) and key stage 4 (KS4) exams sat at the end of primary and secondary school respectively. We could identify each pupil across datasets using a unique reference number. The data also contains a unique LAEstab code of the school in which the pupil sat the exam: this is a key piece of information that we use to identify pupils that were excluded in year 11 and that ended up sitting exams in a different school. While the NPD contains data on pupils in examination years, PLASC contains a record for every pupil for each year that they are in the maintained school sector. PLASC data gives the pupil, year group and school as well as demographic variables such as ethnicity, gender, free school meal eligibility and special educational needs status. We can track pupils through secondary school using the unique pupil identifier. This identifier is common to the NPD enabling us to merge NPD and PLASC data. This gives a panel of pupils with their demographic information, their KS2 and KS4 test results and the school(s) that they attended from year 7 (first year of compulsory secondary education) through to year 11 (final year of compulsory education). For the analysis of pre-2010 academies, we identified the pupils who attended the 208 treatment and control pre-2010 academies at some point from the school years 2001/2 to 2008/9, and we used information on the schools pupils attended in every secondary compulsory year of schooling[28], their demographic information and their exams results at KS4 and KS2. Our analysis focuses on those who are ITT in one of our 94 treatment schools or sit exams in one of our 208 control schools from the school years 2001/2 to 2008/9. For the analysis of post-2010 sponsored academies, we identified the pupils who attended the 254 treatment and control post-2010 sponsored academies from the school years 2005/6 to 2013/4. For the analysis of the post-2010 converter academies, we identified the pupils who attended the 1220 treatment and control post-2010 converter academies from the school years 2005/6 to 2013/4. For the analysis of post-2010 sponsored academies, our analysis focuses on those who are ITT or sit exams in one of our 254 sample schools from the school years 2005/6 to 2013/14. Similarly, for the analysis of post-2010 converter academies, our analysis focuses on those who are ITT or sit exams in one of our 1220 sample schools from the school years 2005/6 to 2013/14. We adopt the same empirical approach for the analysis of pre-2010 and post-2010 academies respectively.

3. Notes on Clustering

For both pre-2010 and post-2010 academies, for each of our treatment and control schools we assign a unique number. It is possible that two pupils from different schools are given the same number should the two differing schools later become the same academy. We identify when schools merge by looking at linked schools in DfE's Edubase. For pre-2010 academies, in one case a single school becomes two separate academies (North Westminster Community School splits into Paddington Academy and Westminster Academy in 2006). Pupils attending the predecessor school are randomly assigned one of the two numbers given to the two academies that open later. In all the analysis, pupils who leave the sample but are ITT or receive exposure are given a unique number equal to the school where they reach year 11. In estimated specifications, standard errors are clustered on this unique number. This results

[28] Strictly speaking this is not true. Some pupils enter the schooling system either from another country or from independent schools. We observe when the pupils enter but not precisely where they came from. These pupils are retained in our analysis.

in 208 clusters for the analysis of pre-2010 academies, in 254 clusters for the analysis of post-2010 sponsored academies, and in 1220 clusters for the analysis of post-2010 converter academies.

4. Outcome Variables and Attainment Measures

The main outcome variable is permanent exclusion in year 11. The NPD contains all records of permanent exclusions from state-maintained schools in England. To be precise, pupil-level information on permanent exclusion is available since the academic year 2001/2, thus covering the entire period of our analysis. Information on temporary and lunchtime exclusion is available from the academic year 2005/6, thus covering the entire period of our analysis for post-2010 academies, but not for pre-2010 academies (i.e., we are unable to assess the impact of academy enrolment for the earliest 18 academies in our sample, that opened from 2002/3 to 2005/6).

For each exclusion, the NPD reports the date of commencement of the exclusion, the reason for the exclusion, and the URN of the school from which the pupil was excluded. Data on temporary exclusion also includes information on the duration of the temporary exclusion. Although the data on exclusion do not report the school year of the excluded pupil, they contain the unique pupil identifier common to all NPD and PLASC datasets. We used this to combine pupil-level information on exclusion with information from PLASC and other NPD datasets. Although we used PLASC data collected in the January census, we were able to identify the schools that excluded pupils prior to the January census by comparing the information on the excluding school in the exclusion data with the school in which a pupil appeared in the January census.

