School Management Takeover, Leadership Change, and Personnel Policy*

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Abstract

Low-performing schools usually lack efficient governance and struggle to attract and retain high-quality teachers. This paper studies a large-scale reform that enables independent organizations, including educational charities and private businesses, to take over the management of under-performing, disadvantaged schools, while funding remains public. Exploiting the staggered introduction of English sponsored academies from the early 2000s onwards, we show that sponsor-led takeovers lead to substantial changes in the school personnel policy and the teaching body. First, the probability that sponsors appoint a new headteacher doubles upon the takeover. New headteachers are, on average, less experienced, but more likely to come from outstanding schools and better paid. Second, the takeover induces teacher sorting, increasing the probability that more experienced and lower-achieving teachers leave, while new hires are younger, more likely to be *Teach First* teachers and to come from outstanding schools. Third, and likely contributing to explain these dynamics, sponsors substantially re-design teachers' pay structure and abandon a seniority-based pay scale, leading to an increase in pay dispersion across equally experienced teachers. Finally, private businesses are more likely to appoint a new headteacher from outside the education sector and attract *Teach First* teachers, potentially contributing to better student performance.

JEL codes: I28; J13; J18.
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1 Introduction

One of the most pressing issues that education ministries around the world constantly face is how to improve low-performing, disadvantaged, public schools. While extensive literature shows that teachers are the most important input for students' outcomes (Rockoff 2004, Rivkin et al. 2005, Rothstein 2010, Chetty et al. 2014, Jackson 2018, Gibbons et al. 2021), a key challenge that such schools experience is how to attract and retain high-quality teachers (Clotfelter et al. 2008, Glazerman et al. 2013, Springer et al. 2016). An increasing number of papers have studied different options to improve teacher recruitment and retention, including financial and career incentives (Swain et al. 2019, Bobba et al. 2021, Morgan et al. 2023), or behavioral strategies (Ajzenman et al. 2024). In parallel, a consolidated literature shows that outsourcing the management of under-performing public schools to external, usually non-profit organizations, is an effective way to improve pupil performance, especially among low-SES students (Dobbie and Fryer Jr 2013, Abdulkadiroğlu et al. 2016, Eyles et al. 2016, Andrews et al. 2017, Eyles and Machin 2019, Romero et al. 2020).

This paper combines these two strands of the literature by considering a large-scale education reform which enables a range of organizations, including educational charities, businesses, religious organizations, and universities, to take over the management of low-performing, disadvantaged, state-funded schools, and studying how this change in management affects the headteacher selection, teacher turnover, sorting, and pay.

Over the last twenty years, English state-funded secondary schools have been allowed to acquire the status of charitable trusts called academies to gain autonomy from the local authority (LA) over major aspects of the school governance. What makes academies especially interesting to study is that the conversion process had been initially conceived as a remedial program for low-performing, disadvantaged schools (Eyles and Machin 2019). When the Department for Education (DfE) identifies a struggling school, it matches it with an independent organization interested in taking over the management of the school, defined as a sponsor. Upon the DfE's approval of the conversion application, while funding remains public, the sponsor takes charge of the school by nominating a board of trustees that, among other things, becomes responsible for setting the school direction and ethos, managing the school budget, and hiring and paying the headteacher and teachers. For this reason, schools acquiring academy status through this route have come to be known as sponsor-led academies, or sponsored academies.

By the school year 2022/23, 25 percent of all secondary schools in England have been converted to sponsored academies.[1] Importantly, by now there is ample evidence that sponsor-led academies have been effective at improving student achievement and educational attainment (Eyles

[1]The 2010 Academies Act also introduced the academy model in the primary sector (see Eyles et al. (2017)). By the school year 2022/23, 10 percent of primary schools have been converted to sponsored academies.

et al. 2016, Andrews et al. 2017, Eyles and Machin 2019). However, as in the case of charter schools in the US, it is still debated what the mechanisms behind these successful stories are (Adonis 2012, Angrist et al. 2013, Dobbie and Fryer Jr 2013, Fryer Jr 2014, Cohodes and Parham 2021, Silva et al. 2023). This paper focuses on the impact of these school management takeovers on sponsors' governance and personnel policy.[2]

To conduct the analysis, we combine school- and teacher-level panel data spanning the entire period of academies' expansion, creating an England-level matched employer-employee data set of schools and teachers. To identify causal effects, we implement two complementary identification strategies. First, we estimate a difference-in-differences (DiD) model that compares the evolution of the outcomes of interest around the year of conversion (from 5 years before to 4 years after) in schools that experience the takeover in a certain year and schools that become sponsored academies a few years later. To mitigate concerns related to the fact that the timing of the takeover is not random, we show that our main findings are unchanged when using a matched difference-in-differences design that compares the evolution of the outcomes of interest in schools experiencing the takeover and schools that are never taken-over, but experience very similar dynamics in pupils' and teachers' characteristics in the years preceding the takeover announcement in treated schools.

The results show that the sponsor-led takeover leads to profound changes in the school leadership, teaching body, and personnel policy. First, the probability that the board of trustees appoints a new headteacher more than doubles in the conversion year in treated vs. control schools, and, on average, it increases by 44 percent after the conversion period compared to the pre-takeover mean. This result echoes the effect of changing school superintendent on the headteacher selection found in Lavy et al. (2023). Importantly, the new headteacher is less experienced (3 percent decrease compared to the pre-takeover mean), but also significantly more likely to hold a master's degree (6 percentage-point increase or 10 percent relative to the pre-takeover mean), better paid (10 percent increase, or around £8,300 - $10,700 - per year),[3] and 3 times more likely to come from an outstanding school in management quality (as certified by the independent inspection authority, the Office for Standards in Education, or OFSTED hereafter). Overall, these results suggest that sponsors have a clear strategy to start improving the school from its leadership.

Second, the change in management generates a sorting effect that echoes the impact of performance-

[2]Eyles and Machin (2019) provide some preliminary evidence that the first sponsored academies, i.e. school takeovers taking place in the early 2000s, changed their headteacher and increased the number of teachers. We substantially expand this analysis by covering the entire period of expansion of sponsored academies and using individual-level teachers' data, which allows us to study the impact of the school takeover on the characteristics of the headteacher, teacher turnover, sorting and pay.

[3]As we will explain more extensively in the next sections, sponsored academies do not obtain any permanent increase in resources. We do not find any evidence either that sponsors provide additional funding to the school. Thus, any increase in the headteacher or teachers' salaries has to come from a more efficient use of available resources. Importantly, note that the increase in the principal's salary represents only the 0.2 percent of the annual average spending of treated schools.

related pay contracts on employees' selection found in the personnel literature (Lazear 2000, Gielen et al. 2010, Leaver et al. 2021). Once the takeover is initiated, there is a large increase in teachers' separations, partly driven by more experienced teachers, a result that mimics the impact of appointing effective public-sector managers found by Fenizia (2022) in the context of the Italian Social Security Agency. Once the takeover is completed, teacher turnover drops by 5.4 percentage points (p.p. hereafter), or one third compared to the pre-conversion mean, but teachers who leave for other schools right after the takeover are significantly less likely to go to an outstanding school in teaching (-1.6 p.p. or 18 percent drop compared to the pre-takeover mean). In parallel, the school takeover leads to a 4 p.p. increase in the share of newly hired teachers, or 26 percent compared to the pre-conversion mean. In stark contrast with leavers, new teachers are significantly younger (1 percent compared to the pre-takeover mean), but 2.5 p.p. or 42 percent more likely to come from an outstanding school in teaching, and 2.1 p.p. or 1 time more likely to be *Teach First* teachers - England's equivalent of *Teach for America* teachers. While, unfortunately, we cannot match pupils to teachers and thus compute teacher-value added measures, these dynamics suggest that the sponsor-led takeover is accompanied by the injection of personnel policies that push away potentially less motivated teachers and attract high-productive ones.

To investigate this hypothesis further, we analyze the impact of the takeover on teacher pay, separately for incumbent and newly hired teachers. The key finding of this part of the analysis is that sponsors abandon a pay scale entirely based on seniority. In detail, by running teacher-level regressions that control for teacher-fixed effects, on top of school and year fixed effects, we find that the new school governing body does not change incumbent teachers' gross pay on average, but returns to experience significantly weaken in treated schools compared to control ones after the takeover. As for new hires, they instead enjoy a significant, but relatively small pay increase of 2 percent per year, on average, or around £840 ($1,100), compared to newcomers in control schools. In turn, the new pay policy adopted by sponsors results into a 9 percent significant increase in pay dispersion across equally experienced teachers. Combined with the insights of a 2014 DfE survey showing that 80 percent of existing sponsor-led academies had changed their performance management system for teachers (Cirin 2014), this result points to increased sponsors' efforts to link pay to performance, echoing the effects of the Wisconsin teacher pay flexibility reform studied by Biasi (2021).

With these results at hand, we conclude the analysis in two ways. First, we conduct a mediation analysis to investigate whether and to what extent sponsors' personnel policy helps explain the improvement in student performance brought about by sponsored academies. We find that changes in teachers' composition, and specifically the increase in the share of *Teach First* teachers and in the share of teachers coming from outstanding schools, explain one fourth of the effects of the takeover. In other words, while sponsors' personnel policy is not the only factor behind the

success of sponsored academies, it plays a non-negligible role in explaining it.

Lastly, we study heterogeneous effects across sponsor types, comparing in particular private businesses to other organizations. For this, we first have to manually identify and classify each academy's sponsor, as the school data only provide the academy trust's name, and there is no official registry of the nature of the organization behind it. We find that, by the end of the sample period (2018/19), educational charities manage 64 percent of sponsored academies, followed by businesses with 16 percent, religious organizations with 10 percent, and universities with 8 percent, while we are not able to recover the sponsor' type for 1.5 percent of schools. Importantly, although sponsors are not randomly assigned to schools, we do not find any significant selection pattern in the characteristics of schools taken over by businesses. In terms of heterogeneous effects, we find suggestive evidence that businesses improve student performance more than other sponsors without bringing more financial resources to the school. Remarkably, they are also more likely to select the new headteacher from outside the education sector and, consistent with the mediation analysis, they are more likely to attract *Teach First* teachers.

These results contribute, first of all, to improve our understanding of why management flexibility has proven to be effective at raising pupil performance (Clark 2009, Dobbie and Fryer Jr 2011, Fryer Jr 2014, Dobbie and Fryer Jr 2015, Abdulkadiroğlu et al. 2016, Eyles et al. 2016, Eyles and Machin 2019, Dobbie and Fryer 2020, Cohodes et al. 2021). In particular, to the best of our knowledge, there is very little evidence on the causal impact of management flexibility on school personnel policy.[4] Our paper shows that effective managers exploit autonomy over hiring and pay to appoint a high-quality headteacher and reshuffle the teaching body, by attracting young and high-performing teachers and inducing the potentially least motivated to leave. In this respect, the closest papers to ours are Romero et al. (2020) and Romero and Sandefur (2022) who study, respectively, the short and long-term impact of a small-scale program taking place in Liberia where the government subsidized private organizations to take over the management of 9 percent of randomly selected public schools. Similar to us, the authors find that the private providers invest in less experienced, but potentially higher-quality teachers. Importantly, in the Liberian context, the management takeover is accompanied by a permanent and large increase in school resources, which is not the case in England, and the program does not necessarily target disadvantaged, struggling schools, which are the focus of our analysis.[5] A related and growing literature provides descriptive and causal evidence that management quality matters for student performance (Bloom et al. 2015, Fryer et al. 2017, Lavy et al. 2023, Hanushek et al. 2024).[6] Our paper complements these studies

[4] A few studies offer insightful descriptive analysis on the characteristics of teachers moving to or leaving charter schools (Baker and Dickerson 2006, Carruthers 2012, Bruhn et al. 2022), and Jackson (2012) and Sorensen and Holt (2021) study the impact of charter schools on teacher quality in surrounding public schools.

[5] As shown by Akhtari et al. 2022, a reshuffle of the teaching body can also be harmful to student performance when spur by political turnover in local elections.

[6] Studies in this strand of the literature include those that measure managerial practices through headteacher inter-

by identifying the personnel practices of effective school managers.

Second and related to this, the paper speaks to the few but growing number of papers that study the impact of pay policy reforms on teacher turnover and pay, and on student performance (Hoxby 1996, Lovenheim and Willén 2019, Biasi 2021, Biasi et al. 2021, Willén 2021, Biasi and Sarsons 2022, Burgess et al. 2022, Hanushek et al. 2023). In a nutshell, this strand of the literature finds that granting school districts (or schools) pay flexibility increases the returns to teacher quality, thus raising schools' chances to attract and retain high-quality teachers, with further positive knock-on effects on student performance. We complement these studies by showing that abandoning a seniority-based pay scheme is especially useful for disadvantaged, failing schools to poach teachers from high-performing schools and attract bright and fresh university graduates into the teaching profession.

Third, this paper complements studies that analyze the impact of financial and non-financial incentives for teachers working in disadvantaged schools (Clotfelter et al. 2008, Lavy 2008, Glazerman et al. 2013, Springer et al. 2016, Swain et al. 2019, Ajzenman et al. 2024, Cabrera and Webbink 2020, Bobba et al. 2021). Taken together, these studies show that monetary bonuses are effective at decreasing teacher turnover, but are often poorly targeted, have mixed effects on pupil achievement, and are costly to scale up.[7] Relative to these studies, our analysis shows that, rather than relying on large financial incentives to attract new teachers, English sponsors invest in promoting practices that reward performance more than seniority. Documenting this strategy is important, when considering that the program of sponsor-led academies has been scaled up effectively nation-wide in the last 20 years.

More broadly, these findings help shed light on what effective public-sector managers do (McCormack et al. 2014, Rasul and Rogger 2018, Janke et al. 2019, Bandiera et al. 2021, Fenizia 2022, Otero and Munoz 2022). Causal evidence on the personnel practices adopted by good managers in public sector organizations is scant, and our paper contributes to show that injecting a culture of high-performance is an effective way to induce less motivated employees to leave and high-achieving ones to join in settings where firing is not an option.

The remainder of the paper proceeds as follows. Section 2 describes the expansion of English sponsor-led academies. Section 3 presents the data and summary statistics. Section 4 illustrates the

views based on the World Management Survey methodology (Bloom et al. 2015, Lemos et al. 2021, Di Liberto et al. 2023), papers that estimate superintendent and principals' value-added (Coelli and Green 2012, Böhlmark et al. 2016, Grissom et al. 2021, Muñoz and Prem 2022, Lavy et al. 2023, Hanushek et al. 2024), and studies that estimate the impact of management training on pupil achievement (Fryer et al. 2017, Muralidharan and Singh 2020, Romero et al. 2022).

[7] With the notable exception of the North Carolina teacher bonus studied by Clotfelter et al. (2008) that only granted a 4% average increase in teacher salary, the incentive programs to attract teachers in hard-to-staff schools that have been studied in the literature usually boost teachers' salary by between 10 and 30 percent per year (Glazerman et al. 2013, Cabrera and Webbink 2020, Bobba et al. 2021).

identification strategy. Section 5 reports the main results, and Section 6 compares the results across the two complementary identification strategies. Section 7 presents the heterogeneity analysis. Section 8 concludes.

2 Institutional setting

The idea of creating the institutional model of sponsored academies emerged in the early 2000s, when a mounting consensus emerged in the English educational community that many secondary schools, and especially those located in poor urban neighborhoods, were failing to provide an adequate educational level to their pupils.[8] To tackle this situation, the then Labour government opted for radically changing the management of low-performing schools. This idea was based on the small-scale experience of the 15 City Technology Colleges (CTCs) created at the end of the 1980s mostly as new independent state-funded schools, managed by businesses and geared towards science, mathematics, technology. Compared to CTCs, Blair's government decided to focus on existing failing, disadvantaged schools and envisaged a large-scale program whereby a broader set of sponsor-managers, including educational charities and private businesses, but also religious organizations, universities and successful schools, would take over the management of the school from the Local Authority, while continuing to receive public funding directly from the State (Adonis 2012).

The takeover process has remained substantially the same since then (Cirin 2014, DfE 2016, DfE 2020). An organization or individual interested in taking over the management of a school, defined as a sponsor, submits an expression of interest to the DfE, where it has to demonstrate it has the skills and expertise to help schools improve. If approved, the DfE matches the sponsor, with a low-performing school, usually located in the same area where the sponsor operates. Importantly, low performance in this context is not based on a single indicator, but rather on a combination of persistent low levels of pupils numbers, unsatisfactory pupil results and negative school OFSTED ratings. The takeover is completed once the sponsor obtains the DfE's approval to convert the school into a charitable trust, a process that presumably takes more than a year to be completed. While data on the application process are only available for a small subset of school conversions (14 percent), we show in the online appendix that, indeed, the median number of months to complete the takeover process is 18 months in these schools (see Appendix Figure B.1), which will be important to bear in mind to understand the timing of the takeover effect.

[8] Secondary school in England comprises grades 7 to 11 (age 11-12 to 15-16). At the end of grade 11, pupils sit externally marked standardized academic (General Certificate of Secondary Education, GCSE) and/or vocational (National Vocational Qualifications, NVQ/British and Technology Council, BTEC) exams. For more information on the structure of the English educational system, see Appendix A.

Upon conversion, the sponsor nominates a board of trustees, usually composed of educational experts, that becomes the decision-making body of the trust. The conversion grants the school board full autonomy from the LA in terms of the organization of the school curriculum, the structure of the school day and year, spending allocation, headteacher and teacher hiring, pay and working conditions. In exchange for these freedoms, the sponsor needs to offer a long-term commitment to run the school and improve pupils' outcomes. Also, the sponsor cannot set selective admission criteria, nor charge fees.

Importantly and related to this, academy trusts are, by definition, non-profit entities, and remain publicly funded. The conversion year, the DfE provides sponsors with a one-off grant (amounting to at most to £400k for a typical school with a capacity of 1000 pupils seats pre-takeover, or 7 percent of the median annual school income) to cover the cost of the conversion process, refurbish the school, buy new uniforms or equipment, and building leadership capacity. Otherwise, like the typical state-funded school, sponsored academies keep receiving funding based on the number of pupils enrolled, as their predecessor schools did. Sponsors can provide additional resources, but we will show that, except for the temporary increase in the school income corresponding to the DfE transfer, converted schools do not experience a further increase in their resources.[9]

Students already enrolled in the school are granted a place at the converted school. Similarly, teachers already employed at the school are (in principle) guaranteed their job at the academy, and retain the pay and working conditions negotiated with the original school, although sponsors can change the allocation of tasks across teachers. Moreover, the sponsor-managed school can negotiate different pay and conditions for newly hired teachers.

Finally, sponsor-managed schools are subjected to the same accountability mechanisms of LA-managed schools, including regular inspections by the independent authority OFSTED and the annual publication of pupil performance tables.

The first three school takeovers took place in the school year 2002/03. Over the following years, the program scaled up and by September 2010, 205 sponsored academies were running. To boost school autonomy at every level of the educational system, the Academies Act, issued in May 2010 by the newly elected Conservative government, introduced a new and simplified conversion route in both the primary and secondary phases. In particular, schools willing to convert under this route did not have to find a sponsor anymore, and, for this reason, the new autonomous schools were simply named *converter academies*. Importantly, however, the Academies Act maintained the rule that, at both primary and secondary level, low-performing schools are only converted through

[9] At the launch of the program, sponsors in principle had to mandatory contribute to capital spending. The initial contribution was set at £2 million, then in 2007/08 this was changed to an equivalent endowment fund. Acknowledging that very few sponsors had fulfilled this requirement, the government finally lifted it in 2010 (National Audit Office 2010).

the sponsor route. By the school year 2022/23, 25 (10) percent of secondary (primary) schools have been converted into sponsor-led academies and 53 (29) percent have acquired the status of converter academy (see Appendix Figure B.2) .

This paper focuses on sponsor-led academies for two reasons. First, the fact that sponsors take over the management of under-performing, disadvantaged schools makes this institutional model especially important to study from a policy point of view. Second, our analysis may help understand to what extent the positive effects that sponsored academies have had on students' outcomes can be explained by the impact of the school takeover on the governance and personnel policy adopted by the sponsor (Eyles et al. 2016, Andrews et al. 2017, Eyles and Machin 2019).[10]

***Teach First* program**. As this feature of the setting analyzed will play an important role in sponsors' personnel policy, we close this section by giving more details on this program. First introduced in England by the educational charity *Teach First* and drawing inspiration from *Teach for America* (Decker et al. 2004, Kane et al. 2008, Antecol et al. 2013, Xu et al. 2011), *Teach First* is an intense six-week teaching-training program that is offered to high-performing fresh university graduates and career changes as a fast track to obtain qualified-teacher status, in exchange for participants' commitment to teach in disadvantaged schools for at least two years. Despite the fact that a lower percentage of *Teach First* teachers remain in state-funded schools after this period compared to traditionally trained teachers of similar age, Allen and Allnutt (2017) find positive effects on pupil performance in end-of-high-school centralized exams in schools participating in the scheme compared to a matched control that participates in later years. These findings echo similar evidence on *Teach for America*, which appears to be especially effective at improving outcomes of high-school students (Xu et al. 2011). It will be important to keep these results in mind to better understand sponsors' hiring strategies.

3 Data and summary statistics

To study how the sponsor-led takeover affects the headteacher selection and teacher turnover, sorting, and pay, we built a unique individual-level teacher panel data set spanning the entire period of school takeovers up to 2018/19,[11] by combining the Database of Teacher Records (DTR) with the School Workforce Census (SWC). The DTR, which provides information on teachers' characteristics as of May of each school year, has been used by DfE for the management of teachers' pension system since the early 1990s. We have access to it from the school year 2001/02 to 2009/10. From the school year 2010/11 onwards, the DfE has discontinued access to the DTR, but has made avail-

[10] While it would be interesting to study the impact of sponsor-led takeovers before and after the expansion of converter academies, the available data only allow us to focus on takeovers taking place from 2007/08, all of which, at least partly, overlap with the emergence of the converter model - see Section 3.

[11] This is the last school year before the COVID-19 pandemic struck and where our analysis stops.

able to researchers the SWC, a teachers' census conducted every year in November that focuses on state-funded schools and is supplemented with information on teachers' qualifications, subject taught, and absences.[12] The DfE has created a unique anonymized teacher identifier for this project that allows us to follow the same teacher across the two data sets. Both data sets provide consistent information on teachers' roles in the school (classroom teacher vs. headteacher), gender, age, full-time status, qualified teacher status, teachers' annual gross and base pay and additional payments. Because a tenure variable is only available in the SWC, we calculate experience as years elapsed since the acquisition of qualified teacher status, which normally takes place just before or after starting the teaching career. This information is missing or not reliable for around 2 percent of the sample. From the qualification data, we construct an indicator variable equal to one if the teacher has at least a master's degree. For one fourth of teachers in the sample, we also know their GPA in the highest degree attained, from which we construct an indicator variable equal to 1 if the teacher graduated with a first-class degree, that means having a GPA in the top 30 percent of a student's cohort.

Both data sets provide a consistent school identifier that we use to match them with school-level data. In particular, we merge the teacher data with publicly-available data extracted from "Get information about schools" (GIAS), a website managed by the DfE and covering all schools in England since the school year 2001/02. Among other things, GIAS provides key information on school phase, type, and LA identifiers, as well as information on pupils' characteristics, such as the share of students eligible for free school meal (FSM), the share of white British pupils, and the share of English native students. We further supplement the resulting data set with: DfE data on schools' conversions, amalgamation and splits, which are crucial to follow a school over time given that the school identifier changes when one of these events takes place; annual data on pupils' performance in externally marked standardized tests taking place at the end of high-school (11th grade) available from the school year 2005/06 onwards; specifically, these data report the headline measure of pupil performance corresponding to the share of students achieving at least 5 A*-C grades in the General Certificate of Secondary Education (GCSE), with a "C" corresponding to a sufficient level of learning. We further use official and standardized indicators of school quality provided by the independent authority OFSTED, whose inspectors periodically visit schools and issue rating scores regarding several dimensions, such as overall effectiveness, and teaching and management quality; these data are also available from the school year 2005/06 onwards; and annual data on school resources and expenditure, also available from the school year 2005/06, and from 2010/11 onwards for academies.

[12] While it would be especially interesting to study the impact of the takeover on teachers' absences, data spanning years before and after the takeover are in practice only available for the last takeover cohort that we study, limiting our ability to use them.

