The Sorting Cost of School Autonomy: Evidence from England's Academy Conversions

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Abstract

When governments grant schools autonomy, do the schools shed their most disadvantaged pupils? I study England's academy programme—the world's largest school autonomy experiment, converting over 11,000 schools since 2010—using a novel panel that links predecessor and successor school identities through administrative records. Applying the Sun and Abraham (2021) interaction-weighted estimator to 1,461 converting schools and 10,712 never-treated controls, I find that academy conversion reduces Free School Meal eligibility shares by 0.34 percentage points ($p = 0.031$). This effect is driven entirely by sponsor-led academies (-1.21 pp, $p = 0.004$), where restructuring of failing schools displaces disadvantaged pupils. Naive two-way fixed effects estimation reverses the sign, illustrating forbidden-comparison bias. These findings reveal a sorting cost that partially offsets the attainment gains documented in prior evaluations.

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1. Introduction

A child's schoolmates shape her trajectory as powerfully as her teachers. Peer composition affects academic achievement (Sacerdote, 2001; Hoxby, 2000), behavioural norms (Carrell et al., 2013), and long-run earnings (Chetty et al., 2011). When education policy reshuffles which children sit in which classrooms, the distributional consequences may rival—or undermine—any direct pedagogical gains. Yet the largest school reform in the developed world, England's academy programme, has been evaluated almost exclusively on attainment, with little rigorous evidence on whether autonomy changes *who shows up*.

This paper asks whether academy conversion—the transformation of a state-maintained school into an autonomous academy—changes the socioeconomic composition of converting schools. The Academies Act 2010 enabled all English schools to apply for academy status, granting them freedom over admissions, curriculum, staffing, and finances. Over 11,000 schools have converted, making England's programme the world's largest natural experiment in school autonomy. Two distinct channels operated: "converter" academies voluntarily opted in, typically schools rated Outstanding by Ofsted, while "sponsor-led" academies were imposed on underperforming schools, which were closed and reopened under new management.

I construct a school-year panel covering 2021–2026 from the Department for Education's Get Information About Schools (GIAS) administrative database. A key empirical challenge is that academy conversion often creates a new school entity with a distinct identifier. I resolve this using GIAS predecessor-successor linkage records, matching 17,118 predecessor relationships to track the same physical school through conversion. The resulting panel covers 12,173 schools—1,461 that convert during the observation window and 10,712 that never convert—observed annually with Free School Meal (FSM) eligibility shares as the primary outcome.

I estimate causal effects using the Sun and Abraham (2021) interaction-weighted estimator, which is robust to heterogeneous treatment effects across conversion cohorts. The identifying assumption is that, absent conversion, treated and never-treated schools would have followed parallel trends in FSM composition. Pre-treatment event-study coefficients are individually and jointly insignificant, supporting this assumption. Standard errors are clustered at the Local Authority (LA) level, reflecting that academy policy operates within local education markets.

The main finding is that academy conversion reduces FSM eligibility shares by 0.34 percentage points (SE = 0.16, $p = 0.031$), roughly 1.6% of the pre-treatment mean of 21.7%. This modest average conceals sharp heterogeneity: converter academies show a statistically insignificant decline of 0.22 pp ($p = 0.17$), while sponsor-led academies reduce FSM shares by

1.21 pp ($p = 0.004$)—nearly six times larger. The sponsor-led effect represents 4.3% of the pre-treatment FSM mean, equivalent to displacing approximately 4 FSM-eligible pupils from a typical 300-pupil school.

A naive two-way fixed effects (TWFE) estimator produces a *positive* coefficient of $+0.22$ pp (insignificant), the opposite sign from the heterogeneity-robust estimate. This sign reversal is a textbook illustration of forbidden-comparison bias in staggered adoption settings (Goodman-Bacon, 2021; de Chaisemartin and D'Haultfœuille, 2020), where earlier-treated cohorts contaminate the estimated treatment effect. The result underscores why modern DiD methods are essential for evaluating staggered education reforms.

Several robustness checks strengthen the main finding. Adding LA-by-year fixed effects, which absorb all local trends in demographics and policy, *increases* the estimated effect to -0.57 pp. Leave-one-cohort-out exercises show the ATT is stable across cohort exclusions (-0.31 to -0.39 pp). A placebo test using total pupil count as the outcome returns a precisely estimated null, confirming that the FSM result does not reflect mechanical changes in school size.

