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DISCUSSION PAPER SERIES

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Andrew McLean
Queen's University Belfast

Duncan McVicar
Queen's University Belfast and IZA

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IZA – Institute of Labor Economics

Schaumburg-Lippe-Straße 5–9
53113 Bonn, Germany

Phone: +49-228-3894-0
Email: publications@iza.org

www.iza.org

ABSTRACT

Does Being Excluded from School Harm Student Achievement? Evidence from Siblings in English Population Data

This paper presents sibling fixed effects estimates of the relationship between school exclusion and subsequent academic achievement from population-wide administrative data on English secondary school students. It complements a growing base of quasi-experimental and individual fixed effects evidence on exclusion effects in predominantly US settings. We find that being excluded is negatively associated with subsequent achievement at school. We assess the extent to which this might reflect a negative causal impact of exclusion.

JEL Classification: I24, I28

Keywords: school exclusion, educational achievement, sibling fixed effects, administrative data

Corresponding author:
Duncan McVicar
Queen's Business School
Queen's University Belfast
Riddel Hall
185 Stranmillis Road
Belfast, BT9 5EE
United Kingdom

E-mail: d.mcvicar@qub.ac.uk

1. Introduction

Exclusion is a disciplinary measure used by schools in response to misbehaviour by students. Exclusions can be permanent or fixed-term, sometimes referred to as expulsion and suspension, respectively (Kinsler, 2013; Pope and Zuo, 2023). They can also be 'in' or 'out of' school (Timpson, 2019; Craig and Martin, 2023). While in-school exclusions remove students from classrooms, they permit them to remain on school grounds, in contrast to out-of-school exclusions. The extent of in-school exclusion is unclear given they are not universally recorded in administrative data (Power and Taylor, 2018). But millions of students in the United States (US) and other OECD countries are exposed to out-of-school exclusion (the focus of this paper) every year, whether directly by being excluded (Cobb-Clark et al., 2015) or indirectly by having an excluded peer (Perry and Morris, 2014; Craig and Martin, 2023). In England, for example, about 1 in every 1,000 students was permanently excluded and about 93 in every 1,000 students were subject to fixed-term exclusion during the 2022/2023 school year (Department for Education, 2024a), with many more potentially affected indirectly as peers.

Exclusion provokes debate, not only in the research and policy communities, but also among parents, teachers, students, and other stakeholders (Kinsler, 2013; Timpson, 2019; Armstrong, 2021).[1] At the debate's core is the question of the extent to which a trade-off exists between the academic achievement (e.g., Kinsler, 2013) and other outcomes (e.g., Dorsett et al., 2023) of excluded students versus those of their peers. For example, the direct achievement effects of exclusion on the excluded may be negative due to lost classroom time, worsened relationships with teachers, or psychological costs (Mendez, 2003; Perry and Morris, 2014). Alternatively, they may be positive if students improve their behaviour in response to being disciplined or fare better when removed from a specific school setting, whether temporarily or permanently. Exclusion may also have positive indirect effects on the academic achievement of classroom peers, e.g., through reduced incidence of classroom disruption or as a deterrent to misbehaviour (Angrist et al., 2013; Machin and Sandi, 2020). However, the indirect effects of exclusion may be negative if it undermines within-school relationships or proxies a wider disciplinarian/punitive school culture (Perry and Morris, 2014; Craig and Martin, 2023). Therefore, exclusion has implications for the education production process for both directly affected students and their peers, and in an uncertain direction in each case (Lazear, 2001; Dearden et al., 2009).

[1] Exclusion and broader issues of school discipline also attract considerable media attention in the UK and other countries e.g., 'No Excuses': inside Britain's strictest school, The Guardian 30-12-2016; 'You Can Hear a Pin Drop': The Rise of Super Strict Schools in England, New York Times 12-03-2024.

There are also distributional concerns about the application of exclusion in schools, including that it exacerbates existing inequalities in educational outcomes (Strand, 2014; Timpson, 2019). Indeed, male, Black, low-income, and students with special educational needs (SEN), all tend to be excluded disproportionally more than others (Jordan and Anil, 2009; Kinsler, 2011; Cobb-Clark et al., 2015). Moreover, exclusion may have different impacts for different types of students. For example, it may have greater academic impacts for socio-economically disadvantaged students with less access to educational and other resources out of school compared to their peers (Blanden and Gregg, 2004; Heckman, 2008; Resnjanskij et al., 2024). For disadvantaged students who qualify for free or reduced-price meals at school, exclusion may also entail nutritional costs, with knock-on impacts for their academic performance (Belot and James, 2011; Schwartz and Rothbart, 2020).

Despite the theoretical ambiguity and societal importance of the effects of exclusion on achievement, much of the evidence base linking the two consists of correlational studies which do not separately identify any causal impacts from the confounding effects of other unobservable (and in some cases observable) differences between excluded and non-excluded students. More recently, a handful of studies adopting causal methods has emerged, exploiting various natural experimental settings almost exclusively within the US (e.g., Lacoe and Steinberg, 2018a; Craig and Martin, 2023; Pope and Zuo, 2023; Bacher-Hicks et al., 2024). However, in addition to the wider institutional and contextual differences between US and UK schools (Timpson, 2019), the specific nature of the natural experiments exploited in these studies may limit the extent to which their findings can be generalised beyond the particular contexts studied. For example, the New York City exclusions studied by Craig and Martin (2023) are in, rather than out-of-school. Further, few studies in this emerging quasi-experimental literature estimate the direct effects of exclusion on the excluded; instead typically estimating net effects (combining direct and indirect effects) of changes in the use of exclusion on average achievement within schools. Finally, none examine explicitly whether exclusion effects are heterogeneous by student socioeconomic status (SES).

This paper studies the direct effects of exclusion on the excluded in an English context. It takes a different approach to the quasi-experimental literature by adopting a sibling fixed effects estimation strategy, enabled by population-wide data on exclusion, pupil characteristics, and the primary and secondary school achievement of students in English schools. That is, we exploit within sibling group variation in receiving exclusion(s) to estimate the extent to which being excluded is associated with subsequent academic achievement, conditioned on a wide range of observables and cohort, school, and family fixed effects. Ours is the first study of exclusion effects to adopt this sibling fixed effects approach. The closest parallels in the existing literature are studies which exploit longitudinal data to present individual fixed effects estimates of the short-term effects of being excluded on academic outcomes (e.g., Chu and Ready, 2018; Hwang, 2018; Lacoe and Steinberg, 2018a). Like most of the quasi-experimental exclusion literature, however, these studies are set in specific US educational contexts, potentially limiting the extent to which their findings might be generalisable. The individual fixed effects approach is also unsuitable for estimating longer-term effects of exclusion, including on outcomes at the end of secondary schooling, as studied here. We discuss the relative merits of our sibling fixed effects approach vis-à-vis an individual fixed effects approach in more detail later in the paper.

