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ORIGINAL PAPER

School Exclusion and Youth Custody

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Abstract

Objective This paper studies whether the disciplinary practice of temporarily excluding children from school causally affects their probability of experiencing incarceration as a young person (youth custody).

Methodology We use linked administrative data on the full population of English secondary school leavers between 2010 and 2013. To isolate the causal impact of exclusion from confounding factors, we employ an instrumental variables approach. Our instrument is academization, a reform program that gave schools increased autonomy over disciplinary procedures, thus quasi-randomly affecting a student's probability of being excluded.

Results We find that schools significantly increased fixed-term exclusions by approximately 1 percentage point after academization. Our results show that fixed-term exclusions caused a 2.6 percentage point increase in the probability of a student being in youth custody by age 18.

Conclusions This finding highlights a direct causal link between a common school disciplinary practice and a severe, long-term negative outcome for young people. It underscores the importance for policymakers and educators to recognize the potential risks associated with disciplinary procedures and the need for alternative, less harmful interventions.

Keywords Disciplinary policies · Prison · Economics of crime · Risky behaviours · Schooling

JEL Classification C36 · I2 · I20 · K42

Claire Cathro was affiliated with the Behavioural Insights team at the time the research was conducted.

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Introduction

Exposure to strict disciplinary policies, a crucial aspect of managing student behaviour, is a significant predictor of subsequent criminal activities. The most widely used formal disciplinary measure is the fixed-term exclusion (or suspension), which temporarily removes students from the school environment. While schools can also permanently exclude students, this is a far less common sanction. The implementation of exclusionary practices may contribute to early involvement of affected students in the criminal justice system. Yet, little evidence indicates whether this relationship is causal.

The use of exclusions is a subject of controversy. While proponents argue for their necessity in maintaining a productive and disruption-free learning environment, critics contend that they disrupt the education of those removed. There is a positive correlation between school exclusion and later-life engagement with the criminal justice system (Fabelo et al. 2011), disproportionately affecting minority students (Okonofua et al. 2020). Moreover, disciplinary practices such as fixed term exclusions may affect students' long-run outcomes by leading them to associate with at-risk peers or by labelling them as troublemakers. Custody itself affects subsequent labour market outcomes, reducing both employment and wages (Dorsett and Thomson 2023).

In the early 1990s, the United States implemented school discipline reforms centred around the concept of "zero tolerance" in response to rising concerns about violence and disorder in schools. These policies have highlighted tension among educators and policymakers concerning the effectiveness of removal from school as a response to student misbehaviour, highlighting the scarcity of empirical evidence on the effects of these practises on removed pupils and their impact on non-excluded peers (Lacoe and Steinberg 2019). A notable exception is the evidence provided by Bacher-Hicks et al. (2019) who exploit exogenous variation in school assignment and find that students assigned to a high-fixed term exclusion school have lower graduation rates and higher rates of future crime.

In England, schools that became academies as part of a school improvement program in the 2000s increased exclusion rates, employing strict 'No Excuses' disciplinary procedures (Machin and Sandi 2020). Despite the prevalent use of these disciplinary practices and the scale of the policy change, evidence on the magnitude of their causal effect on removed pupils' involvement in criminal activities still lacks.

The association between school exclusion and later involvement in the justice system has been labelled as the 'school-to-prison pipeline' (Wald and Losen 2003; Bacher-Hicks et al. 2019). While this concise term is rhetorically useful, it is not informative of whether this relationship is causal. Being excluded from school may increase the risk of later imprisonment beyond selection processes or pre-existing behavioural issues (Butler et al. 2021). In England, among those sentenced to custody who completed secondary school between 2013 and 2015, 91 percent of those with sentences of 6 months or less and 85 percent of those with sentences over 24 months had experienced a temporary exclusion during their school years (Department for Education 2022). It is possible that behavioural problems may lead to both exclusion and imprisonment. Alternatively, exclusion might exacerbate existing behavioural issues by providing an opportunity for criminal activity, subsequently resulting in involvement with the criminal justice system (Cohen and Felson 2010).

In this paper, we aim to address this research gap. We focus on year 11 pupils (those in the final year of compulsory schooling; such pupils are considered to have academic age

15 and turn 16 in the course of the year) in state-funded schools in England, in the years between 2011 and 2013.[1] We track their outcomes using administrative data and estimate the impact of fixed term exclusion on the probability of experiencing youth custody at academic age 16–18.[2] This study focuses on the impact of fixed-term exclusions (suspensions) on youth custody. We do not examine permanent exclusions, as the number of individuals permanently excluded in our dataset is too small (fewer than 400) to support a robust statistical analysis.

Our estimation strategy relies on an Instrumental Variable (IV) approach to identify the causal effect of fixed term exclusion. We leverage a policy-induced variation in the probability of being temporarily removed from school to isolate the effect of school removal on the probability of custody. Machin and Sandi (2020) indicate that following the transition to academy status, which allowed schools to be run in an autonomous manner free from local authority control, schools tend to embrace more stringent disciplinary policies, leading to a significant increase in the number excluded students post-academization. To limit concerns about the possibility that academization affects other dimensions of the educational process besides stricter disciplinary policies, we restrict the analysis to academies in their first academic year of operation. This is because changes that may affect attainment, for example, are likely to take longer to be implemented.

We find that, for Year 11 pupils, academization significantly increases the probability of exclusion by 0.9 percentage points. The effect compares to an average fixed term exclusion rate of 2.3 percent for the whole sample. The significance of this relationship provides a partial justification for the use of academization as an instrument.

Our main result is that being excluded significantly increases the probability of custody by age 18. Specifically, receiving a fixed term exclusion increases the probability of custody by 2.6 percentage points (statistically significant at 90 percent). Against the custody rate of 0.06 percent among pupils in the general school population, this constitutes a sizeable increase. However, the estimated impact relates to compliers – those who are excluded because of academization. The heterogeneity analysis indicates that receiving any exclusion increases the probability of custody by 5 percentage points for high ability pupils. This evidence aligns with the post-2010 academization wave, during which the government prioritized converting 'outstanding' schools, resulting in compliers who are positively selected and attend these top-performing schools.

To understand whether the impact operates through lower human capital accumulation due to less time spent in school because of fixed term exclusions, we assess the causal impact of exclusions on academic attainment, as captured by results in math and English examinations undertaken at the end of Key Stage 4 (KS4), the final year of compulsory schooling, failure to pass one of the KS4 assessments and KS5, the stage of secondary education in England between the age of 16 and 18. The substantial effect sizes suggest that the impact on youth custody may partly operate through its effect on KS4 attainment but not secondary or post-secondary school completion.

[1] These are academic years, running from September to August. For example, academic year 2011 spans September 2010 to August 2011.

[2] We do not consider crime as an outcome since this is not recorded in the data but rather focus on custody, i.e. whether conviction of a crime of sufficient severity to warrant imprisonment has occurred by the age of 18. By determining whether exclusion exclusion leads to an increased risk of custody, we understand that the intermediate step involves the commission of a criminal offense.

We test the robustness of our results through tests of the exclusion restriction, a falsification test and qualitative evidence on the changes implemented by Head teachers in the first years following academization. These additional analyses serve indirect tests of the identification strategy.

We propose an alternative specification that limits the sample to schools without sixth forms, as these schools eliminate the risk of additional years of exposure to potential academization effects on attainment. In these schools, exclusions increase the likelihood of youth custody by 5.9 percentage points. In schools serving a more disadvantaged student population, higher exclusion rates post academization can harm the average pupil, unlike in outstanding schools, where mostly high-ability pupils face an increased risk of custody after exclusion. This highlights the potential for exclusion policies to exacerbate negative outcomes in lower socio-economic contexts.

Our finding that exclusion increases the probability of youth custody has important implications for school discipline and criminal justice policies. This is particularly the case given the negative impact of youth custody on subsequent labour market outcomes (Dorsett and Thomson 2023) and costs associated with custody.[3] Greater sensitivity to this issue is particularly necessary given the long-standing and widespread use of exclusion in England.

Our results highlight a previously unquantified negative criminal justice outcome of education policy. This evidence does not necessarily mean that exclusion as a policy should cease, although some may advocate for that. Rather, it implies that headteachers, teachers, and policymakers should take seriously the idea of exclusion as an intervention that warrants better understanding in terms of its short- and long-term trade-offs. This can be achieved through better use of data and evidence already available to the government to examine the impacts of exclusion on the excluded students as well as their peers, who may experience unquantified benefits. Additionally, it requires the willingness of the government, multi-academy trusts, and schools to rigorously test behaviour management approaches, including those that are promoted as effective but lack evidence of their efficacy in improving behaviour or other outcomes.