The main limitation of our resulting data set is that we cannot match teachers to pupils and thus build teacher value-added measures. To try to overcome this drawback, we will exploit the rich information on schools' and teachers' characteristics, including school externally certified scores in terms of managerial and teaching quality and overall performance, but also teachers' experience in the educational sector, performance in their own educational career, and whether they are part of the *Teach First* program. Although these are all imperfect proxies of teacher quality, they can help us improve our understanding of how effective managers select their personnel (Fenizia 2022, Minni 2023).

Another limitation of the school data set is that it does not contain any information on either the name of the sponsor or its nature - whether it is a charity, a business or another organization. To further characterize sponsors' choices and compare their personnel practices, we thus complemented this data set with our own data-collection effort to classify the sponsor's type for each of the 623 schools converted to a sponsored academy between 2002/03 to 2018/19, starting from information provided in schools' websites, Wikipedia, and magazines focused on the educational sector.

Table 1 presents summary statistics of the main variables of interest. The table aims to introduce the reader to the first identification strategy we adopt to study the effects of interest. Column 1 reports school and teacher characteristics of schools that have never been converted to sponsor-led academies by 2018/19. The figures are averages (and standard deviations) computed from 2002/03 to 2011/12. Columns 2 and 3 refer to the 589 academies that are conversions of pre-existing secondary schools taking place between 2007/08 and 2018/19. Specifically, Column 2 reports pre-conversion school and teacher characteristics of schools that experience a takeover between 2007/08 and 2012/13. These are the "cohorts" of school takeovers that will form our treatment group primarily because we can observe teachers' outcomes in these schools from at least 5 years before to four years after the takeover. Column 3 reports instead teacher and school characteristics, computed over the same period as in Column 2, for cohorts of schools that experienced the takeover six years after each cohort of schools included in the previous column. We will explain in the next section why we focus on this specific comparison group.[13]

This table offers two main insights. First, while the average school that never experiences a takeover over the period considered (Column 1) appears different from schools that eventually

[13]Thus, we exclude the 32 takeovers that happen between the school years 2002/03 and 2006/07 for which we only observe few pre-conversion years, and 2 takeovers happening, respectively in 2014 and 2018 for which we have poor teacher data. We further disregard newly created sponsor-led academies, for which we would not have pre-treatment data (37 schools); we also exclude conversions from the CTCs which were already enjoying high autonomy (12 schools) and conversions involving a merge between primary and secondary schools or secondary schools and colleges (46 schools) as we would not have secondary school standardized exam results for part of the institution prior to the merge; finally, we disregard conversions from private or special schools (10 schools), to focus on state-funded mainstream secondary schools only.

Table 1: Summary statistics

	Never converted 2002/03 to 2011/12	Conversions between 2007/08 to 2012/13 5 to 1 year before takeover of schools in Col 2	Conversions 6 years later
	(1)	(2)	(3)
School characteristics			
Number of pupils	1,066	1,009	1,062
	(360)	(388)	(382)
% FSME students	0.13	0.24	0.19
	(0.12)	(0.13)	(0.12)
% Students with 5 A*-C	0.57	0.31	0.40
	(0.18)	(0.11)	(0.12)
% OFSTED low score	0.31	0.82	0.62
	(0.46)	(0.39)	(0.49)
% in urban LA	0.85	0.94	0.85
	(0.36)	(0.24)	(0.35)
Headteacher's characteristics			
Age	49	49	50
	(6)	(6)	(5)
Experience	27	27	28
	(6)	(7)	(6)
Master's or more	0.66	0.57	0.63
	(0.48)	(0.50)	(0.48)
Top GPA in education	0.05	0.08	0.02
	(0.22)	(0.26)	(0.13)
Annual gross pay	84,253	83,528	85,288
	(19,272)	(18,279)	(18,577)
Teachers' characteristics			
Number of teachers	59	58	59
	(24)	(23)	(24)
Pupil-teacher ratio	19	18	19
	(5)	(5)	(4)
% New hires	0.14	0.17	0.16
	(0.10)	(0.10)	(0.09)
% Leaving in t+1	0.12	0.17	0.11
	(0.09)	(0.12)	(0.08)
Age	40	40	40
	(3)	(2)	(3)
Experience	15	14	14
	(3)	(2)	(3)
% Master's or more	0.66	0.61	0.63
	(0.10)	(0.10)	(0.10)
% Top GPA in education	0.05	0.04	0.05
	(0.08)	(0.07)	(0.07)
% Teach First	0.001	0.003	0.002
	(0.01)	(0.02)	(0.02)
% New hires from outstanding school	0.006	0.003	0.002
	(0.03)	(0.01)	(0.01)
% Part-time	0.14	0.10	0.11
	(0.09)	(0.07)	(0.07)
Annual base pay	38,406	37,986	38,591
	(3,297)	(2,891)	(2,842)
Share additional payments	0.03	0.05	0.04
	(0.03)	(0.03)	(0.03)
N schools	2,329	289	300

Source: DTR, 2001/02-2009/10, SWC, 2010/11-2018/19, GIAS, 2001/02-2018/19.

Notes: This table presents summary statistics (mean and standard deviation) of school, pupils', the principal's, and teachers' characteristics for three groups of schools. Figures in Column 1 refer to schools that never become sponsored academies throughout the period considered, and are calculated over the period 2002/03-2011/12. Figures in Column 2 refer to schools that become sponsored academies between 2007/08 and 2012/13, and are calculated from five to one year prior to the conversion year. Figures in Column 3 refer to schools that convert six years after the schools in Column 2, and are calculated over the same period as in Column 2. Information on student standardized exams and OFSTED school performance is only available from 2005/06. Information on teachers' GPA in education is only available for one fourth of teachers in the sample.

become sponsor-led academies (Columns 2 and 3) along many dimensions, ever sponsor-managed schools are more similar to each other over the years before schools in Column 2 are converted. Specifically, ever sponsor-managed schools have a (50-80 percent) larger proportion of pupils eligible for free school meals, a (30-50 percent) smaller proportion of pupils achieving a sufficient level of learning in grade-11 standardized exams, and are (2-3 times) more likely to be classified as "requires improvements" or "inadequate" by OFSTED. Moreover, although schools in Column 2 are more likely to have a headteacher with top GPA in his/her own educational career, the headteacher in ever-sponsored academies is less likely to have a master's degree (5-14 percent lower probability). As for teachers, ever sponsor-managed schools have less experienced teachers (7-percent lower level), a (5-8 percent) lower proportion of teachers with a master's degree, and a (50-70 percent) smaller share of new hires who come from an outstanding school in teaching. Schools in Column 2 also have a 40 percent higher turnover rate and are 20 percent less likely to have teachers with top GPA in their own educational career.

The second takeaway of this table comes from the comparison of Columns 2 and 3, which suggests that the overall school performance (student numbers, pupil performance in standardized exams, school OFSTED scores, teacher turnover) may deteriorate in the years preceding the takeover, an important point over which we will return at greater length when presenting our regression results.

The last point to consider in Table 1 concerns the structure of teacher pay. The last two rows of the table show that, in the typical state-funded school, the bulk of a teacher's gross pay is composed of the base pay, but, 4 to 5 percent of it comes, on average, from what we have called additional payments. It is important to make clear that these are not pay-for-performance bonuses, but a top-up that teachers usually can earn if the headteacher assigns them so-called additional "teaching and learning responsibilities", or TLR, such as coordinating the teachers in a specific subject area. Also, TLR are usually given to teachers who have at least a few years of experience in the educational sector, with the probability of being assigned TLR jumping from 22 percent for teachers with at most five years of experience to 39 percent for more experienced teachers. These details are important because we will see that sponsors abandon the use of TLR to distribute tasks across teachers, and that returns to experience significantly decrease in sponsored academies.

We conclude the descriptive analysis of the data by introducing the distribution of sponsors' types. The data that we have collected tell us that educational charities have rapidly become the most prevalent type of sponsor and, by the end of the sample period (2018/19), they manage 64 percent of sponsored academies,[14] followed by businesses with 16 percent of schools, religious organizations with 10 percent, and universities with 8 percent, while we have not been able to recover

[14]Note that we include in this category cases where groups of schools found a charity together to sponsor nearby low-performing schools.

the sponsor' type for 1.5 percent of schools (see Appendix Figure B.3). Furthermore, the average number of schools managed by each sponsor has increased over time, with companies managing on average 14 secondary schools by the end of the sample, charities 8, religious organizations 5, and universities 2, an organizational feature that will be important to consider when discussing the heterogeneous effects across sponsor types.

4 Identification strategy

To identify the causal impact of the school management takeover on the school governance and personnel policy, we adopt two complementary identification strategies.

With the first strategy, we focus on schools that eventually become sponsor-led academies over the period considered, and estimate a difference-in-differences model that compares the evolution of the outcomes of interest in schools that acquire academy status in a certain year and in schools that convert k years after. We will discuss below how we choose k in our main specification and later show that our results do not change when using a control group that converts in a different year. As for the treated cohorts, we consider school takeovers taking place from 2007/08 to 2012/13, for which we observe at least five pre-conversion years and four post-takeover years on the main outcomes of interest. Note that our estimation sample will comprise several pairs of treated and control conversion cohorts. For instance, we will compare schools converting in 2007/08 to schools converting k years after, schools converting in 2010/11 to schools converting k years after, and so on. We can think about these pairs of conversion cohorts as being different subsamples that we stack together in the final estimation sample. For this reason, studies using this methodology refer to it as a stacked difference-in-differences design (see, for instance, Deshpande and Li 2019, Eyles and Machin 2019, Crema 2022, Adams-Prassl et al. 2024). When studying school-level outcomes, our regression model looks as follows:

$$Y_{sct} = \alpha_s + \theta_t + \gamma_c + \beta\, SponsoredAcademy_{sct} + u_{sct} \quad (1)$$

where s stands for a school that acquires academy status in year c (treated group) or $c+k$ (control group). We choose t to go from $c-5$ to $c+4$. Y_{sct} is the outcome of interest, which is either the probability that the school appoints a new headteacher, a measure of teacher turnover, as described below, teachers' characteristics such as educational background or experience, school expenditure, and pay dispersion, as defined later; α_s are θ_t are, respectively, school and school-year fixed effects; and γ_c are takeover-year (or cohort) fixed effects. Our main coefficient of interest is β associated to the indicator variable $SponsoredAcademy_{sct}$, which is equal to one in treated schools from the year the takeover is completed onwards. Finally, we use heteroskedasticity-robust

standard errors clustered at the school-cohort level. To further take into account that we study the impact of the school takeover on many outcomes, in the regression tables we will report both the standard p-values associated to the coefficients estimated, as well as sharpened False Discovery Rate (FDR) q-values introduced by Benjamini et al. (2006).[15]

To test the parallel-trend assumption and study the dynamic impact of the takeover on the outcomes of interest, we will present event-study estimates obtained by running the following dynamic specification:

$$Y_{sct} = \alpha_s + \theta_t + \gamma_c + \sum_{e=-4}^{4} \beta_e (Treated_{sc} * \mathbf{1}[t - c = e]) + u_{sct}, \quad (2)$$

where $Treated_{sc}$ is a dummy variable equal to 1 for schools experiencing the takeover in year c, e refers to the year relative to the conversion year, and $\mathbf{1}[t - c = e]$ is an indicator variable that takes value 1 when $t - c = e$ and 0 otherwise. In what follows, we will take the event year -5 as the reference period.

Finally, when studying the impact of the school takeover on teacher pay, we run teacher-level regressions that control for teacher-fixed effects and teacher time-varying controls (age squared, age to the cube, and experience), on top of school and year fixed effects:

$$Y_{isct} = \delta_i + \alpha_s + \theta_t + \gamma_c + \beta\ SponsoredAcademy_{sct} + X'_{isct}\pi + u_{isct}, \quad (3)$$

where Y_{isct} is the log of teachers' annual pay, and i is either an incumbent teacher, that is a teacher already employed at the school before the takeover, or a teacher hired from the takeover year onwards, as further explained in Section 5.4.

In the main specification, we choose as control group schools that experience a management takeover six years after the treated group. The main benefit of using the 6-year time window between treated and control cohorts is that it allows us to study the dynamic effects of the takeover up to four years after the event (when control cohorts are still two years away from converting and at least one year from starting the conversion process). However, to take into consideration that this choice could be considered arbitrary, we will show that our results are robust to use as control group schools that experience the takeover four or five years after treated schools (although this implies restricting the number of years post-takeover that we can study).

This identification strategy has three strengths. First, it focuses on low-performing schools that sooner or later experience a takeover, and thus we know that this is the route that has been

[15] Anderson (2008) provides a related Stata code to estimate sharpened q-values. As noted in his code, sharpened q-values can be smaller than unadjusted p-values when many hypotheses are rejected. This usually happens for null hypothesis that would not be rejected even without implementing any correction for multiple-hypothesis testing.

chosen for these schools to improve. Next, from an econometric point of view, the stacked design ensures that once a school is treated, we will not use it as control for schools converting in subsequent years, which allows us to avoid the risk of bias created by "forbidden comparisons" in the presence of dynamic treatment effects (Callaway and Sant'Anna 2021, Goodman-Bacon 2021, Sun and Abraham 2021, Borusyak et al. 2024). Third, when estimating event-study specifications, we do not have to worry about composition effects that may affect the interpretation of long-term leads and lags, as we observe teacher outcomes for all sub-samples from five years before to four years after the takeover.

However, to identify the effects of interest, this strategy fundamentally exploits variations in the timing of the takeover between treated and control cohorts. Therefore, it is especially important to exclude that our results pick the effect of a deterioration of the school environment in the years preceding the takeover of control schools. To mitigate these concerns, we complement this strategy with the estimation of a matched difference-in-differences design that compares the evolution of the outcomes of interest in the same treated schools used in the first method and a matched control group of schools that will never experience the takeover. As the average school that never experiences a takeover tends to be quite different from treated schools (see Table 1), the matching ensures that in the pool of never-treated schools, we identify those that are the most comparable. Specifically, we consider each treated school two years before the takeover completion and find its nearest-neighbor match based on pupil numbers from three years before, the share of FSME students, the share of pupils achieving at least 5 A*-C grades, school OFSTED performance, whether the school is fully-managed by the LA, whether it is located in an urban area, the share of teachers leaving the school, and whether the school has changed the headteacher that year. When considering teacher-level outcomes, we find the nearest-neighbor match based on age, gender, experience, whether the teacher has a master's degree, whether the school is fully-managed by the LA, and whether it is located in an urban area. Once the matching is performed, we stack again the different sub-samples of treated schools and their matched control and estimate the same difference-in-differences model presented above, augmented with match-specific fixed effects.

Matched difference-in-differences designs have been used in several recent papers, including Aneja and Xu (2022), Jensen and Zhang (2023), Adams-Prassl et al. (2024).[16] Compared to the first strategy, with the matched DiD, we do not have to worry about the control group being about to be treated. At the same time, it is important to consider the nature of the matched control. By construction, these are schools that must be performing as bad as treated schools before the latter experience the sponsor-led conversion, but that are able to escape the takeover, potentially by

[16] As in our case, both Jensen and Zhang (2023), Adams-Prassl et al. (2024) complement the matched (stacked) DiD design with a stacked DiD model comparing pairs of cohorts experiencing the treatment at different points in time.

improving their performance in different ways. Thus, with this identification strategy we should expect to estimate a lower bound of the positive effect of the takeover on pupil performance. In this respect, it becomes especially interesting to compare the personnel practices adopted by sponsors vis-à vis this alternative control group. In what follows, we start by presenting the results obtained when using the first identification strategy and compare them with those obtained using the matched difference-in-differences design in Section 6.

5 Results

5.1 Pupil and school performance

Using pupil-level data and a stacked DiD design that compares schools taken over earlier or later, Eyles et al. (2016), Andrews et al. (2017), Eyles and Machin (2019) show that sponsor-led takeovers improve pupil performance in end-of-high school centralized exams by 7 to 12 percent of a standard deviation, depending on the specific treated cohorts considered and control group used.

While we cannot exactly replicate these results because we do not have access to pupil-level data for this project,[17] we start our analysis by re-estimating the effect of the takeover on pupil performance using the available information in school-level data. Consistent with the findings of these studies, Figure 1, Panel A, shows that the share of pupils achieving five or more A*-C GCSE grades is comparable across treated and control schools two years before the conversion of treated schools.[18] However, the year right before the takeover completion, when plausibly the sponsor is already exerting some control on the school management, pupil performance begins to significantly improve. Once the takeover is completed, the improvement accelerates and persists over the following years. Table 2 shows that, on average, the takeover leads to a 7 p.p improvement in pupil performance, or a 23 percent increase compared to the pre-takeover mean of treated schools.

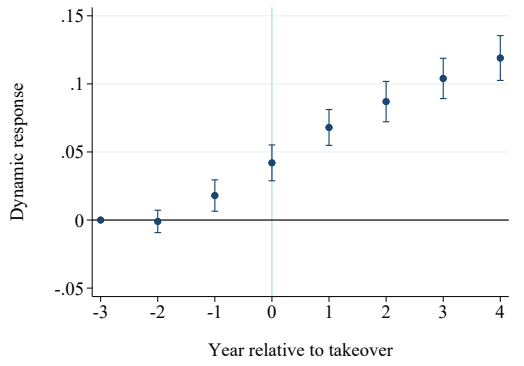
We complement these results by estimating the impact of the takeover on the school performance in OFSTED inspections. Every three to five years - and more often if a school is underperforming - the independent authority OFSTED performs two-day-long school inspections to evaluate the school management, teaching quality, and the overall effectiveness of the school at

[17] See Appendix Section B for more information on the studies cited above and on the differences between pupil-level and school-level data. Note, in particular, that with pupil-level data these studies can estimate the effect of the takeover on the performance of legacy pupils, that is, students who were already enrolled in the school before the conversion, and thus isolate the effect on performance from the progressive post-takeover improvement in pupil intake that these studies also document.

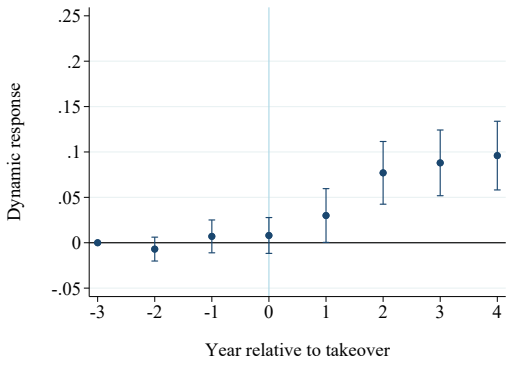
[18] We run shorter event-study specifications for pupil and school performance outcomes because these data are only available from 2005/06. We also exclude the 2007/08 takeover cohort from these regressions, for which we only observe two pre-takeover years. However, in Appendix Section B.2, we show that the average takeover effect changes little when this cohort is included in the estimation sample (see Appendix Table B.2).

providing an appropriate and safe learning environment for its pupils. On each dimension (overall effectiveness, teaching, and management quality), schools are rated on a scale of four scores, with 1 for outstanding, 2 for good, 3 for "requires improvement", and 4 for schools that are judged inadequate.

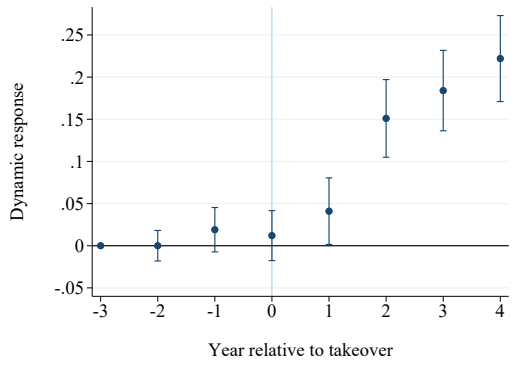
Figure 1: Pupil and school OFSTED performance



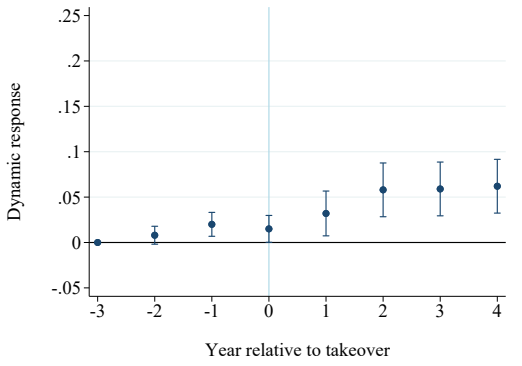
(A) Share pupils with 5 A*-C



(B) Outstanding



(C) Outstanding in management



(D) Outstanding in teaching

Source: GIAS and OFSTED, 2005/06-2018/19.
Notes: This graph presents the dynamic impact of the school management takeover on pupil performance in end-of-high school centralized exams and school OFSTED performance. Pupil performance is measured as the share of pupils achieving a sufficient level of learning, i.e., at least 5 A*-C grades, including in English and Math. These results are estimated by running regression 2 on the outcome displayed in each graph. The estimation sample includes schools that convert between 2008/09 to 2012/13 (treated schools) and schools that convert six years after each treated cohort (control schools). The estimation period includes five years before to four years after the takeover of treated schools. Each graph also reports 90-percent confidence intervals corresponding to robust standard errors clustered at the school-cohort level.

Figure 1, Panels B-D show the evolution of the likelihood of being deemed outstanding on each dimension in treated vs. control schools around the year of the takeover. To run these regressions, we assign the rating score of the last inspection to years in which a school does not receive an inspection. While there is no differential trend in these outcomes before treated schools experience the takeover, in just two years after this is completed, the likelihood of receiving an outstanding score for the school overall performance increases by up to 10 p.p., or, on average, 5 times compared to the pre-takeover mean (Table 2).

Table 2: Pupil and school OFSTED performance

	Share of pupils with 5 A*-C (1)	Outstanding (2)	Outstanding in management (3)	Outstanding in teaching (4)
Sponsored academy	0.070***	0.048***	0.089***	0.030***
	(0.005)	(0.012)	(0.017)	(0.011)
p-value	(0.000)	(0.000)	(0.000)	(0.004)
q-value	[0.001]	[0.001]	[0.001]	[0.008]
Observations	4,432	4,432	4,432	4,432
Adjusted R^2	0.696	0.436	0.373	0.387
School FE	✓	✓	✓	✓
Year FE	✓	✓	✓	✓
Cohort FE	✓	✓	✓	✓
Pre-SA Mean T	0.31	0.01	0.03	0.01
Pre-SA Mean C	0.41	0.06	0.09	0.02

Source: GIAS, OFSTED, 2005/06-2018/19.

Notes: This table presents the impact of the school management takeover on pupil performance in end-of-high school centralized exams and school OFSTED performance. Pupil performance is measured as the share of pupils achieving a sufficient level of learning, i.e., at least 5 A*-C including in English and Math. These results are estimated by running regression 1 on the outcomes displayed on top of each column. Robust standard errors clustered at the school-cohort level are displayed in parenthesis. The table also reports p-values associated to the estimated coefficients and sharpened FDR q-values accounting for multiple hypothesis testing. The estimation sample includes schools that convert between 2008/09 to 2012/13 (treated schools) and schools that convert six years after each treated cohort (control schools). The estimation period includes five years before to four years after the takeover of treated schools.

*** p<0.01, ** p<0.05, * p<0.1.

Importantly, both the probability of obtaining an outstanding score for management and teaching quality also increase steadily after the takeover (Figure 1, Panels C-D), with the effect on this outcome starting to materialize the year before the takeover completion. On average, the takeover increases the likelihood of these outcomes by 9 and 3 p.p., respectively, or by 3 times com-

pared to the pre-takeover mean (Table 2), suggesting that sponsors' new governance and personnel policy may be important to explain the substantial improvement in pupils' outcomes brought about by sponsored academies.[19]

5.2 Leadership change

The analysis of school performance in OFSTED inspections reveals that the takeover is accompanied by a substantial improvement in both management and teaching quality. In what follows, we will dig deeper into these results, by analyzing sponsors' choices in terms of school leadership, reshuffling of the teaching body, school resources, and teacher pay policy.

One of the key responsibilities that the academy board of trustees acquires is that of appointing the school headteacher, who will become responsible for the operational and day-to-day running of the academy trust. Figure 2 shows that the probability that the governing body selects a new headteacher shoots up the year that the takeover is completed, with a 29 p.p. increase, doubling the pre-conversion mean. In turn, Table 3, Column 1, shows that, on average, the takeover increases the probability of appointing a new headteacher by 11 p.p., or 44 percent compared to the pre-takeover mean. This result echoes the findings of Lavy et al. (2023) who show that, in the context of Israel, the appointment of a new school superintendent increases the probability of changing the school headteacher, an effect that is larger when the school is matched to a higher-quality school superintendent.