We assess the extent to which our sibling fixed effects estimates are robust to selection on (within-family) unobservables using Oster-style sensitivity analyses (Oster, 2019). One potential (tentative) interpretation of the resulting bias-adjusted estimates is that they bound the causal effect of exclusion on the excluded in these data. We also explore the sensitivity of our results to potential spillovers between siblings (see e.g., Nicoletti and Rabe, 2019) by comparing estimates for subsamples consisting of siblings in the same school years and of siblings in different school years, for whom we might expect stronger/weaker spillovers, respectively. We contribute further by estimating whether the direct effects of exclusion are greater for socially disadvantaged students who, as is common in the literature (Belot and James, 2011; Burgess et al., 2015; Craig and Martin, 2023), we identify as those eligible for free school meals (FSMs). The SES achievement gap in UK education is several times larger than those associated with students' sex or ethnicity and has widened in recent years (Strand, 2014; Farquharson et al., 2024).

We find that being excluded is strongly and negatively associated with subsequent achievement at school, across several measures of achievement, and that these relationships are qualitatively robust to, although attenuated in magnitude by, conditioning on observable covariates (including prior academic achievement) as well as cohort, school, and sibling fixed effects. For example, our sibling fixed effects estimates show that exclusion is associated with a 13 percentage point (pp) decrease in the probability of achieving 5 or more General Certificates of Secondary Education qualifications (GCSEs) at grades A*-C including English and Mathematics (a widely used benchmark for achievement at the end of secondary schooling in England). We also find evidence suggestive of a dose-response relationship between exclusion and achievement, with larger estimated effects for permanent than for fixed-term exclusions, and for repeated fixed-term exclusions compared to one-off fixed-term exclusions.

For example, our sibling fixed effects estimates show that being permanently excluded is associated with a 12 percentile rank penalty in the number of GCSEs achieved, whereas receiving one (two or more) fixed-term exclusion(s) is associated with a 4 (9) percentile rank penalty. Moreover, using Oster (2019)-style methods, we show that our conclusions are robust to selection on (within-family) unobservables under standard proportionality assumptions. Finally, we show robustness to the nature of the sibling groups included in the analysis, suggesting that spillover effects between siblings do not substantially bias our estimates. Taken together, our results are suggestive of exclusion exerting an economically and statistically significant negative causal impact on the academic achievement of excluded students. In line with previous evidence for Australia, however, we do not find that being excluded is associated with larger academic penalties for socially disadvantaged students (Cobb-Clark et al., 2015).

The rest of the paper is structured as follows. Section 2 reviews existing research on exclusion and educational achievement. Section 3 describes our data and presents descriptive statistics. We set out our approach to estimation in Section 4, with results presented in Section 5. Section 6 concludes.

2. Existing Literature

Much of the quantitative literature linking exclusion to educational outcomes consists of correlational studies which reveal that excluded students, or schools with higher exclusion rates, tend to do worse in terms of student achievement or related outcomes (e.g., Ekstrom et al., 1986; Davis and Jordan, 1994, Mendez, 2003; Rausch and Skiba, 2004; Arcia, 2006; Christle et al., 2007). Noltemeyer et al. (2015) provides a comprehensive meta-analysis of these studies. However, it is not clear from this body of research to what extent the negative association typically found between exclusion and achievement is causal, as opposed to the result of unobserved (or observed but uncontrolled) confounding differences between those excluded and those not, i.e., selection effects. And if we are to inform policymakers and schools about the trade-off between the achievement of would-be excluded students and that of their peers, it is causal estimates (or at least closer-to-causal estimates) that we need.

In an early attempt to estimate such causal exclusion effects, using a structural modelling approach for North Carolina middle school students, Kinsler (2013) presents estimates that suggest exclusion does not impact detrimentally on student achievement. Another approach has been to mitigate selection on unobservables by estimating individual fixed effects models of the impact of being excluded on academic achievement in the short run, so far largely in US school settings (Chu and Ready, 2018; Hwang, 2018; Lacoe and Steinberg, 2018a). Like the earlier generation of correlational studies, these studies typically report negative associations between exclusion and achievement. However, as these studies acknowledge, the extent to which this is driven by a causal effect of exclusion, as opposed to the confounding effects of time-varying unobservables, remains unclear. Having said that, this kind of individual fixed effects approach seems likely to give estimates of associations that are closer to causal effects than earlier correlational studies.

A more recent literature, so far also largely from the US, has sought to estimate the causal effects of exclusion on educational outcomes by exploiting plausibly exogeneous variation in exclusion rates generated by natural experiments such as school district boundary changes or exclusion bans (e.g., Lacoe and Steinberg, 2018a; Lacoe and Steinberg, 2018b; Cleveland, 2023; Craig and Martin, 2023; Pope and Zuo, 2023; Bacher-Hicks et al., 2024). Although this is the more promising approach at first glance, most of these studies estimate net effects of exclusion (combining direct and indirect effects) rather than the direct effects of exclusion on the excluded. Evidence from this literature is mixed but leans towards exclusion having a negative net effect on academic achievement, on average. For example, Bacher-Hicks et al. (2024) reports negative impacts on student achievement of being assigned to schools with relatively high exclusion rates following a 2002 boundary change in Charlotte-Mecklenburg. Cleveland (2023) reports improvements in dropout rates, graduation rates, and performance in English language arts following a Massachusetts reform designed to reduce the use of exclusion in schools. Craig and Martin (2023) report increased mathematics and reading achievement for students following a ban on the use of exclusion in response to certain types of misbehaviour in New York City. In contrast, Pope and Zuo (2023) report a decline in average achievement following a decline in the use of exclusions in Los Angeles. In a rare quasi-experimental study for the UK, Machin and Sandi (2020) exploit the academisation of schools (removal of schools from local authority control) in England, finding that the resulting changes in propensity to exclude did not explain test score gains in such schools.

The main exception to this focus on net effects in the emerging quasi-experimental literature is Lacoe and Steinberg (2018a), which presents both individual fixed effects estimates and instrumental variables estimates of the direct effect of exclusion on the excluded, the latter exploiting a ban on the use of exclusion for non-violent behavioural infractions in the School District of Philadelphia. In both cases they report negative effects on academic achievement from being excluded. A partial exception is Pope and Zuo (2023) which reports that the academic achievement of those with a higher exclusion propensity (high-risk students) improved following the New York City ban on exclusions, in contrast to the negative average effect. Although it is difficult to interpret this contrast, it is consistent with a negative direct effect of exclusion coupled with a positive indirect effect. In sum, although there is a growing literature which is suggestive of a negative impact of being excluded on academic achievement, explicit quantitative evidence of such an effect remains sparse.

Existing evidence on whether exclusion impacts socially disadvantaged students more or less than their counterparts is similarly sparse, despite several studies reporting heterogeneity in exclusion effects along other (included correlated) dimensions. For example, Bacher-Hicks et al. (2024) finds that the negative (net) impacts of exclusion are larger for male and ethnic minority students, but do not test explicitly for heterogeneity by social disadvantage. The heterogeneity in exclusion effects by exclusion propensity reported by Pope and Zuo (2023) could also reflect larger impacts for socially disadvantaged students given existing evidence that shows such students are disproportionately excluded (e.g., Jordan and Anil, 2009; Cobb-Clark et al., 2015), although it might also reflect other underlying differences. A study in the correlational literature which directly tests for heterogeneous associations between exclusion and achievement by household welfare receipt (an indicator of social disadvantage) finds no difference (Cobb-Clark et al., 2015).