We also model the impact of academy enrolment on unauthorised absence from school. Our dependent variable in this case is calculated as the fraction of sessions missed by a pupil without authorization during the school year in year 11. We also model the impact of academy enrolment on the likelihood to be "persistently absent". This is defined as a pupil that missed at least 20 percent of sessions during the school year in year 11. Even though the 2010 Coalition government changed the official definition of persistent truant in school, we keep this definition constant over the entire analysis of pre-2010 and post-2010 academies[29].

We also look at the KS4 impact of academy enrolment. For this, we follow closely what Eyles and Machin (2015) do. The main KS4 qualification in the UK is the GCSE (General Certificate of Secondary Education). GCSEs are graded from A*-G. The current points score calculations give an A* a score of 58 and a G a score of 16 with grades in between going up in increments of 6 between adjacent grades as follows:

[29] See, e.g., https://www.gov.uk/government/news/persistent-absence-government-changes-definition-to-deal-with-reality-of-pupil-absenteeism-in-schools

Grade	Points	Grade	Points
A*	58	D	34
A	52	E	28
B	46	F	22
C	40	G	16

New scale

Prior to this an A* was given a score of 8 and G a score of 1 with scores rising in unit increments.

Grade	Points	Grade	Points
A*	8	D	4
A	7	E	3
B	6	F	2
C	5	G	1

Old scale

As well as GCSEs there are a wide range of equivalent qualifications focusing on more vocational subjects. These include GNVQs and BTecs. Depending upon the type of equivalent these are often worth multiple GCSEs and are often graded as a combination of GCSE grades i.e. a distinction in an intermediate GNVQ is equivalent to gaining two GCSEs with one at grade A and the other at grade A*.[30] The points score given to the qualification reflects the underlying GCSE grades that it is based upon so that under the new scoring system the aforementioned qualification would be given a score of 110. The points system we use is as follows:

Grade	Points	Grade	Points
A*	10	D	4
A	8	E	3
B	7	F	2
C	6	G	1

Scale used in the paper

[30] Most equivalents are graded as pass, merit or distinction but the Department for Education equates these categories, combinations of, A*-G grades.

The points system we use addresses some of the concerns expressed pertaining to the 16-58 and 1-8 scales used over the course of our sample.[31] The non-linearity reflects the fact that it appears hardest to jump from grades D to C and from A to A*.

We cap points scores at best 8 qualifications. To do this we normalize raw point scores by their GCSE equivalent i.e. a qualification worth 4 GSCEs and 208 points (under the 16-58 scale) is normalized to be worth 52 points. We then convert these points to our new measure and rank them highest to lowest. We then add up the grade weightings (in terms of GCSEs), taking fractions of qualifications if need be, until we reach 8. All those in the top 8 are then multiplied through by their weight and summed to give the points score.

Our decision to cap at 8 is motivated by two concerns. Total points scores have the problem that pupils can appear to do well by entering many exams and performing poorly in them. Similarly using, for instance, 5 best means that those who focus very narrowly on a small set of exams may appear better than those who perform well over a larger selection of subjects/qualifications. Our decision to cap at 8 balances these two concerns.

Finally, it is worth noting that our point measures create some notable discrepancies with the official method. For instance, an equivalent qualification worth two GCSEs graded CD is worth 74 points under the 16-58 scale meaning that it is worth more than a A* at GCSE. Using our system such a qualification is worth 10 points (the sum of the points scores for grades of C and D) – the equivalent of a GCSE at grade A*. A further example is a BTEC that is worth 76 points on the old scale and equivalent to 4 GCSEs. This is the same as achieving grades of 2 Fs and 2 Gs. In our system this is equivalent to a point score of 6. Thus our points mean the qualification is the same as getting a C at GCSE whereas the old measure means that the qualification is again worth more than an A*. In general our system reduces the relative points scores of equivalent qualifications compared to the official method. Despite this our results remain unchanged when using the (standardized) old (1-8) and new (16-58) points systems and when using total rather than capped scores.

[31] We are grateful to Tim Leunig and Mike Treadaway for very helpful correspondence on this.