Literature

A fundamental problem for making causal claims about the life-course effects of exclusion is the lack of evidence from randomised trials.[4] The evidence that exists is largely correlational but indicates that excluded students are more likely to engage in antisocial behaviour, to be involved with the criminal justice system, and are less likely to complete school in both the short and long term. Excluded youth are more likely to be arrested during the month of exclusion (Monahan et al. 2014). Within a year of fixed term exclusion, excluded youth are also more likely to engage in antisocial behaviour (Hemphill et al. 2013, 2006). Twelve years after fixed term exclusion, excluded youth were less likely to have earned bachelor's degrees or high school diplomas, and more likely to have been arrested and on probation

[3] The cost of housing a convicted criminal in prison in England and Wales was £51,100 a in the year in (2022)/23. *Costs per place and costs per prisoner by individual prison*, HM Prison & Probation Service Annual Report and Accounts (2022)–23, Ministry of Justice, 2023.

[4] The systematic review by Valdebenito et al. 2019 identifies randomized control trials of interventions to reduce or prevent exclusions.

(Rosenbaum 2020). As adults, temporarily excluded students are more likely to experience criminal victimization, criminal involvement, and incarceration (Wolf and Kupchik 2017).

There is limited causal empirical evidence on the relationship between the adoption of strict disciplinary policies – as opposed to exclusion—and students' later criminal behaviour. Bacher-Hicks et al. (2019), exploiting exogenous variation in school assignment caused by a boundary change, find that students who are quasi-randomly assigned to schools with stricter disciplines and higher conditional fixed term exclusion rates are significantly more likely to be arrested and incarcerated as adults. Sorensen et al. (2022) use changes in school principals as a source of exogenous variation in disciplinary inclinations, finding that having a stricter principal reduced reported rates of minor student misconduct. However, this deterrence effect comes at the cost of higher juvenile justice complaints and lower high school graduation rates. Further, that design cannot separately identify the impact of principals from the broader school context.

A more common focus is on the effect of exclusion on educational attainment. This is relevant since this is a possible channel through which the effect of exclusion on custody operates. A meta-analysis by Noltemeyer et al. (2015) summarizes the correlational research across 34 studies in the education literature and finds a negative correlation between achievement and being excluded. Individual studies have deployed a range of estimation techniques. Cornish and Brennan (2025), using a target trial emulation approach which relies on observational data, find a significant association between school exclusion and an increased likelihood of involvement in violent behaviour later in life in UK administrative data. Anderson et al. (2017), using dynamic panel methods, provide a potentially counter-intuitive finding of zero to positive impact of fixed term exclusions on test scores in Arkansas. In Los Angeles, Pope and Zuo (2023) use the interaction between district-wide fixed-term exclusion rate changes with initial school fixed-term exclusion rates levels as an instrument for exclusion. Their findings indicate that a reduction in fixed term exclusion rates decreases average maths and English test scores, decreases GPAs, and increases absences. The impact is driven by small but diffuse spill-overs produced by more lenient disciplinary environments, with these negative externalities partially being offset by large and concentrated benefits for the small number of students who are no longer excluded. Using a difference-in-difference identification strategy that leverages a policy-induced variation in the probability of being excluded, Steinberg and Lacoe (2018) find that lower fixed term exclusion decreases absences and increases maths achievement for previously excluded students. The design, however, does not rely on exogenous variation in the school-level decision to comply or not with the policy change. Using propensity score matching, Jabbari and Johnson (2023) find that high fixed term exclusion schools are negatively associated with students' math achievement and college attendance. Using the same technique in data for England, Sutherland and Eisner (2014) find that fixed term school exclusion increases the probability of unemployment.

Lastly, in respect of labour market outcomes, Madia et al. (2022) find that both fixed term and permanent exclusion increases the risk of becoming NEET at the age of 19/20, and then remaining economically inactive at the age of 25/26, as well as experiencing higher unemployment risk and earning lower wages also at the age of 25/26.

Overall, although existing evidence suggests there are positive relationships between exclusions and involvement with the criminal justice system and a few studies isolate the causal effect of fixed term exclusion on adult custody, the causal evidence on the link between exclusion and youth custody remains largely unexplored. To our knowledge, ours is the first attempt to assess whether there is a causal relationship between school exclusion and youth custody in England.

The Institutional Context

The Introduction of Academy Schools

Until the early 2000 s, most state-funded schools in England were run by local authorities. Starting in the 1990 s, there was a wide acknowledgement that some of these schools were failing to provide a high-quality education to their pupils and suffered from endemic discipline issues (Machin and Sandi 2020). Under the 1997–2010 Labour government, a remedial programme for poorly performing schools was adopted to enable state-funded schools to run autonomously from the local authority, leading to the creation of "academy schools", similar to charter schools in the US. These schools, in order to convert, were required to sign up a sponsor.

Academy schools are independent, non-selective and state-funded schools. They have autonomy to operate in areas such as hiring and pay of teachers, pupil admissions (subject to national rules), curriculum (except for core subjects), and decisions about the structure and length of the school day. Critically, autonomy over school policies meant that academy schools could set up and enforce their own disciplinary procedures.

The academization process of the English education system changed when the newly elected Conservative/Liberal Democrats coalition government introduced the Academies Act of 2010. The programme aimed at bringing greater autonomy and competition to the entire school sector. It was no longer limited to poorly performing schools and a sponsor was no longer required for conversion to take place (Machin and Sandi 2020).

The new policy regime resulted in a remarkable increase in the number of academies in England. Compared to the pre-2010 academies, these academies were relatively high-performing schools. Eyles et al. (2018) show that the schools converting during the two batches of academy introduction – i.e. before and after 2010—were very different, in terms of both the characteristics of schools that became academies and the changes in pupil intake that occurred post-conversion. Schools that converted after May 2010 had high attainment and a low share of disadvantaged students before conversion. They did not experience any changes in terms of the ability of the students they enrolled, and only limited changes in the percentage of children eligible for free school meals. The opposite is true for schools that became academies before May 2010, which showed significant improvements in the ability of enrolled students and more substantial declines in the percentage of disadvantaged pupils after conversion. Due to the important differences between the academization programmes before and after 2010, our analysis focuses on the process of academization that occurred after May 2010 only. Although a number of new Labour-type 'sponsored' academies have opened since, more than 80 per cent of the expansion of the academy sector has happened through coalition-type 'converters'. Both 'sponsored' and 'converter' academies enjoy similarly wide margins of autonomy.

Disciplinary Measures

In English secondary schools, disciplinary actions range from minor sanctions to permanent exclusions, which represent the most severe response to student misbehaviour. These measures are designed to be proportionate to the severity of the misconduct and aim to maintain a safe and orderly learning environment.

Sanctions are varied and escalate according to the nature of the infraction; however, detailed data on their application is not formally recorded. Teachers may address minor misconduct through verbal warnings or reprimands, reminding students of behavioural expectations. In more serious cases, schools may involve parents or carers by issuing formal letters, seeking their cooperation in resolving the issue. Detentions are another common sanction, requiring students to remain at school during breaks, lunchtime, or after hours as a consequence of their behaviour. Other measures include the temporary loss of privileges, such as participation in extracurricular activities, and the confiscation of items deemed inappropriate for school, such as mobile phones or prohibited materials.

Exclusions, the most severe disciplinary measure, are used to address significant behavioural issues and are governed by strict guidelines to ensure fairness. According to the Department for Education (2019), decisions to exclude must be "lawful, rational, reasonable, fair, and proportionate." Exclusion is permitted only for disciplinary reasons; it is unlawful to exclude a pupil for poor academic performance. Common reasons for exclusion include persistent disruptive behaviour, bullying, theft, physical assault, verbal abuse, vandalism, and drug- or alcohol-related offences.

A fixed-term exclusion temporarily removes a pupil from school for up to 45 school days in an academic year. This is typically used for breaches of the school's behaviour policy that are serious but do not warrant permanent removal and when other sanctions have proven insufficient. Unlike maintained schools, academies are not required to follow legislation permitting off-site direction for behaviour improvement (Sect. 29(3), Education Act 2002). As a result, exclusions remain the primary tool for managing severe behavioural issues in academies.

Empirical Strategy

The primary challenge in the literature examining the impact of school exclusion on youth custody lies in disentangling the observed correlation between exclusions and custody outcomes from the influence of individual characteristics, unobserved factors, and broader community or school-level influences. At the individual level, unobserved psychological traits such as low self-control, impulsivity, high levels of aggression, and lack of trust in others play a significant role in both exclusion and eventual involvement with the criminal justice system (Wikström and Treiber 2007; Burt 2020). Additionally, parenting practices crucial to behavioural development and educational success, such as parental beliefs, monitoring, interest in the child's progress, and parent–child relationships, are often unmeasured. For example, children's behavioural norms learned at home—whether they align with school rules—can have profound implications (Tremblay and Craig 1995).