Our paper contributes to this literature by providing new evidence on the direct effect of being excluded on achievement at the individual-level, uniquely adopting a sibling fixed effects approach to mitigate family-level unobserved confounders, coupled with sensitivity analyses designed to assess robustness to selection on remaining within-family unobservables and the potential for spillover effects between siblings. We also test explicitly whether direct exclusion effects are heterogeneous by social disadvantage. Finally, by presenting evidence on exclusion effects for the population of English school pupils across several cohorts, we also help to broaden the evidence base beyond the specific US contexts of recent quasi-experimental and individual fixed effects studies.

3. Data

We exploit the Longitudinal Education Outcomes (LEO) administrative dataset for England, curated by the UK Government Department for Education, and made available recently for approved use by accredited researchers via the Secure Research Service of the Office for National Statistics. Among other things, LEO longitudinally tracks all students in state-maintained (i.e., publicly-funded) schools in England, from their first year of schooling through to their last, and includes de-identified data on exclusions, education outcomes such as test scores and qualifications achieved, and student socio-economic and demographic characteristics (Department for Education, 2024b). Education in England follows a national curriculum organised into blocks of years called Key Stages, with Key Stages 1 and 2 making up students' years of primary education, and Key Stages 3 and 4 corresponding to their secondary schooling (Department for Education, 2013). Our base dataset consists of five cohorts of students who finished their Key Stage 4 (KS4) schooling between the 2009/2010 and 2013/2014 academic years, for whom we have complete information on their characteristics, experience of school exclusion, and academic achievement. This results in a base dataset of 2,667,917 individuals.

We measure students' exposure to both permanent and fixed-term exclusion across their Key Stage 3 (KS3) and KS4 careers, which corresponds to five academic years, when they are usually aged between 11/12 and 15/16 years.[2] In England, permanent exclusion from a mainstream school prevents a student from re-attending that school. Fixed-term exclusion, on the other hand, temporarily removes students from schools for periods ranging from half a day to a maximum of 45 days in a single academic year (Department for Education, 2013; Timpson, 2019; Department for Education, 2024c). Our measure of permanent exclusion is binary and equals one for students permanently excluded one or more times during KS3 or KS4, and zero otherwise. We construct two binary measures of fixed-term exclusion. The first equals one for students who experienced exactly one fixed-term exclusion during KS3 and KS4 but no permanent exclusion. The second equals one for students who experienced two or more fixed-term exclusions during KS3 and KS4 but no permanent exclusion. Note that the information we use to measure exclusion is recorded administratively by English schools and is therefore less likely to be reported with error than in studies relying on exclusion information reported by students or students' parents (Cobb-Clark et al., 2015; Madia et al., 2022). Table 1 shows that 0.3% of our sample experienced one or more permanent exclusions during their KS3 and KS4 years, 6.7% experienced exactly one fixed-term exclusion and no permanent exclusion, and 7.2% experienced two or more fixed-term exclusions but no permanent exclusion.

Our academic achievement outcomes for students are measured at the end of KS4, when GCSE standardised exams are usually taken. They include the achievement of 5 or more GCSEs at grades A*-C including English and Mathematics, the number of GCSEs at grades

[2] Exclusion is rare but not unknown in English primary schools, with only 0.01% of primary pupils permanently excluded and 1.8% fixed-term excluded in 2022/2023 (Timpson, 2019; Department for Education, 2025).

A*-G achieved, and GCSE points score. We assign students percentile ranks for the latter two (quasi-)continuous measures for ease of comparison across measures and because the points system changes during our analysis period. These measures each have different pros and cons. The first, although widely used as a benchmark (e.g., Department for Education, 2012; Strand, 2014; Gorard et al., 2022), sets a comparatively high bar for academic achievement, with only 58.6% of pupils in our sample meeting the threshold (see Table 1). The second and third measures, although less widely used, may be more suitable for capturing marginal achievement effects from exclusion across the distribution.

The richness of the LEO dataset also enables us to control for a set of individual-level factors which existing research suggests are correlated with both school exclusion and academic achievement (Kinsler, 2011; Noltemeyer et al., 2015; Timpson, 2019). These include birth order, sex, language, ethnicity, special educational needs (SEN) status, free school meal (FSM) eligibility (our proxy for socio-economic disadvantage) and, crucially, prior achievement (which we define for students in terms of their total KS2 points score, expressed as a percentile rank).[3] Note that, in the LEO dataset, students' FSM eligibility reflects their FSM registration status rather than their eligibility (via low-income and/or benefit receipt) per se. Recent studies, however, have shown that the two are very highly correlated (Sahota et al., 2014; Borbely et al., 2024).[4] We measure all individual-level controls for students when they are in their last year of KS4, with the exception of our prior achievement control, which is measured at the end of KS2, when students are usually aged 10/11 years. Table 1 presents descriptive statistics for these variables. Finally, our base dataset also includes unique sibling group, school, and cohort identifiers.

In line with existing evidence, Table 1 shows that exclusion is associated with several observable characteristics including being male, Black, FSM eligible, and having special educational needs (Mendez, 2003; Kinsler, 2011; Noltemeyer et al., 2015; Graham et al., 2019; Timpson, 2019). Further, excluded students in our sample fare considerably worse in terms of their academic achievement than others. For example, Table 1 reports that only 10.2% of students who experienced one or more permanent exclusions achieved 5 or more GCSEs at grades A*-C versus 58.6% for the full sample. Students excluded during KS3 and/or KS4 also have lower prior achievement. For example, the average KS2 test score percentile rank for the

[3] Total KS2 point score is the sum of a student's KS2 English points score and KS2 Maths points score, with each recorded on a scale ranging from 0-51. These scores are from tests sat at the end of KS2.
[4] Furthermore, being FSM registered does not guarantee that a student is indeed taking up FSM.

full sample is 63 versus 49 and 28 for students who ended up receiving one fixed-term exclusion and one or more permanent exclusions, respectively.

Table 2 shows that 14.1% of our sample are classed as FSM eligible. As in Machin and Vignoles (2004) and Hobbs (2016), the average academic achievement of FSM students in our sample is lower than that of their non-eligible counterparts. For example, the average number of GCSEs achieved at grades A*-G percentile rank is 62 for non-FSM students and 29 for FSM students, with 35.3% of FSM students achieving 5 or more GCSEs at grades A*-C including English and Mathematics versus 62.4% of non-FSM students. In line with existing evidence that disadvantaged students are disproportionately excluded (Jordan and Anil, 2009; Graham et al., 2019), Table 2 also reports that 5.8% of non-FSM students experienced two or more fixed-term exclusions versus 15.4% of FSM students, and 0.73% of FSM students experienced permanent exclusion versus 0.23% of non-FSM students.

Of the approximately 2.7 million students in our sample, Table A1 in the appendix reports that 895,121 are siblings with at least one other present in the data, and 913,141 are siblings with no others present in the data (henceforth sibling singletons). Sibling singletons are predominantly students whose sibling(s) fall outside the five cohorts analysed here, whether completing KS4 prior to our first cohort or after our final cohort, and therefore for whom we have incomplete information. For the purposes of estimating sibling fixed effects models, they are treated as singletons and dropped from the estimation sample. Table A1 shows that the observable characteristics of sibling singletons are very similar to those of siblings with others present in the data. This also holds for academic achievement at both primary and secondary school stages and experience of exclusion.