Selected Examples From the Media

s://www.tes.com/news/school-news/breaking-news/academies-move-difficult-pupils-other-schools-boost-their-results

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Academies move difficult pupils to other schools to boost their results, study suggests

Richard Vaughan
13th November 2015 at 00:04

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GCSE Behaviour Accountability

EXCLUSIVE

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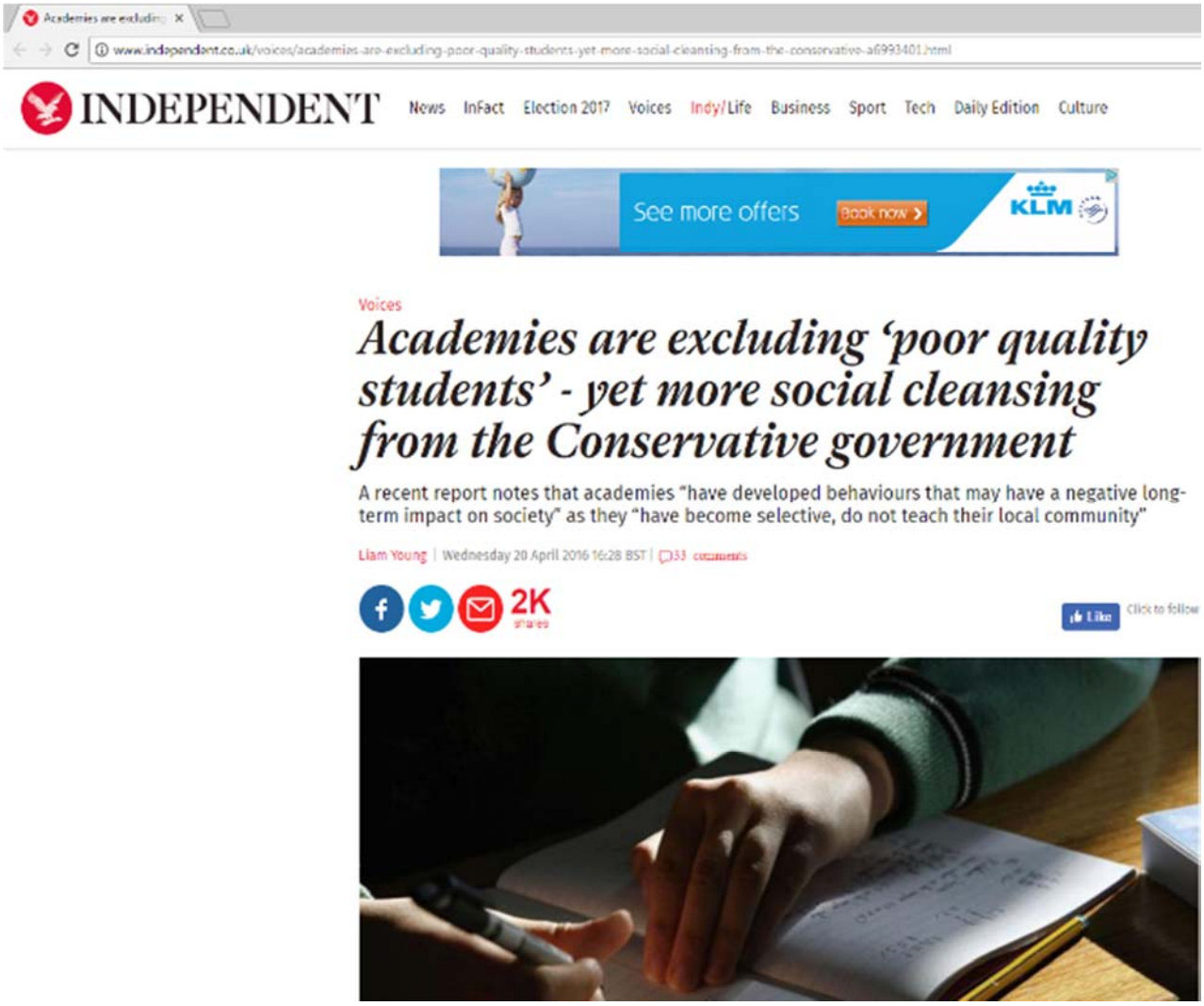

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League tables 'mislead on school success'

By Marc Ashdown
BBC London News

31 January 2017 | London

Share

League tables are currently worked out from how many children were on-roll in January of their final year

Some schools would have performed much worse than official league tables show if "churn rates" had been taken into account, it has been claimed.

ools May Have Higher Test Scores – Mother Jones

-students-disabilities-test-scores/

The Disturbing Reason Why Some Charter Schools May Have Higher Test Scores

It's devastating for black students and children with special needs.