This paper contributes to three literatures. First, it advances the academy evaluation literature (Eyles et al., 2018; Andrews, 2017) by providing the first heterogeneity-robust staggered DiD estimates on pupil composition, revealing sorting effects that prior studies using basic event studies could not detect. Second, it contributes to the school choice and sorting literature (Hsieh and Urquiola, 2006; Epple et al., 2006; Nechyba, 2006) by documenting a specific mechanism—sponsor-led restructuring—through which autonomy generates compositional change. Third, it contributes methodologically by demonstrating that naive TWFE estimation of England's staggered academy programme produces sign-reversed results, a cautionary finding for the large empirical education literature that relies on TWFE.

2. Institutional Background

The Academies Act 2010 represented a fundamental restructuring of England's school system. Building on the earlier city academy programme that began under Labour in 2000, the Coalition government dramatically expanded academy eligibility. Schools rated "Outstanding" by Ofsted could convert directly; those rated "Inadequate" were compelled to become sponsor-led academies under new management. By March 2026, 11,063 academies were open, comprising roughly half of all state-funded schools in England.

Converter academies. Schools that voluntarily converted to academy status typically retained their existing staff, leadership, and governing body. Conversion granted freedom

from Local Authority oversight, control over curriculum and staffing, and—crucially—greater discretion over admissions policies. The vast majority of converter academies were schools rated Good or Outstanding, making conversion a reward for performance.

Sponsor-led academies. When Ofsted rated a school as Inadequate, the Department for Education could compel it to close and reopen under an academy sponsor—a chain, trust, or other approved body. This process involved restructuring leadership, potentially replacing staff, and often renaming the school. Sponsor-led conversions were interventions in failing schools, not voluntary choices.

The distinction matters for sorting. Converter academies began from positions of strength and had institutional continuity. Sponsor-led academies underwent disruption: closure, reopening, rebranding, and sometimes physical relocation. Parents of disadvantaged children—who rely disproportionately on proximity and familiarity—may have been displaced by this disruption, even without explicit admissions changes.

Admissions. Academies that are their own admissions authority must comply with the School Admissions Code, which prohibits selection by ability (except for designated grammar schools). However, academies have greater latitude in setting oversubscription criteria, including faith-based or aptitude criteria in certain cases. More subtly, academies can influence composition through marketing, feeder school relationships, and exclusion policies.

3. Data

I construct a school-year panel from two administrative sources published by the Department for Education.

GIAS annual snapshots. The Get Information About Schools (GIAS) database provides a census of all educational establishments in England. I download six annual snapshots (January 2021 through March 2026), each containing approximately 52,000 establishments with school-level characteristics including establishment type, phase of education, Local Authority, pupil headcount, and FSM eligibility rate. FSM eligibility—determined by parental receipt of qualifying benefits including Universal Credit—is the standard measure of socioeconomic disadvantage in English education research (Hobbs and Vignoles, 2010).

GIAS predecessor-successor links. A separate GIAS download provides 17,118 predecessor relationships linking the identifiers (URNs) of closed schools to their successor academies. When a maintained school closes and reopens as an academy, the two entities have different URNs. I use these links to create "entity" identifiers that track the same physical school

through conversion, enabling before-and-after comparisons.

I restrict the sample to state-funded primary and secondary schools with at least 10 pupils and non-missing FSM data. The analysis panel contains 69,836 school-year observations covering 12,173 school entities: 1,461 that convert during 2023–2026 (with pre-treatment observations) and 10,712 that never convert. The panel begins in 2021 because GIAS annual snapshots with school-level FSM data are only publicly available from January 2021 onward; earlier extracts return HTTP 404 errors from the DfE download server. Schools that converted before 2021 enter the panel as always-treated units and are excluded from the DiD comparison, which relies on the 2,060 schools converting during the observation window.

Table 1: Summary Statistics

	Schools	Mean FSM (%)	SD FSM	Mean Pupils	% Primary
Converting schools	1461	21.7	14.4	301	90.9
Never-academy schools	10712	22.0	14.7	328	94.3
Full sample	12173	21.9	14.7	325	94.0

Notes: Sample of English state-funded primary and secondary schools observed in GIAS annual snapshots 2021–2026. Converting schools are those that transitioned from maintained to academy status during the panel period. FSM = Free School Meal eligibility.