Socio-economic background, another commonly considered factor, serves as a noisy proxy for a complex set of influences, including developmental opportunities, resource availability, and the broader behavioural context. Variations within this category could obscure the true drivers of the observed outcomes (Huesmann et al. 2002; Baker et al. 2023). Furthermore, at the school level, unmeasured aspects such as the "school climate," which is known to influence behaviours like bullying (e.g., Paluck et al. 2016), could significantly affect exclusion and custody pathways.

Additionally, we don't observe data on the neighbourhoods of the children, which are linked to both developmental trajectories and later outcomes. Evidence shows that the burden of violence in a child's community environment alter a child's behaviour and his/her probability of adult's arrest (Sharkey et al. 2012; Baker et al. 2023). These neighbourhood factors are not purely individual but depend on how they are experienced and interpreted—for instance, the same event in a neighbourhood might have a profound effect on one child but leave another unaffected. These unobserved factors collectively complicate causal interpretations, suggesting the need for robust methodologies to isolate the specific role of school exclusions.

There may also be school-level responses to factors related to poor achievement and attendance. The process of school is not random and may have multiple influences, including subjective judgments, disciplinary policies and institutional practices. The non-random selection of students for exclusion can bias estimates of impact. If certain groups of students (such as those from marginalized communities, those with pre-existing behavioural issues or those enrolled in schools with stricter disciplinary policies) are both more likely to be excluded and have a higher probability of youth custody, the failure to account for these confounding characteristics can result in inflated estimates of the impact of exclusion.

To address this, we estimate the effect of exclusion on youth custody using an instrumental variables approach. We leverage a policy-induced variation in the probability of receiving an exclusion to isolate the effect of exclusion on the probability of custody for excluded students. In a nutshell, we looked for a factor that could influence the chance that a pupil is excluded but does not have any other effect on the pupil's likelihood to experience custody.

Previous research has suggested that after converting to academy status, schools adopt stricter disciplinary policies, resulting in a significant increase in the number of pupils excluded (Machin and Sandi 2020). If pupil selection into an academy school is exogenous to its disciplinary regime, differences in exclusion rates between academies and non-academies are attributable to the effect of the academic status and not individual characteristics of the pupil, as are differences in outcomes.

However, pupils' selection into schools once they become academies may not be random. To address this, we consider only children who were already enrolled in the school before it converted to academy status. This ensures that academy conversion was exogenous to enrolment in secondary school. Thus, our analysis excludes pupils that switch schools between year 10 and 11 or during year 11. Similarly, we drop from the analysis pupils that are in custody at any point while in year 11 as it is not possible to determine if custody happened before or after the school academized. Next, we drop students who repeat year 11 to avoid counting twice pupils who may be more exposed to exclusion. Finally, we exclude the small group of pupils who were permanently excluded from school, as including them would otherwise misclassify them as pupils who had not received a fixed term exclusion.

In estimating impacts, we also want to compare academizing schools with a set of similar schools. Here, we follow Eyles et al. (2017), Eyles and Machin (2019) and Machin and Sandi (2020) to only select the sample of all schools that became academies over the period between the academic years 2010/11 and 2013/14. If the timing of academization is not correlated with unobservable factors that may affect disciplinary policies, this restriction ensures that unobservable common reasons for conversion to an academy are accounted for in the analysis.

The research design adopted here defines academized schools as becoming academies following the 2010 general election and by the academic year 2013/14. In each academic year, the control group includes schools that became academies later but by the end of the academic year 2013/14. In defining academies, we follow Eyles et al. 2018 and define a school as being open as an academy in a given academic year if it became an academy *in or before* September of that year. For instance, a school that converted in September 2011 would be classed as being an academy from the 2011–12 academic year, whereas a school opening in October 2011 would be classed as opening as an academy in 2012–13. A school's first year of treatment is thus the first full academic year that it begins with academy status.

Since the introduction of this new policy regime, there has been a remarkable increase in the number of academies in England. As of 2021, 79 percent of all secondary schools and 38 percent of all primary schools are academies, over 85 percent of which are run as part of a chain of schools, called multi-academy trusts.[5]

Table 1 reports the number of schools that converted to academies in each academic year. In each year, schools that converted are considered the treatment group and are compared to schools scheduled to convert by the 2013/14 academic year. For example, schools that converted in the academic year 2010/11 are compared to those that converted between 2011/12 and 2013/14. Similarly, schools that converted in 2011/12 are compared to those converting between 2012/13 and 2013/14, with schools that converted in 2010/11 excluded from the sample, as this is one-year post-conversion. Finally, schools that converted in 2012/13 are compared to those converting in 2013/14, excluding schools that converted in 2010/11 and 2011/12. Most of the schools in our sample convert in 2011/12 and 2012/13.[6]

A crucial assumption of our analysis strategy is that academization affects the likelihood of custody *only through its impact on exclusion*. That is, being in a school that academizes does not increase or decrease the probability of experiencing custody later in life, *except through* the higher probability of being excluded.

An immediate objection to this instrument is that academization was intended to improve attainment and so may not satisfy that requirement. This may be especially true for schools that became academies in the pre-2010 period, as they showed significant improvements in the ability of enrolled students. It may also be a consequence of stricter disciplinary policies used to manipulate scores, as the removal of disruptive children may l improve the student body composition, and thus attainment. Evidence from Worth (2015) demonstrates that academies converting post-2010 did not show significant progress in attainment com-

Table 1 Number of academies converting between 2010/11 and 2014

Year when school academize	2010/11	2011/12	2012/13	2013/14	Total
No. of conversion to academy by year	96	684	317	115	1,212

[5] Department for Education, 2021. Open academies, free schools, studio schools and University Technical Colleges.

[6] These figures are smaller than the official statistics published by the DfE due to the fact that our sample includes only schools that converted to academies between the years 2010/11 and 2013/14. Nonetheless, our data accurately reflect the trend in sector expansion.

pared to similar non-converting schools after two years. Similarly, evidence from Andrews et al., (2017) shows that, in contrast to the pattern of sponsored academies, there was no real change to the primary school test scores of incoming pupils once the schools become converter academies. A possible explanation for this is that converter academies already had a relatively high attaining intake and so continued to attract and admit a similar cohort of pupils. Andrews et al., also show a slight decline in KS4 the year the school convert to an academy. However, they show that this effect emerges a year before conversion, indicating that it may not directly reflect the impact of academization itself.

Evidence from Eyles and Machin (2019) shows that positive effects of academization on KS4 attainment took at least three years to materialize, with small improvements starting to manifest one year after conversion. This evidence is drawn from pre-2010 academies and is not available for post-2010 academies. Machin and Sandi's (2020) analysis of the effect of academy attendance on end-of-secondary school GCSE pupil performance found a statistically significant 0.1 standard deviation impact of academization for pre-2010 academies, and no significant impact for post-2010 academies. Their results indicate that the higher exclusion rates post-academization may reflect the more rigorous discipline enforced by the academies, rather than functioning as a means of test score manipulation. Similarly, evidence from Eyles et al. (2017) based on the academization of primary schools finds no evidence of pupil performance improvements resulting from academization of primary schools.

To mitigate the risk that academization leads to better attainment, we apply two additional restrictions to the analysis. First, we restrict the analysis of exclusions to the year academization takes place, as we hypothesise that newly formed academies can swiftly enforce new discipline policies while other changes to improve attainment will take longer to implement. This is supported by Eyles and Machin (2019) and anecdotal evidence from some converted academies where newly appointed principals introduced strict discipline policies promptly.[7]

Second, we focus our analysis on pupils in year 11 only. Evidence from Eyles and Machin (2019) suggests that considering educational outcomes beyond the conversion year may raise concerns, as academization could lead to improvements in attainment over time. By limiting our sample to year 11 pupils, we mitigate the risk that the academization process has contributed to any improvements in their attainment. There are two other reasons for focusing on pupils in year 11. First, by age 15 puberty has reached its peak and pupils are particularly susceptible to peers' influences (Steinberg and Monahan 2007), and thus more likely to take risks (Blakemore and Robbins 2012). Second, as we are unable to observe custody for pupils prior to age 15, focusing on exclusion in year 11 reduces the gap between when we observe a pupil receive an exclusion and when they may start a custodial sentence.