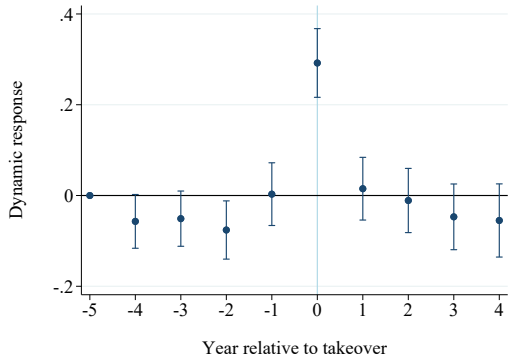
Figure 2, Panels B-D and Table 3, Columns 2-4 further show that this effect is driven by an increase in the probability of hiring a new head from a different school or appointing a new head who previously worked in a different sector, with both of these probabilities doubling compared to the pre-takeover mean. Importantly, Table 3, Columns 5-10 show that the new headteacher tends to be less experienced (3 percent decrease compared to the pre-takeover mean), but 6 p.p. more likely to have a master's degree (10 percent increase compared to the pre-takeover mean).[20] The new head's gross pay also increases after the takeover, by 9.9 percent, or around £8,300 ($10,700) per year.[21] Finally, the new head is 3 times more likely to come from an outstanding school in management quality (as certified by OFSTED), pointing to a clear strategy of the sponsor to start improving the school from its leadership.

[19] In Appendix Section B.2, we show that the takeover leads to a corresponding decrease in the probability that the school obtains the two worst OFSTED rating scores, while the probability of being deemed "good" also significantly increases after the takeover.

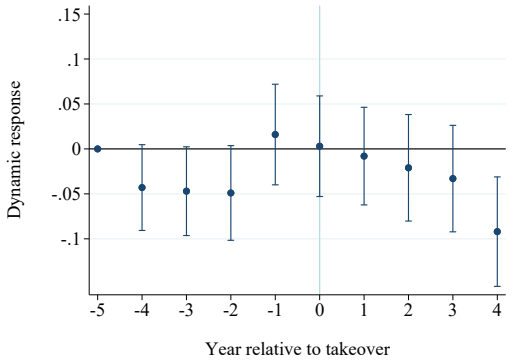
[20] These two results are just marginally insignificant when accounting for multiple hypothesis testing.

[21] In Appendix Section B.3, we show that this average effect is the result of a progressive higher pay growth, rather than a large immediate pay raise (see Appendix Figure B.6). Also note that, while this may represent a large salary increase from the point of view of the headteacher, it only amounts to 0.2 percent of a school annual spending on teachers, as we will further discuss in Section 5.4.

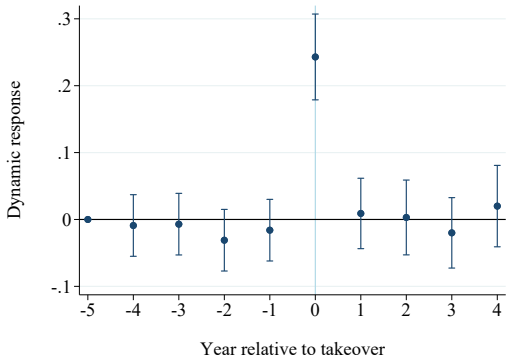
Figure 2: Probability of appointing a new headteacher



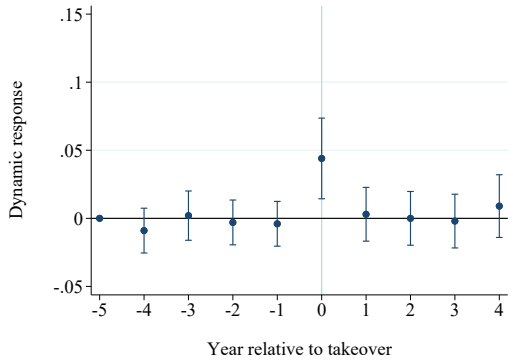
(A) New head



(B) Promoted internally



(C) Hired from other school



(D) Hired from other sector

Source: DTR, 2001/02-2009/10, SWC, 2010/11-2018/19, GIAS, 2001/02-2018/19.
Notes: These graphs present the dynamic impact of the school management takeover on the probability that the school changes headteacher. These results are estimated by running regression 2 on the outcomes displayed in each graph. The estimation sample includes schools that convert between 2007/08 to 2012/13 (treated schools) and schools that convert six years after each treated cohort (control schools). The estimation period includes five years before to four years after the takeover of treated schools. Each graph also displays 90-percent confidence intervals corresponding to robust standard errors clustered at the school-cohort level.

Table 3: New headteacher

		Where from			Headteacher's characteristics					
	New head	Promoted internally	Hired from other school	Hired from other sector	Age	Years of experience	Master's or more	Top GPA in education	Log annual pay	Coming from outstanding in management
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Sponsored academy	0.114***	0.004	0.091***	0.017***	-0.689	-0.847*	0.056*	-0.024	0.099***	0.021**
	(0.023)	(0.018)	(0.017)	(0.006)	(0.450)	(0.514)	(0.033)	(0.035)	(0.015)	(0.010)
p-value	(0.000)	(0.837)	(0.000)	(0.006)	(0.126)	(0.099)	(0.0913)	(0.495)	(0.000)	(0.040)
q-value	[0.001]	[0.484]	[0.001]	[0.011]	[0.123]	[0.111]	[0.101]	[0.312]	[0.001]	[0.057]
Observations	5,589	5,589	5,589	5,589	5,589	5,582	5,589	1,413	5,589	3,606
Adjusted R^2	0.068	0.041	0.008	0.012	0.378	0.391	0.494	0.722	0.453	-0.004
School FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Cohort FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Pre-SA Mean T	0.26	0.15	0.11	0.01	49	27	0.57	0.08	83,528	0.007
Pre-SA Mean C	0.20	0.09	0.09	0.01	50	28	0.63	0.02	85,288	0.007

Source: DTR, 2001/02-2008/09, SWC, 2010/11-2018/19, GIAS, 2001/02-2018/19.

Notes: This table presents the impact of the school management takeover on the probability of appointing a new headteacher and the characteristics of the headteacher. These results are estimated by running regression 1 on the outcomes displayed on top of each column. Robust standard errors clustered at the school-cohort level are displayed in parenthesis. The table also reports p-values associated to the estimated coefficients and sharpened FDR q-values accounting for multiple hypothesis testing. The estimation sample includes schools that convert between 2007/08 to 2012/13 (treated schools) and schools that convert six years after each treated cohort (control schools). The estimation period includes five years before to four years after the takeover of treated schools. Information on teachers' GPA in education is only available for 25 percent of the sample. Schools' OFSTED performance is available from the school year 2005/06.

*** p<0.01, ** p<0.05, * p<0.1.

5.3 Teacher sorting

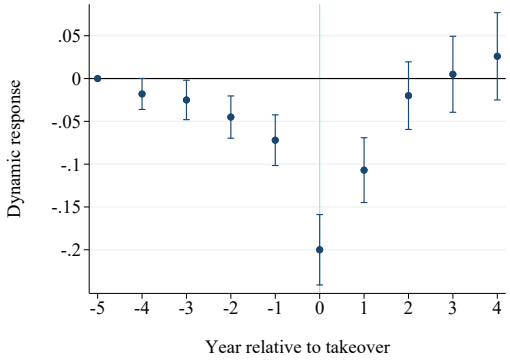
The corporate finance literature shows that firm takeovers typically involve a process of substantial workforce restructuring, which are important to understand how these events affect firm productivity (Davis et al. 2019, Nishesh et al. 2024).

A priori, it is unclear whether state-funded school takeovers may lead to large changes in the teaching body, considering that sponsors cannot fire teachers who were already employed at the school, and that the school remains publicly funded. However, sponsors acquire substantial autonomy over key aspects of the school management, including spending allocation, headteacher and teacher hiring, and teacher pay policy. We have just seen that they make use of this freedom to drastically change the school leadership. Potentially, they may also exploit their autonomy to induce less motivated teachers to leave and high-achieving teachers to join the sponsored academy. To explore this hypothesis, we start here by documenting the dynamics of teacher turnover and sorting associated to the takeover, and show in the next section that these dynamics are consistent with the new pay policy adopted by sponsors.

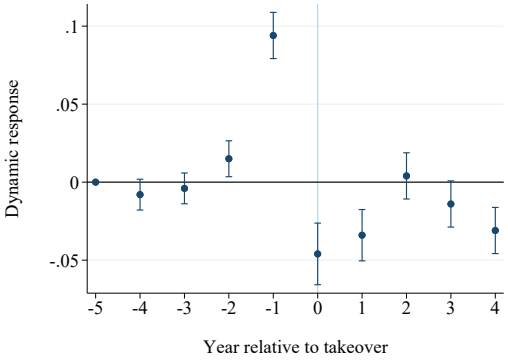
Figure 3, Panel A shows the evolution of teacher numbers before and after the takeover in treated vs. control schools. The corporate finance literature suggests that the process of worker reallocation may begin before the completion of a firm takeover (Agrawal and Tambe 2019). A similar dynamic seems to take place around school takeovers. A few years before the event, we see that the size of the teaching body starts shrinking. This has to be read in parallel with the larger decline in pupil numbers, reported in Appendix Figure B.5, which probably signals a deterioration of the school environment leading to the takeover.[22] However, while pupil numbers steadily decrease, the decline in teacher numbers accelerates dramatically between the year preceding the takeover completion - when this has most likely already started - and the year in which this is finalized. Figure 3, Panel B tells us that this dynamic is driven by a spike in teacher separations that increase by 9 p.p relative to the reference period. In Appendix Section B.3, we further show that this surge in separations is mostly driven by teachers moving to other schools. Remarkably, this trend in teacher turnover completely reverses following the takeover, with a significant 5.4 p.p. decline in teacher separations, or 32 percent drop compared to the pre-takeover mean. In parallel, teacher hiring significantly and steadily increases after the takeover, with an average effect of 4 p.p., or 26 percent rise compared to the pre-takeover mean, driven both by new hires from other schools and new recruits from outside the education sector (see Appendix Figure B.7 and Table B.4).

[22]Note that the fact that pupil and teacher numbers start declining a few years before the takeover suggests that, by comparing early and later takeovers, the average effect on these outcomes results at least in part from the dynamics experienced by the control group before its own takeover. This is why it will be especially important to complement this strategy with the estimation of the matched DiD, which explicitly selects a control group that never experiences the takeover.

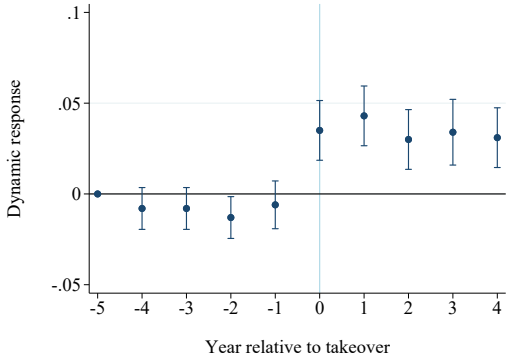
Figure 3: Teacher turnover



(A) Log number of teachers



(B) Share of teachers leaving in t+1



(C) Share of new hires

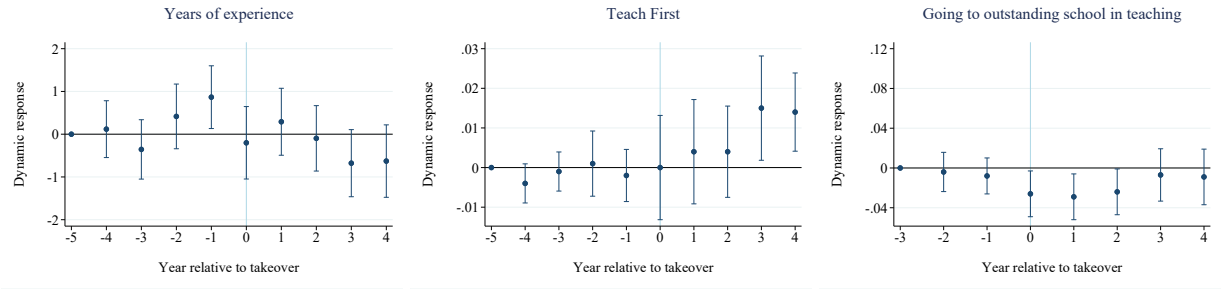
Source: DTR, 2001/02-2009/10, SWC, 2010/11-2018/19, GIAS, 2001/02-2018/19.
Notes: These graphs present the dynamic impact of the school management takeover on teacher turnover. These results are estimated by running regression 2 on the outcomes displayed in each graph. The estimation sample includes schools that convert between 2007/08 to 2012/13 (treated schools) and schools that convert six years after each treated cohort (control schools). The estimation period includes five years before to four years after the takeover of treated schools. Each graph also displays 90-percent confidence intervals corresponding to robust standard errors clustered at the school-cohort level.

Together, the decline in teacher separations and the increase in hiring drive an expansion of the teaching body that accompanies a recovering of pupil numbers.[23]

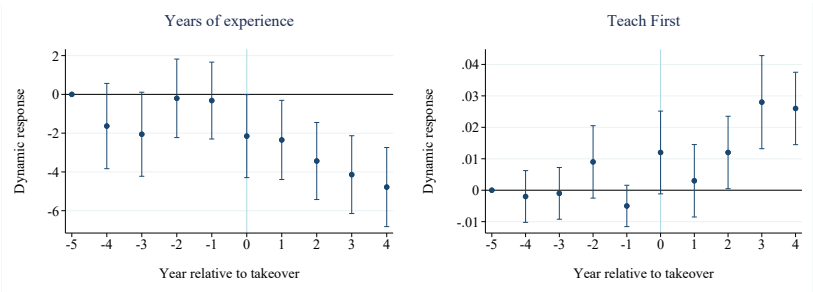
Thus, sponsor-led takeovers are indeed accompanied by a substantial process of worker re-allocation. What is especially interesting is to understand the dynamics of teacher sorting that accompany this process. Starting from teachers leaving the school, the top left graph in Figure 4,

[23] In Appendix Section B.3 we further show that the takeover does not change, on average, the pupil-teacher ratio, although the dynamic effect oscillates as the school recovers from the spike in teacher separations.

Figure 4: Teacher sorting: leavers



(A) Leavers to other school



(B) Leavers to other sector

Source: DTR, 2001/02-2009/10, SWC, 2010/11-2018/19, GIAS, 2001/02-2018/19.
Notes: These graphs present the dynamic impact of the school management takeover on teacher sorting. These results are estimated by running regression 2 on the outcomes displayed in each graph. The estimation sample includes schools that experience a takeover between 2007/08 to 2012/13 and schools that experience this event six years after each treated cohort. Each graph also displays 90-percent confidence intervals corresponding to robust standard errors clustered at the school-cohort level. The cohort 2007/08 is excluded from the third graph of Panel A because schools' OFSTED performance is only available from 2005/06.

Panel A shows that the spike in separations preceding the takeover is partly driven by more experienced teachers. This result mimics the findings of Fenizia (2022) who show that, in the Italian Social Security Agency, older workers tend to retire when a good manager is assigned to their office. And while overall teacher turnover decreases after the takeover, Figure 4, Panel A and Table 4, Panel A show that teachers who leave right after the takeover completion are significantly less likely to go to outstanding schools in teaching (-1.6 p.p. or 18 percent drop compared to the pre-takeover mean).[24]

In stark contrast, Figure 5, Panel A, and Table 5, Panel A tell us that new hires from other schools tend to be younger (-1 percent compared to the pre-takeover mean), and potentially less experienced, but 2.5 p.p. (or 42 percent) more likely to come from outstanding schools in teaching

[24]Note that we estimate a short event-study for this outcome because school OFTSED scores are only available from 2005/06.

quality,[25] and even more likely to come from the same school of the new head (0.8 p.p. increase or 1.3 times the pre-takeover probability). As for teachers coming from outside the education sector (Figure 5, Panel B, and Table 5, Panel B), they also tend to be younger (-2 percent compared to the pre-takeover mean), but 2.1 p.p. or 1 time more likely to be *Teach First* teachers and, consistent with this, more likely to be top-performers in their own education, although this result is less precisely estimated.

Table 4: Teacher sorting: leavers

	Age	Years of experience	Master's or more	Top GPA in education	Teach First	Going to outstanding in teaching
	(1)	(2)	(3)	(4)	(5)	(6)
A. Leavers to other school						
Sponsored academy	-0.299	-0.527*	0.002	-0.014	0.008**	-0.016*
	(0.275)	(0.269)	(0.012)	(0.013)	(0.004)	(0.009)
p-value	(0.278)	(0.051)	(0.882)	(0.296)	(0.049)	(0.087)
q-value	[0.222]	[0.066]	[0.493]	[0.230]	[0.066]	[0.100]
Observations	5,329	5,329	5,329	3,871	5,329	4,429
Adjusted R^2	0.106	0.083	0.106	0.012	0.168	0.082
Pre-SA Mean T	36	10	0.68	0.06	0.008	0.09
Pre-SA Mean C	35	9	0.70	0.05	0.004	0.09
B. Leavers to other sector						
Sponsored academy	-2.400***	-2.364***	-0.014	0.003	0.015***	
	(0.443)	(0.542)	(0.019)	(0.013)	(0.004)	
p-value	(0.000)	(0.000)	(0.461)	(0.843)	(0.001)	
q-value	[0.001]	[0.001]	[0.296]	[0.484]	[0.002]	
Observations	4,091	4,091	4,091	2,842	4,091	
Adjusted R^2	0.174	0.153	0.063	0.026	0.269	
Pre-SA Mean T	43	17	0.48	0.05	0.007	
Pre-SA Mean C	42	17	0.50	0.05	0.003	

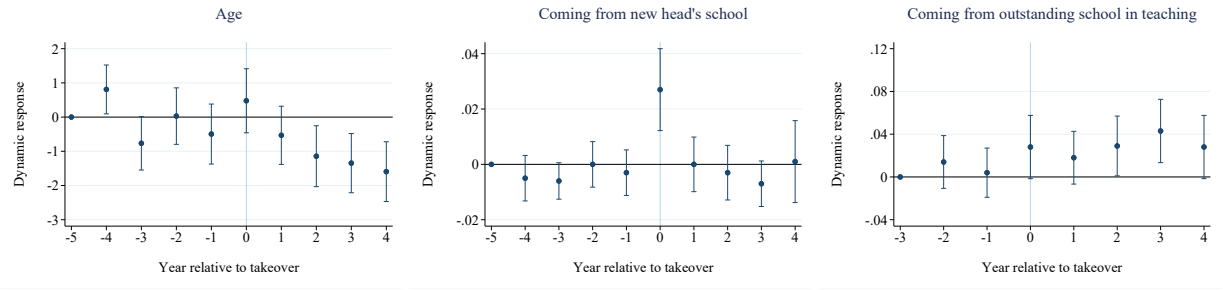
Source: DTR, 2001/02-2009/10, SWC, 2010/11-2018/19, GIAS, 2001/02-2018/19.

Notes: This table presents the impact of the school management takeover on teacher sorting. Panel A (B) refers to teachers leaving for other schools (the educational sector or for a school in different education phases). These results are estimated by running regression 1 on the outcomes displayed on top of each column. All regressions include school, year, and cohort fixed effects. Robust standard errors clustered at the school-cohort level are displayed in parenthesis. The table also reports p-values and sharpened FDR q-values associated to the estimated coefficients. Information on teachers' GPA in education is only available for 25 percent of the sample. Schools' OFSTED performance is available from the school year 2005/06.

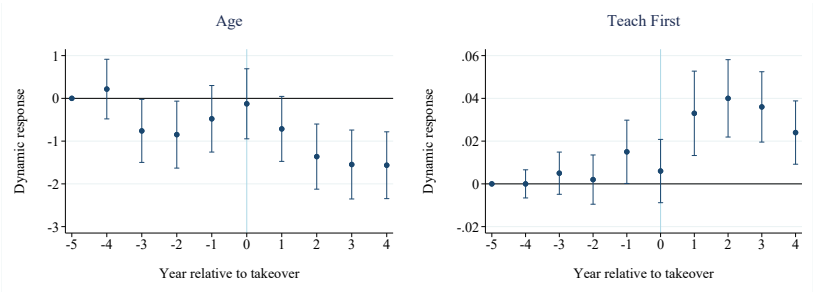
*** p<0.01, ** p<0.05, * p<0.1.

[25] Note that these teachers are unlikely to come from control schools, given that only 2 percent of them are outstanding in teaching quality in the pre-takeover period. This argument also applies to the headteacher, as only 9 percent of control schools are deemed outstanding in management quality. Nonetheless, this result is important when considering the general equilibrium effect of this program, and we will return on this point in the conclusion. On the regression sample, note that also for this outcome we estimate a shorter event-study, as we can observe the OFSTED performance of new hires' school of origin only from 2006/07.

Figure 5: Teacher sorting: new hires



(A) New hires from other school



(B) New hires from other sector

Source: DTR, 2001/02-2009/10, SWC, 2010/11-2018/19, GIAS, 2001/02-2018/19.
Notes: These graphs present the dynamic impact of the school management takeover on teacher sorting. These results are estimated by running regression 2 on the outcomes displayed in each graph. The estimation sample includes schools that experience a takeover between 2007/08 to 2012/13 and schools that experience this event six years after each treated cohort. Each graph also displays 90-percent confidence intervals corresponding to robust standard errors clustered at the school-cohort level. The cohorts 2007/08 and 2008/09 are excluded from the third graph of Panel A because schools' OFSTED performance is only available from 2005/06.

In turn and in line with the evidence from the literature on *Teach First* (Allen and Allnutt 2017), Figure 4, Panels A and B show that, 2 to 3 years after the takeover, teachers leaving are more likely to be *Teach First* teachers (and thus less experienced), as their two-year training program comes to an end.

In sum, sponsors clearly choose to invest in younger and less experienced teachers, but who have either previously served in outstanding schools or who are high-performing fresh university graduates willing to start their career in disadvantaged schools.[26] Importantly, the sorting patterns that the takeover triggers echo the impact of pay-for-performance contracts on employees' selection found in the personnel literature, suggesting that the sponsor-led takeover is also accompanied by the injection of personnel practices that push away less motivated teachers and attract high-quality ones (Lazear 2000, Gielen et al. 2010, Leaver et al. 2021). The next section further explores this hypothesis by studying sponsors' pay policy.

[26] We report the overall effect on teacher composition in Appendix Table B.5.

Table 5: Teacher sorting: new hires

	Age	Years of experience	Master's or more	Top GPA in education	Teach First	Coming from new head's school	Coming from outstanding in teaching
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
A. New hires from other school							
Sponsored academy	-0.520*	-0.287	0.012	-0.011	0.002	0.008**	0.025**
	(0.293)	(0.276)	(0.014)	(0.013)	(0.002)	(0.004)	(0.011)
p-value	(0.076)	(0.299)	(0.413)	(0.385)	(0.421)	(0.022)	(0.018)
q-value	[0.092]	[0.230]	[0.275]	[0.265]	[0.278]	[0.035]	[0.030]
Observations	5,232	5,232	5,232	3,397	5,232	5,232	3,229
Adjusted R^2	0.085	0.053	0.053	0.007	0.090	0.027	0.081
Pre-SA Mean T	37	10	0.66	0.05	0.002	0.006	0.06
Pre-SA Mean C	36	10	0.69	0.05	0.001	0.004	0.05
B. New hires from other sector							
Sponsored academy	-0.536*		-0.005	0.022	0.021***		
	(0.273)		(0.015)	(0.014)	(0.007)		
p-value	(0.050)		(0.721)	(0.112)	(0.001)		
q-value	[0.066]		[0.432]	[0.119]	[0.003]		
Observations	4,990		4,990	3,668	4,990		
Adjusted R^2	0.175		0.159	0.035	0.252		
Pre-SA Mean T	32		0.58	0.06	0.02		
Pre-SA Mean C	31		0.63	0.08	0.01		

Source: DTR, 2001/02-2009/10, SWC, 2010/11-2018/19, GIAS, 2001/02-2018/19.

Notes: This table presents the impact of the school management takeover on teacher sorting. Panel A (B) refers to new hires from other schools (from other sectors or schools in different education phases). These results are estimated by running regression 1 on the outcomes displayed on top of each column. All regressions include school, year, and cohort fixed effects. Robust standard errors clustered at the school-cohort level are displayed in parenthesis. The table also reports p-values and sharpened FDR q-values associated to the estimated coefficients. Information on teachers' GPA in education is only available for 25 percent of the sample. Schools' OFSTED performance is available from the school year 2005/06. *** p<0.01, ** p<0.05, * p<0.1.