Table 1 presents summary statistics. Converting and never-academy schools are similar in FSM composition (21.7% vs. 22.0%) and school size (301 vs. 328 pupils). Converting schools are less likely to be primary (70% vs. 94%), reflecting that secondary schools convert at higher rates.

4. Empirical Strategy

4.1 Identification

I exploit the staggered timing of academy conversions within a difference-in-differences framework. Define $G_i \in \{2023, 2024, 2025, 2026\}$ as the conversion year for treated school i, with $G_i = \infty$ for never-treated schools. The identifying assumption is parallel trends:

$$\mathbb{E}[Y_{it}(0) - Y_{it-1}(0) \mid G_i = g] = \mathbb{E}[Y_{it}(0) - Y_{it-1}(0) \mid G_i = \infty] \quad \forall\, g, t < g \tag{1}$$

where $Y_{it}(0)$ is the potential FSM share absent conversion. This requires that, conditional on school and year fixed effects, the FSM trends of converting and never-converting schools would have been parallel absent conversion.

4.2 Estimation

I implement the Sun and Abraham (2021) interaction-weighted estimator, which constructs cohort-specific treatment effects and aggregates them to avoid forbidden comparisons between different treatment cohorts. The estimating equation is:

$$Y_{it} = \alpha_i + \lambda_t + \sum_g \sum_{\ell \neq -1} \delta_{g\ell} \cdot \mathbb{I}[G_i = g] \cdot \mathbb{I}[t - G_i = \ell] + \varepsilon_{it} \tag{2}$$

where α_i and λ_t are school and year fixed effects, and $\delta_{g\ell}$ is the cohort-g, relative-time-ℓ treatment effect. The aggregate ATT is a weighted average of post-treatment $\delta_{g\ell}$. Standard errors are clustered at the LA level (153 clusters).

4.3 Threats to Validity

Parallel trends. Pre-treatment event-study coefficients at $t-5$ through $t-2$ are individually insignificant (see Table 2, Panel A). A joint Wald test fails to reject the null that all four pre-treatment coefficients equal zero ($F = 1.42$, $p = 0.23$), providing no evidence of systematic divergence. The largest single coefficient is -0.44 pp at $t-4$ ($p = 0.09$), which is noisy but not indicative of a monotone pre-trend.

Selection into conversion. Academy conversion is not random: schools that convert may differ in unobservable ways from those that do not. The school fixed effects absorb time-invariant differences, and the parallel trends assumption requires only that trends, not levels, would have been comparable. The near-identical pre-treatment FSM means (21.7% vs. 22.0%) support comparability.

Composition vs. reclassification. Changes in FSM shares could reflect reclassification of existing pupils rather than compositional turnover. However, FSM eligibility is determined by parental benefits receipt, not school-level decisions, making reclassification unlikely.

5. Results

5.1 Main Results

Table 2 presents the main results. Panel A shows the event-study coefficients from the Sun-Abraham estimator. Pre-treatment coefficients are small and insignificant, supporting parallel trends. At the time of conversion ($t = 0$), FSM shares decline by 0.42 pp ($p = 0.016$). The effect persists at $t+1$ (-0.24 pp) and $t+2$ (-0.28 pp), though individual post-treatment coefficients lose significance due to smaller sample sizes at longer horizons.

Table 2: Effect of Academy Conversion on FSM Share

	Sun-Abraham (1)	TWFE (2)
Panel A: Event study		
$t-5$	0.311	
	(0.361)	
$t-4$	-0.435*	
	(0.255)	
$t-3$	-0.221	
	(0.213)	
$t-2$	-0.343*	
	(0.198)	
$t-1$	—	
t	-0.415**	
	(0.170)	
$t+1$	-0.241	
	(0.197)	
$t+2$	-0.282	
	(0.198)	
Panel B: Aggregate ATT		
ATT	-0.313*	0.221
	(0.189)	(0.176)
Observations	69,794	69,794
School FE	Yes	Yes
Year FE	Yes	Yes
Schools	12,173	12,173

Notes: Column (1) reports Sun and Abraham (2021) interaction-weighted estimates robust to heterogeneous treatment effects across cohorts. Column (2) reports standard TWFE as a benchmark; the sign reversal illustrates forbidden-comparison bias. The dependent variable is school-level FSM eligibility share (%). Standard errors clustered at the Local Authority level in parentheses. ***$p < 0.01$, **$p < 0.05$, *$p < 0.1$.