Additionally, we include a set of robustness checks to further ensure the validity of the exclusion restriction and the identifying assumption. First, we examine the impact of academization on educational attainment and a proxy for score manipulation. If most changes beyond the enforcement of stricter disciplinary policy take time to occur, we do not expect any significant change on attainment and outcomes that measure attainment and are proxies for score manipulation in the year of conversion.

[7] One example comes from schools under the Inspiration Trust academy sponsors' umbrella. In 2017, the BBC covered the sudden change in discipline policy in newly converted Inspiration Trust academies in a series of articles (examples available here and here).

Second, we conduct a falsification test using unauthorised absences, a proxy for disruptive behaviour that is not directly linked to academization. Unauthorised absences are positively correlated with youth custody and exclusions: pupils who eventually enter custody have unauthorised absences four times higher than the average (see Table 2), and those excluded have more than double the rate of unauthorised absences compared to non-excluded peers (see Table 3). However, unlike exclusions, unauthorised absences should not be directly affected by academization. If academization indirectly impacted youth custody through broader cultural or policy shifts, we would expect significant effects on unauthorised absences.

Third, our identifying assumption, as explained above, is that variation in academization is uncorrelated with other factors determining a student's probability of exclusion, conditional on the set of observable controls, secondary school and cohort fixed effects. To address concerns about potential systematic differences in unobservable, time-varying school-level factors between schools that academize earlier and those that do so later, we demonstrate that pre-trends in exclusion rates do not differ significantly between the two groups.

Table 2 Descriptive statistics by custody status

	Pupils never in custody	Pupils in Custody
	Mean	Mean
	(SD)	(SD)
Exclusion	0.023	0.144
If school attended is an academy	0.29	0.208
Female	0.492	0.086
White Ethnicity	0.81	0.677
Black Ethnicity	0.034	0.124
Asian Ethnicity	0.072	0.058
Mixed Ethnicity	0.032	0.073
Other Ethnicity	0.052	0.069
FSM (up to 11)	0.191	0.642
Statement or EHC plan (up to 11)	0.05	0.073
English as additional language	0.157	0.177
SEN (up to 11)	0.322	0.721
IDACI score	0.179	0.325
	(0.157)	(0.184)
Unauthorised absences up to 12	0.004	0.021
	(0.023)	(0.061)
KS2 English	0.116	−0.711
	(0.918)	(1.04)
KS2 math	0.105	−0.582
	(0.936)	(0.973)
KS4 English	0.161	−1.818
	(0.878)	(1.298)
KS4 math	0.159	−1.597
	(0.905)	(1.209)
Observations	707,789	452

Data from National Pupil Database for students in comprehensive (non-selective) state schools and from the National Client Caseload Information System. Statistics are unweighted. Descriptives for children in care had to be suppressed for statistical disclosure control

Table 3 Descriptive statistics by exclusion status

	Students not excluded	Students excluded
	Mean	Mean
	(SD)	(SD)
Custody before 18	0.001	0.004
If school attended is an academy	0.286	0.445
Female	0.497	0.291
White Ethnicity	0.81	0.794
Black Ethnicity	0.033	0.059
Asian Ethnicity	0.072	0.05
Mixed Ethnicity	0.032	0.044
Other Ethnicity	0.052	0.052
FSM (up to 11)	0.186	0.415
Statement or EHC plan (up to 11)	0.05	0.055
English as additional language	0.158	0.135
SEN (up to 11)	0.317	0.559
IDACI score	0.178	0.234
	(0.156)	(0.176)
Unauthorised absences up to 12	0.004	0.01
	(0.022)	(0.033)
KS2 English	0.129	−0.423
	(0.913)	(0.988)
KS2 math	0.115	−0.339
	(0.934)	(0.945)
KS4 English	0.179	−0.629
	(0.868)	(0.986)
KS4 math	0.176	−0.63
	(0.897)	(0.974)
Observations	691,803	16,438

Data from National Pupil Database for students in comprehensive (non-selective) state schools and from the National Client Caseload Information System. Statistics are unweighted. Descriptives for children in care had to be suppressed for statistical disclosure control

Fourth, as a sensitivity analysis, we restrict the sample to schools without a sixth form. This approach accounts for the fact that schools with a sixth form provide students additional years of exposure to potential academization effects on attainment.

Finally, we present evidence from the Department for Education's 2014 survey, which reports headteachers' perceived changes following academization. Together, these additional analyses and evidence strengthen the validity of the exclusion restriction and the identifying assumption.

As already mentioned, to avoid results to be driven by changes in the ability composition of schools after they become academies, our analysis selects only children who were already enrolled before their school converted.

Under these restrictions, it is plausible to view the academization of local authority schools as representing an exogenous "shock" to the risk of exclusion facing pupils at those schools.

Data

This paper uses data from the National Pupil Database (NPD), a comprehensive register maintained by the Department for Education in England. The NPD provides detailed records of all pupils in state schools across England, tracking them from their early years through to post-16 education. It includes data on academic attainment, pupil backgrounds, absences, and exclusions, enabling us to determine if a pupil has experienced fixed-term exclusions during year 11. More specifically, it provides information on pupils' school, gender, age, ethnicity, whether English is an additional language (EAL), Special Educational Needs or disabilities (SEN) and entitlement to Free School Meals (FSM). It records whether the child has been "looked after" in state care (CLA), whether "in need" in some other way (CiN) and whether the child has an Education, Health and Care (EHC) plan. It also provides a measure of local deprivation relevant to children (IDACI—income deprivation affecting children index). Information on whether the individual is in youth custody by the age of 18 is sourced from the National Client Caseload Information System (NCCIS), one of the component tables of the NPD.

Our analysis focuses on three full cohorts of English maintained secondary school leavers: those completing KS4 in the academic years 2009/10, 2010/11, 2011/2012 and 2012/2013.[8] The estimation sample includes 708,241 observations. Table 2 summarises pupil characteristics, split by whether observed to be in custody by age 18 or never in custody. Those with experience of custody are small sub-groups, accounting for only 0.06 percent of cases.

Our 'treatment' variable is a binary indicator of having been temporarily excluded in a given school year as observed from the NPD School Census. Those entering custody are six times more likely to have experienced an exclusion than those who do not (24–26 percent versus 2.3 percent).

Young people who are male, of a Black ethnic origin, eligible for FSM and have SEN are overrepresented among those in custody. Similarly, attainment at the end of primary school at age 11 years measured with Key Stage 2 (KS2) is lower on average and individuals in this group are more likely to live in deprived areas as measured by the IDACI index of multiple deprivation. Youngsters in custody have a rate of unauthorized absences by the age of 12 four times higher than that among those who do not enter custody (2 percent versus 0.5 percent). KS2 and KS4 scores are below the average for those who end up in custody.

Table 3 presents descriptive statistics by exclusion status. The rate of custody is four times higher for students excluded: 0.4 percent of students who have been excluded are in custody by age 18 compared to 0.1 percent among students never excluded. Males, students with Black ethnicity, those receiving free school meal and students with special educational needs are over-represented among the excluded, who also show lower KS2 and KS4 attainment than their non-excluded peers.

Estimating the Effect of Exclusion on Youth Custody

The aim of our empirical work is to estimate the effect of school exclusion on the probability of youth custody. The outcome is a binary indicator of custody, defined as 1 if pupil

[8] We exclude pupils enrolled in the academic year 2013/14 given this is our cut-off for conversion to academy status, there would be no control schools in that year.

i in school s, cohort t is in custody before age 18 (and after age 16) and 0 otherwise.[9] We estimate the model using a linear probability specification:

$$y_{ist} = \beta_0 + \beta_1 exc_{ist} + \beta_2 X_{ist} + \sigma_s + \tau_t + u_{ist} \tag{1}$$

The variable exc_{ist} is a binary indicator equal to 1 if year 11 pupil i attending school s in academic year t received a fixed term exclusion for any disciplinary infraction, and 0 otherwise. β_1 will estimate the impact of exclusion relative to not being excluded.

X_{ist} is a vector of controls measured at the pupil level. These characteristics include prior attainment (KS2 scores in English and maths); ethnicity dummies; whether English is an additional language; free school meal eligibility; having an Education, Health and Care plan; special educational needs; whether ever looked after by the state, whether ever classified as in need; gender; and unauthorised absences at the age 12 years. We also control for area deprivation measured at the Lower-layer Super Output Area level (neighbourhood). σ_s and τ_t are a set of school level and cohort level fixed effects. We cluster the standard errors at the school level to account for correlation among students attending the same school.