5.4 Teacher pay

While sponsor-led academies are financed through public funding, as their predecessors, one of the key dimensions over which the sponsors acquire autonomy concerns the allocation of resources. In particular, sponsors may re-design teachers' pay structure and also decouple teachers' pay growth from a rigid seniority-based pay scale. To investigate these decisions, we analyze, in turn, the impact of the takeover on teachers' annual pay, pay dispersion, and resources and expenditure.

We first look at incumbent teachers, that is teachers who were already employed at the school the year before the takeover. We compare the evolution of their annual pay with that of teachers who, in that year, were employed at schools that only convert six years after the control group. We follow incumbent teachers from five years before the school takeover till four years afterwards, irrespective of whether they were already employed at the school before the event year -1, or after

the conversion. In this respect, the β coefficient in regression 3 could be considered an intention-to-treat (ITT) estimate.

When considering newly hired teachers, we compare the pay dynamics of teachers hired both the year of the takeover and in the two subsequent years in treated vs. control schools, from five years before the takeover to four years afterwards.[27]

To measure pay dispersion, we follow Biasi (2021) and first estimate a Mincerian regression of teachers' annual base pay on gender, a cubic polynomial of age, years of experience, level of education, and school times school-year fixed effects.[28] We then take the standard deviation of the residuals from this regression, divide it by teachers' average annual base pay, and study the evolution of this variable at the school level.

As anticipated in Section 3, Figure 6, Panel A, shows that the new school governing body concentrates teacher salary in the base pay, while reducing the use of additional payments to distribute tasks across teachers.[29] In turn, incumbent teachers' gross pay does not change on average after the takeover, but Table 7 shows that the returns to experience significantly decrease, while the returns to having a master's degree and possibly those to be a top graduate increase in sponsor-led schools.

Figure 6, Panel B shows that sponsors re-design the salary structure for new teachers as well, but, on average, they also offer new hires an overall 2.1 percent higher gross annual pay, or £840 ($1,100) per year. To put this into context, note that the typical financial incentive scheme studied in the literature offers teachers a salary increase ranging between 10 and 30 percent per year to work in hard-to-staff schools (Clotfelter et al. 2008, Glazerman et al. 2013, Springer et al. 2016, Cabrera and Webbink 2020), suggesting that teachers who join sponsor-led academies are not necessarily moved by a pure financial motive.

Finally, Figure 6, Panel C shows that the new pay policy adopted by sponsors results into a 9 percent significant increase in pay dispersion among equally experienced teachers in treated schools compared to control ones after the takeover.

Taken together, these results suggest that sponsors abandon a seniority-based pay policy where salary growth is tightly linked to experience. Unfortunately, with our data we cannot tell whether this implies that higher value-added teachers are rewarded more after the takeover. Yet,

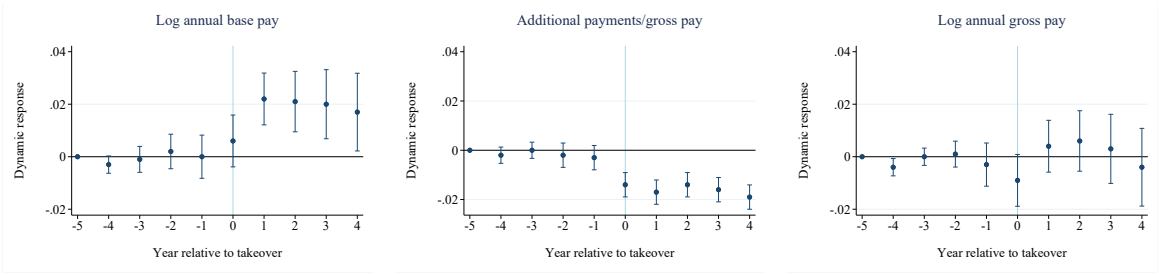
[27]This implies that we observe teachers hired at time 0 from five years before to four years after this event. In contrast, we observe teachers hired the year (two years) after the conversion from six (seven) years before the hiring happens to three (two) years afterwards. To take this differential timing into account, in the event-study specification, we pull the effects from two years after the takeover onward into one lag "2+", while the reference period in this case comprises five to seven years before the takeover.

[28]To estimate this regression, we restrict the sample to full-time teachers to avoid capturing variations in hours worked.

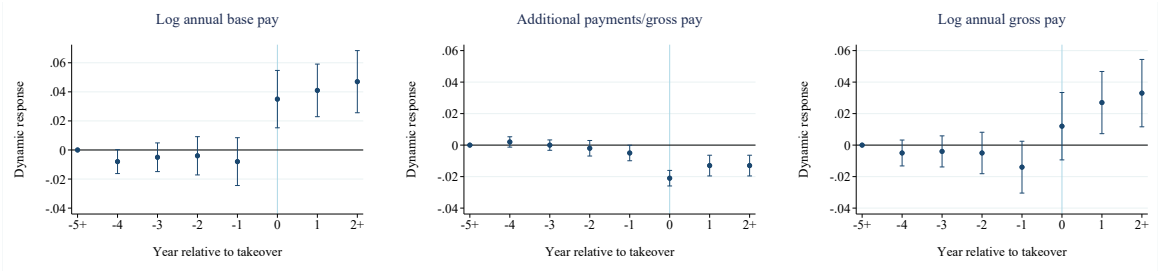
[29]Note that this does not contradict the fact that sponsors cannot modify pay and working conditions of incumbent teachers, because TLR are renegotiated more frequently than the contractual pay.

a survey conducted by the DfE in 2014 shows that 80 percent of secondary schools that had been converted to a sponsor-led academy by then had changed their performance management system for teachers, by intensifying performance monitoring through both classroom observations and pupil progress, and by linking pay to performance (Cirin 2014).

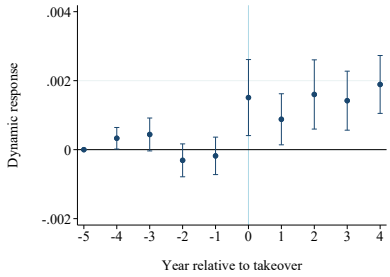
Figure 6: Teacher pay



(A) Incumbent teachers



(B) New teachers



(C) Pay dispersion

Source: DTR, 2001/02-2009/10, SWC, 2010/11-2018/19, GIAS, 2001/02-2018/19.
Notes: These graphs present the dynamic impact of the school management takeover on teachers' pay and pay dispersion. The results in Panels A and B are estimated by running the dynamic specification of regression 3 on the outcomes displayed in each graph. The Figure in Panel C is obtained by running regression 2 on pay dispersion. The estimation sample includes schools that convert between 2007/08 to 2012/13 (treated schools) and schools that convert six years after each treated cohort (control schools). The estimation period includes five years before to four years after the takeover of treated schools. Each graph also displays 90-percent confidence intervals corresponding to robust standard errors clustered at the school-cohort level.

Thus, our findings are consistent with the results of Biasi (2021) who show that a 2011 reform that allowed school districts in Wisconsin to abandon a strict seniority-based pay scale increased the returns to teacher quality. Similarly, it seems plausible that sponsors promote a culture that rewards performance more than seniority, which, in turn, is consistent with the patterns of teacher sorting triggered by the takeover.

Table 6: Teacher pay

	Incumbent teachers			New hires			
	Log base pay (1)	Additional payments /Gross pay (2)	Log gross pay (3)	Log base pay (4)	Additional payments /Gross pay (5)	Log gross pay (6)	Pay dispersion (7)
Sponsored academy	0.022***	-0.014***	0.007	0.041***	-0.018***	0.021**	0.00141***
	(0.004)	(0.002)	(0.004)	(0.009)	(0.002)	(0.009)	(0.00035)
p-value	(0.000)	(0.000)	(0.102)	(0.000)	(0.000)	(0.022)	(0.000)
q-value	[0.001]	[0.001]	[0.111]	[0.001]	[0.001]	[0.035]	[0.001]
Observations	255,517	255,517	255,517	47,630	47,630	47,630	5,589
Adjusted R^2	0.601	0.484	0.618	0.620	0.428	0.636	0.100
Teacher FE	✓	✓	✓	✓	✓	✓	
School FE	✓	✓	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓	✓
Cohort FE	✓	✓	✓	✓	✓	✓	✓
Pre-SA Mean T	39,219	0.06	41,737	37,874	0.05	40,113	0.0156
Pre-SA Mean C	39,536	0.05	41,638	36,833	0.04	38,687	0.0148

Source: DTR, 2001/02-2008/09, SWC, 2010/11-2018/19, GIAS, 2001/02-2018/19.

Notes: This table presents the impact of the school management takeover on teachers' pay and pay dispersion. The results in Column 1-6 are estimated by running regression 3 on the outcomes displayed on top of each column. The results in Column 7 are estimated by running regression 1 on pay dispersion. Robust standard errors clustered at the school-cohort level are displayed in parenthesis. The table also reports p-values associated to the estimated coefficients and sharpened FDR q-values accounting for multiple hypothesis testing. The estimation sample includes schools that convert between 2007/08 to 2012/13 (treated schools) and schools that convert six years after each treated cohort (control schools). The estimation period includes five years before to four years after the takeover of treated schools.

*** p<0.01, ** p<0.05, * p<0.1.

To complement these results, we study how sponsors manage their overall budget, by exploiting school expenditure data, although these are only available for takeovers taking place from 2010/11.[30] As explained in Section 2, sponsors receive a one-off grant to manage the conversion process, corresponding to, at most, 7 percent of the school annual income based on pupil seats pre-takeover. Figure 7 shows that indeed school income jumps up by 6 percent in the takeover year, reversing a negative trend due to declining pupil numbers - the effect appears larger when looking at income per pupil (or per teacher) as the school does not yet operate at full capacity the year the sponsor takes the lead. Consistent with the purpose of the grant, the additional resources are mostly allocated to non-teaching expenditure, including the hiring of temporary administrative staff to manage the conversion and other running costs such as the building refurbishment.

[30]The other main drawback of this data set is that it does not have information on capital spending.

Table 7: Returns to experience - incumbent teachers

	Log annual gross pay	
	(1)	(2)
Experience*Treated	0.015***	0.020***
	(0.001)	(0.002)
p-value	(0.000)	(0.000)
q-value	[0.001]	[0.001]
Experience*Treated*Post	-0.008***	-0.007***
	(0.000)	(0.000)
p-value	(0.000)	(0.000)
q-value	[0.001]	[0.001]
Master's or more*Treated	-0.131***	-0.017
	(0.016)	(0.067)
p-value	(0.000)	(0.806)
q-value	[0.001]	[0.476]
Master's or more*Treated*Post	0.067***	0.048**
	(0.004)	(0.021)
p-value	(0.000)	(0.024)
q-value	[0.001]	[0.038]
Top GPA in education*Treated		-0.042
		(0.069)
p-value		(0.540)
q-value		[0.343]
Top GPA in education*Treated*Post		0.022
		(0.039)
p-value		(0.573)
q-value		[0.355]
Observations	255,517	57,135
Adjusted R^2	0.590	0.641

Source: DTR, 2001/02-2008/09, SWC, 2010/11-2018/19, GIAS, 2001/02-2018/19.

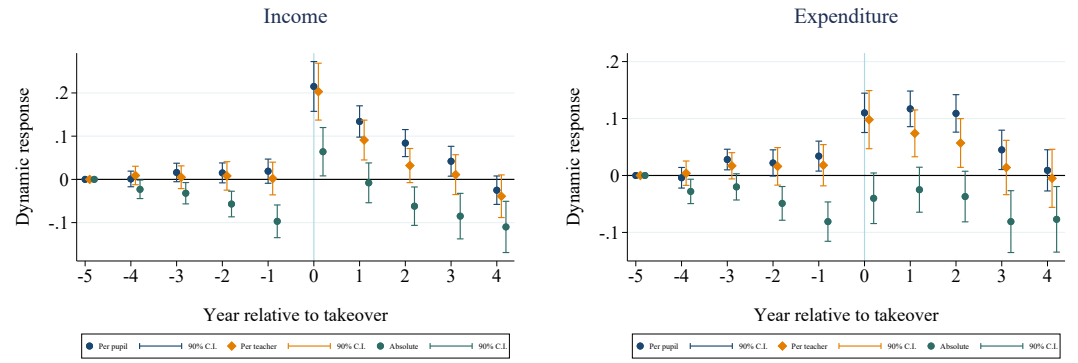
Notes: This table presents the impact of the school management takeover on the return to teachers' observable characteristics. The regressions also control for teachers' experience, and teacher, school, year, and cohort fixed effects. Robust standard errors clustered at the school-cohort level are displayed in parenthesis. The table also reports p-values associated to the estimated coefficients and sharpened FDR q-values accounting for multiple hypothesis testing. Information on teachers' GPA in education is only available for 25 percent of the sample.

*** p<0.01, ** p<0.05, * p<0.1.

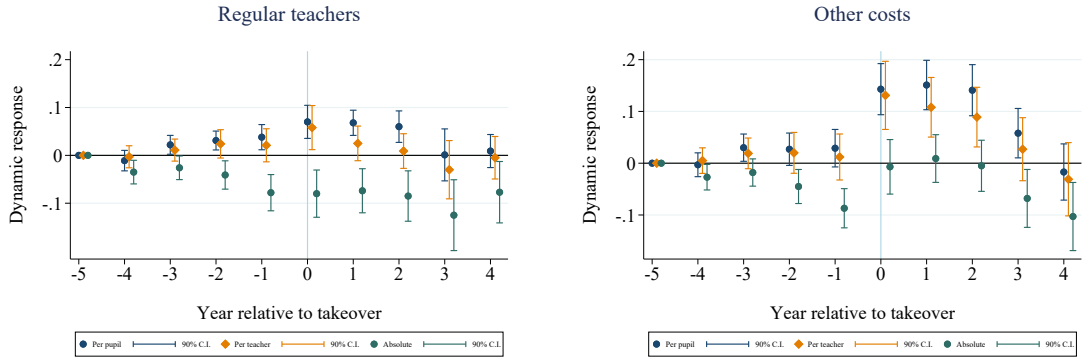
The takeover year, teaching expenditure per teacher significantly increases by 6 percent, or around £3,400 ($4,450), most likely to secure the hiring of the new headteacher, but, on average, it does not significantly change in treated schools compared to control ones (see Appendix Table B.7).

These results suggest two things. First, on average, the takeover is not accompanied by a permanent increase in school resources and spending. How can this be squared with the salary increase that we have documented for both the new headteacher and the new hires? We detail

Figure 7: School expenditure and resources



(A) Log income and expenditure



(B) Log expenditure items

Source: GIAS, 2005/06-2018/19.
Notes: These graphs present the dynamic impact of the school management takeover on school resources and expenditure. The results are estimated by running regression 2 on the outcomes displayed in each graph. Blue (orange) dots (diamonds) refer to outcomes per pupil (teacher), and green dots to outcomes measured in absolute terms. The estimation sample includes schools that convert between 2010/11 to 2012/13 (treated schools) and schools that convert six years after each treated cohort (control schools). The estimation period includes five years before to four years after the takeover of treated schools. Each graph also displays 90-percent confidence intervals corresponding to robust standard errors clustered at the school-cohort level.

our calculations in Appendix Section B.4 but the short answer is that these additional resources allocated to newcomers represent only 0.6 percent of the average school annual spending, and thus, should be relatively easy to find for the sponsors who acquire the autonomy to manage the school budget. Second, a recent meta-analysis conducted by Jackson and Mackevicius (2024) concludes that, on average, a $1,000 per pupil increase in school spending sustained over four years increases test scores by 0.0316σ. Our results show that the DfE grant translates into an average increase in spending per pupil of around £390 ($510) per year over 5 years (see Table B.7), while, together, Eyles et al. (2016), Andrews et al. (2017), and Eyles and Machin (2019) show that sponsor-led academies improve student performance by 0.07-0.12σ. Thus, although the temporary increase in resources may contribute to explain these effects, it seems unlikely that it can fully explain the takeover impact. In the next section, we attempt to quantify to what extent the reshuffle of the teaching body brought about by the takeover may play a role in explaining sponsors' effectiveness.

5.5 Mediation analysis

The results of Eyles et al. (2016), Andrews et al. (2017), and Eyles and Machin (2019) show that sponsor-led takeovers significantly improve student performance. Our results provide evidence that sponsors profoundly restructure the school personnel policy, and that the takeover is accompanied by a substantial reshuffle of the teaching body. In Table 8, we attempt to link these two results. Column 1 reports the effect of the takeover on the share of pupils achieving at least a sufficient level of learning in end-of-high-school final exams, that we introduced in Table 2. Column 2 re-estimates this regression on the sub-sample of schools for which we observe all characteristics of both the headteacher and teachers, which has no impact on the takeover effect. In Columns 3-8, we progressively augment this regression with the inclusion of the headteacher and teachers' characteristics that the takeover has changed, i.e. the probability that the headteacher comes from an outstanding school in management, the probability that the principal has at least a master's degree, the share of teachers coming from outstanding schools in teaching, the share of *Teach First* teachers, and the share of teachers with top-GPA in their education. We include each of them by themselves and interacted with the takeover dummy. Each of these variables seems to explain part of the effect of the takeover on pupil performance. In particular, when included together in Column 8, they collectively seem to explain 26 percent of the takeover effect. Although the results of this sort of mediation analysis should always be taken with caution, these estimates suggest that, while sponsors' personnel policy is not the only factor behind the success of sponsor academies, it plays a non-negligible role in explaining it.

Table 8: Mediation analysis

	Share pupils with 5 A*-C							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Sponsored academy (SA)	0.070***	0.068***	0.068***	0.059***	0.065***	0.061***	0.064***	0.050***
	(0.005)	(0.005)	(0.005)	(0.008)	(0.006)	(0.005)	(0.006)	(0.008)
New head from outstanding in management			0.003					0.004
			(0.012)					(0.012)
SA*New head from outstanding in management			-0.003					-0.004
			(0.017)					(0.017)
New head with master's or more				0.003				0.003
				(0.005)				(0.005)
SA*New head with master's or more				0.014				0.010
				(0.009)				(0.009)
Share from outstanding in teaching					-0.098			-0.082
					(0.141)			(0.138)
SA*Share from outstanding in teaching					0.432*			0.386*
					(0.223)			(0.215)
Share Teach First						0.254**		0.244**
						(0.118)		(0.121)
SA*Share Teach First						0.392***		0.373**
						(0.135)		(0.144)
Share Top GPA in education							0.023	0.009
							(0.031)	(0.030)
SA*Share Top GPA in education							0.067	0.023
							(0.062)	(0.058)
Observations	4,432	4,160	4,160	4,160	4,160	4,160	4,160	4,160
Adjusted R-squared	0.696	0.672	0.672	0.672	0.672	0.678	0.672	0.678
School FE	✓	✓	✓	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓	✓	✓
Sample FE	✓	✓	✓	✓	✓	✓	✓	✓

Source: DTR, 2001/02-2008/09, SWC, 2010/11-2018/19, GIAS, 2001/02-2018/19.

Notes: This table presents a mediation analysis that studies the contribution of changes in teacher composition in explaining the takeover effect on pupil performance. Pupil performance is measured as the share of pupils achieving a sufficient level of learning, i.e., at least 5 A*-C including in English and Math. These results are estimated by running regression 1 on pupil performance. Robust standard errors clustered at the school-cohort level are displayed in parenthesis. In Column 1, the estimation sample includes schools that convert between 2008/09 to 2012/13 (treated schools) and schools that convert six years after each treated cohort (control schools). In the following columns, the sample is restricted to school-year observations with non-missing information on the headteacher and teachers' characteristics included in Column 8. The estimation period includes five years before to four years after the takeover of treated schools.

*** p<0.01, ** p<0.05, * p<0.1.

6 Matched DiD results

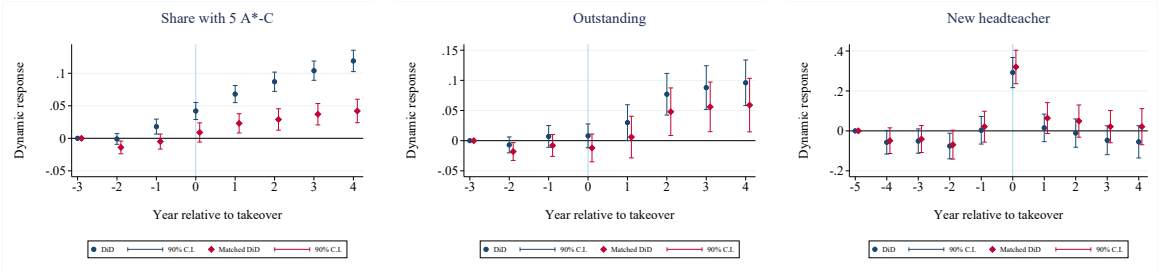
So far, we have estimated the impact of the takeover on sponsors' personnel policy by comparing the evolution of teachers' outcomes in schools that experience the takeover earlier and those that acquire academy status a few years later. In particular, as control group, we chose cohorts of schools that convert to academy six years after treated cohorts, which allowed us to study the dynamic effects of the takeover till four years after this event. However, as this choice could be considered arbitrary, in Appendix Section B.5, we show that our results are practically unchanged when restricting the estimation window to two years post-takeover and using as control group cohorts of schools that convert either four or five years after the takeover.

Still, any of these alternative specifications exploit variations in the timing of the takeover to estimate its effect on sponsors' personnel policy. In an effort to exclude that our results pick anticipation effects in control schools, we always exclude from the estimation window the immediate year before the completion of the takeover in the control group. Yet, one may still be worried that our estimates partly capture the effect of a deterioration of the school environment in control schools prior to the takeover. To mitigate these concerns, here we re-run our analysis by implementing a matched DiD design that focuses on the same treated cohorts considered so far, but uses as control group schools that will never be taken over by a sponsor, but that experience similar dynamics in pupils' and teachers' outcomes in the years preceding the takeover of treated schools. Importantly, this means that the matching strategy identifies, in the never-treated pool, schools that perform as badly as treated schools before the latter are taken over, but that for some reason escape the takeover. In other words, these schools must have done something other than the takeover to improve their performance and avoid the academy conversion. Our analysis offers some interesting insights in this respect.

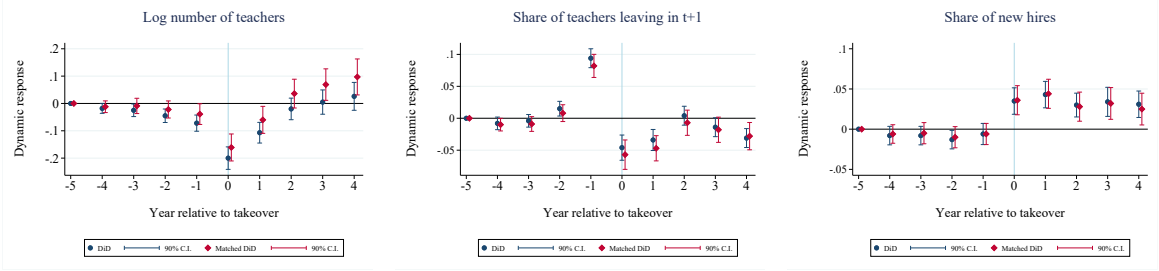
Figures 8 and 9 compare the results of our two identification strategies.[31] The blue dots refer to the estimates presented so far, obtained from estimation of the DiD comparing early and later takeovers. The red diamonds are obtained from the estimation of the matched DiD. The very first thing to notice is that, as anticipated, the takeover effect on pupil performance and the probability that schools are deemed outstanding by OFSTED are still positive and significant under the matched DiD design but lower than the estimated impact presented in Section 5.1. When focusing on sponsors' personnel policy, we see that all the estimates and dynamic evolution of the effects are remarkably similar across the two specifications, which supports the validity of our first identification method. However, there is one strategy that the matched control seems to share with sponsors, which is the use of *Teach First* teachers, as the estimated effect on this outcome becomes small and insignificant with the matched DiD design (see Figure 9, Panel D).

[31] Average effects are reported in Appendix Section C.