4. Approach to Estimation

To examine the relationship between being excluded and subsequent achievement, we estimate linear models for each of our outcomes: first without adjusting for observables; second adjusting for observables as well as cohort and school fixed effects; and finally including sibling fixed effects. For our binary outcome measure (the achievement of 5 or more GCSEs at grades A*-C including English and Mathematics) these are linear probability models. The model including the full set of observable controls and cohort, school, and sibling fixed effects takes the form:

$$(1) \qquad Y_{ifsc} = \mu_c + \delta_s + \alpha_f + \boldsymbol{X'_{ifsc}\beta} + \gamma_1 P_{ifsc} + \gamma_2 F_{ifsc} + \gamma_3 FT_{ifsc} + \varepsilon_{ifsc}$$

In (1), Y_{ifsc} denotes academic achievement for student i, in family f, school s, and cohort c; μ_c, δ_s, and α_f, respectively, denote cohort, school, and sibling fixed effects; X'_{ifsc} is a vector of observable individual-level controls as set out in Table 1; P_{ifsc} is a binary variable indicating whether a student experienced one or more permanent exclusions; F_{ifsc} is a binary variable indicating whether a student experienced exactly one fixed-term exclusion; and FT_{ifsc} is a binary variable indicating whether a student experienced two or more fixed-term exclusions; and ε_{ifsc} is the error term. We cluster standard errors at the school level when estimating (1) (Clarke et al., 2015; Cameron and Miller, 2015).

We explore heterogeneity in the effects of exclusion on achievement by students' socioeconomic status (SES) in two ways. First, we split our sample according to our proxy for lower SES (students' FSM eligibility), re-estimating Equation (1) on each subsample (omitting the FSM variable from the set of individual-level controls). Second, using the full sample, we estimate an augmented version of Equation (1) including interaction terms between the variables indicating FSM eligibility and each type of exclusion. In England, as in other countries, most of the FSM eligibility criteria relate to household benefit receipt and/or income, which has informed its widespread use as an indicator of lower SES in academic research (Burgess et al., 2013; Ilie et al., 2019; Schwartz and Rothbart, 2020; Adamecz et al., 2024) and policymaking (Pattaro et al., 2020; Gorard et al., 2022). Note, however, that families can move in and out of FSM eligibility over time. Because most of the siblings in our data complete secondary schooling in different academic years (i.e., they are in different school cohorts), this leads to some (albeit limited) variation in FSM eligibility even within sibling groups.

Arguably, the closest parallels to our sibling fixed effects approach in the existing literature on school exclusion are the individual fixed effects approaches adopted by Chu and Ready (2018), Hwang (2018), and Lacoe and Steinberg (2018a). The individual fixed effects approach removes scope for bias from confounding time-invariant unobserved differences between those excluded and those not excluded, although potential for bias from time-varying unobservables remains. In contrast, the sibling fixed effects approach adopted here removes scope for bias from confounding family-level unobservables common to siblings, but potential bias from unobserved individual-level (within-family) confounders remains. Both are useful methods for mitigating some aspects of selection bias, but we adopt the sibling fixed effects approach here, as opposed to an individual fixed effects approach, for the following reasons. First, our interest is in the longer-run impacts of exclusion on academic achievement as measured at the end of secondary schooling i.e., the end of KS4, rather than the short-run effects of exclusion on outcomes within quarter, semester, or year. The LEO data also best support our focus on longer-term academic impacts of exclusion, given that achievement is only recorded in LEO at the end of each Key Stage and not in between. In contrast, because it relies on within-individual variation in exclusion and achievement over time, the individual fixed effects approach does not lend itself to estimating such longer-run effects (Chu and Ready, 2018). Second, persistent (e.g., beyond quarter/semester/year) effects of exclusion may bias individual fixed effects estimates of short-run exclusion effects because outcomes during within-individual exclusion-free periods may themselves be impacted by past exclusions. Indeed, an implicit assumption of individual fixed effects approaches in this context is that exclusion effects are symmetrical, i.e., that a period in which a student is not excluded is the same whether it falls before or after a period in which they are. Third, the individual fixed effects approach omits students who are excluded in each period studied, for whom we might expect the largest cumulative impacts (Lacoe and Steinberg, 2018a).

On the other hand, in adopting a sibling fixed effects approach, we cannot rule out that selection on within-family unobservables may bias our estimates, and we therefore do not interpret them as causal. Ex ante, our expectation is that the sign of any remaining selection bias is likely to be negative, because unobserved confounders that are positively correlated with exclusion are likely to be negatively correlated with achievement. Our estimates may also be biased if there are spillover effects between siblings, such that the exclusion of one sibling impacts on the achievement of another. Evidence for achievement spillover effects between siblings, using a predecessor of the LEO data, is presented by Nicoletti and Rabe (2019). They find them to be positive in sign, such that a lower-achieving sibling lowers own achievement. If there are direct effects of exclusion on achievement, and if such achievement spillover effects exist between siblings in our cohorts, then our sibling fixed effects estimates are potentially biased towards zero, as would most likely be the case for sibling spillovers via other mechanisms.

We explore the sensitivity of our estimates to such potential biases in two ways. First, we test the robustness of our sibling fixed effects estimates to selection on (within-family) unobservables using Oster (2019) style analysis, adapted for the fixed effects nature of our model following the approach of Bryan et al. (2022). This approach explores the sensitivity of our sibling fixed effects estimates to selection on remaining unobservables by exploiting the change in estimated exclusion coefficients when moving from fixed effects models with no observable (individual-level) controls to fixed effects models with our complete set of observable controls, under an assumption of proportionality between the effects of selection on observable and unobservable controls, net of fixed effects. One (tentative) potential interpretation of the resulting bias-adjusted estimates is that they represent a lower bound on the absolute magnitude of the causal effect of exclusion on achievement (Oster, 2019). This interpretation can also hold in the presence of sibling spillovers if they attenuate the sibling fixed effects estimates. Following Bryan et al. (2022), this approach also allows us to compare the sensitivity of our estimates to selection on between-family unobservables versus selection on within-family unobservables. Second, we explore the sensitivity of our estimates to potential spillovers between siblings by comparing estimates for siblings (including twins) in the same school years, for whom we might expect the strongest spillover effects, to those for siblings in different school years.

5. Results

Table 3 reports key estimates from Equation (1), for each of the three outcome measures, first unadjusted, then including observable covariates and cohort and school fixed effects, and finally also including sibling fixed effects. In all versions of the model, we find large, negative and highly statistically significant associations between exclusion and achievement. The estimated exclusion effects attenuate in magnitude as we increase the extent of statistical adjustment, but remain negative, large and statistically significant in all cases in the sibling fixed effects models. In these models, receiving one (two or more) fixed-term exclusion(s) is associated with an 8 (13) pp decrease in the probability of achieving 5 or more GCSEs at grades A*-C including English and Mathematics, a 4 (10) percentile rank decrease in the number of GCSEs achieved at grades A*-G, and a 4 (7) percentile rank decrease in GCSE points score. The equivalent estimates for experiencing one or more permanent exclusions are decreases of 13pp and 12 and 8 percentile ranks, respectively. Note that the attenuation in estimated exclusion effects between columns 2 and 3 of Table 3 is not driven by sample differences between the full sample and the siblings sample. Table A2 shows that these estimated effects, and their relative magnitudes, are robust to re-estimation of all models on the siblings sample.