The control variables in Eq. (1) account for certain observable variables that are correlated with exclusion rates. However, estimates of β_1 will remain biased if unobservable time-varying, within-school characteristics are correlated with $excl$ and also with y^*. This is the motivation for our IV strategy, that addresses the endogeneity of β_1 by using academization as a source of exogenous variation in the probability that a school excludes a pupil.

The first stage equation:

$$e_{ist} = \delta_1 A_{st} + \delta_2 X_{ist} + \sigma_s + \tau_t + \varepsilon_{it} \tag{2}$$

relates exclusion to A_{st}, a binary indicator equal to 1 if pupil i attended year 11 in school s that became an academy in the same year t and 0 if the pupil's school had not yet converted to an academy by the start of the academic year. In each year, schools that converted are considered the treatment group and are compared to schools scheduled to convert by the 2013/14 academic year. Treated schools are only retained in the sample during the academic year of their conversion. They are excluded in the subsequent academic years, which eliminates the issue of using already treated schools as comparators. We focus on pupils when they are in year 11 in the academic years 2010/11 to 2012/13.

Our identifying assumption is that being in a school that academizes does not increase or decrease the probability of experiencing custody later in life, *except through* the higher probability of being excluded. The exclusion restriction requires that the instrument be uncorrelated with the structural error term u_{ist} in Eq. 1, which contains across-pupils,

[9] The NCCIS data do not include record-level offence data for individuals in youth custody, preventing us from verifying whether the offence leading to custody occurred before or after a pupil's exclusion. However, we argue that the likelihood of pupils committing an offence in Year 10, being excluded in Year 11, and subsequently entering custody after the end of Year 11 is minimal. First, data from 2013 indicate that the average time from arrest to case completion in the youth justice system was 89 days (MoJ, (2014), suggesting that only a small number of pupils would be affected by such a sequence of events. Additionally, according to a MoJ-DfE report (2022), 88% of pupils are excluded before their first conviction for serious violent offences. While we acknowledge this as a limitation of our study, we believe it applies to only a very small minority of cases and is unlikely to significantly impact our findings.

within-school and across-time variation in custody not accounted for by individual controls, or period and school fixed effects.

As previously described, the exclusion restriction is violated if changes in custody rates are influenced by factors other than the implementation of stricter disciplinary policies in academized schools. To address this, we conduct a set of additional specifications described in the Robustness checks section.

Results

Before correcting for the endogeneity of β_1 in Eq. (1), Table 4 reports school fixed effects estimates of the effects of exclusions on custody. The chosen specifications include one without controls and one with the full set of individual controls and time and school level fixed effects as indicated in Eq. (1). According to these fixed effects estimates, exclusion significantly increases the probability of custody by age 18. Fixed term exclusion increases the probability of youth custody by 0.25 percentage points. This might be an indication that there is negative self-selection into exclusion by those individuals who are more likely to engage in disruptive behaviour and criminal activities and selection cannot be accounted for with this specification.

Our instrumental variable strategy exploits the higher likelihood of being excluded after the school converts to an academy. We provide evidence of the impact of a school's academization on the probability of being excluded while in year 11 in Table 5.

Table 5 shows the results from the estimation of the first stage as specified in Eq. (2), by presenting the coefficient of 'academy' conversion on the probability of exclusion, with and without controls. Both columns 1 and 2 give very similar results: being a year 11 pupil in a school after it converted to an academy increases the probability of receiving a fixed term exclusion by 0.9 percentage points on average. They are statistically significant at 1 percent level. The effect on exclusion compares to a raw fixed term exclusion rate of 2.3 percent of all pupils in our sample. 0.93 percentage points is therefore a sizable increase: it means moving about 1 to 2 pupils excluded in every two classes of pupils (52 pupils in total).[10]

Table 4 Fixed effects model of exclusion on youth custody

	(1)	(2)
VARIABLES	Y: custody by 18	
If student excluded	0.0034***	0.0025***
	[0.001]	[0.001]
Individual controls	No	Yes
Observations	708,241	708,241

*** $p<0.01$, ** $p<0.05$, * $p<0.1$. School fixed effect linear regression estimated. Standard errors clustered at school level. Controls: KS2 Maths, KS2 English, EAL, FSM (up to 11), Statement or EHC plan (up to 11), SEN (up to 11), Ever looked after up to age 16, Pupil had ever been in need, Female, Unauthorized absence up to 12, Ethnicity dummies, IDACI score decile dummies, year dummies, dummies for missing KS2 and missing unauthorized absences

[10] To find the number of pupils that receive fixed term exclusions in the absence of academization, we multiply the average exclusion rate among pupils attending schools that have not yet converted to academies in our sample (0.023) by the approximate number of students in two classes (26*2=52). This is 0.023*52=1 pupils. This compares to the number of pupils in two classes that would receive exclusions given the change in exclusion rate as a result of academization (the "treatment effect"). We multiply the exclusion rate post-

Table 5 First stage: the effect of academization on exclusions

	(1)	(2)
	Y: student suspended	Y: student suspended
Academy	0.0088***	0.0092***
	[0.001]	[0.001]
	No	Yes
Fraction of suspensions	0.0232	0.0232
Observations	708,241	708,241
F-test	36.12	40.16
Prob > F	0.000	0.000

*** $p<0.01$, ** $p<0.05$, * $p<0.1$. School fixed effect linear regression estimated. Standard errors clustered at school level. Controls: KS2 Maths, KS2 English, EAL, FSM (up to 11), Statement or EHC plan (up to 11), SEN (up to 11), Ever looked after up to age 16, Pupil had ever been in need, Female, Unauthorized absence up to 12, Ethnicity dummies, IDACI score decile dummies, year dummies, dummies for missing KS2 and missing unauthorized absences

Table 6 2SLS estimates of exclusion on youth custody

	(1)	(2)
VARIABLES	Y: custody before 18	
If student excluded	0.0267*	0.0264*
	[0.015]	[0.014]
First stage		
Academy	0.0088***	0.0092***
	[0.001]	[0.001]
Individual controls	No	Yes
Mean youth custody	0.0006	0.0006
F-test	36.12	40.16
Prob > F	0.000	0.000
Observations	708,241	708,241

*** $p<0.01$, ** $p<0.05$, * $p<0.1$. School fixed effect two stages least square linear regression estimated. Standard errors clustered at school level. Controls: KS2 Maths, KS2 English, EAL, FSM (up to 11), Statement or EHC plan (up to 11), SEN (up to 11), Ever looked after up to age 16, Pupil had ever been in need, Female, Unauthorized absence up to 12, Ethnicity dummies, IDACI score decile dummies, year dummies, dummies for missing KS2 and missing unauthorized absences

The F-statistic in column 2 shows that conversion to an academy significantly affects the probability of exclusion, and the F-test is larger than the conventional level of 10 used to indicate a good instrument, suggesting that academization is highly predictive and appears a strong instrument.

Table 6 reports the Two-Stage Least Squares estimates. Consistent with our previous findings, we find that fixed term exclusions increase youth custody (significant at the 90 percent level). The effect of exclusion is 2.64 percentage points. Compared to the exclusion effect estimates in Table 4, the IV coefficient—corrected for the endogeneity of exclusion—

academization ($0.023+0.0093=0.032$) by the approximate number of number of pupils in two classes (52). These are $0.032*52=1.67$ pupils.

is substantially larger, nearly ten times the OLS estimate. While this highlights the potential importance of addressing endogeneity, the magnitude of the IV estimate is strikingly large, and should therefore be interpreted with caution. The smaller size in the fixed effect estimates in Table 4 suggests that unobservable factors correlated with the risk of exclusion are negatively correlated with those that increase the likelihood of youth custody. Unlike the fixed effect model, the 2SLS results estimate the local average treatment effect, capturing the impact on compliers, students who are excluded due to their school converting to an academy. The fixed effect model smaller size may arise if the marginally affected group primarily comprises high-ability individuals or individuals with a low propensity for risky activity. To investigate this further, we perform a heterogeneity analysis to assess how the average observable characteristics of students excluded and later entering youth custody changed after their schools convert to academies.

To explore heterogeneity in impacts, Tables 7 and 8 present how the effects vary across different pupil subgroups. We consider two dimensions along which the impact on custody may differ: academic attainment at KS2, classified into three ability tertiles based on the distribution within each secondary school (Table 7), and eligibility for free school meals, used as a proxy for socio-economic disadvantage (Table 8).