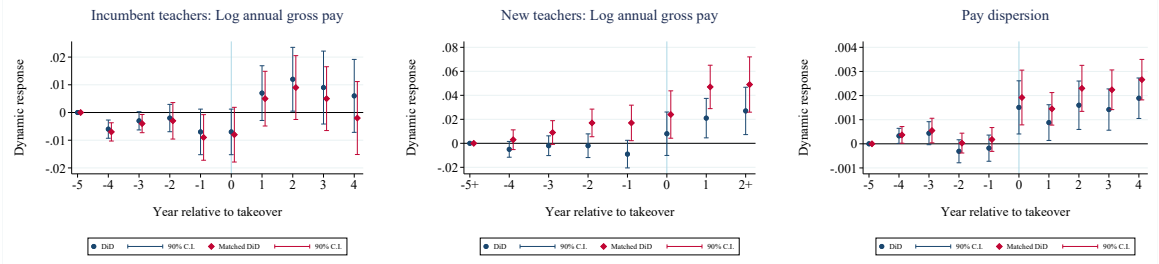
Figure 8: Main outcomes - Matched DiD



(A) Pupil and school performance, new head



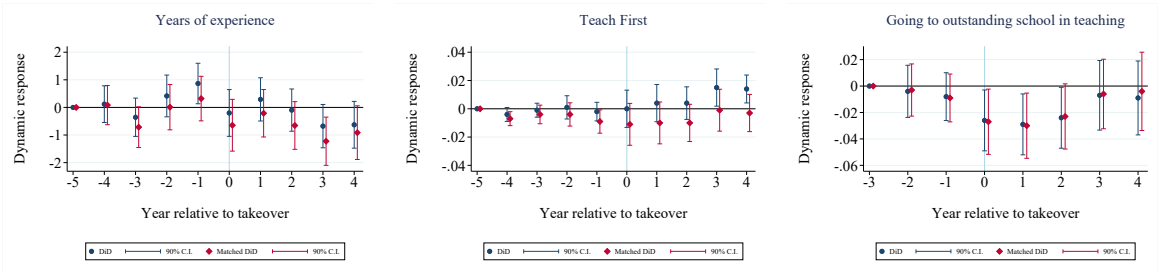
(B) Teacher turnover



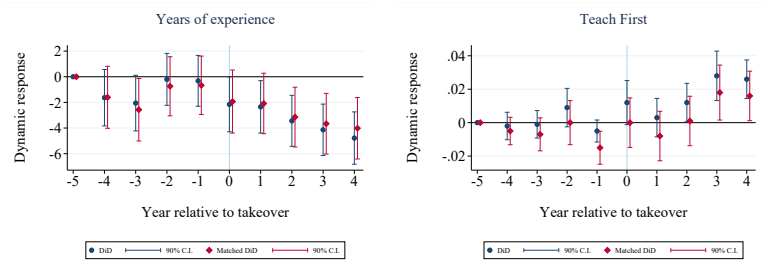
(C) Teacher pay

Source: DTR, 2001/02-2009/10, SWC, 2010/11-2018/19, GIAS, 2001/02-2018/19.
Notes: These graphs compare the dynamic impact of the school management takeover on pupil and school performance, the headteacher selection, and teacher turnover and pay, estimated using the two alternative identification strategies. For each outcome, the blue dots represent the estimates obtained when comparing earlier to later takeovers, while red diamonds refer to the estimates obtained using the matched DiD design. See Section 4 for more details on the two identification methods and estimation samples. Each graph also displays 90-percent confidence intervals corresponding to robust standard errors clustered at the school-cohort level.

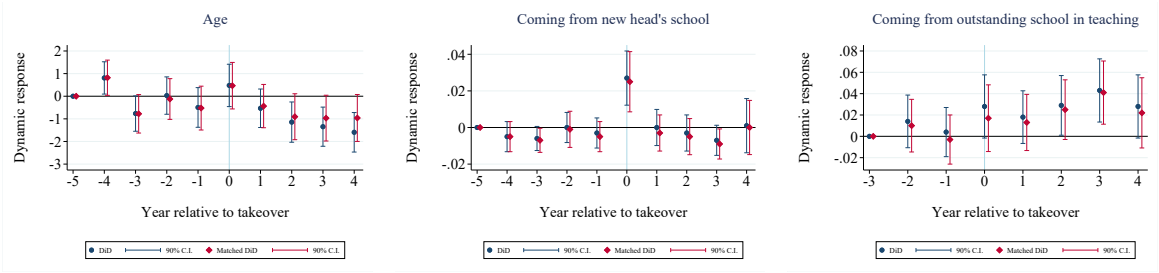
Figure 9: Teacher sorting - Matched DiD



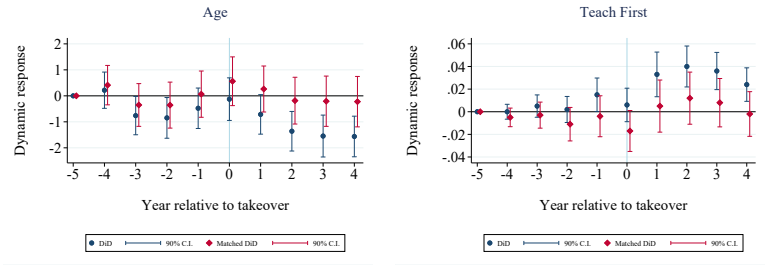
(A) Leavers to other school



(B) Leavers to other sector



(C) New hires from other school



(D) New hires from other sector

Source: DTR, 2001/02-2009/10, SWC, 2010/11-2018/19, GIAS, 2001/02-2018/19.
Notes: These graphs compare the dynamic impact of the school management takeover on teacher sorting, estimated using the two alternative identification strategies. For each outcome, the blue dots represent the estimates obtained when comparing earlier to later takeovers, while red diamonds refer to the estimates obtained using the matched DiD design. See Section 4 for more details on the two identification methods and estimation samples. Each graph also displays 90-percent confidence intervals corresponding to robust standard errors clustered at the school-cohort level. The cohort(s) 2007/08 (and 2008/09) is (are) excluded in the third graph of Panel A (C) because schools' OFSTED performance is only available from 2005/06.

Taken together these results offer three main insights. First, consistent with the findings of the literature studying the effects of *Teach for America* and *Teach First*, the fact that the matched control employs a similar share of *Teach First* teachers may help explain why the improvement in pupil and school performance brought about by the takeover appears smaller when using the matched DiD. Second, the success of sponsor-led takeover is not just the result of this choice, as sponsored academies significantly improve student and school OFSTED performance even compared to the matched control. Third, and in line with the findings of the personnel literature, our results show that effective managers implement a bundle of personnel practices, including the selection of new leaders (Bloom et al. 2015, Lavy et al. 2023) and a flexible pay policy that decouples employees' pay growth from a rigid seniority-based pay scale (Biasi et al. 2021, Willén 2021), which, in turn, allows them to attract high-performing employees and induce the least motivated to leave (Romero et al. 2020, Fenizia 2022, Minni 2023).

7 Heterogeneous effects across sponsors

As explained in Section 3, one of the peculiarities of sponsor-led takeovers is that they involve a variety of different types of sponsors, namely educational charities, successful schools, further and tertiary education institutions, religious organizations, and businesses. In every trust, the board of trustees is the key decision maker. Its members, who perform their role on a voluntary basis, are accountable for any decision made, including the selection and supervision of the headteacher. But importantly, trustees are appointed and can be removed by the founding sponsor who has the ultimate responsibility to ensure that the governance of the trust is effective.

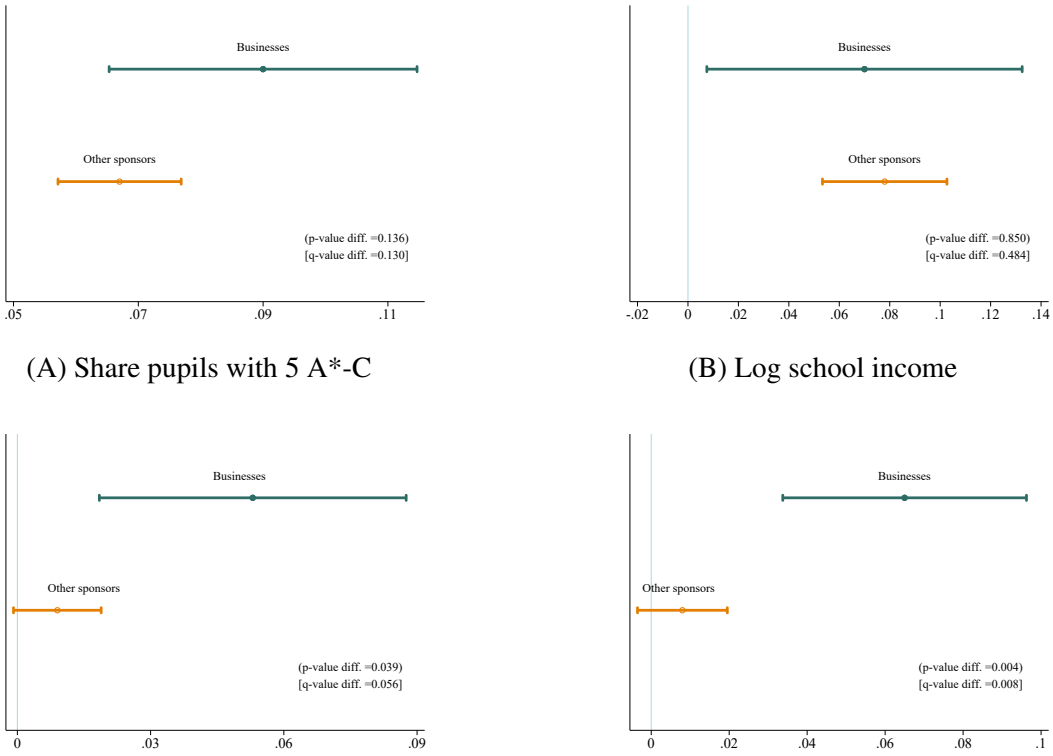
Given the heterogeneous nature of sponsoring organizations, it thus seems interesting to investigate whether different types of sponsors promote distinct personnel practices and if they ultimately obtain different results in terms of pupil and school performance. It is especially intriguing to compare businesses drawn from the private sector with all the other non-profit organizations, as it is a priori ambiguous what we should expect. On the one hand, businesses may be able to tap on a larger pool of candidates for leadership positions, and appeal more to fresh new graduates participating in the *Teach First* program. They may also be keener to abandon a seniority-based pay scheme. On the other hand, they should have less experience of the education sector, and this may limit their effectiveness.

Importantly, in this setting, sponsors are not randomly assigned to schools - as it happens in the Liberian program studied by Romero et al. (2020) and Romero and Sandefur (2022) - thus, we should be cautious in giving a causal interpretation to heterogeneous actions across sponsor types. Having said this, we do not find clear sorting patterns when correlating the probability that a school is taken over by a company with pupils and teachers' characteristics as of two years before

the takeover completion (see Appendix Table D.1).

We highlight here only the key heterogeneous results and report all the other estimates in Appendix Section D. Figure 10, Panel A shows that companies seem to improve pupil performance more than other sponsors, although the coefficients across the two subgroups are not statistically different. Importantly, this is not due to the fact that businesses bring more resources to the school, as Panel B shows. Instead, Panels C and D show that companies are significantly more likely to hire a new headteacher from outside the education sector, and to employ *Teach First* teachers.[32]

Figure 10: Heterogeneous effects across sponsor types



(A) Share pupils with 5 A*-C

(B) Log school income

(C) New head from other sector

(D) Share *Teach First* teachers

Source: DTR, 2001/02-2009/10, SWC, 2010/11-2018/19, , GIAS, 2001/02-2018/19, school websites, Wikipedia.
Notes: These graphs present the heterogeneous effects of the takeover across sponsor types. These results are estimated by running regression 1 on the outcomes displayed in each graph, by subgroup. The estimation sample includes schools that experience a takeover between 2007/08 to 2012/13 and schools that experience this event six years after, with non-missing information on the sponsor type. Expenditure data are only available for the 2010/11-2012/13 cohorts and their respective controls. Each graph also displays 90-percent confidence intervals corresponding to robust standard errors clustered at the school-cohort level, and the p-value and associated FDR sharpened q-value of the t-test on the equality of coefficients across subgroups.

[32]In Appendix Section D, we also show that private businesses hire overall more teachers after the takeover to compensate for a larger spike in teacher separations pre-takeover (see Appendix Table D.5 and Figure D.1).

These results are consistent with the considerations made in the previous paragraph. Hiring bright fresh university graduates into the teaching profession seems helpful to turn around the performance of struggling schools, and this may contribute to explain why sponsors who are able to attract a larger share of *Teach First* teachers obtain larger improvement in pupil performance. In turn, this may partly explain why, over time, the DfE assigns, on average, more schools to companies than other sponsors. At the same time, all types of sponsors systematically invest in new school headteachers and decouple teacher pay from seniority,[33] suggesting that there is a common perception that these are key personnel decisions.

8 Conclusion

One of the most pressing issues that education ministries around the world constantly face is how to improve low-performing, disadvantaged, public schools. This paper studies a large-scale reform that enables a range of independent organizations to take over the management of underperforming, high-poverty schools, while funding remains public. Exploiting the staggered expansion of English sponsored academies since the early 2000s, we show that the sponsor-led takeover leads to substantial changes in the school personnel policy, the school leadership, and the teaching body, which, in turn, contribute to explain the significant improvement in pupil performance.

To conclude, we want to stress two important points. First, our analysis suggests that the policy of delegating the management of low-performing, disadvantaged, state-funded schools to external organizations can help leveling the playing field, in that both the headteacher and teachers appointed by the new management are more likely to come from outstanding schools. Further considering the potential general equilibrium effects of the program, note that sponsored academies stimulate an expansion of teacher supply, as they attract both headteachers and teachers from outside the education system. An important avenue for future research is the analysis of spillover effects of school takeovers to nearby schools, as the expansion of sponsor-led academies is likely to have increased competition for the best teachers (Jackson 2012, Biasi et al. 2021).

Second, the sponsor management takeover is a multifaceted form of "treatment" and changes in the school personnel policy are likely not the only factor explaining the improvement in pupil performance following the takeover. Yet, documenting its effects on personnel practices helps open the black box of what effective managers do, which is especially important in a setting where employees, which in this case are teachers, but the same argument would hold for other public-sector workers like doctors or police officers, are a key input in the organization's production function.

[33] If anything, the point estimates reported in Appendix Section D suggest that the increase in pay dispersion is driven by schools sponsored by organizations other than businesses.

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Online Appendix

School Management Takeover, Leadership Change, and Personnel Policy

Emma Duchini Victor Lavy Stephen Machin Shqiponja Telhaj

A The English educational system

This section provides a brief overview of the main features of the English educational system.

Educational phases. Compulsory education in England is organized into Key Stages (KSs). Pupils enter school at age 4–5 and for two years are enrolled in the Foundation Stage. Then they progress to KS1, up until age 6–7, when they enter the last stage of primary education, called KS2. By the end of it, at age 10-11, students take KS2 standardized national tests, which assess their knowledge in English, mathematics and science. Next, they enter secondary school which lasts from age 11-12 to age 15-16, and is divided into KS3 (up to age 13-14) and KS4. At the end of this stage, pupils sit externally marked academic (GCSE) and/or vocational (NVQ/BTEC) exams in a range of subjects, with English, math and science being the only compulsory ones. Finally, after compulsory education, pupils can keep on studying in so-called sixth-form colleges, which normally last for two years, and then move to university. Around 93 percent of students keep on studying at upper secondary school level.

School types. As for the institutional organization, before the advent of academies, there were four types of state-funded schools at both the primary and secondary stage:[A.1] community, voluntary controlled (VC), voluntary aided (VA) and foundation schools. *Community schools*, representing slightly more than 50 percent of state-funded schools before the advent of academies, are fully managed by the LA, and because of this they are often called LA-maintained schools. The LA sets the admission criteria of community schools, and allocates funding based on pupils' numbers and presence of specific categories of children, such as pupils with special education needs (SEN); it also owns schools' land and building, and supports them in the provision of administrative and accounting activities. Importantly, the LA has historically managed community schools' personnel hiring and pay, though a pay reform introduced in 2014 has granted schools more autonomy in this respect (see the dedicated paragraph below). Finally, the local governing body of community schools includes members of staff, representatives of the LA and parents. *Voluntary-controlled* and *voluntary-aided schools*, representing, respectively, 11 and 19 percent of state-funded schools before 2010/11, are mostly faith schools, and their land and building are owned by a charity, typically a church, rather than the LA. As in the case of community schools, VC schools are managed by the LA, which employs the staff and sets the entrance criteria, while the charity supporting the school only appoints some members of the governing body. For this reason, throughout the paper, we refer to community and VC schools as "fully managed by the LA". In contrast, in the case of VA schools, it is the charity supporting the school that appoints the majority of members of the

[A.1] In this paper, we do not consider private, or so-called independent schools, which represent around 6 percent of English schools as of 2019 (Independent Schools Council 2019).

governing body, and this is responsible for managing the staff and setting the admission criteria.[A.2] *Foundation schools*, representing around 9 percent of state-funded schools before the advent of academies, derive their name from the fact that they are usually supported by a Trust or Foundation. Though they are not necessarily faith schools, they enjoy the same autonomy of VA schools in terms of staff and entrance criteria. Importantly, all these four types of schools are funded by the local authority, they follow the National Curriculum, and they do not charge entrance fees.

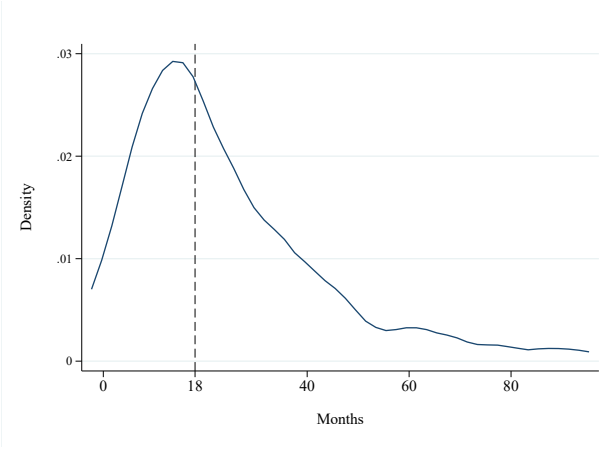
2014 Pay Reform. While academies can set teachers' pay autonomously, until 2014, community schools had to follow a national pay scale. Teachers' base salary was determined by their scale point, which in turn was a function of their years of experience as a teacher, and geographical location (Burgess et al. 2022). In September 2013, the government approved a major reform of teachers' pay, which replaced the existing pay scale with a pay framework that only sets minima and maxima for teachers and headteachers' pay. In essence, this reform granted all state-funded schools the autonomy that academies already had to determine individual teachers' pay. The reform first affected teachers' pay in the school year 2014/15. Burgess et al. (2022) find that this national pay reform increased teachers' pay progression, retention and students' performance in schools located in high-wage LAs, compared to schools located in labor markets with worse outside options. Interestingly, and in contrast to the effect of sponsor-led takeovers, the reform has not led to an increase in pay dispersion, suggesting that affected schools uniformly adjusted all teachers' pay to local economy conditions, rather than using the reform to differentiate teachers based on their performance. Still, because this reform is introduced in the post-takeover period of some of the conversion cohorts that we study, we show in Appendix Section B that the effect on pay dispersion is unchanged if we exclude these cohorts from the estimation sample (see Appendix Table B.6).

[A.2] In case of over-subscription, for instance, VA schools can select students based on religious affiliation. Yet, they are not allowed to select based on ability.

B Further results and robustness checks

B.1 Length of the conversion process

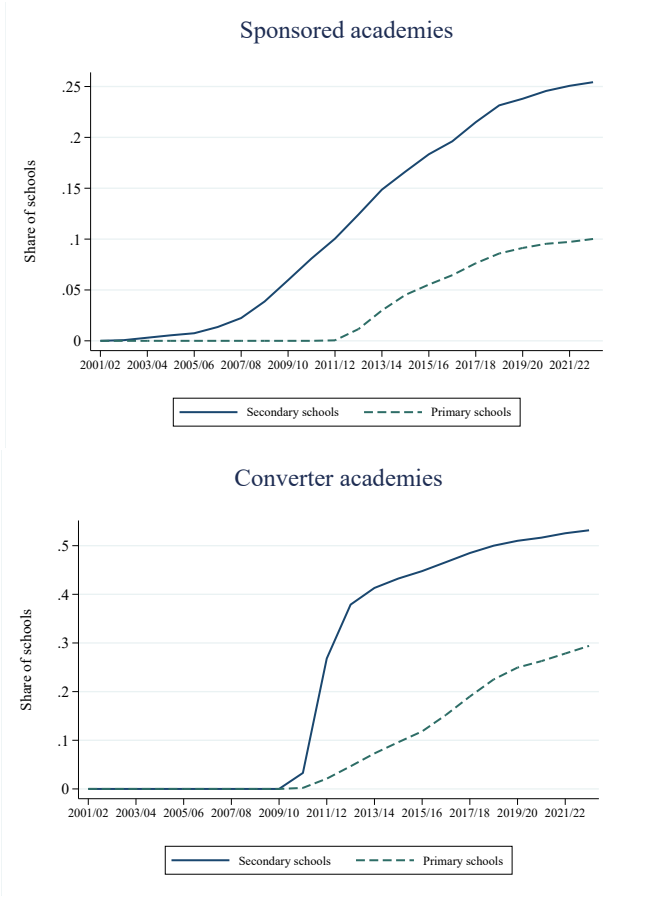
Figure B.1: Months from application to conversion



Source: GIAS, 2010/11-2018/19.

Notes: This graph reports the distribution of the number of months between the application date and the conversion date among the 14 percent of schools converting to sponsored academies after 2010/11 that have provided these data to the DfE.

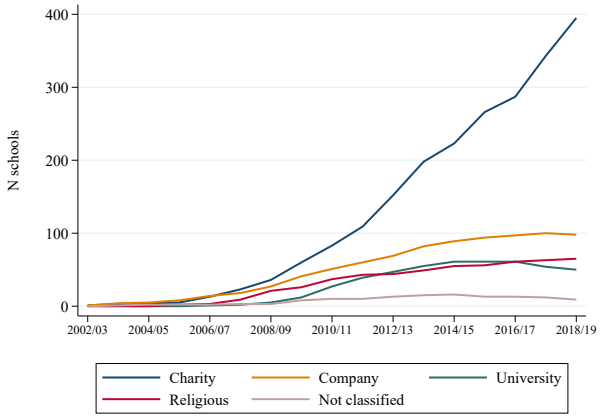
Figure B.2: Academies' expansion among English schools



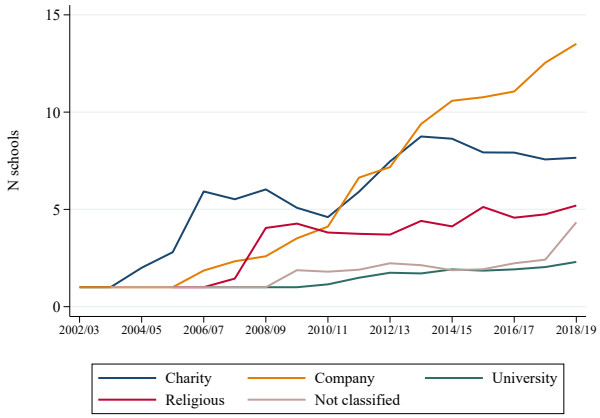
Source: GIAS, 2001/02-2022/23.

Notes: These graphs present the expansion of academies over time in English secondary and primary schools.

Figure B.3: Sponsors' types



(A) Total number of schools per type



(B) Number of schools per sponsor

Source: Data collected by the authors from schools' websites, Wikipedia, and magazines.
Notes: This graph shows the distribution of sponsors' types over time. The "Charity" category includes both educational charities and schools founding a charity to sponsor other low-performing schools.

B.2 Pupil and school outcomes

The impact of sponsor-led takeovers on pupil performance and educational attainment has been studied by Eyles et al. (2016), Andrews et al. (2017), and Eyles and Machin (2019). All the three studies implement a stacked DiD design, combined with an IV approach. Similar to Abdulkadiroğlu et al. (2016) who study charter takeovers in New Orleans, these papers focus on legacy pupils, i.e., pupils who are already enrolled in the school before the conversion, and compare their end-of-high school performance with that of pupils who are enrolled in schools that will convert a few years later. All the three studies estimate both OLS and IV specifications, where the legacy enrollment variable is used as an instrument for the actual school attended, to take into account that a few pupils leave the academy before grade 11. The studies differ in the treated cohorts considered or the control group used, with Eyles et al. (2016) and Eyles and Machin (2019) focusing on earlier treated cohorts and slightly different control groups, while the report by Andrews et al. (2017) compare the effects across conversion cohorts.

Taken together, these studies find that sponsor-led academies improve pupil performance in secondary school standardized exams by 7 to 12% of a standard deviation, depending on the specific treated cohorts considered and control group used. They also find that, after the takeover, the school intake begins to change as sponsor-led academies gradually start attracting pupils with higher end-of-primary school grades (Andrews et al. 2017, Eyles and Machin 2019), and both the share of FSM-eligible students and the share of white British students significantly decrease following the takeover (Eyles and Machin 2019).