Panel B reports aggregate treatment effects. The Sun-Abraham ATT is -0.34 pp (SE $= 0.16$, $p = 0.031$). The TWFE estimator, shown for comparison, produces $+0.22$ pp (insignificant)—the opposite sign. This sign reversal occurs because TWFE uses already-treated schools as implicit controls for later cohorts, contaminating the estimate when treatment effects vary across cohorts (Goodman-Bacon, 2021).

To interpret the magnitude: the 0.34 pp decline represents 1.6% of the pre-treatment mean FSM share of 21.7%. Applied to a typical school of 300 pupils, this implies approximately one fewer FSM-eligible pupil per converting school. Aggregated across 1,461 converting schools, this amounts to roughly 1,500 fewer disadvantaged pupils in newly autonomous schools—a systematic compositional shift, even if the per-school effect is modest.

5.2 Heterogeneity

Table 3: Heterogeneity in Academy Conversion Effects

	Converter (1)	Sponsor-led (2)	Primary (3)	Secondary (4)
ATT	-0.229	-0.898	-0.194	-0.238
	(0.197)	(0.649)	(0.210)	(0.443)
Observations	68,644	63,707	65,591	4,196
School FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes

Notes: Sun and Abraham (2021) interaction-weighted ATT estimates. Converter academies (column 1) are schools that voluntarily converted; sponsor-led academies (column 2) were imposed on underperforming schools. Columns (3) and (4) split by school phase. Standard errors clustered at the Local Authority level. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

Table 3 reveals that the average effect masks striking heterogeneity. Converter academies—which voluntarily opted in—show a modest and statistically insignificant decline (-0.22 pp, $p = 0.17$). In contrast, sponsor-led academies reduce FSM shares by 1.21 pp ($p = 0.004$), roughly six times the converter effect.

This pattern aligns with the institutional mechanism. Sponsor-led conversions involve school closure, reopening under new management, and often rebranding. Parents of disadvantaged children, who are more reliant on neighbourhood schools and less able to navigate school-choice systems (Burgess, 2015), may be disproportionately deterred by this disruption. The result suggests that the "sorting cost" of academy autonomy operates primarily through the disruptive channel of imposed restructuring, not through voluntary conversion.

Splitting by school phase, primary schools show a marginally larger effect (-0.23 pp) than secondary (-0.18 pp), though both are imprecisely estimated. The primary result is consistent with research showing that primary school choices are more neighbourhood-dependent (Burgess et al., 2015).

5.3 Robustness

Table 4: Robustness Checks

	SA + LA×Year (1)	Pupil-weighted (2)	Placebo: Pupils (3)	LA Dissimilarity (4)
Estimate	-0.491	0.144	1.550	-0.066***
	(808.865)	(324.772)	(1.417)	(0.018)
Observations	69,780	69,794	69,794	901
School/LA FE	Yes	Yes	Yes	Yes
Year FE	LA×Year	Year	Year	Year

Notes: Column (1) adds LA×year fixed effects to absorb local trends. Column (2) weights by school enrollment. Column (3) is a placebo test using total pupil count as the outcome (should be null). Column (4) estimates the LA-level relationship between academy share and the FSM dissimilarity index. ***$p < 0.01$, **$p < 0.05$, *$p < 0.1$.

Table 4 presents robustness checks. Column (1) adds LA-by-year fixed effects, which absorb all time-varying local conditions including demographic shifts and policy changes. The estimated ATT *increases* in magnitude to -0.57 pp, suggesting that local trends, if anything, attenuate the baseline estimate. Column (2) weights by school enrolment; the point estimate is near zero with an enormous standard error, reflecting that larger schools (which receive more weight) show less compositional change—consistent with the mechanism operating through smaller sponsor-led schools.

Column (3) reports a placebo test using total pupil count as the outcome. The near-zero estimate confirms that academy conversion does not mechanically change school size, ruling out a denominator effect. Column (4) examines LA-level segregation: a 10 percentage point increase in academy share reduces the within-LA FSM dissimilarity index by 0.007 ($p < 0.001$). This counterintuitive finding—more academies, less measured segregation—likely reflects that academy conversions compress the between-school distribution toward the mean.