Results in Table 7 show a positive and significant relationship with high KS2 attainment across all measures and a negative and significant relationship with low KS2 attainment. Impacts are concentrated among those in the middle and upper third of the prior attainment distribution. Following school conversion to an academy, estimates indicate that exclusions increase the likelihood of custody by 2.2 percentage points for middle-ability pupils and 5.4 percentage points for high-ability pupils, although the latter is significant at the 10 percent level. In contrast, low-ability pupils experience a non-significant 1.7 percentage point increase in custody probability. The first stage results report how the impact of academization on exclusions varied across different groups and it indicated that exclusions were concentrated on low ability pupils.

Table 7 Impacts by subgroup - KS2 tertiles

	(1)
VARIABLES	Y: custody by 18
If student excluded	0.0169
	[0.011]
Exclusion*KS2 middle tertile	0.0344**
	[0.014]
Exclusion*KS2 high tertile	0.0509*
	[0.027]
First stage	
Academy	0.0241***
	[0.001]
Academy*KS2 middle tertile	−0.0279***
	[0.001]
Academy*KS2 high tertile	−0.0262***
	[0.001]
Observations	708,241
F-test	703.9
Prob>F	0.000

*** $p<0.01$, ** $p<0.05$, * $p<0.1$. School fixed effect two stages least square linear regression estimated. Standard errors clustered at school level. Controls: KS2 Maths, KS2 English, EAL, FSM (up to 11), Statement or EHC plan (up to 11), SEN (up to 11), Ever looked after up to age 16, Pupil had ever been in need, Female, Unauthorized absence up to 12, Ethnicity dummies, IDACI score decile dummies, year dummies, dummies for missing KS2 and missing unauthorized absences

Table 8 Impacts by subgroup – FSM

	(1)
VARIABLES	Y: custody by 18
If student excluded	0.0351**
	[0.016]
Exclusion*FSM	−0.0426***
	[0.012]
First stage	
Academy	0.0095***
	[0.001]
Academy*FSM	−0.0115***
	[0.001]
FSM	−0.0200***
	[0.000]
Observations	708,241
F-test	205.3
Prob>F	0.000

*** $p<0.01$, ** $p<0.05$, * $p<0.1$. School fixed effect two stages least square linear regression estimated. Standard errors clustered at school level. Controls: KS2 Maths, KS2 English, EAL, FSM (up to 11), Statement or EHC plan (up to 11), SEN (up to 11), Ever looked after up to age 16, Pupil had ever been in need, Female, Unauthorized absence up to 12, Ethnicity dummies, IDACI score decile dummies, year dummies, dummies for missing KS2 and missing unauthorized absences

Table 8 results indicate that pupils eligible for free school meals are 0.043 percentage points less likely to face youth custody following the school's conversion to an academy. The first stage results indicate that academization significantly decreases the probability of exclusion for low ability students.

This evidence, indicating that marginal individuals exposed to higher custody rates tend to be high-ability and less deprived pupils, aligns with the context of the post-2010 academization wave. At the onset of this wave, the government focused on converting 'outstanding' schools, which is why early Academies predominantly consisted of high-achieving institutions. The majority of these schools were rated as either outstanding or good by the Office for Standards in Education, Children's Services and Skills (Ofsted). Consequently, the compliers impacted by the change are likely to be positively selected, attending these high-performing schools. This finding is also consistent with the evidence presented in Machin and Sandi (2020), who argued that the higher exclusion rates in academies stem from the enforcement of stricter disciplinary standards rather than test score manipulation.

Our ability to assess possible mechanisms of this effect is limited by the fact that our data do not report anything about the time spent outside of schools and possible students' interactions once excluded from schools. However, to determine if the effect of school exclusion is due to reduced human capital accumulation from less time spent in school because of exclusions, we evaluate the causal impact of these exclusions on KS4 Maths and English outcomes, on the probability of failing to pass one of the KS4 exams and on KS5 attainment, the stage of secondary education in England where students typically study A-levels and other level 3 qualifications between the age of 16 and 18.[11] In the sample, 38 percent of pupils attain A-level, while 17 percent achieve other Level 3 qualifications. The remaining pupils obtain Level 2 (KS4) qualifications or lower.

[11] In addition to A-levels, Level 3 qualifications in England include a variety of vocational and technical qualifications. These include, but are not limited to, BTECs (e.g., National Diplomas, National Extended Diplomas), T Levels, the International Baccalaureate Diploma, Cambridge Pre-U, Advanced Apprenticeships, Access to Higher Education Diplomas, NVQ Level 3, and OCR Cambridge Technicals.

Results are reported in Table 9. The effects on KS4 are both large in magnitude and statistically significant, indicating that exclusions reduce test scores by approximately two standard deviations of KS4. This effect is significant at the 99% level for mathematics and at the 90% level for English. Additionally, while the likelihood of failing one or more KS4 assessments is positively associated with exclusion, the effect is small and not statistically significant. For the probability of completing A-level or other Level 3 qualifications, the effect is negative but not statistically significant. The considerable effect sizes on KS4 suggest that the impact of exclusion on youth custody may, at least in part, be mediated through its negative impact on KS4 attainment, although not through any effect on secondary or post-secondary education completion.

Robustness Checks

While it is not possible to confirm the validity of the exclusion restriction and the identifying assumption, we present in this section some additional results that aim to probe the robustness of the results.

To corroborate the validity of exclusion restriction, we assess whether academization has any impact on educational attainment. To this end, we include in this analysis the sample of schools that academize even in the years after the conversion, and estimate an event study equation, as the following one:

$$ks_{ist} = \delta_0 + \sum_{k=-K}^{J} \delta_k T_{s,t-k} + \beta_2 X_{ist} + \sigma_s + \tau_t + \nu_{ist} \tag{3}$$

where ks_{ist} is the maths and English KS4 attainment level of pupil i in school s, cohort t. $T_{s,t-k}$ is the event-time dummy, taking value 1 when the difference between the calendar year, t, and the academic year school s converted to an academy is k. Our baseline reference category is the year of school conversion, as we defined a school as being open as an acad-

Table 9 2SLS estimates of exclusion on KS4 and KS5 attainment and KS4 failure

	(1)	(2)	(3)	(4)	(5)
VARIABLES	KS4 math	KS4 Eng	KS4 fail	KS5 A level	KS5 other qual
If student excluded	−2.1943***	−2.4371*	0.0623	−0.2329	−0.2985
	[0.838]	[1.330]	[0.299]	[0.309]	[0.226]
First stage					
Academy	0.0092***	0.0092***	0.0092***	0.0092***	0.0092***
	[0.001]	[0.001]	[0.001]	[0.001]	[0.001]
Mean outcomes	0.1575	0.1601	0.0324	0.3760	0.1222
F-test	173.9	173.9	173.9	173.9	173.9
Prob>F	0.000	0.000	0.000	0.000	0.000
Observations	708,241	708,241	708,241	708,241	708,241

*** $p<0.01$, ** $p<0.05$, * $p<0.1$. School fixed effect two stages least square linear regression estimated. Standard errors clustered at school level. Controls: KS1 Maths, KS1 Reading, KS1 Writing, KS2 Maths, KS2 English, EAL, FSM (up to 11), Statement or EHC plan (up to 11), SEN (up to 11), Ever looked after up to age 16, Pupil had ever been in need, Female, Unauthorized absence up to 12, Ethnicity dummies, IDACI score decile dummies, year dummies, dummies for missing KS2 and missing unauthorized absences emy in a given academic year if it became an academy by September of that year (k=0). The control group is dynamic in the sense that it consists of all schools in the sample that have not yet converted to academy status at any given point in time. The 2013/14 converting schools serve as part of the dynamic control group for all earlier academy conversions throughout the 2010–2013 period. Within our 2010–2014 data, they are not observed in a post-treatment state, effectively functioning as a "never-treated" or "always control" group for the purposes of identifying the effects of earlier conversions. Figure 1 displays the estimated coefficients for years before and after conversion relative to the year of conversion, separately for English and maths. The pre-conversion coefficients provide evidence on the trends before conversion, while the post-conversion coefficients reflect the dynamic treatment effects of academization.