With school-level data, we cannot exactly replicate the results of these studies for three reasons. First, while these papers focus on legacy pupils, with school-level data, we only have an aggregate measure of pupil performance and, thus, our results capture a mixture of the effect on legacy pupil performance and the composition effects of the takeover.

Second, as measure of pupil performance, school-level data only provide the headline variable introduced in Section 3, i.e., the share of pupils with 5 A*-C including English and Math, while the studies cited above construct a numerical variable measuring the pupil-level standardized best 8 exams points score to circumvent changes in the conversion of points to grades that have taken place through the period under analysis.

Third, the variable measuring the share of pupils with 5 A*-C including English and Math is only available from 2005/06 which limits the possibility to estimate long event-study specifications. As for the variable measuring average primary-school performance for pupils enrolled in secondary schools, this is only available from the school year 2010/11, thus we refer to the studies cited above for the analysis on this outcome, but exploit the school-level data to present the takeover effect on pupils' socio-demographic characteristics.

With these caveats in mind, in Section 5.1, we showed that the takeover effect on pupil

performance that we estimate with school-level data is consistent with the findings of Eyles et al. (2016), Andrews et al. (2017), and Eyles and Machin (2019). We further extended their analysis by studying the impact of the takeover on schools' probability of being judged "outstanding" by OFSTED, which, as in the case of pupil performance, is only available in school performance tables starting from 2005/06.

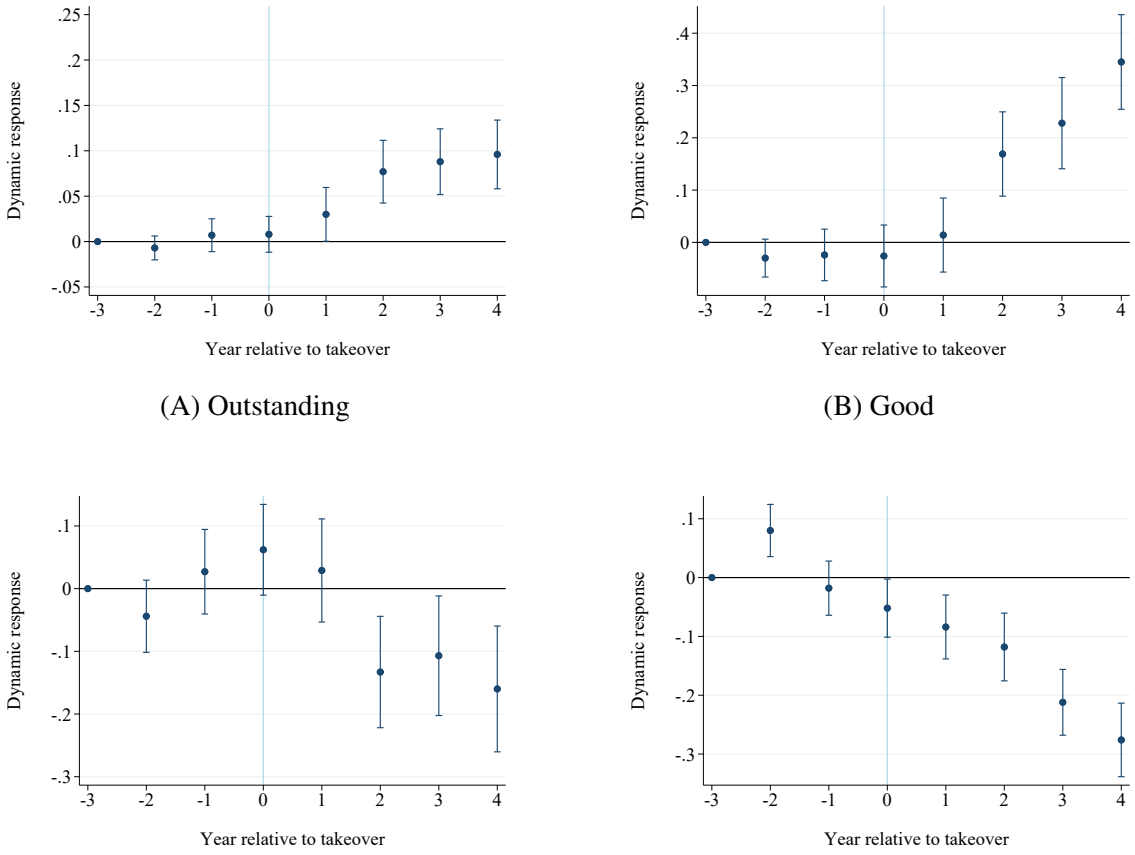
Here, we complement the analysis of Section 5.1 in three ways. First, in Figure B.4 and Table B.1, we show that the takeover also increases a school's probability of being deemed "good" by OFSTED, and decreases both the probability that it is judged "inadequate" and the likelihood that the school "requires improvements".

Second, in Table B.2, we complement the results of Table 2, by showing that the average takeover effect on pupil and school performance changes little when we also include the 2007/08 cohort in the estimation sample.

Third, in Figure B.5 and Table B.3, we present the evolution of pupil numbers and composition around the takeover year. The first important thing to notice is that the takeover is preceded by a decline in pupil numbers, which may contribute to activate the takeover process (Panel A). In Section 5.3, we show that teacher numbers follow a similar, though less pronounced, trend. Importantly, this implies that, by comparing early and later takeovers, the average effect on these outcomes results, at least in part, from the dynamics experienced by the control group before its own takeover. This is why it is especially important to complement this strategy with the estimation of the matched DiD, which explicitly selects a control group that never experiences the takeover.

Finally, and consistent with the results of Eyles and Machin (2019), Figure B.5 and Table B.3 show that pupil socio-demographic composition gradually changes around the takeover year. In particular, sponsored academies become more diverse relative to their predecessor school, as both the share of white British students and the share of English native pupils significantly decrease after the takeover, while the share of FSM-eligible students starts declining by the end of the estimation period.

Figure B.4: School OFSTED performance



(A) Outstanding

(B) Good

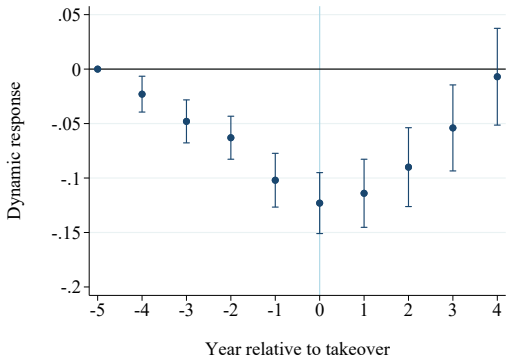
(C) Requires improvement

(D) Inadequate

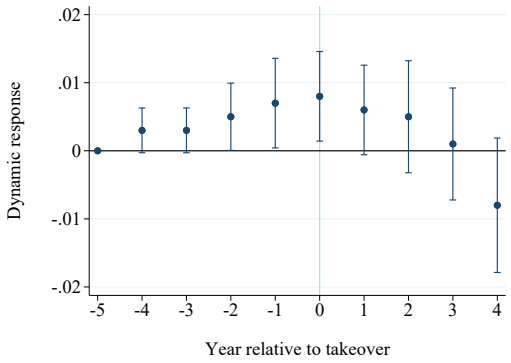
Source: OFSTED, 2005/06-2018/19.

Notes: These graphs present the dynamic impact of the school management takeover on school OFSTED performance. These results are estimated by running regression 2 on the outcome displayed in each graph. The estimation sample includes schools that convert between 2008/09 to 2012/13 (treated schools) and schools that convert six years after each treated cohort (control schools). The estimation period includes five years before to four years after the takeover of treated schools. Each graph also reports 90-percent confidence intervals corresponding to robust standard errors clustered at the school-cohort level.

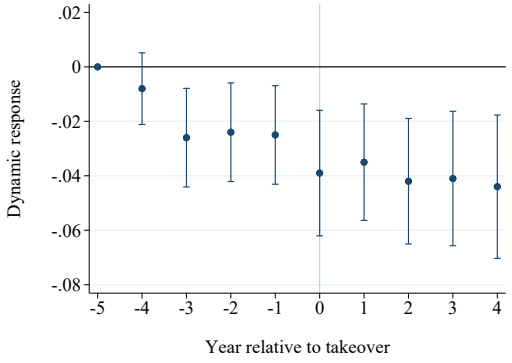
Figure B.5: Pupil numbers and socio-demographic composition



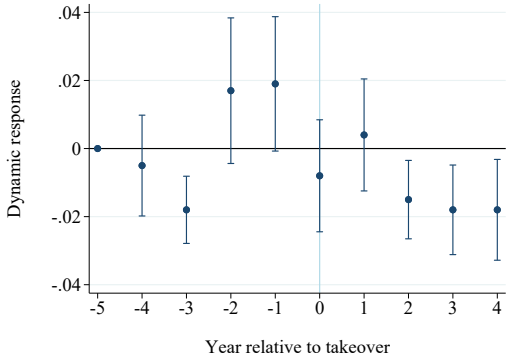
(A) Log number of pupils



(B) Share FSM-eligible



(C) Share white British



(D) Share English native

Source: GIAS, 2001/02-2018/19.

Notes: These graphs present the dynamic impact of the school management takeover on pupil numbers and socio-demographic composition. These results are estimated by running regression 2 on the outcome displayed in each graph. The estimation sample includes schools that convert between 2007/08 to 2012/13 (treated schools) and schools that convert six years after each treated cohort (control schools). The estimation period includes five years before to four years after the takeover of treated schools. Each graph also reports 90-percent confidence intervals corresponding to robust standard errors clustered at the school-cohort level.

Table B.1: School OFSTED performance

	Outstanding	Good	Requires Improvement	Inadequate
	(1)	(2)	(3)	(4)
Sponsored academy	0.048***	0.111***	-0.033	-0.136***
	(0.012)	(0.030)	(0.032)	(0.020)
p-value	(0.000)	(0.000)	(0.291)	(0.000)
q-value	[0.001]	[0.001]	[0.229]	[0.001]
Observations	4,432	4,432	4,432	4,432
Adjusted R^2	0.436	0.279	0.259	0.151
School FE	✓	✓	✓	✓
Year FE	✓	✓	✓	✓
Cohort FE	✓	✓	✓	✓
Pre-SA Mean T	0.01	0.14	0.56	0.16
Pre-SA Mean C	0.06	0.28	0.47	0.05

Source: OFSTED, 2005/06-2018/19.

Notes: This table presents the impact of the school management takeover on school OFSTED performance. These results are estimated by running regression 1 on the outcomes displayed on top of each column. Robust standard errors clustered at the school-cohort level are displayed in parenthesis. The table also reports p-values associated to the estimated coefficients and sharpened FDR q-values accounting for multiple hypothesis testing. The estimation sample includes schools that convert between 2008/09 to 2012/13 (treated schools) and schools that convert six years after each treated cohort (control schools). The estimation period includes five years before to four years after the takeover of treated schools.

*** p<0.01, ** p<0.05, * p<0.1.

Table B.2: Pupil and school OFSTED performance - different estimation samples

	Share pupils with 5 A*-C		Outstanding		Outstanding in management		Outstanding in teaching	
	2008-13 (1)	2009-13 (2)	2008-13 (3)	2009-13 (4)	2008-13 (5)	2009-13 (6)	2008-13 (7)	2009-13 (8)
Sponsored academy	0.071***	0.070***	0.049***	0.048***	0.090***	0.089***	0.030***	0.030***
	(0.005)	(0.005)	(0.012)	(0.012)	(0.016)	(0.017)	(0.010)	(0.011)
p-value	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.002)	(0.004)
q-value	[0.001]	[0.001]	[0.001]	[0.001]	[0.001]	[0.001]	[0.006]	[0.008]
Observations	5,048	4,432	5,048	4,432	5,048	4,432	5,048	4,432
Adjusted R^2	0.696	0.696	0.439	0.436	0.387	0.373	0.403	0.387
School FE	✓	✓	✓	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓	✓	✓
Cohort FE	✓	✓	✓	✓	✓	✓	✓	✓
Pre-SA Mean T	0.31	0.31	0.01	0.01	0.03	0.03	0.01	0.01
Pre-SA Mean C	0.40	0.41	0.05	0.06	0.08	0.09	0.02	0.02

Source: GIAS and OFSTED, 2005/06-2018/19.

Notes: This table presents the impact of the school management takeover on pupil performance in end-of-high school centralized exams and school OFSTED performance. Pupil performance is measured as the share of pupils achieving a sufficient level of learning, i.e., at least 5 A*-C including in English and Math. Robust standard errors clustered at the school-cohort level are displayed in parenthesis. The table also reports p-values associated to the estimated coefficients and sharpened FDR q-values accounting for multiple hypothesis testing. These results are estimated by running regression 3 on the outcomes displayed on top of each column. For each outcome, the estimation sample in the first (second) column includes schools that convert between 2007/08 (2008/09) to 2012/13 and schools that convert six years after each treated cohort. The estimation period includes five years before to four years after the takeover of treated schools.

*** p<0.01, ** p<0.05, * p<0.1.

Table B.3: Pupil numbers and socio-demographic composition

	Log number of pupils (1)	Share FSM eligible (2)	Share white British (3)	Share English native (4)
Sponsored academy	-0.031**	-0.000	-0.020***	-0.014*
	(0.015)	(0.003)	(0.007)	(0.007)
p-value	(0.032)	(0.944)	(0.003)	(0.060)
q-value	[0.048]	[0.530]	[0.006]	[0.076]
Observations	5,589	5,589	5,556	5,216
Adjusted R^2	0.873	0.923	0.880	0.837
School FE	✓	✓	✓	✓
Year FE	✓	✓	✓	✓
Cohort FE	✓	✓	✓	✓
Pre-SA Mean T	1,009	0.24	0.78	0.76
Pre-SA Mean C	1,062	0.19	0.78	0.81

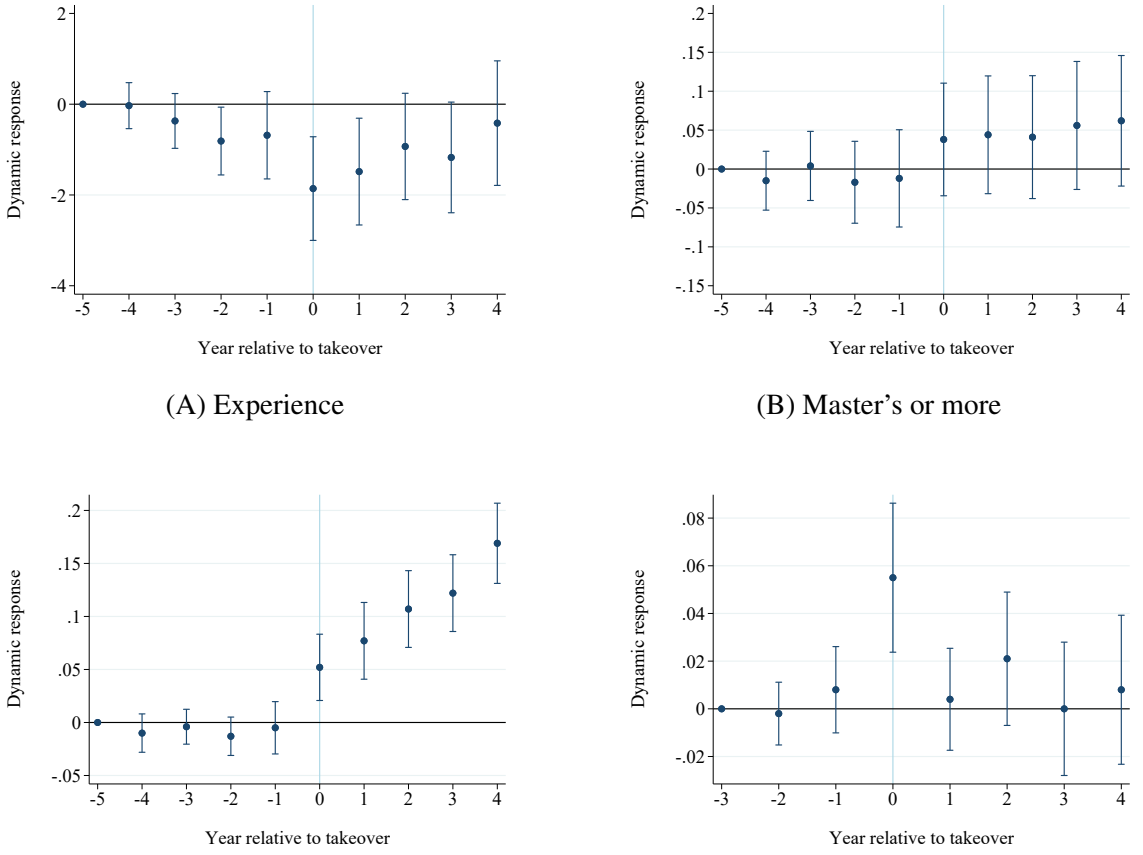
Source: GIAS, 2001/02-2018/19.

Notes: This table presents the impact of the school management takeover on pupil numbers and socio-demographic composition. These results are estimated by running regression 1 on the outcomes displayed on top of each column. Robust standard errors clustered at the school-cohort level are displayed in parenthesis. The table also reports p-values associated to the estimated coefficients and sharpened FDR q-values accounting for multiple hypothesis testing. The estimation sample includes schools that convert between 2007/08 to 2012/13 (treated schools) and schools that convert six years after each treated cohort (control schools). The estimation period includes five years before to four years after the takeover of treated schools. Information on the share of white British (English native) pupils is missing for 1 (7) percent of school-year observations.

*** p<0.01, ** p<0.05, * p<0.1.

B.3 Additional results

Figure B.6: Headteacher's characteristics



(A) Experience

(B) Master's or more

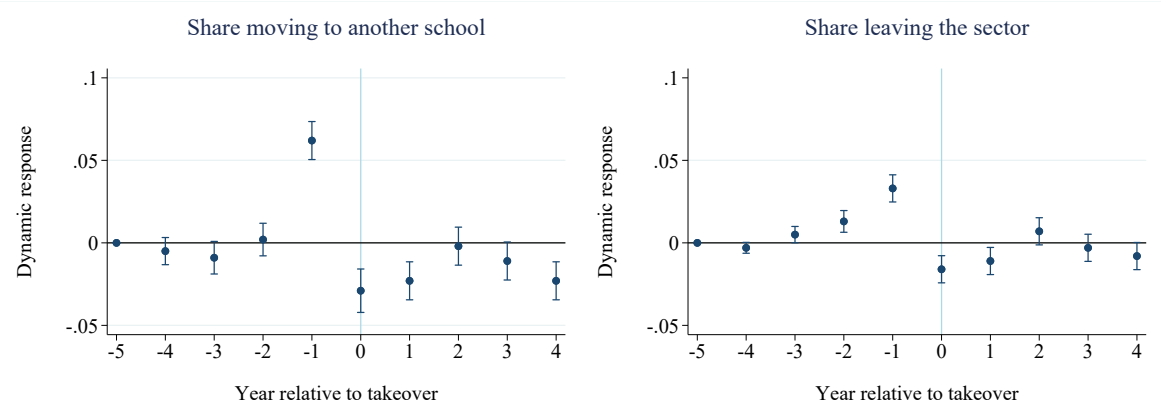
(C) Log annual gross pay

(D) From outstanding school in management

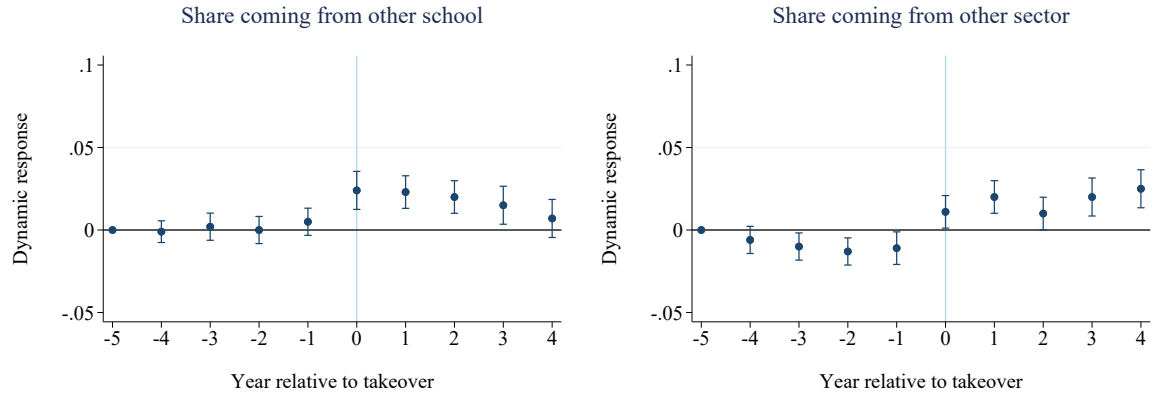
Source: DTR, 2001/02-2009/10, SWC, 2010/11-2018/19, GIAS, 2001/02-2018/19.

Notes: These graphs present the dynamic impact of the school management takeover on the headteacher's traits. These results are estimated by running regression 2 on the outcomes displayed in each graph. The estimation sample includes schools that convert between 2007/08 to 2012/13 (treated schools) and schools that convert six years after each treated cohort (control schools). The estimation period includes five years before to four years after the takeover of treated schools. Each graph also displays 90-percent confidence intervals corresponding to robust standard errors clustered at the school-cohort level. The graph in Panel D excludes the 2007/08 cohort, because school OFSTED performance is only available from the school year 2005/06.

Figure B.7: Teacher turnover - where to and from



(A) Share teachers leaving in t+1

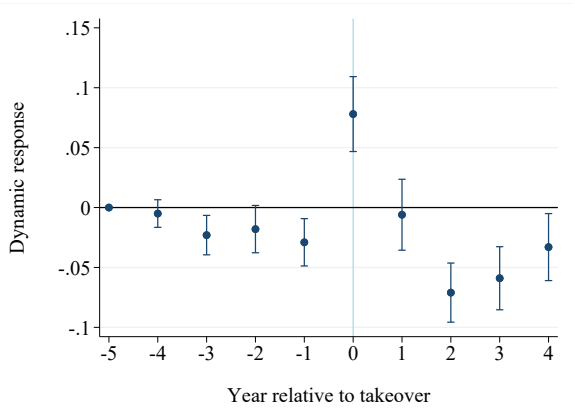


(B) Share of new hires

Source: DTR, 2001/02-2009/10, SWC, 2010/11-2018/19 GIAS, 2001/02-2018/19.

Notes: These graphs present the dynamic impact of the school management takeover on teacher turnover. These results are estimated by running regression 2 on the outcomes displayed in each graph. The estimation sample includes schools that convert between 2007/08 to 2012/13 (treated schools) and schools that convert six years after each treated cohort (control schools). The estimation period includes five years before to four years after the takeover of treated schools. Each graph also displays 90-percent confidence intervals corresponding to robust standard errors clustered at the school-cohort level.

Figure B.8: Pupil-teacher ratio



Source: DTR, 2001/02-2009/10, SWC, 2010/11-2018/19 GIAS, 2001/02-2018/19.

Notes: This graph presents the dynamic impact of the school management takeover on the pupil-teacher ratio. These results are estimated by running regression 2 on the outcomes displayed in each graph. The estimation sample includes schools that convert between 2007/08 to 2012/13 (treated schools) and schools that convert six years after each treated cohort (control schools). The estimation period includes five years before to four years after the takeover of treated schools. The graph also displays 90-percent confidence intervals corresponding to robust standard errors clustered at the school-cohort level.

Table B.4: Teacher turnover

		Share teachers leaving in t+1			Share new hires			
	Log number of teachers (1)	Any destination (2)	To other school (3)	To other sector (4)	Any origin (5)	From other school (6)	From other sector (7)	Log pupil-teacher ratio (8)
Sponsored academy	-0.044**	-0.054***	-0.034***	-0.020***	0.044***	0.019***	0.025***	0.013
	(0.018)	(0.006)	(0.004)	(0.003)	(0.005)	(0.004)	(0.003)	(0.012)
p-value	(0.014)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.267)
q-value	[0.024]	[0.001]	[0.001]	[0.001]	[0.001]	[0.001]	[0.001]	[0.219]
Observations	5,589	5,589	5,589	5,589	5,589	5,589	5,589	5,589
Adjusted R^2	0.810	0.359	0.260	0.469	0.356	0.220	0.357	0.653
School FE	✓	✓	✓	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓	✓	✓
Cohort FE	✓	✓	✓	✓	✓	✓	✓	✓
Pre-SA Mean T	58	0.17	0.12	0.05	0.17	0.08	0.09	18
Pre-SA Mean C	59	0.11	0.09	0.03	0.16	0.07	0.09	19

Source: DTR, 2001/02-2008/09, SWC, 2010/11-2018/19 GIAS, 2001/02-2018/19.