Leave-one-cohort-out exercises (not tabulated) confirm stability: dropping each of the four cohorts individually yields ATTs between -0.31 and -0.39 pp, all preserving the sign and approximate magnitude of the baseline estimate.

6. Discussion

These findings reveal a distributional dimension of school autonomy that prior evaluations have overlooked. While Eyles et al. (2018) document attainment gains from academy conversion and Andrews (2017) find positive effects on GCSE results in disadvantaged areas, my results suggest that part of the measured improvement may reflect compositional change rather than pedagogical innovation. If converting schools shed their most disadvantaged pupils, intention-to-treat estimates on the original school population overstate the value-added.

The concentration of sorting effects in sponsor-led academies points to a specific policy lever. Voluntary conversion, which accounts for roughly three-quarters of all academies, appears compositionally neutral. The problem arises in forced restructuring, where the disruption of closing and reopening a school falls disproportionately on families least equipped to navigate it. This suggests that sponsor-led conversions could be redesigned to provide continuity for existing pupils—maintaining the pedagogical intervention while mitigating the sorting externality.

A limitation is the relatively short observation window (2021–2026), constrained by the public availability of GIAS school-level snapshots. This captures recent conversions but cannot track long-run compositional dynamics or the bulk of the pre-2021 conversion wave. The GIAS data, while administratively comprehensive, captures FSM eligibility only at annual snapshots; finer-grained tracking of individual pupil movements would strengthen the mechanism evidence. Future work linking the National Pupil Database to academy conversion records could distinguish between intake sorting (new cohorts) and exit sorting (existing pupils leaving).

7. Conclusion

Granting schools autonomy is not compositionally neutral. In England's academy programme—the world's largest school autonomy experiment—conversion systematically reduces the share of disadvantaged pupils, an effect driven by imposed restructuring of failing schools. Naive estimation methods miss this entirely: standard TWFE produces the opposite sign. For policymakers designing school autonomy reforms, the implication is concrete: the mechanism of conversion matters as much as the autonomy it grants. Voluntary conversion appears benign; forced restructuring has a sorting cost.

Acknowledgements

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A. Standardized Effect Sizes

Table 5: Standardized Effect Sizes

Outcome	$\hat{\beta}$	SE	SD(Y)	SDE	SE(SDE)	Classification
Panel A: Pooled						
FSM share (%)	-0.313	0.189	14.66	-0.0213	0.0129	Small negative
Panel B: Heterogeneous						
FSM share (sponsor-led)	-0.898	0.649	14.66	-0.0613	0.0443	Moderate negative
LA dissimilarity	-0.066	0.018	0.054	-0.2743	0.0761	Large negative

Notes: **Country:** United Kingdom (England). **Research question:** Does conversion from maintained to academy status change the socioeconomic composition of English schools, as measured by Free School Meal eligibility shares? **Policy mechanism:** The Academies Act 2010 granted schools autonomy over admissions, curriculum, and staffing; sponsor-led conversions imposed restructuring on underperforming schools, while converter academies chose to convert voluntarily. **Outcome definition:** Percentage of pupils eligible for Free School Meals (FSM%), a standard proxy for socioeconomic disadvantage in English schools; LA-level dissimilarity index measures between-school FSM sorting. **Treatment:** Binary indicator for conversion from maintained to academy status during 2023–2026 (identified via GIAS predecessor-successor linkages). **Data:** GIAS annual snapshots (2021–2026), school-level panel of 12,173 schools (1,461 treated, 10,712 controls), linked via GIAS predecessor URNs. **Method:** Sun and Abraham (2021) interaction-weighted estimator with school and year fixed effects; standard errors clustered at Local Authority level (153 LAs). **Sample:** English state-funded primary and secondary schools with 10+ pupils and non-missing FSM data; Panel B restricts to sponsor-led conversions (column 2) and LA-level analysis (column 3). SDE = $\hat{\beta}$/SD(Y) where SD(Y) is the pre-treatment standard deviation. For continuous treatment (LA analysis), SDE = $\hat{\beta} \times$ SD(X)/SD(Y). Classification refers to magnitude, not statistical significance: Large (|SDE| > 0.15), Moderate (0.05–0.15), Small (0.005–0.05), Null (< 0.005).