We find that the pre-conversion coefficients are positive and significant at the 10% level, suggesting a downward trend in KS4 performance after conversion. One-year post-conversion, the estimated effect is close to zero and statistically insignificant in both subjects, indicating no immediate impact. A negative and significant effect on maths attainment emerges only two years after conversion, with no comparable effect on English. These results

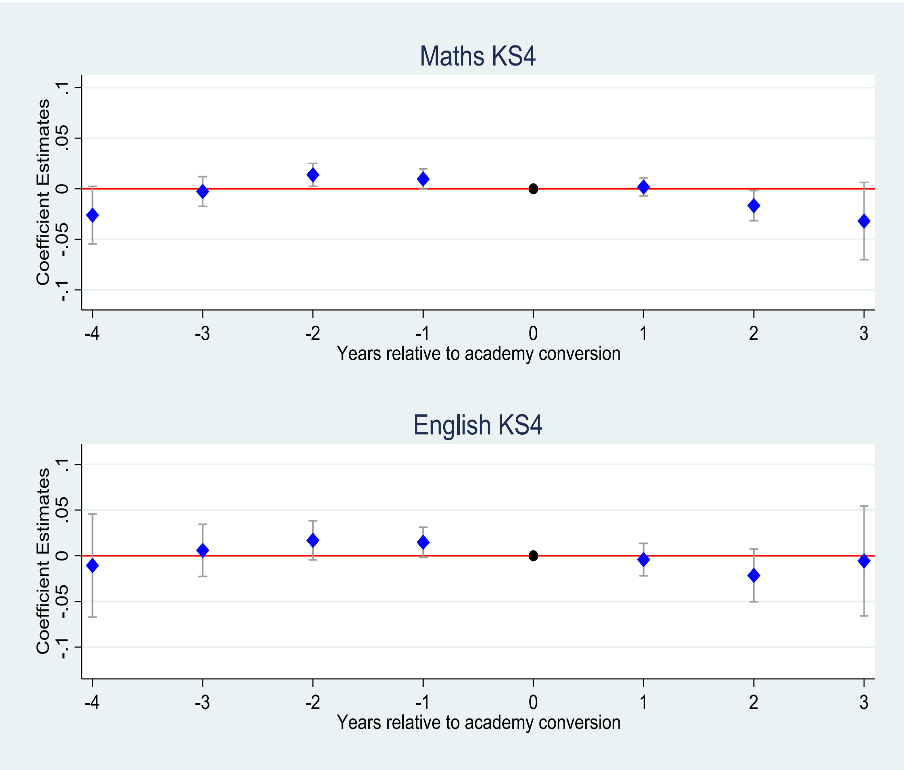

Fig. 1 Effect of conversion timing on KS4: years before and after conversion. Notes: *** $p<0.01$, ** $p<0.05$, * $p<0.1$. School fixed effect model estimated. Residual clustered at school level. Controls: KS1 Maths, KS1 Reading, KS1 Writing, KS2 Maths, KS2 English, EAL, FSM (up to 11), Statement or EHC plan (up to 11), SEN (up to 11), Ever looked after up to age 16, Pupil had ever been in need, Female, Unauthorized absence up to 12, Ethnicity dummies, IDACI score decile dummies, year dummies, dummies for missing KS2 and missing unauthorized absences

are consistent with the evidence provided by Andrews et al., (2017), who report modest declines in KS4 attainment following conversion and that this effect is first apparent at least year before conversion takes place, indicating it does not necessarily reflect the impact of academisation per se.

Further evidence from Hill et al. (2017) supports this position by highlighting that long-term improvements in school performance, often attributed to academization, are typically driven by changes implemented within the first three years. However, these performance gains are not always immediate. In fact, schools that achieved the most significant long-term improvements typically did not see measurable test score gains until the third year of implementation. This delayed effect supports the argument that any positive impact from academization on academic performance is gradual, not instant.

Evidence from Ingram et al. (2018) suggests that some schools may resort to alternative practices to improve their GCSE performance, as excessively high exclusion rates can raise concerns. In an attempt to enhance their GCSE performance metrics, certain schools have employed strategies that effectively distance students, particularly those deemed less likely to succeed, from the exam process. One such strategy involves limiting the number of students sitting for their GCSEs by placing them in home-schooling programs, off-site provisions, or transferring them to other schools where they would not be included in the school's official exam cohort. By excluding these students from the exam process, schools can reduce the number of lower-performing students whose results might negatively impact the overall GCSE success rates.

We assess whether academies have adopted this strategy by examining changes in the number of students who leave during Year 11. While it is difficult to distinguish between students who leave due to gaming practices and those who leave for other reasons, we argue that this distinction should not pose a significant issue, provided there is no substantial difference in the reasons for student departures between academies and non-academies.

We re-estimate Eq. (1) replacing youth custody with a dummy identifying pupils leaving in Y11. Results in cols 1 in Table 10 show that academization has no impact on this outcome, providing reassurance on the instrument not operating through changes in students sitting the final GSCEs assessment.

Table 10 The effect of academization on leavers and absences

	(1)	(2)
VARIABLES	Leaver	Unauthorised absences
Academy	0.0006	0.0005
	[0.001]	[0.001]
Constant	0.0070***	0.0117***
	[0.000]	[0.000]
Mean outcome	0.0119	0.0177
Observations	708,241	706,419

*** $p<0.01$, ** $p<0.05$, * $p<0.1$. School fixed effect model estimated. Residual clustered at school level. Controls: KS1 Maths, KS1 Reading, KS1 Writing, KS2 Maths, KS2 English, EAL, FSM (up to 11), Statement or EHC plan (up to 11), SEN (up to 11), Ever looked after up to age 16, Pupil had ever been in need, Female, Unauthorized absence up to 12, Ethnicity dummies, IDACI score decile dummies, year dummies, dummies for missing KS2 and missing unauthorized absences

We conducted a falsification test on unauthorised absences, a proxy for disruptive behaviour not directly linked to academization. Results in column 3 in Table 10 shows that the effect estimate of academization is zero, supporting the exclusion restriction.

While school-level fixed effects account for unobserved differences that may be correlated with the timing of academization, concerns may still arise regarding unobservable, time-varying school-level factors that influence the timing of conversion. To assess this, we examine pre-2010 trends in exclusion rates, estimating the equivalent of Eq. 3, but replacing the dependent variable with school level exclusion rates. Our analysis relies on total exclusion rates across year groups from 2006/07, as these data are publicly available and this was the first year exclusions data was collected using School Census.

Figure 2 displays the estimated coefficients for the years preceding and following academy conversion, using the year of conversion as the reference point. The average fixed term exclusion rate across all year groups is 6.26. The value is approximately 4 percentage points higher than the Year 11 exclusion rate observed in the data. This difference reflects the tendency of schools to issue more exclusions in earlier school years compared to Year 11.

The coefficients for the years prior to conversion test the parallel trends assumption. The figure suggests converter schools were experiencing declining exclusion rates before conversion. Given that our estimated impact of academization on exclusion rates is positive (indicating an increase in exclusions post-conversion), this pre-existing downward trend, if anything, suggests that our estimated positive impact is downward biased, i.e. likely smaller than the true underlying effect would be in the absence of the pre-trend.

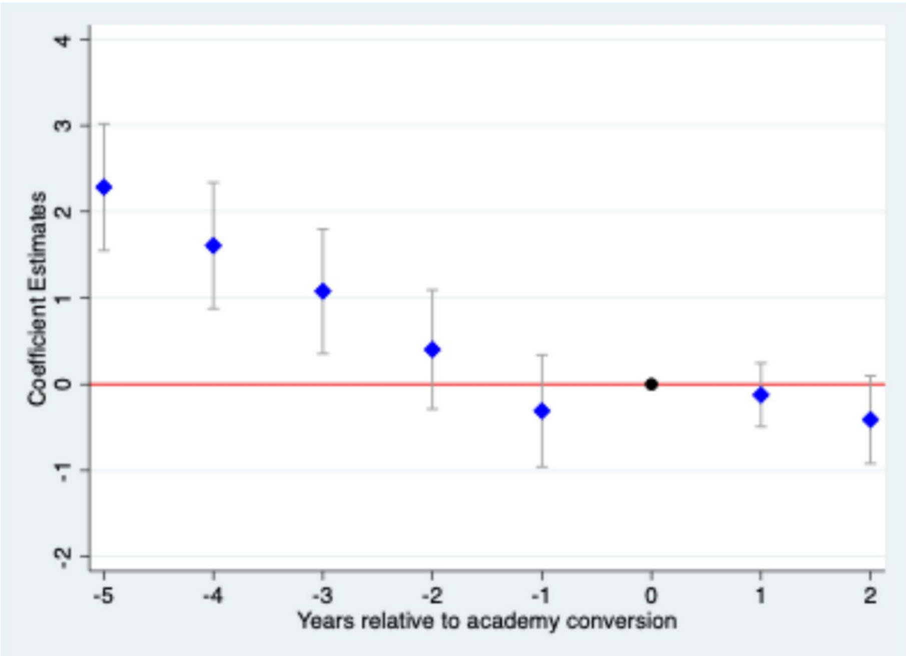

Fig. 2 Effect of conversion timing on fixed term exclusion rates: years before and after conversion. Notes: *** $p<0.01$, ** $p<0.05$, * $p<0.1$. School fixed effect linear regression estimated. The dependent variable is the school-year fixed term exclusion rate across all year groups. Standard errors clustered at school level

The δ_{-1} coefficient captures the average difference in KS4 attainment between schools one year prior to conversion ($k = -1$) and the comparison group of schools that had not yet converted, relative to the baseline period, the year of conversion ($k=0$). The δ_{-2} coefficient similarly captures this difference for schools two years prior to conversion, relative to the year of conversion. Neither the δ_{-1} and the δ_{-2} are statistically significant. This suggests that for schools that converted within two years of each other, their pre-conversion KS4 attainment trends were broadly parallel, lending credibility to the parallel trends assumption for immediate pre-conversion periods. However, the δ_{-3} coefficient is statistically significant at the 95% level, indicating a divergence in pre-trends for schools three years prior to their conversion. This suggests that groups of schools whose conversion dates were separated by three or more years may have had different underlying exclusion trajectories even before conversion.