Notes: This table presents the impact of the school management takeover on teacher turnover and the pupil-teacher ratio. These results are estimated by running regression 1 on the outcomes displayed on top of each column. Robust standard errors clustered at the school-cohort level are displayed in parenthesis. The table also reports p-values associated to the estimated coefficients and sharpened FDR q-values accounting for multiple hypothesis testing. The estimation sample includes schools that convert between 2007/08 to 2012/13 (treated schools) and schools that convert six years after each treated cohort (control schools). The estimation period includes five years before to four years after the takeover of treated schools.

*** p<0.01, ** p<0.05, * p<0.1.

Table B.5: Teachers' characteristics

	Age	Years of experience	Master's or more	Top GPA in education	Teach First	Going to outstanding school in teaching	Coming from new head's school	Coming from outstanding school in teaching
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Sponsored academy	-1.093***	-0.915***	-0.006	0.011***	0.007***	-0.004***	0.001	0.002***
	(0.134)	(0.130)	(0.005)	(0.004)	(0.001)	(0.001)	(0.001)	(0.001)
p-value	(0.000)	(0.000)	(0.235)	(0.005)	(0.000)	(0.000)	(0.373)	(0.003)
q-value	[0.001]	[0.001]	[0.201]	[0.010]	[0.001]	[0.001]	[0.261]	[0.007]
Observations	5,589	5,589	5,589	5,589	5,589	4,432	5,589	3,606
Adjusted R^2	0.728	0.758	0.796	0.548	0.568	0.154	0.033	0.165
School FE	✓	✓	✓	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓	✓	✓
Cohort FE	✓	✓	✓	✓	✓	✓	✓	✓
Pre-SA Mean T	40	14	0.61	0.04	0.003	0.001	0.001	0.003
Pre-SA Mean C	40	14	0.63	0.05	0.002	0.006	0.001	0.002

Source: DTR, 2001/02-2008/09, SWC, 2010/11-2018/19.

Notes: This table presents the impact of the school management takeover on teacher composition. These results are estimated by running regression 1 on the outcomes displayed on top of each column. Robust standard errors clustered at the school-cohort level are displayed in parenthesis. The table also reports p-values associated to the estimated coefficients and sharpened FDR q-values accounting for multiple hypothesis testing. The estimation sample includes schools that convert between 2007/08 to 2012/13 (treated schools) and schools that convert six years after each treated cohort (control schools). The estimation period includes five years before to four years after the takeover of treated schools. Information on teachers' GPA in education is only available for 25 percent of the sample. Schools' OFSTED performance is available from the school year 2005/06.

*** p<0.01, ** p<0.05, * p<0.1.

Table B.6: Pay dispersion - Pre vs. post Pay Reform

	Entire Sample (1)	2007/08-2009/10 (2)	2010/11-2012/13 (3)	P-value Difference (4)	Q-value Difference (5)
Sponsored academy	0.00141***	0.00138***	0.00164***	0.738	[0.386]
	(0.00035)	(0.00047)	(0.00059)		
Observations	5,589	2,634	2,955		
Adjusted R^2	0.100	0.130	0.084		
Pre-SA Mean T	0.0156	0.0157	0.0155		
Pre-SA Mean C	0.0148	0.0144	0.0153		

Source: DTR, 2001/02-2008/09, SWC, 2010/11-2018/19, GIAS, 2001/02-2018/19.

Notes: This table compares the takeover effect on pay dispersion across cohorts, with those in Column 2 that are not affected by the 2014 Pay reform, and cohorts in Column 3 that could be partially affected in some post-takeover years. The results are estimated by running regression 1 on pay dispersion by subgroup. All regressions include school, year, and cohort fixed effects. Robust standard errors clustered at the school-cohort level are displayed in parenthesis. Columns 4 and 5 report the p-value and FDR sharpened q-value of the t-test on the equality of coefficients across subgroups. The estimation sample includes schools that convert between 2007/08 to 2012/13 (treated schools) and schools that convert six years after each treated cohort (control schools). The estimation period includes five years before to four years after the takeover of treated schools.

*** p<0.01, ** p<0.05, * p<0.1.

Table B.7: School resources and expenditure

			Log expenditure on	
	Log income (1)	Log expenditure (2)	regular teachers (3)	other costs (4)
A. Absolute				
Sponsored academy	0.014	-0.002	-0.040*	0.015
	(0.020)	(0.018)	(0.021)	(0.020)
p-value	(0.488)	(0.894)	(0.061)	(0.435)
q-value	[0.311]	[0.497]	[0.076]	[0.286]
Adjusted R^2	0.831	0.840	0.810	0.799
Pre-SA Mean T (,000)	6,489	6,433	3,422	3,024
Pre-SA Mean C (,000)	6,663	6,594	3,623	2,972
B. Per pupil				
Sponsored academy	0.074***	0.058***	0.020*	0.076***
	(0.014)	(0.012)	(0.012)	(0.019)
p-value	(0.000)	(0.000)	(0.089)	(0.000)
q-value	[0.001]	[0.001]	[0.100]	[0.001]
Adjusted R^2	0.632	0.652	0.451	0.697
Pre-SA Mean T	6,758	6,712	3,524	3,197
Pre-SA Mean C	6,290	6,219	3,396	2,822
C. Per teacher				
Sponsored academy	0.049***	0.033*	-0.005	0.051**
	(0.018)	(0.017)	(0.015)	(0.024)
p-value	(0.007)	(0.054)	(0.768)	(0.034)
q-value	[0.013]	[0.070]	[0.455]	[0.050]
Adjusted R^2	0.492	0.508	0.459	0.565
Pre-SA Mean T (,000)	111	110	58	52
Pre-SA Mean C (,000)	104	103	56	47
Observations	2,642	2,642	2,642	2,642
School FE	✓	✓	✓	✓
Year FE	✓	✓	✓	✓
Cohort FE	✓	✓	✓	✓

Source: GIAS, 2005/06-2018/19.

Notes: This table presents the impact of the school management takeover on school resources and expenditure. These results are estimated by running regression 1 on the outcomes displayed on top of each column. Robust standard errors clustered at the school-cohort level are displayed in parenthesis. The table also reports p-values associated to the estimated coefficients and sharpened FDR q-values accounting for multiple hypothesis testing. The estimation sample includes schools that experience a takeover between 2010/11 to 2012/13 (treated schools) and schools that experience this event six years after treated cohorts (control schools). The estimation period includes five years before to four years after the takeover of treated schools.

*** p<0.01, ** p<0.05, * p<0.1.

B.4 Resources allocated to new hires

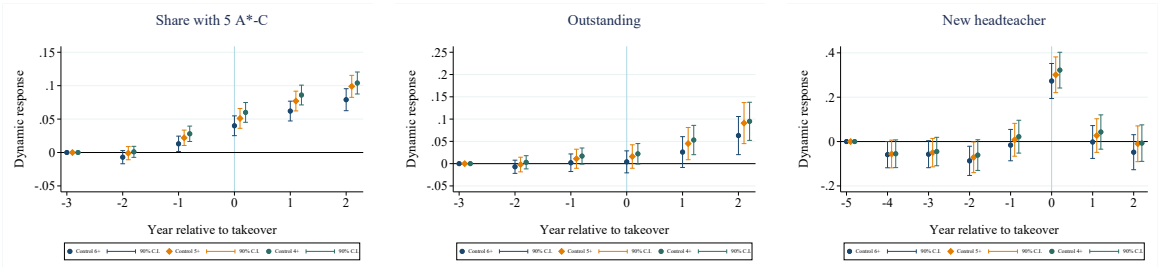
In Section 5.2, we showed that the headteacher of a sponsor-led academy is paid almost 10 percent more per year, or £8,300 ($10,700), than in the predecessor school. In Section 5.4, we further showed that new hired teachers enjoy, on average, a 2 percent annual increase in their gross pay when joining converted schools, £840 ($1,100) per year.

Pre-takeover, new hires constitute on average 17 percent of the teaching body, or 10 teachers, relative to an average size of 58 teachers. Sponsors increase the share of new hires by 26 percent compared to the pre-takeover mean, meaning that they hire on average 3 more teachers per year.

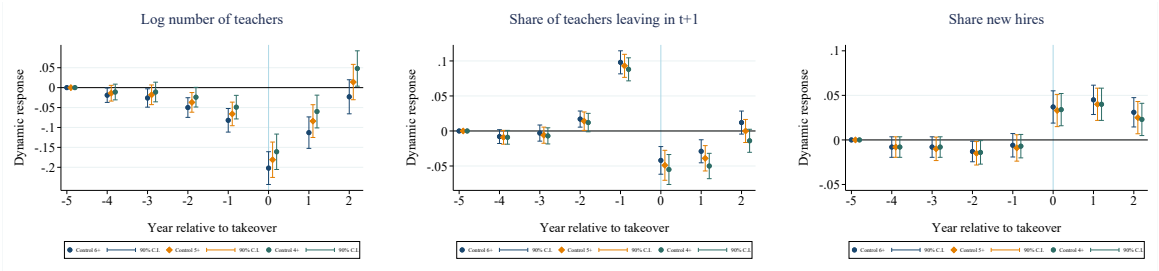
The increase in annual resources that sponsors allocate to newcomers thus amounts to £8,300 for the headteacher, plus £840*13 for new teachers, or £19,220 in total. The average annual spending on regular teachers pre-takeover totals £3,422,000 and decreases by 4 percent afterwards, getting to an average of £3,285,120 per year. The additional resources allocated to newcomers, therefore, constitute only 0.6 percent of the annual expenditure of the typical sponsor-led school. While it is not straightforward to identify how sponsors reallocate funding across expenditure items to liberate these resources, this seems a relatively small sum of money to find in the school budget once sponsors are allowed to set their own pay policy.

B.5 Alternative control groups

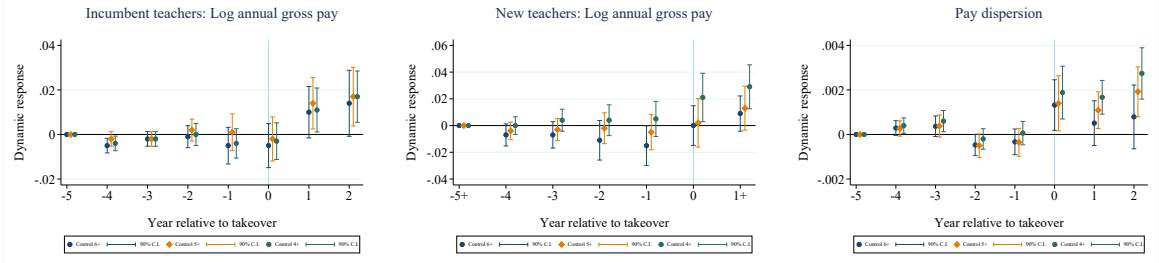
Figure B.9: Main outcomes - Alternative control groups



(A) Pupil and school performance, new head



(B) Teacher turnover

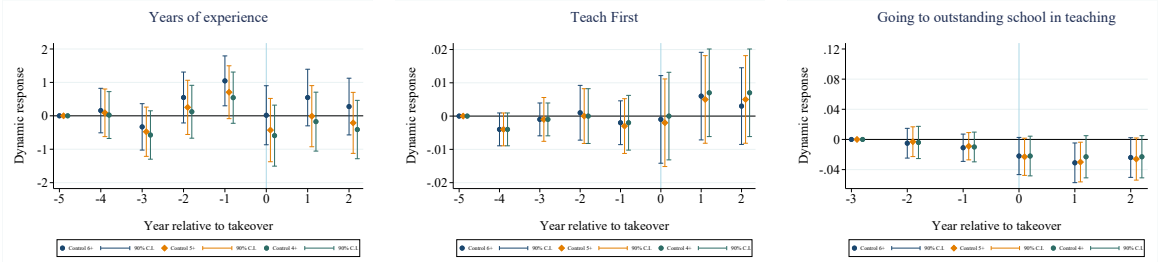


(C) Teacher pay

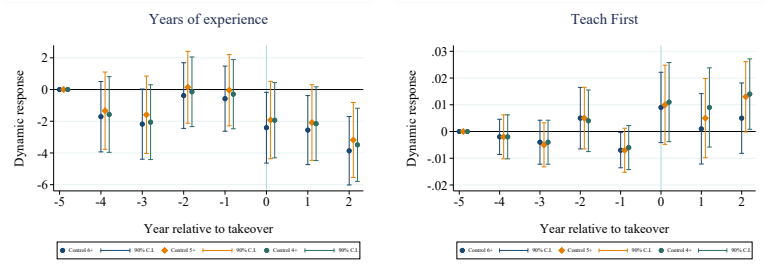
Source: DTR, 2001/02-2009/10, SWC, 2010/11-2018/19, GIAS, 2001/02-2018/19.

Notes: These graphs compare the dynamic impact of the school management takeover on pupil and school performance, the headteacher selection, and teacher turnover and pay, when using different control groups. These results are estimated by running regression 2 on the outcomes displayed in each graph. The estimation sample includes schools that experience a takeover between 2007/08 to 2012/13 and schools that experience this event four (green dots), five (orange diamonds), or six years (blue dots) after each treated cohort. The estimation period includes five years before to two years after the takeover of treated schools. The graph also displays 90-percent confidence intervals corresponding to robust standard errors clustered at the school-cohort level.

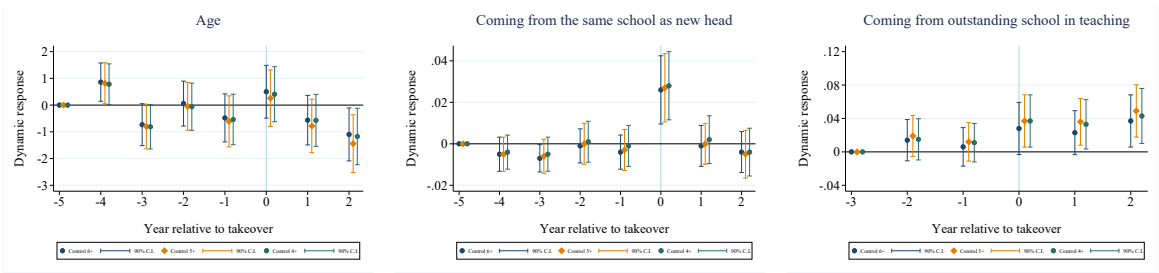
Figure B.10: Teacher sorting - Alternative control groups



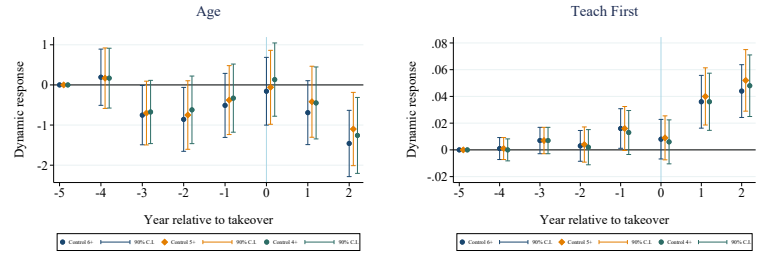
(A) Leavers to other school



(B) Leavers to other sector



(C) New hires from other school



(D) New hires from other sector

Source: DTR, 2001/02-2009/10, SWC, 2010/11-2018/19, GIAS, 2001/02-2018/19.

Notes: These graphs compare the dynamic impact of the school management takeover on teacher sorting, when using different control groups. These results are estimated by running regression 2 on the outcomes displayed in each graph. The estimation sample includes schools that experience a takeover between 2007/08 to 2012/13 and schools that experience this event four (green dots), five (orange diamonds), or six years (blue dots) after each treated cohort. The estimation period includes five years before to two years after the takeover of treated schools. The graph also displays 90-percent confidence intervals corresponding to robust standard errors clustered at the school-cohort level.

C Matched DiD

Table C.1: Pupil and school OFSTED performance - Matched DiD

	Share pupils with 5 A*-C		Outstanding	
	DiD	Matched DiD	DiD	Matched DiD
	(1)	(2)	(3)	(4)
Sponsored academy	0.070***	0.030***	0.048***	0.028**
	(0.005)	(0.006)	(0.012)	(0.014)
Observations	4,432	4,139	4,432	4,139
Adjusted R^2	0.696	0.718	0.436	0.404
School FE	✓	✓	✓	✓
Year FE	✓	✓	✓	✓
Cohort FE	✓	✓	✓	✓
Match FE		✓		✓
Pre-SA Mean T	0.31	0.31	0.01	0.01
Pre-SA Mean C	0.41	0.34	0.06	0.04

Source: GIAS and OFSTED, 2005/06-2018/19.

Notes: This table compares the impact of the school management takeover on pupil performance in end-of-high school centralized exams and school OFSTED performance, estimated using the two alternative identification strategies. For each outcome, the first column reports the estimates obtained when comparing earlier to later takeovers, while the second column presents the estimates obtained using the matched DiD design. See Section 4 for more details on the two identification methods and estimation samples. Robust standard errors clustered at the school-cohort level are displayed in parenthesis. Pupil performance is measured as the share of pupils achieving a sufficient level of learning, i.e., at least 5 A*-C including in English and Math.

*** p<0.01, ** p<0.05, * p<0.1.

Table C.2: New head - Matched DiD

	New head		Where from				Headteacher's characteristics							
			Hired from other school		Hired from other sector		Years of of experience		Master's or more		Log annual pay		Coming from outstanding school in management	
	DiD	Matched DiD	DiD	Matched DiD	DiD	Matched DiD	DiD	Matched DiD	DiD	Matched DiD	DiD	Matched DiD	DiD	Matched DiD
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)
Sponsored academy	0.114***	0.160***	0.091***	0.124***	0.017***	0.018**	-0.847*	-1.171**	0.056*	0.016	0.099***	0.069***	0.021**	0.037***
	(0.023)	(0.025)	(0.017)	(0.018)	(0.006)	(0.007)	(0.514)	(0.578)	(0.033)	(0.036)	(0.015)	(0.016)	(0.010)	(0.010)
Observations	5,589	4,741	5,589	4,741	5,589	4,741	5,582	4,733	5,589	4,741	5,589	4,741	3,606	3,408
Adjusted R^2	0.068	0.081	0.008	0.019	0.012	0.019	0.391	0.400	0.494	0.459	0.453	0.435	-0.004	0.014
School FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Cohort FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Match FE		✓		✓		✓		✓		✓		✓		✓
Pre-SA Mean T	0.26	0.26	0.11	0.11	0.01	0.01	27	26	0.57	0.57	83,528	83,577	0.007	0.007
Pre-SA Mean C	0.20	0.22	0.09	0.10	0.01	0.01	28	27	0.63	0.60	85,288	86,617	0.007	0.011

Source: DTR, 2001/02-2008/09, SWC, 2010/11-2018/19, GIAS, 2001/02-2018/19.

Notes: This table compares the impact of the school management takeover on the probability of appointing a new headteacher and the characteristics of the headteacher, estimated using the two alternative identification strategies. For each outcome, the first column reports the estimates obtained when comparing earlier to later takeovers, while the second column presents the estimates obtained using the matched DiD design. See Section 4 for more details on the two identification methods and estimation samples. Robust standard errors clustered at the school-cohort level are displayed in parenthesis.

*** p<0.01, ** p<0.05, * p<0.1.

Table C.3: Teacher turnover - Matched DiD

	Log number of teachers		Share teachers leaving in t+1		Share new hired teachers	
	DiD	Matched DiD	DiD	Matched DiD	DiD	Matched DiD
	(1)	(2)	(3)	(4)	(5)	(6)
Sponsored academy	-0.044**	-0.015	-0.054***	-0.060***	0.044***	0.042***
	(0.018)	(0.021)	(0.006)	(0.008)	(0.005)	(0.006)
Observations	5,589	4,741	5,589	4,741	5,589	4,741
Adjusted R^2	0.810	0.763	0.359	0.325	0.356	0.346
School FE	✓	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓
Cohort FE	✓	✓	✓	✓	✓	✓
Match FE		✓		✓		✓
Pre-SA Mean T	58	58	0.17	0.17	0.17	0.17
Pre-SA Mean C	59	60	0.11	0.15	0.16	0.16

Source: DTR, 2001/02-2008/09, SWC, 2010/11-2018/19, GIAS, 2001/02-2018/19.

Notes: This table compares the impact of the school management takeover on teacher turnover, estimated using the two alternative identification strategies. For each outcome, the first column reports the estimates obtained when comparing earlier to later takeovers, while the second column presents the estimates obtained using the matched DiD design. See Section 4 for more details on the two identification methods and estimation samples. Robust standard errors clustered at the school-cohort level are displayed in parenthesis.

*** p<0.01, ** p<0.05, * p<0.1.

Table C.4: Teacher sorting: leavers - Matched DiD

	Years of experience		Teach First		Going to outstanding school in teaching	
	DiD	Matched DiD	DiD	Matched DiD	DiD	Matched DiD
	(1)	(2)	(3)	(4)	(5)	(6)
A. To other school						
Sponsored academy	-0.527*	-0.682**	0.008**	-0.002	-0.016*	-0.015
	(0.269)	(0.297)	(0.004)	(0.005)	(0.009)	(0.010)
Observations	5,329	4,561	5,329	4,561	4,429	4,152
Adjusted R^2	0.083	0.094	0.168	0.150	0.082	0.117
Pre-SA Mean T	10	10	0.008	0.008	0.09	0.09
Pre-SA Mean C	9	9	0.004	0.004	0.09	0.08
B. To other sector						
Sponsored academy	-2.364***	-1.611***	0.015***	0.010**		
	(0.542)	(0.616)	(0.004)	(0.005)		
Observations	4,091	3,537	4,091	3,537		
Adjusted R^2	0.153	0.135	0.269	0.197		
Pre-SA Mean T	17	17	0.007	0.007		
Pre-SA Mean C	17	18	0.003	0.006		

Source: DTR, 2001/02-2009/10, SWC, 2010/11-2018/19, GIAS, 2001/02-2018/19.

Notes: This table compares the impact of the school management takeover on teacher sorting, estimated using the two different identification strategies. For each outcome, the first column reports the estimates obtained when comparing earlier to later takeovers, while the second column presents the estimates obtained using the matched DiD design. See Section 4 for more details on the two identification methods and estimation samples. All regressions include school, year, and cohort fixed effects. Matched-DiD regressions further include match-id fixed effects. Robust standard errors clustered at the school-cohort level are displayed in parenthesis. Panel A (B) refers to teachers leaving for other schools (leaving the educational sector or leaving for a school in a different phase of education). Schools' OFSTED performance is available from the school year 2005/06.

*** p<0.01, ** p<0.05, * p<0.1.

Table C.5: Teacher sorting: new hires - Matched DiD

	Age		Teach First		Coming from new head's school		Coming from outstanding in teaching	
	DiD	Matched DiD	DiD	Matched DiD	DiD	Matched DiD	DiD	Matched DiD
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
A. From other school								
Sponsored academy	-0.520*	-0.208			0.008**	0.008**	0.025**	0.022**
	(0.293)	(0.334)			(0.004)	(0.004)	(0.011)	(0.011)
Observations	5,232	4,441			5,232	4,441	3,229	3,187
Adjusted R^2	0.085	0.083			0.027	0.056	0.081	0.101
Pre-SA Mean T	37	37			0.006	0.006	0.06	0.05
Pre-SA Mean C	36	37			0.004	0.004	0.05	0.05
B. From other sector								
Sponsored academy	-0.536*	0.183	0.021***	0.005				
	(0.273)	(0.315)	(0.007)	(0.008)				
Observations	4,990	4,235	4,990	4,235				
Adjusted R^2	0.175	0.185	0.252	0.236				
Pre-SA Mean T	32	32	0.02	0.02				
Pre-SA Mean C	31	32	0.01	0.01				

Source: DTR, 2001/02-2009/10, SWC, 2010/11-2018/19, GIAS, 2001/02-2018/19.

Notes: This table compares the impact of the school management takeover on teacher sorting, estimated using the two different identification strategies. For each outcome, the first column reports the estimates obtained when comparing earlier to later takeovers, while the second column presents the estimates obtained using the matched DiD design. See Section 4 for more details on the two identification methods and estimation samples. All regressions include school, year, and cohort fixed effects. Matched-DiD regressions further include match-id fixed effects. Robust standard errors clustered at the school-cohort level are displayed in parenthesis. Panel A (B) refers to new hires from other schools (new hires from outside the education sector or from a different phase of education). Schools' OFSTED performance is available from the school year 2005/06.