KRISTINA RIZGA MAR. 18, 2016 10:00 AM

Kindergarten teacher and students at KIPP Thrive Academy, a new school in what had been the closed Eighteenth Avenue School in Newark, New Jersey. AP Photo/Mel Evans

Additional Tables

Fractions of Pupils that Sit Exams in Excluding Schools

Table A1. Fraction of Pupils that Sit Exams in the Excluding School by Month of Permanent Exclusion

Month of Permanent Exclusion in Year 11	**Pre-2010 Academies and All Schools**		**Post-2010 Academies and All Schools**		**Post-2010 Sponsored Academies and All Schools**		**Post-2010 Converter Academies and All Schools**	
	Share who Sit Exams in Excluding School	Number of Permanent Exclusions	Share who Sit Exams in Excluding School	Number of Permanent Exclusions	Share who Sit Exams in Excluding School	Number of Permanent Exclusions	Share who Sit Exams in Excluding School	Number of Permanent Exclusions
September	0.030	1,079	0.045	1,016	0.046	949	0.044	979
October	0.048	1,118	0.059	1,077	0.057	1,019	0.060	1,044
November	0.106	1,363	0.099	1,328	0.094	1,262	0.099	1,277
December	0.197	802	0.172	699	0.169	650	0.167	681
January	0.421	801	0.395	582	0.391	562	0.395	552
February	0.651	484	0.622	410	0.623	398	0.629	399
March	0.788	392	0.795	308	0.8	295	0.796	289
April	0.782	110	0.853	68	0.846	65	0.853	68
May	0.873	63	0.947	57	0.945	55	0.945	55
June	0.789	38	0.938	16	0.938	16	0.938	16
July	0.615	26	0.333	3	0.333	3	0.333	3
Total	0.245	6,276	0.219	5,564	0.220	5,274	0.218	5,363

Notes: Table A1 shows the fraction of pupils in our treatment schools and in all other schools that sit GCSE exams in the excluding school by month of permanent exclusion in year 11.

Table A2. Fraction of Pupils that Sit Exams in the Excluding School by Month of Permanent Exclusion

Month of Permanent Exclusion in Year 11	**Pre-2010 Academies and Control Schools**		**Post-2010 Academies and Control Schools**		**Post-2010 Sponsored Academies and Control Schools**		**Post-2010 Converter Academies and Control Schools**	
	Share who Sit Exams in Excluding School	Number of Permanent Exclusions	Share who Sit Exams in Excluding School	Number of Permanent Exclusions	Share who Sit Exams in Excluding School	Number of Permanent Exclusions	Share who Sit Exams in Excluding School	Number of Permanent Exclusions
September	0.015	130	0.044	477	0.067	105	0.038	372
October	0.030	133	0.066	487	0.073	123	0.063	364
November	0.107	131	0.097	609	0.106	141	0.094	468
December	0.173	104	0.177	284	0.152	66	0.183	218
January	0.418	91	0.410	278	0.300	70	0.447	208
February	0.667	63	0.686	188	0.667	45	0.692	143
March	0.773	44	0.818	138	0.794	34	0.817	104
April	0.75	8	0.875	32	0.667	3	0.897	29
May	1	3	0.95	20	0	1	1	19
June	0.75	4	0.9	10	1	2	0.875	8
July	0.5	2	0	1	0	1	-	0
Total	0.231	713	0.227	2524	0.208	591	0.234	1,933

Notes: Table A2 shows the fraction of pupils in our treatment and control schools that sit GCSE exams in the excluding school by month of permanent exclusion in year 11.

Balancing Tests

Table B1. Pre-Conversion Characteristics and Tests of Balancing for Pre-2010 Academies

	Percent eligible for free school meals	Percent male	Percent white	Percent incidence of permanent exclusion (X 100)	Percent incidence of strategic permanent exclusion (X 100)	Key stage 2 points score (mean)	Number of schools
All Schools	0.137	0.506	0.829	0.070	0.037	63.457	3003
All Pre-2010 Academies[32]	0.240	0.524	0.773	0.123	0.051	56.807	220
Current academies (treatment group)	0.263	0.536	0.730	0.118	0.031	55.408	94
Future academies (control group)	0.232	0.515	0.812	0.135	0.070	56.480	114
Difference	0.031 (0.019)	0.021 (0.015)	-0.082* (0.040)	-0.017 (0.056)	-0.039 (0.027)	-1.072 (0.798)	

Notes: Standard errors clustered at school level reported in parentheses. Both panels refer to characteristics in the 2001/02 school year. The top panel is maintained schools in the UK, which do not convert to academies prior to, or in, the academic year 2010/11. All variables refer to characteristics of the year 11 cohort in 2001/02 i.e. year 11 pupils in the school year 2001/02, before any academies had opened. * denotes significance at the 5% level, ** at the 1% level.