Our sample design, with schools converting between 2010/11 and 2013/14, means that comparisons for shorter pre-conversion periods (up to two) are more abundant and typically involve 'cleaner' control groups of schools that will convert later, providing a more relevant counterfactual. Comparisons involving schools that converted in 2011/12 or 2012/13 are limited to differences of no more than two years, supporting the credibility of the parallel trend assumption. Assuming the trends divergence hold for Y11 exclusions too, we re-estimate the main model excluding 2013/14 converters to ensure robustness in comparisons involving the 2010/11 group. This adjustment reduces the sample size by approximately 10 percent. The revised 2SLS estimate of the effect of exclusion on youth custody is 2.1 percentage points, 0.5 percentage points lower than the estimate reported in Table 6. The standard error remains unchanged, resulting in a slight drop in statistical significance to the 88 percent level.

As discussed, another sensitivity analysis involves estimating impacts only for schools without a 6th form, since in such schools there is no scope for academization to exert an influence post year 11 through improved teaching practice, for example. Results in Table 11 indicate that the size of the effects in schools without 6th form are substantially higher than in the full sample of schools, and significant. Academization is associated with a 1.35 percentage points increase in the probability of exclusion, slightly higher than the estimated increase in the full sample (about 1 percentage point). The impact of exclusion on custody, significant at 99 percent level, is 5.92 percentage points, more than double the impact estimated for the full sample.

A possible explanation for the higher coefficient could be that schools without a sixth form differ from those that offer a sixth form, potentially serving a more disadvantaged student population, who do not wish to study beyond year 11. This intuition is supported by a comparison of the average pupil characteristics between schools with and without sixth forms. Pupils in schools without sixth forms tend to have lower KS2 and KS4 attainment on average, a higher incidence of FSM eligibility, and reside in areas with higher deprivation indices.[12]

This evidence highlights that exclusions result in higher youth custody for pupils in disadvantaged settings, where lower socio-economic backgrounds and possibly weaker school resources amplify students' vulnerabilities. In contrast, outstanding schools, which often serve pupils from stronger socio-economic backgrounds and benefit from more robust sup-

[12] Tables available upon request.

Table 11 2SLS estimates of exclusion on youth custody, on schools with no sixth form

	(1)	(2)
VARIABLES	Y: custody before 18	Y: custody before 18
If student excluded	0.0611***	0.0592***
	[0.023]	[0.022]
First stage		
Academy	0.0130***	0.0135***
	[0.001]	[0.001]
Individual controls	No	Yes
F-test	80.11	88.45
Prob>F	0.000	0.000
Mean youth custody	0.0007	0.0007
Observations	185,383	185,383

*** $p<0.01$, ** $p<0.05$, * $p<0.1$. School fixed effect two stages least square linear regression estimated. Standard errors clustered at school level. Controls: KS2 Maths, KS2 English, EAL, FSM (up to 11), Statement or EHC plan (up to 11), SEN (up to 11), Ever looked after up to age 16, Pupil had ever been in need, Female, Unauthorized absence up to 12, Ethnicity dummies, IDACI score decile dummies, year dummies, dummies for missing KS2 and missing unauthorized absences

port systems, see an increase in youth custody following exclusion that is half the rate observed in more disadvantaged schools.

Finally, we present qualitative evidence on the changes implemented post-academization. We draw on the Department for Education (2014) survey, which captures headteachers' reported changes following academization, to argue that the reforms implemented by academies are unlikely to directly affect the probability of youth custody for pupils enrolled in these schools. The Department for Education distributed a questionnaire to all 2,919 academies open as of May 2013, encompassing primary and secondary schools as well as both converter and sponsor academies. The survey received 720 responses detailing the ways academy freedoms were utilized, as summarized in Table 12 below.

The survey findings indicate that academies primarily implement changes in operational and administrative aspects. These include adjustments related to procurement, cost savings, and capital expenditure, as well as governance reforms, such as reconstituting governing bodies and modifying school leadership structures. Additionally, academies frequently report changes in teacher pay structures and increased collaboration with other schools.

The only change potentially linked to student outcomes is curriculum modifications; however, such changes would typically be expected to impact academic attainment, which, as shown in Tables 10 and 11, was not observed. Therefore, in the short term, this evidence provides supportive arguments in favour of the hypothesis that academization is unlikely to have significantly impacted other dimensions correlated with youth custody, beyond the enforcement of stricter disciplinary policies.

While disciplinary policies are not explicitly listed, they play a crucial role in the leadership change, as the authority to exclude a student from school lies with the Head of School, who is often replaced during the transition to an academy. Additionally, the report highlights the adoption of zero-tolerance policies for poor behaviour as another major change, alongside those listed in the table.

Table 12 Changes made by academies by opening date

Changes made by academies by opening date	% of Academies
Procured services that were previously provided by the LA	87
Collaborated with other schools in more formalised partnerships	60
Changed your pattern of capital expenditure	59
Introduced savings in back-office functions	58
Reconstituted your governing body	58
Changed the performance management system for teachers	56
Changed the curriculum you offer	55
Added non-teaching positions	48
Changed school leadership	47
Introduced or increased revenue-generating activities	35
Increased the number of pupils on roll	33
Changed staff pay structures	24
Changed your admission criteria	22
Hired teachers without qualified teacher status	16
Sought to attract pupils from a different geographical area	10
Increased the length of the school day	8
Changed the length of school terms	4
Reduced the number of pupils on roll	2

Weighted average computed across schools converted to academies in each year between 2010–2013. Elaborated from DfE report, 2014

Conclusions

Largely correlational evidence indicates that students excluded from school are more likely to engage in antisocial and/or criminal behaviour. The literature shows a positive correlation between school exclusion and later-life involvement with the criminal justice system. Despite the widespread use of disciplinary policies, a crucial aspect of managing student behaviour in education production, there is very little evidence on the causal relationship between school exclusion and crime. In this paper, we use novel administrative data comprising education records and custody outcomes to study the impacts of school exclusion on youth custody. As a result of linkages in administrative datasets, we were able to look at the trajectories of pupils from education through to young adulthood. We leverage policy-induced variations in the likelihood of school exclusion to determine its effect on the probability of custody. We show that academization significantly increases the probability of exclusion in year 11.

Using academization as an instrumental variable, we provide evidence of a statistically significant effect of exclusion on the probability of youth custody. Although small in absolute terms – roughly 2.6 percentage point – it is a notable increase when seen in the context of the very small proportion of young people experiencing custody (<1 per cent).

In schools with a more disadvantaged student body, higher rates of exclusion lead to two times higher rates of custody than in high-performing schools, where mostly high-ability students face a greater risk of youth custody following exclusion. This suggests that exclusion policies may worsen the outcomes for vulnerable students in lower socio-economic environments.

Given the costs – both monetary and non-monetary – associated with both exclusion and custody, this finding has potentially important implications. In particular, it underscores the need for headteachers, teachers, and policymakers to understand the risk consequences of disciplinary practice at schools and the relevance of the context where the exclusion happens. Furthermore, custody is an end-point of often lengthy criminal justice processes; it is reserved for more serious offenses, and its use for under-18 s has been steadily decreasing in recent years (Bowyer et al. 2023). There is significant attrition between crimes committed and final criminal justice outcomes, particularly custody. Consequently, we are examining the narrow end of the criminal justice 'funnel', since very few young people ultimately go to prison. Crimes leading up to prison and crimes resulting in a lesser punishment are not observed in the available data. Examining the effect of exclusion on those outcomes remains the focus of future studies.

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Data Availability The data supporting the findings of this study are available from the Secure Research Service (SRS). However, due to confidentiality and the secure settings under which the data were used, access to these data is restricted and they are not publicly available.

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