*** p<0.01, ** p<0.05, * p<0.1.

Table C.6: Teacher pay - Matched DiD

	Incumbent teachers		New hires			
	Log gross pay		Log gross pay		Pay dispersion	
	DiD	Matched DiD	DiD	Matched DiD	DiD	Matched DiD
	(1)	(2)	(3)	(4)	(5)	(6)
Sponsored academy	0.007	0.006*	0.021**	0.024***	0.00141***	0.00176***
	(0.004)	(0.004)	(0.009)	(0.009)	(0.00035)	(0.00035)
Observations	255,517	185,247	47,630	35,221	5,589	4,741
Adjusted R^2	0.618	0.628	0.636	0.671	0.100	0.186
Teacher FE	✓	✓	✓	✓		
School FE	✓	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓
Cohort FE	✓	✓	✓	✓	✓	✓
Match FE		✓		✓		✓
Pre-SA Mean T	41,737	41,736	40,113	40,065	0.0156	0.0156
Pre-SA Mean C	41,638	42,043	38,687	38,979	0.0148	0.0158

Source: DTR, 2001/02-2008/09, SWC, 2010/11-2018/19.

Notes: This table compares the impact of the school management takeover on teachers' pay and pay dispersion, estimated using the two alternative identification strategies. For each outcome, the first column reports the estimates obtained when comparing earlier to later takeovers, while the second column presents the estimates obtained using the matched DiD design. See Section 4 for more details on the two identification methods and estimation samples. Robust standard errors clustered at the school-cohort level are displayed in parenthesis.

*** p<0.01, ** p<0.05, * p<0.1.

D Heterogeneous effects

Table D.1: School-Sponsor match

	Probability (Sponsor = private business)						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Log number of pupils	-0.028						-0.068
	(0.093)						(0.104)
% FSME pupils		-0.345					-0.469
		(0.354)					(0.410)
% 5 A*-C pupils			-0.062				-0.079
			(0.253)				(0.234)
% Teacher separations				0.070			-0.050
				(0.316)			(0.363)
Fully managed by the LA					0.026		0.021
					(0.072)		(0.070)
Inadequate						0.079	0.076
						(0.071)	(0.079)
Observations	238	238	238	238	238	238	238
Adjusted R^2	0.166	0.169	0.166	0.166	0.166	0.172	0.154
LA FE	✓	✓	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓	✓

Source: DTR, 2001/02-2008/09, SWC, 2010/11-2018/19.

Notes: This table shows the correlation between school characteristics as of two years pre-takeover and the probability that the sponsor is a private company. The estimation sample includes schools that convert between 2007/08 to 2012/13 (treated schools) with non-missing information on the sponsor' nature. Robust standard errors clustered at the Local-Authority (LA) level are displayed in parenthesis.

*** p<0.01, ** p<0.05, * p<0.1.

Table D.2: Pupil and school OFSTED performance - Heterogeneous effects across sponsor types

	Entire Sample (1)	Businesses (2)	Other sponsors (3)	P-value Difference (4)	Q-value Difference (5)
A. Pupil performance					
Sponsored academy	0.070***	0.090***	0.067***	0.136	[0.130]
	(0.005)	(0.015)	(0.006)		
Observations	4,432	645	3,674		
Adjusted R^2	0.696	0.624	0.714		
Pre-SA Mean T	0.31	0.31	0.32		
Pre-SA Mean C	0.41	0.40	0.42		
B. OFSTED outstanding					
Sponsored academy	0.048***	0.030	0.048***	0.669	[0.412]
	(0.012)	(0.039)	(0.014)		
Observations	4,432	645	3,674		
Adjusted R^2	0.436	0.388	0.446		
Pre-SA Mean T	0.01	0.01	0.01		
Pre-SA Mean C	0.06	0.13	0.05		

Source: GIAS and OFSTED, 2005/06-2018/19.

Notes: This table presents heterogeneous effects across sponsor types on pupil and school OFSTED performance. Pupil performance is measured as the share of pupils achieving a sufficient level of learning, i.e., at least 5 A*-C including in English and Math. These results are estimated by running regression 1 on the outcomes displayed on top of each panel, by subgroup. All regressions include school, year, and cohort fixed effects. Robust standard errors clustered at the school-cohort level are displayed in parenthesis. Columns 4 and 5 report the p-value and FDR sharpened q-value of the t-test on the equality of coefficients across subgroups. The estimation sample includes schools that convert between 2008/09 to 2012/13 (treated schools) and schools that convert six years after each treated cohort (control schools). In Columns 2-3, it is restricted to schools with non-missing information on the sponsor's nature. The estimation period includes five years before to four years after the takeover of treated schools.

*** p<0.01, ** p<0.05, * p<0.1.

Table D.3: New head: where from - Heterogeneous effects across sponsor types

	Entire Sample (1)	Businesses (2)	Other sponsors (3)	P-value Difference (4)	Q-value Difference (5)
A. New head					
Sponsored academy	0.114***	0.139**	0.114***	0.693	[0.416]
	(0.023)	(0.060)	(0.025)		
Observations	5,589	855	4,563		
Adjusted R^2	0.068	0.081	0.059		
Pre-SA Mean T	0.26	0.24	0.26		
Pre-SA Mean C	0.20	0.21	0.19		
B. Promoted internally					
Sponsored academy	0.004	0.002	0.012	0.859	[0.484]
	(0.018)	(0.052)	(0.020)		
Observations	5,589	855	4,563		
Adjusted R^2	0.041	0.025	0.038		
Pre-SA Mean T	0.15	0.15	0.14		
Pre-SA Mean C	0.09	0.08	0.09		
C. Hired from other school					
Sponsored academy	0.091***	0.095**	0.089***	0.909	[0.455]
	(0.017)	(0.046)	(0.019)		
Observations	5,589	855	4,563		
Adjusted R^2	0.008	0.014	0.006		
Pre-SA Mean T	0.11	0.08	0.11		
Pre-SA Mean C	0.09	0.11	0.09		
D. Hired from other sector					
Sponsored academy	0.017***	0.053**	0.009	0.039	[0.056]
	(0.006)	(0.021)	(0.006)		
Observations	5,589	855	4,563		
Adjusted R^2	0.012	0.026	0.010		
Pre-SA Mean T	0.01	0.01	0.01		
Pre-SA Mean C	0.01	0.02	0.01		

Source: DTR, 2001/02-2008/09, SWC, 2010/11-2018/19, GIAS, 2001/02-2018/19.

Notes: This table presents heterogeneous effects across sponsor types on the probability of appointing a new headteacher. These results are estimated by running regression 1 on the outcomes displayed on top of each panel, by subgroup. All regressions include school, year, and cohort fixed effects. Robust standard errors clustered at the school-cohort level are displayed in parenthesis. Columns 4 and 5 report the p-value and FDR sharpened q-value of the t-test on the equality of coefficients across subgroups. The estimation sample includes schools that convert between 2007/08 to 2012/13 (treated schools) and schools that convert six years after each treated cohort (control schools). In Columns 2-3, it is restricted to schools with non-missing information on the sponsor's nature. The estimation period includes five years before to four years after the takeover of treated schools.

*** p<0.01, ** p<0.05, * p<0.1.

Table D.4: Headteacher's characteristics - Heterogeneous effects across sponsor types

	Entire Sample (1)	Businesses (2)	Other sponsors (3)	P-value Difference (4)	Q-value Difference (5)
A. Experience					
Sponsored academy	-0.847*	-0.616	-0.920	0.824	[0.484]
	(0.514)	(1.247)	(0.582)		
Observations	5,582	855	4,556		
Adjusted R^2	0.391	0.433	0.383		
Pre-SA Mean T	27	27	26		
Pre-SA Mean C	28	28	28		
B. Master's or more					
Sponsored academy	0.056*	-0.007	0.066*	0.366	[0.261]
	(0.033)	(0.072)	(0.039)		
Observations	5,589	855	4,563		
Adjusted R^2	0.494	0.438	0.502		
Pre-SA Mean T	0.57	0.67	0.55		
Pre-SA Mean C	0.63	0.65	0.63		
C. Log annual gross pay					
Sponsored academy	0.099***	0.109**	0.101***	0.864	[0.484]
	(0.015)	(0.043)	(0.016)		
Observations	5,589	855	4,563		
Adjusted R^2	0.453	0.379	0.468		
Pre-SA Mean T	83,528	84,486	83,437		
Pre-SA Mean C	85,288	89,557	84,703		
D. Coming from outstanding school in management					
Sponsored academy	0.021**	-0.013	0.024**	0.179	[0.167]
	(0.010)	(0.025)	(0.011)		
Observations	3,606	489	3,025		
Adjusted R^2	-0.004	0.009	-0.003		
Pre-SA Mean T	0.007	0.006	0.007		
Pre-SA Mean C	0.007	0	0.007		

Source: DTR, 2001/02-2008/09, SWC, 2010/11-2018/19, GIAS, 2001/02-2018/19.

Notes: This table presents heterogeneous effects across sponsor types on the probability of appointing a new headteacher and the headteacher's characteristics. These results are estimated by running regression 1 on the outcomes displayed on top of each panel, by subgroup. All regressions include school, year, and cohort fixed effects. Robust standard errors clustered at the school-cohort level are displayed in parenthesis. Columns 4 and 5 report the p-value and FDR sharpened q-value of the t-test on the equality of coefficients across subgroups. The estimation sample includes schools that convert between 2007/08 to 2012/13 (treated schools) and schools that convert six years after each treated cohort (control schools). In Columns 2-3, it is restricted to schools with non-missing information on the sponsor's nature. The estimation period includes five years before to four years after the takeover of treated schools.

*** p<0.01, ** p<0.05, * p<0.1.

Table D.5: Teacher turnover - Heterogeneous effects across sponsor types

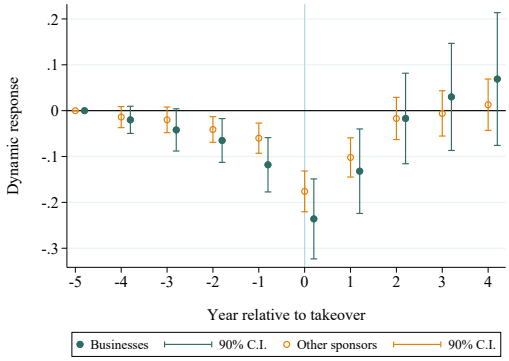
	Entire Sample (1)	Businesses (2)	Other sponsors (3)	P-value Difference (4)	Q-value Difference (5)
A. Log number of teachers					
Sponsored academy	-0.044**	-0.056	-0.042**	0.750	[0.451]
	(0.018)	(0.042)	(0.020)		
Observations	5,589	855	4,563		
Adjusted R^2	0.810	0.797	0.808		
Pre-SA Mean T	58	55	59		
Pre-SA Mean C	59	57	60		
B. Share separations					
Sponsored academy	-0.054***	-0.056***	-0.051***	0.794	[0.472]
	(0.006)	(0.015)	(0.007)		
Observations	5,589	855	4,563		
Adjusted R^2	0.359	0.303	0.370		
Pre-SA Mean T	0.17	0.17	0.17		
Pre-SA Mean C	0.11	0.12	0.11		
C. Share new hires					
Sponsored academy	0.044***	0.075***	0.040***	0.014	[0.024]
	(0.005)	(0.013)	(0.006)		
Observations	5,589	855	4,563		
Adjusted R^2	0.356	0.349	0.355		
Pre-SA Mean T	0.17	0.17	0.17		
Pre-SA Mean C	0.16	0.18	0.16		
D. Log pupil-teacher ratio					
Sponsored academy	0.013	0.009	0.012	0.948	[0.530]
	(0.012)	(0.031)	(0.013)		
Observations	5,589	855	4,563		
Adjusted R^2	0.653	0.656	0.656		
Pre-SA Mean T	18	19	18		
Pre-SA Mean C	19	19	19		

Source: DTR, 2001/02-2008/09, SWC, 2010/11-2018/19, GIAS, 2001/02-2018/19.

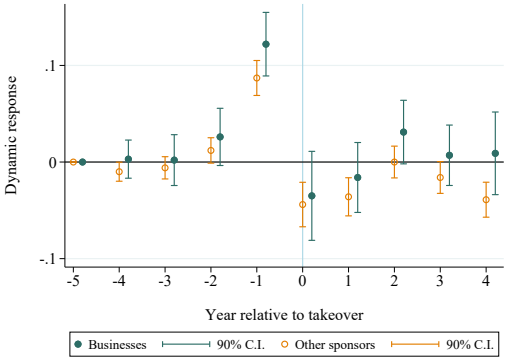
Notes: This table presents heterogeneous effects across sponsor types on teacher turnover and the pupil-teacher ratio. The results are estimated by running regression 1 on the outcomes displayed on top of each panel, by subgroup. All regressions include school, year, and cohort fixed effects. Robust standard errors clustered at the school-cohort level are displayed in parenthesis. Columns 4 and 5 report the p-value and FDR sharpened q-value of the t-test on the equality of coefficients across subgroups. The estimation sample includes schools that convert between 2007/08 to 2012/13 (treated schools) and schools that convert six years after each treated cohort (control schools). In Columns 2-3, it is restricted to schools with non-missing information on the sponsor's nature. The estimation period includes five years before to four years after the takeover of treated schools.

*** p<0.01, ** p<0.05, * p<0.1.

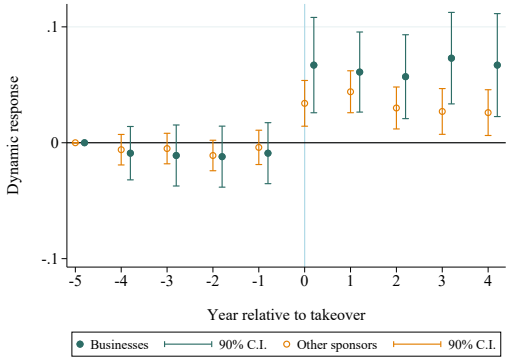
Figure D.1: Teacher turnover - Heterogeneous effects across sponsor types



(A) Log number of teachers



(B) Share teachers leaving in t+1



(C) Share new hires

Source: DTR, 2001/02-2009/10, SWC, 2010/11-2018/19 GIAS, 2001/02-2018/19.

Notes: These graphs present heterogeneous effects across sponsor types on teacher turnover. These results are estimated by running regression 2 on the outcomes displayed in each graph, by subgroup. The estimation sample includes schools that experience a takeover between 2007/08 to 2012/13 and schools that experience this event six years later, with non-missing information on the sponsor's nature. The estimation period includes five years before to four years after the takeover of treated schools. Each graph also displays 90-percent confidence intervals corresponding to robust standard errors clustered at the school-cohort level.

Table D.6: Leavers - Heterogeneous effects across sponsor types

	To other school					To other sector				
	Entire	Businesses	Other sponsors	P-value Difference	Q-value Difference	Entire	Businesses	Other sponsors	P-value Difference	Q-value Difference
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
A. Years of experience										
Sponsored academy	-0.527*	-0.212	-0.473	0.695	[0.416]	-2.364***	-3.217**	-2.177***	0.476	[0.305]
	(0.269)	(0.598)	(0.306)			(0.542)	(1.344)	(0.611)		
Observations	5,329	823	4,343			4,091	623	3,338		
Adjusted R^2	0.083	0.068	0.090			0.153	0.088	0.162		
Pre-SA Mean T	10	10	10			17	18	18		
Pre-SA Mean C	9	10	9			17	14	17		
B. Teach First										
Sponsored academy	0.008**	0.014**	0.003	0.089	[0.100]	0.015***	0.026**	0.008*	0.117	[0.119]
	(0.004)	(0.006)	(0.004)			(0.004)	(0.011)	(0.004)		
Observations	5,329	823	4,343			4,091	623	3,338		
Adjusted R^2	0.168	0.228	0.164			0.269	0.302	0.258		
Pre-SA Mean T	0.008	0.007	0.009			0.007	0.006	0.007		
Pre-SA Mean C	0.004	0.012	0.003			0.003	0.012	0.003		
C. Going to outstanding school in teaching										
Sponsored academy	-0.016*	-0.002	-0.023**	0.408	[0.274]					
	(0.009)	(0.023)	(0.011)							
Observations	4,429	659	3,656							
Adjusted R^2	0.082	0.096	0.081							
Pre-SA Mean T	0.09	0.07	0.09							
Pre-SA Mean C	0.09	0.10	0.08							

Source: DTR, 2001/02-2009/10, SWC, 2010/11-2018/19, GIAS, 2001/02-2018/19.

Notes: This table presents heterogeneous effects across sponsor types on leavers' characteristics. These results are estimated by running regression 1 on each outcome, by subgroup. All regressions include school, year, and cohort fixed effects. Robust standard errors clustered at the school-cohort level are displayed in parenthesis. Columns 4-5 (9-10) report the p-value and FDR sharpened q-value of the t-test on the equality of coefficients across subgroups. The estimation sample includes schools that convert between 2007/08 to 2012/13 (treated schools) and schools that convert six years after each treated cohort (control schools). In Columns 2-3 and 7-8, it is restricted to schools with non-missing information on the sponsor's nature. The estimation period includes five years before to four years after the takeover of treated schools.

*** p<0.01, ** p<0.05, * p<0.1.

Table D.7: New hires - Heterogeneous effects across sponsor types

	From other school					From other sector				
	Entire	Businesses	Other sponsors	P-value Difference	Q-value Difference	Entire	Businesses	Other sponsors	P-value Difference	Q-value Difference
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
A. Age										
Sponsored academy	-0.520*	0.829	-0.732**	0.077	[0.092]	-0.536*	-1.327**	-0.158	0.107	[0.115]
	(0.293)	(0.832)	(0.324)			(0.273)	(0.667)	(0.303)		
Observations	5,232	808	4,268			4,990	776	4,058		
Adjusted R^2	0.085	0.124	0.080			0.175	0.200	0.168		
Pre-SA Mean T	37	37	37			32	32	31		
Pre-SA Mean C	36	36	36			31	32	31		
B. Teach First										
Sponsored academy						0.021***	0.065***	0.008	0.004	[0.008]
						(0.007)	(0.019)	(0.007)		
Observations						4,990	776	4,058		
Adjusted R^2						0.252	0.293	0.243		
Pre-SA Mean T						0.02	0.02	0.02		
Pre-SA Mean C						0.01	0.02	0.01		
C. Coming from new head's school										
Sponsored academy	0.008**	0.026**	0.006*	0.123	[0.122]					
	(0.004)	(0.013)	(0.004)							
Observations	5,232	808	4,268							
Adjusted R^2	0.027	0.034	0.028							
Pre-SA Mean T	0.006	0.006	0.005							
Pre-SA Mean C	0.004	0.008	0.003							
D. Coming from outstanding school in teaching										
Sponsored academy	0.025**	-0.012	0.026**	0.248	[0.206]					
	(0.011)	(0.031)	(0.012)							
Observations	3,229	434	2,720							
Adjusted R^2	0.081	0.094	0.079							
Pre-SA Mean T	0.06	0.06	0.06							
Pre-SA Mean C	0.05	0.05	0.05							

Source: DTR, 2001/02-2009/10, SWC, 2010/11-2018/19, GIAS, 2001/02-2018/19.

Notes: This table presents heterogeneous effects across sponsor types on new hires' characteristics. These results are estimated by running regression 1 on each outcome, by subgroup. All regressions include school, year, and cohort fixed effects. Robust standard errors clustered at the school-cohort level are displayed in parenthesis. Columns 4-5 (9-10) report the p-value and FDR sharpened q-value of the t-test on the equality of coefficients across subgroups. The estimation sample includes schools that convert between 2007/08 to 2012/13 (treated schools) and schools that convert six years after each treated cohort (control schools). In Columns 2-3 and 7-8, it is restricted to schools with non-missing information on the sponsor's nature. The estimation period includes five years before to four years after the takeover of treated schools.

*** p<0.01, ** p<0.05, * p<0.1.

Table D.8: Teacher pay - Heterogeneous effects across sponsor types

	Entire Sample (1)	Businesses (2)	Other sponsors (3)	P-value Difference (4)	Q-value Difference (5)
A. Log gross pay - Incumbent teachers					
Sponsored academy	0.007	0.018*	0.007	0.367	[0.261]
	(0.004)	(0.011)	(0.005)		
Observations	255,517	32,238	190,388		
Adjusted R^2	0.618	0.652	0.626		
Pre-SA Mean T	41,737	42,278	41,866		
Pre-SA Mean C	41,638	43,329	41,742		
B. Log gross pay - New teachers					
Sponsored academy	0.021**	-0.020	0.016	0.185	[0.171]
	(0.009)	(0.019)	(0.019)		
Observations	47,630	14,547	10,030		
Adjusted R^2	0.636	0.708	0.731		
Pre-SA Mean T	40,113	40,933	39,079		
Pre-SA Mean C	38,687	40,364	38,264		
C. Pay dispersion					
Sponsored academy	0.00141***	0.00042	0.00159***	0.154	[0.147]
	(0.00035)	(0.00071)	(0.00042)		
Observations	5,589	855	4,563		
Adjusted R^2	0.100	0.097	0.095		
Pre-SA Mean T	0.0156	0.0155	0.0155		
Pre-SA Mean C	0.0148	0.0143	0.0149		

Source: DTR, 2001/02-2008/09, SWC, 2010/11-2018/19, GIAS, 2001/02-2018/19.

Notes: This table presents heterogeneous effects across sponsor types on teacher pay and pay dispersion. The results in Panels A and B are estimated by running regression 3 on the outcomes displayed on top of each panel, by subgroup. The results in Panel C are estimated by running regression 1 on pay dispersion by subgroup. All regressions include school, year, and cohort fixed effects. Regressions in Panels A and B further include teacher fixed effects. Robust standard errors clustered at the school-cohort level are displayed in parenthesis. Columns 4 and 5 report the p-value and FDR sharpened q-value of the t-test on the equality of coefficients across subgroups. The estimation sample includes schools that convert between 2007/08 to 2012/13 (treated schools) and schools that convert six years after each treated cohort (control schools). In Columns 2-3, it is restricted to schools with non-missing information on the sponsor's nature. The estimation period includes five years before to four years after the takeover of treated schools.

*** p<0.01, ** p<0.05, * p<0.1.

Table D.9: School resources and expenditure - Heterogeneous effects across sponsor types

	Entire Sample (1)	Businesses (2)	Other sponsors (3)	P-value Difference (4)	Q-value Difference (5)
A. Log income per pupil (p.p.)					
Sponsored academy	0.074***	0.070*	0.078***	0.850	[0.484]
	(0.014)	(0.039)	(0.015)		
Observations	2,642	303	2,286		
Adjusted R^2	0.632	0.534	0.662		
Pre-SA Mean T	6,758	6,591	6,780		
Pre-SA Mean C	6,290	6,975	6,254		
B. Log expenditure per pupil (p.p.)					
Sponsored academy	0.058***	0.036	0.063***	0.560	[0.349]
	(0.012)	(0.046)	(0.013)		
Observations	2,642	303	2,286		
Adjusted R^2	0.652	0.642	0.673		
Pre-SA Mean T	6,712	6,567	6,732		
Pre-SA Mean C	6,219	6,973	6,179		
C. Log expenditure on regular teachers (p.p.)					
Sponsored academy	0.020*	0.060*	0.018	0.239	[0.201]
	(0.012)	(0.034)	(0.013)		
Observations	2,642	303	2,286		
Adjusted R^2	0.451	0.572	0.446		
Pre-SA Mean T	3,524	3,453	3,539		
Pre-SA Mean C	3,396	3,857	3,371		
D. Log expenditure on other costs (p.p.)					
Sponsored academy	0.076***	0.010	0.085***	0.311	[0.233]
	(0.019)	(0.073)	(0.020)		
Observations	2,642	303	2,286		
Adjusted R^2	0.697	0.643	0.718		
Pre-SA Mean T	3,197	3,114	3,203		
Pre-SA Mean C	2,822	3,115	2,808		

Source: GIAS, 2005/06-2018/19.

Notes: This table presents heterogeneous effects across sponsor types on school resources and expenditure. These results are estimated by running regression 1 on the outcomes displayed on top of each column, by subgroup. All regressions include school, year, and cohort fixed effects. Robust standard errors clustered at the school-cohort level are displayed in parenthesis. Columns 4 and 5 report the p-value and FDR sharpened q-value of the t-test on the equality of coefficients across subgroups. The estimation sample includes schools that convert between 2010/11 to 2012/13 (treated schools) and schools that convert six years after each treated cohort (control schools). In Columns 2-3 and 7-8, it is restricted to schools with non-missing information on the sponsor's nature. The estimation period includes five years before to four years after the takeover of treated schools.

*** p<0.01, ** p<0.05, * p<0.1.