Full estimation results for these models are reported in Appendix Tables A3-A5. Although there is some flipping of the signs of estimated coefficients for some observable controls between models with and without sibling fixed effects, reflecting the within-family nature of the latter, estimated coefficients for observable controls in the sibling fixed effects models take expected signs, with one partial exception. For example, all three achievement measures show achievement is higher for girls than for boys, on average (Strand, 2014; Graham et al., 2019), lower for students with special educational needs (Timpson, 2019; Tuckett et al., 2024) and increasing in students' prior (KS2) achievement (Gibbons et al., 2013; Leckie and Prior, 2022). Given limited within-family variation, language and ethnicity are uncorrelated with achievement in our sibling fixed effects models. Estimates for these variables in our other models of student achievement (i.e., excluding sibling fixed effects), however, are consistent with recent UK evidence showing that most ethnic groups outperform White British students by the end of secondary education (Tuckett et al., 2024). In line with evidence concerning birth order effects on educational outcomes (Booth and Kee, 2009), higher birth order is negatively associated with academic achievement in our sibling fixed effects models. Note that some birth order estimates, however, take opposite signs in models excluding sibling fixed effects, reflecting the confounding effects of family-level unobservables. Moreover, in models without sibling fixed effects, FSM eligibility is negatively associated with all three of our measures of academic achievement, in line with existing evidence on the SES achievement gap in education (Strand, 2014; Findlay and Hermansson, 2019; Farquharson et al., 2024; Resnjanskij et al., 2024). In sibling fixed effects models for two of our three outcome measures, however, FSM eligibility is uncorrelated with achievement, most likely reflecting limited within-family variation, given eligibility at a particular time is determined at the household level. The aforementioned partial exception is the estimated effect of FSM eligibility in the sibling fixed effects model for GCSE points score, which takes a positive sign. We interpret this cautiously, given we do not observe the reason for within-family variation in FSM eligibility. However, it is consistent with a beneficial effect on students' performance of being provided with nutrition at school (Belot and James, 2011; McEwan, 2013; Schwartz and Rothbart, 2020).

Tables A3-A5 also allow us to compare the magnitudes of our estimated exclusion coefficients to those for students' observables. The estimated effects of exclusion in sibling fixed effects models are relatively large. For example, column 3 of Table A3 shows that being male is associated with a 5 pp lower probability of achieving 5 or more GCSEs at grades A*-C including English and Mathematics, whereas experiencing one (two or more) fixed-term exclusion(s) is associated with an 8 (13) pp decrease in this probability. Across all three of our achievement outcomes, estimated effects of being designated as having special educational needs (SEN) fall between those of receiving one and two or more fixed-term exclusions, but are always smaller than the estimated effects of experiencing one or more permanent exclusions. The magnitude of the effect of experiencing one or more permanent exclusions on our binary measure of achievement is equivalent to having a prior achievement score that is lower by approximately 16 percentile ranks. For our (quasi-)continuous measures of achievement, the effect of experiencing one or more permanent exclusions is equivalent to having a prior achievement score that is lower by approximately 30 percentile ranks.

Our estimates are also consistent with a dose-response relationship between exclusion and achievement. For example, column 3 of Table 3 shows that receiving one fixed-term exclusion is associated with a decrease of 4 percentile ranks in the number of GCSEs achieved at grades A*-G, receiving two or more fixed-term exclusions is associated with a 10 percentile rank penalty, and receiving one or more permanent exclusions is associated with a 12 percentile rank penalty. Few existing studies provide this level of detail when it comes to individual-level experiences of exclusion, instead typically focussing on school-level effects (e.g., Perry and Morris, 2014; Machin and Sandi, 2020; Craig and Martin, 2023; Bacher-Hicks et al., 2024) or the effects of single, catchall, binary exclusion measures (e.g., Cobb-Clark et al., 2015).

5.1 Heterogeneity by SES

We find no evidence that being excluded is associated with larger penalties, in terms of our three achievement outcomes, for lower-SES students, as identified by their FSM eligibility. This holds in unadjusted, partially adjusted, and sibling fixed effects models of student achievement, estimated on samples consisting only of FSM and non-FSM students respectively, the results of which are reported in Table 4. Rather, in terms of absolute magnitude, the evidence here points to the opposite being the case, i.e., that the direct effects of exclusion on achievement may be smaller for lower-SES students. For example, in sibling fixed effects models, experiencing one fixed-term exclusion is associated with an 8 pp decrease in the probability of achieving 5 or more GCSEs at grades A*-C including English and Mathematics for non-FSM students, versus a 6 pp decrease for FSM students, respectively.

Table A6 in the appendix presents full-sample estimates of an augmented version of (1) with interaction terms between FSM status and our exclusion dummies. The relevant estimates from the sibling fixed effects models similarly suggest that the relationship between exclusion and achievement differs statistically significantly according to students' FSM eligibility, such that exclusion effects are smaller for the FSM eligible. There are exceptions, however, where no statistically significant difference is apparent. The gap in the estimated effects of exclusion between lower-SES students and others is largest in terms of permanent exclusion, with estimated permanent exclusion effects for FSM students, in sibling fixed effects models, between half and two thirds the magnitude of those for non-FSM students.

Note that this finding does not reflect baseline academic achievement being lower among FSM students, i.e., that the achievement of non-FSM students has further to fall, on average, following an exclusion. Table A7 shows that exclusion effects for FSM students, at least in terms of our (quasi-)continuous achievement outcomes, are also smaller (permanent exclusion) or no larger (fixed-term exclusion) in relative terms. Instead, our conjecture is that the larger estimated effect of permanent exclusion for non-FSM pupils might reflect, at least in part, our use of registered FSM eligibility as a proxy for socio-economic disadvantage. For example, if the most disadvantaged FSM eligible households are less likely to register their children due to language, literacy (including digital literacy given that FSM registration is often online), or other barriers, then our proxy may fail to capture the lower tail of the SES distribution, where household resources may be least able to compensate for the academic impacts of exclusion (Chevalier et al., 2013; Sahota et al., 2014; Francesconi and Heckman, 2016).

5.2 Robustness to Selection on Unobservables

We test the robustness of our sibling fixed effects estimates to selection on remaining unobservables using Oster (2019) style analysis, adapted for the fixed effects nature of our model following Bryan et al. (2022). First, we estimate bias adjusted treatment effects of exclusion (β^*) under the assumption that selection on unobservables and observables is proportional, following adjustment for fixed effects. Second, we estimate degrees of selection on unobservables relative to observables ($\tilde{\delta}$) that would be required to nullify our estimated effects of exclusion, again following adjustment for fixed effects. We assume R_{max} equals $1.3\tilde{R}$ or $2.2\tilde{R}$ where, in both cases, $\tilde{R}$ is the (within) R-squared value from the respective fully adjusted model of achievement on exclusion. Estimates are presented in Table 5.