[32] This includes 12 conversions from City Technology Colleges (CTCs) that, similarly to Eyles and Machin (2015), we do not include in the regression analysis. This is because these schools were already very autonomous and they were performing well prior to conversion to academy status (Eyles and Machin 2015).

Table B2. Pre-Conversion Characteristics and Tests of Balancing for Post-2010 Academies

	Percent eligible for free school meals	Percent male	Percent white	Percent incidence of permanent exclusion (X 100)	Percent incidence of strategic permanent exclusion (X 100)	Key stage 2 points score (mean)	Number of schools
All Schools	0.140	0.506	0.776	0.116	0.093	64.443	1371
Post-2010 Academies (Pre-conversion)	0.107	0.505	0.817	0.097	0.071	66.368	1474
Current academies (treatment group)	0.102	0.503	0.816	0.098	0.071	66.716	1273
Future academies (control group)	0.141	0.516	0.820	0.088	0.070	64.196	201
Difference	-0.039** (0.009)	-0.013 (0.010)	-0.004 (0.019)	0.010 (0.019)	0.001 (0.018)	2.520** (0.346)	

Notes: Standard errors clustered at school level reported in parentheses. Both panels refer to characteristics in the 2009/10 school year. The top panel is maintained schools in the UK, which do not convert to academies prior to, or in, the academic year 2016/17. All variables refer to characteristics of the year 11 cohort in 2009/10 i.e. year 11 pupils in the school year 2009/10, before any academies had opened. * denotes significance at the 5% level, ** at the 1% level.

Table B3. Pre-Conversion Characteristics and Tests of Balancing for Post-2010 Sponsored Academies

	Percent eligible for free school meals	Percent male	Percent white	Percent incidence of permanent exclusion (X 100)	Percent incidence of strategic permanent exclusion (X 100)	Key stage 2 points score (mean)	Number of schools
All Schools	0.117	0.504	0.794	0.102	0.078	65.833	2591
Post-2010 Sponsored Academies (Pre-conversion)	0.180	0.522	0.835	0.146	0.114	61.568	254
Current academies (treatment group)	0.181	0.520	0.840	0.152	0.124	60.886	157
Future academies (control group)	0.178	0.525	0.828	0.136	0.098	62.627	97
Difference	0.003 (0.016)	-0.005 (0.013)	0.012 (0.030)	0.016 (0.046)	0.026 (0.041)	-1.742** (0.451)	

Notes: Standard errors clustered at school level reported in parentheses. Both panels refer to characteristics in the 2009/10 school year. The top panel is maintained schools in the UK, which do not convert to academies prior to, or in, the academic year 2016/17. All variables refer to characteristics of the year 11 cohort in 2009/10 i.e. year 11 pupils in the school year 2009/10, before any academies had opened. * denotes significance at the 5% level, ** at the 1% level.

Table B4. Pre-Conversion Characteristics and Tests of Balancing for Post-2010 Converter Academies

	Percent eligible for free school meals	Percent male	Percent white	Percent incidence of permanent exclusion (X 100)	Percent incidence of strategic permanent exclusion (X 100)	Key stage 2 points score (mean)	Number of schools
All Schools	0.147	0.508	0.786	0.120	0.096	63.994	1625
Post-2010 Converter Academies (Pre-conversion)	0.092	0.501	0.813	0.087	0.062	67.320	1220
Current academies (treatment group)	0.091	0.501	0.812	0.091	0.064	67.488	1116
Future academies (control group)	0.108	0.508	0.813	0.044	0.044	65.581	104
Difference	-0.017 (0.010)	-0.007 (0.016)	-0.001 (0.025)	0.047** (0.016)	0.020 (0.016)	1.907** (0.473)	

Notes: Standard errors clustered at school level reported in parentheses. Both panels refer to characteristics in the 2009/10 school year. The top panel is maintained schools in the UK, which do not convert to academies prior to, or in, the academic year 2016/17. All variables refer to characteristics of the year 11 cohort in 2009/10 i.e. year 11 pupils in the school year 2009/10, before any academies had opened. * denotes significance at the 5% level, ** at the 1% level.

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