Under the assumption that R_{max} equals $1.3\tilde{R}$, we find the estimated effects of being excluded to be highly robust to selection on unobservables, becoming only marginally smaller in magnitude following adjustment for bias. For example, where sibling fixed effects models suggest the effect of one (two or more) fixed-term exclusion(s) on GCSE point score is a 4 (7) percentile rank penalty, Table 5 shows that the corresponding bias-adjusted exclusion effects are penalties of 3 and 6 percentile ranks, with all exclusion estimates remaining statistically significant at the 99.9% level following bias adjustment. Further, the $\tilde{\delta}$ values reported in Table 5 suggest that unobservables would need to be at least four times as important as observables for selection to explain entirely our estimated effects of being excluded in sibling fixed effects models. If we assume instead that R_{max} equals $2.2\tilde{R}$, which sets a higher bar, estimated bias-adjusted exclusion effects are further attenuated in magnitude and, in some cases, no longer statistically significant. Nevertheless, we continue to see evidence suggestive of non-trivial magnitude and statistically significant detrimental exclusion effects, even for single fixed-term exclusions, with estimates of $\tilde{\delta}$ nowhere smaller than 1.

Note the relative robustness of our sibling fixed effects estimates, in this respect, versus those presented by Cobb-Clark et al. (2015), who apply an earlier approach to evaluating robustness to omitted variable bias (see Altonji et al., 2005). This suggests that the scope for selection on unobservables is much reduced by the sibling fixed effects approach. Further, this analysis can provide suggestive evidence of the relative roles of between-family and within-family unobserved heterogeneity in our estimated exclusion effects. For example, the finding that our estimated exclusion effects are attenuated to a greater degree by the inclusion of sibling fixed effects that by the bias adjustment (under R_{max} equals $1.3\tilde{R}$), is consistent with family-level unobservables being more salient in our case than within-family unobservables. If we instead assume that R_{max} equals $2.2\tilde{R}$, the suggestion is that family-level unobservables and within-family unobservables may confound estimates without sibling fixed effects (such as those presented in model 2 of Table 3) to a broadly similar degree.

5.3 Spillover Effects between Siblings

We also explore the sensitivity of our exclusion estimates to potential spillovers between siblings (e.g., see Nicoletti and Rabe, 2019). To do so, we construct subsamples consisting only of siblings (including twins) in the same school years and of siblings in different school years, respectively. Then, for each subsample, we estimate unadjusted, partially adjusted, and sibling fixed effects models (i.e. (1)) for KS4 student achievement. The resulting estimates (see Table 6) show that our preferred exclusion estimates are broadly robust to the nature of the sibling groups included in our sibling fixed effects analysis. For example, for different-year siblings, as for the full siblings sample, the effect of one (two or more) fixed exclusion(s) on the number of GCSEs achieved at grades A*-G, is a 4 (10) percentile rank penalty. For same-year siblings, the corresponding penalties are 3 and 8 percentile ranks, i.e., slightly smaller, although these effects remain statistically significant at the 99.9% level. The exception to this is that the estimated effects of permanent exclusion for same-year siblings are both smaller and statistically insignificant at conventional levels for two of the three achievement measures, although they remain statistically significant for the number of GCSEs achieved at grades A*-

G. This may reflect larger spillover effects between same-year siblings in the case of permanent exclusion, driving sibling fixed effects estimates towards zero, or it may reflect that we are pushing at the limits of the data in this case, with only 0.2% of the (much smaller) same-year siblings sample experiencing permanent exclusion.

The tentative finding that our estimated exclusion effects appear larger for different-year siblings than for same-year siblings, particularly for permanent exclusion, suggests that we cannot rule out that spillovers between siblings bias our preferred sibling fixed effects estimates of exclusion effects to a degree. Further, such spillovers appear to act in the expected direction, i.e., attenuating estimated exclusion effects. Therefore, if anything, the potential existence of such spillovers reinforces our tentative interpretation of the bias-adjusted estimates presented in Table 5 as plausible lower bounds on the effect of exclusion on achievement.

6. Conclusions

Millions of students are excluded from schools around the world every year (Cobb-Clark et al., 2015; Machin and Sandi, 2020) and go on to achieve at below average levels educationally (Noltemeyer et al., 2015). But the extent to which this association reflects a causal effect of exclusion, as opposed to unobserved differences between excluded and non-excluded students, remains unclear. Recently, two new strands have emerged in the exclusion literature seeking to address this question, in a small number of (predominantly US) school settings: the first exploiting plausibly exogenous variation in the use of exclusion across schools; the second estimating individual fixed effects models for exclusion effects in the short run. Each approach has its advantages and disadvantages, but together the studies that make up these strands of the literature mostly point to exclusion having a negative effect on educational achievement for those excluded and at the wider school level. We complement this growing evidence base by presenting sibling fixed effects estimates of the effects of being excluded on educational achievement, which is a novel approach in this literature, using population-wide data for students in England. This approach is closest in design to the individual fixed effects strand of the exclusion literature, but has some relative advantages, including being better suited to estimating longer-run exclusion effects on achievement as measured at the end of compulsory schooling.

We find negative associations between being excluded and our measures of subsequent achievement, at the individual-level, which survive conditioning on a wide range of observable characteristics and three-way (sibling, school and cohort) fixed effects. These estimated effects are large (typically around three times the magnitude of the effect of being male and larger or in line with the effect of being recorded as having special educational needs) and highly statistically significant. We also find evidence suggestive of their robustness to selection on (within-family) unobservables, to potential spillovers between siblings, and of dose response to the number and severity of exclusions. We do not interpret our estimates causally but, given the extent to which our approach mitigates scope for selection bias, consistency with dose response and robustness to these sensitivity analyses, we argue that they are strongly suggestive of an economically significant detrimental effect of exclusion on educational achievement. We find no evidence that this effect is larger for socio-economically disadvantaged students, although the disproportionate use of exclusion for such students (and for boys and those with special education needs) still acts to exacerbate existing inequalities in educational outcomes.

Our novel approach therefore returns estimates that are broadly consistent with those from recent fixed effects and quasi-experimental studies on school exclusion, but that complement these earlier studies in several crucial and distinct ways: the use of data for the population of school pupils in England; the focus on cumulative effects of exclusion throughout secondary schooling on educational achievement at the end of secondary schooling; the focus on the direct effect of exclusion on the excluded at the individual level; and the distinction between different types and frequencies of exclusion. Taken together with the evidence from these earlier studies, the strong suggestion is that exclusion has a detrimental effect on the academic achievement of excluded pupils, on average, and in different school and country contexts. In making decisions about the use of exclusion, school heads and policy makers must trade-off this detrimental effect against the potential for positive indirect effects on the classroom peers of excluded pupils and on teachers and other school staff. This remains difficult, however, because such indirect effects remain largely unquantified, although the emerging quasi-experimental exclusion literature suggests that the school-level net effects of at least some types of exclusion in some school settings can be negative. Further, our own evidence suggests that this trade-off is likely to vary for different types of exclusion and by the number of times an individual pupil is excluded. These are important avenues for further research.

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Table 1. Observables and Achievement by Exclusion

Variable/Sample	Full Sample		One Fixed-Term Exclusion		Two or More Fixed-Term Exclusions		One or More Permanent Exclusions	
	Frequency	Percent	Frequency	Percent	Frequency	Percent	Frequency	Percent
Birth Order								
First born	1,721,466	64.52	115,113	64.65	121,458	63.38	5,001	61.89
Second born	806,901	30.24	52,620	29.55	57,389	29.95	2,433	30.11
Third born	124,548	4.67	9,051	5.08	11,013	5.75	557	6.89
Born fourth or later	15,002	0.56	1,274	0.72	1,768	0.92	89	1.10
Sex								
Female	1,310,449	49.12	61,268	34.41	55,611	29.02	1,875	23.21
Male	1,357,468	50.88	116,790	65.59	136,017	70.98	6,205	76.79
Language								
English	2,374,323	89.00	154,707	86.89	173,319	90.45	7,298	90.32
Not English	293,594	11.00	23,351	13.11	18,309	9.55	782	9.68
Ethnicity								
White	2,207,398	82.74	138,297	77.67	154,506	80.63	6,160	76.24
Black	109,287	4.10	13,342	7.49	13,318	6.95	686	8.49
Asian	205,907	7.72	13,907	7.81	9,877	5.15	425	5.26
Other	145,325	5.45	12,512	7.03	13,927	7.27	809	10.01
Special Educational Needs Status								
Non-SEN	2,075,594	77.80	116,275	65.30	81,088	42.32	2,796	34.60
SEN	592,323	22.20	61,783	34.70	110,540	57.68	5,284	65.40
Free School Meal Eligibility								
Non-FSM	2,292,711	85.94	138,217	77.62	133,878	69.86	5,335	66.03
FSM	375,206	14.06	39,841	22.38	57,750	30.14	2,745	33.97
Academic Achievement								
Achieved 5 or more GCSEs at Grades A*-C including English and Mathematics	1,562,236	58.56	63,244	35.52	31,914	16.65	824	10.20
	Median		Median		Median		Median	
GCSE Points Score at KS4 Percentile Rank	61		40		23		11	
Number of GCSEs at Grades A*-G at KS4 Percentile Rank	52		34		16		8	
KS2 Points Score Percentile Rank	63		49		28		28	
	Frequency	Percent	Frequency	Percent	Frequency	Percent	Frequency	Percent
Observations	2,667,917	100	178,058	6.67	191,628	7.18	8,080	0.30

Table 2. Observables, Exclusion and Achievement by FSM

Variable/Sample	Full Sample		Non-FSM		FSM	
	Frequency	Percent	Frequency	Percent	Frequency	Percent
Birth Order						
First born	1,721,466	64.52	1,475,406	64.35	246,060	65.58
Second born	806,901	30.24	705,438	30.77	101,463	27.04
Third born	124,548	4.67	101,367	4.42	23,181	6.18
Born fourth or later	15,002	0.56	10,500	0.46	4,502	1.20
Sex						
Female	1,310,449	49.12	1,125,130	49.07	185,319	49.39
Male	1,357,468	50.88	1,167,581	50.93	189,887	50.61
Language						
English	2,374,323	89.00	2,077,208	90.60	297,115	79.19
Not English	293,594	11.00	215,503	9.40	78,091	20.81
Ethnicity						
White	2,207,398	82.74	1,944,251	84.80	263,147	70.13
Black	109,287	4.10	76,385	3.33	32,902	8.77
Asian	205,907	7.72	158,827	6.93	47,080	12.55
Other	145,325	5.45	113,248	4.94	32,077	8.55
Special Educational Needs Status						
Non-SEN	2,075,594	77.80	1,848,648	80.63	226,946	60.49
SEN	592,323	22.20	444,063	19.37	148,260	39.51
Fixed-Term Exclusion						
One Fixed-Term Exclusion	178,058	6.67	138,217	6.03	39,841	10.62
Two or More Fixed-Term Exclusions	191,628	7.18	133,878	5.84	57,750	15.39
Permanent Exclusion						
One or More Permanent Exclusions	8,080	0.30	5,335	0.23	2,745	0.73
Academic Achievement						
Achieved 5 or more GCSEs at Grades A*-C including English and Mathematics	1,562,236	58.56	1,429,980	62.37	132,256	35.25
	Median		Median		Median	
GCSE Points Score at KS4 Percentile Rank	61		61		39	
Number of GCSEs at Grades A*-G at KS4 Percentile Rank	52		62		29	
KS2 Points Score Percentile Rank	63		63		47	
	Frequency	Percent	Frequency	Percent	Frequency	Percent
Observations	2,667,917	100	2,292,711	85.94	375,206	14.06

Table 3. Estimated Effects of Exclusion on Achievement

Variable/Model	(1) Without Controls	(2) With Observable Controls and Cohort and School Fixed Effects	(3) With Observable Controls and Cohort, School and Sibling Fixed Effects
		Achieving 5 or more GCSEs at Grades A*-C including English and Mathematics	
Fixed-Term Exclusion			
One fixed-term exclusion	-0.285***	-0.137***	-0.0796***
	(0.003)	(0.001)	(0.003)
Two or more fixed-term exclusions	-0.474***	-0.209***	-0.125***
	(0.004)	(0.002)	(0.003)
Permanent Exclusion			
One or more permanent exclusions	-0.538***	-0.239***	-0.133***
	(0.011)	(0.007)	(0.010)
Observed Covariates	N	Y	Y
Cohort Fixed Effects	N	Y	Y
School Fixed Effects	N	Y	Y
Sibling Fixed Effects	N	N	Y
Observations	2,667,917	2,667,917	895,121
R-squared	0.080	0.462	0.765
		Number of GCSEs at Grades A*-G at KS4 Percentile Rank	
Fixed-Term Exclusion			
One fixed-term exclusion	-17.52***	-8.477***	-4.423***
	(0.181)	(0.119)	(0.149)
Two or more fixed-term exclusions	-32.44***	-15.55***	-9.609***
	(0.309)	(0.204)	(0.197)
Permanent Exclusion			
One or more permanent exclusions	-41.58***	-19.34***	-12.11***
	(1.046)	(0.609)	(0.637)
Observed Covariates	N	Y	Y
Cohort Fixed Effects	N	Y	Y
School Fixed Effects	N	Y	Y
Sibling Fixed Effects	N	N	Y
Observations	2,667,917	2,667,917	895,121
R-squared	0.100	0.498	0.827
		GCSE Points Score at KS4 Percentile Rank	
Fixed-Term Exclusion			
One fixed-term exclusion	-12.53***	-6.436***	-3.619***
	(0.177)	(0.103)	(0.225)
Two or more fixed-term exclusions	-23.19***	-11.52***	-7.259***
	(0.320)	(0.144)	(0.242)
Permanent Exclusion			
One or more permanent exclusions	-33.67***	-14.00***	-8.324***
	(1.587)	(0.482)	(0.782)
Observed Covariates	N	Y	Y
Cohort Fixed Effects	N	Y	Y
School Fixed Effects	N	Y	Y
Sibling Fixed Effects	N	N	Y
Observations	2,667,917	2,667,917	895,121
R-squared	0.053	0.255	0.648

Notes: Standard errors clustered at the school level. ***,**,* denotes statistical significance at the 99.9%, 99% and 95% levels respectively.

Table 4. Estimated Effects of Exclusion on Achievement, by FSM Status

	Non-FSM Sample			FSM Sample		
Variable/Model	(1) Without Controls	(2) With Observable Controls and Cohort and School Fixed Effects	(3) With Observable Controls and Cohort, School and Sibling Fixed Effects	(1) Without Controls	(2) With Observable Controls and Cohort and School Fixed Effects	(3) With Observable Controls and Cohort, School and Sibling Fixed Effects
			Achieving 5 or more GCSEs at Grades A*-C including English and Mathematics			
Fixed-Term Exclusion						
One fixed-term exclusion	-0.283***	-0.143***	-0.083***	-0.178***	-0.108***	-0.063***
	(0.003)	(0.002)	(0.004)	(0.003)	(0.002)	(0.006)
Two or more fixed-term exclusions	-0.481***	-0.229***	-0.141***	-0.306***	-0.156***	-0.095***
	(0.003)	(0.002)	(0.004)	(0.003)	(0.002)	(0.006)
Permanent Exclusion						
One or more permanent exclusions	-0.552***	-0.262***	-0.151***	-0.351***	-0.183***	-0.106***
	(0.011)	(0.007)	(0.015)	(0.010)	(0.008)	(0.016)
Observed Covariates	N	Y	Y	N	Y	Y
Cohort Fixed Effects	N	Y	Y	N	Y	Y
School Fixed Effects	N	Y	Y	N	Y	Y
Sibling Fixed Effects	N	N	Y	N	N	Y
Observations	2,292,709	2,292,709	731,872	375,198	375,198	106,502
R-squared	0.072	0.447	0.755	0.061	0.420	0.743
			Number of GCSEs at Grades A*-G at KS4 Percentile Rank			
Fixed-Term Exclusion						
One fixed-term exclusion	-17.06***	-8.831***	-4.458***	-10.57***	-6.608***	-3.986***
	(0.192)	(0.131)	(0.175)	(0.207)	(0.137)	(0.340)
Two or more fixed-term exclusions	-32.33***	-16.73***	-10.20***	-21.60***	-12.11***	-8.195***
	(0.273)	(0.206)	(0.220)	(0.325)	(0.199)	(0.323)
Permanent Exclusion	-42.09***	-21.28***	-14.15***	-28.70***	-14.80***	-9.151***
One or more permanent exclusions	(0.966)	(0.612)	(0.879)	(1.053)	(0.557)	(0.962)
Observed Covariates	N	Y	Y	N	Y	Y
Cohort Fixed Effects	N	Y	Y	N	Y	Y
School Fixed Effects	N	Y	Y	N	Y	Y
Sibling Fixed Effects	N	N	Y	N	N	Y
Observations	2,292,709	2,292,709	731,872	375,198	375,198	106,502
R-squared	0.087	0.473	0.815	0.087	0.467	0.805
			GCSE Points Score at KS4 Percentile Rank			
Fixed-Term Exclusion						
One fixed-term exclusion	-12.35***	-6.657***	-3.559***	-7.824***	-5.315***	-3.586***
	(0.166)	(0.103)	(0.261)	(0.213)	(0.169)	(0.481)
Two or more fixed-term exclusions	-22.96***	-12.05***	-7.423***	-16.83***	-9.954***	-6.586***
	(0.289)	(0.154)	(0.285)	(0.279)	(0.180)	(0.453)
Permanent Exclusion						
One or more permanent exclusions	-33.19***	-14.89***	-9.124***	-27.17***	-11.95***	-5.932***
	(1.450)	(0.496)	(1.113)	(1.750)	(0.563)	(1.452)
Observed Covariates	N	Y	Y	N	Y	Y
Cohort Fixed Effects	N	Y	Y	N	Y	Y
School Fixed Effects	N	Y	Y	N	Y	Y
Sibling Fixed Effects	N	N	Y	N	N	Y
Observations	2,292,709	2,292,709	731,872	375,198	375,198	106,502
R-squared	0.045	0.240	0.641	0.046	0.244	0.632

Notes: Standard errors clustered at the school level. ***,**,* denotes statistical significance at the 99.9%, 99% and 95% levels respectively.

Table 5. Sensitivity To Unobservables: Bias Adjusted Exclusion Estimates and Relative Degrees of Selection on Unobservables Required for β=0, Sibling Fixed Effects Models

Variable/Outcome	(1) Achieving 5 or more GCSEs at Grades A*-C including English and Mathematics	(2) Number of GCSEs at Grades A*-G (Percentile Rank)	(3) GCSE Points Score (Percentile Rank)	(1) Achieving 5 or more GCSEs at Grades A*-C including English and Mathematics	(2) Number of GCSEs at Grades A*-G (Percentile Rank)	(3) GCSE Points Score (Percentile Rank)
		$\beta^{*} under\ \delta = 1\ and\ Rmax = 1.3\widetilde{R2}$			$\beta^{*} under\ \delta = 1\ and\ Rmax = 2.2\widetilde{R2}$	
Fixed-Term Exclusion						
One fixed-term exclusion	-0.064***	-3.515***	-2.939***	-0.018*	-0.791	-0.897
	(0.004)	(0.209)	(0.311)	(0.008)	(0.426)	(0.613)
Two or more fixed-term exclusions	-0.095***	-7.848***	-5.986***	-0.007	-2.564***	-2.168**
	(0.004)	(0.279)	(0.336)	(0.008)	(0.586)	(0.672)
Permanent Exclusion						
One or more permanent exclusions	-0.111***	-10.672***	-7.285***	-0.043	-6.357***	-4.168
	(0.014)	(0.887)	(1.084)	(0.028)	(1.780)	(2.153)
		$\tilde{\delta}\ for\ \beta = 0\ under\ Rmax = 1.3\widetilde{R2}$			$\tilde{\delta}\ for\ \beta = 0\ under\ Rmax = 2.2\widetilde{R2}$	
Fixed-Term Exclusion						
One fixed-term exclusion	5.162	4.871	5.318	1.290	1.218	1.330
Two or more fixed-term exclusions	4.222	5.456	5.704	1.056	1.364	1.426
Permanent Exclusion						
One or more permanent exclusions	5.925	8.420	8.012	1.481	2.105	2.003

Notes: Standard errors calculated using the delta method. ***,**,* denotes statistical significance at the 99.9%, 99% and 95% levels respectively.

Table 6. Estimated Effects of Exclusion on Achievement, Same and Different Year Siblings

	Same Year Siblings			Different Year Siblings		
Variable/Model	(1) Without Controls	(2) With Observable Controls and Cohort and School Fixed Effects	(3) With Observable Controls and Cohort, School and Sibling Fixed Effects	(1) Without Controls	(2) With Observable Controls and Cohort and School Fixed Effects	(3) With Observable Controls and Cohort, School and Sibling Fixed Effects
			Achieving 5 or more GCSEs at Grades A-C including English and Mathematics at KS4*			
Fixed-Term Exclusion						
One fixed-term exclusion	-0.291***	-0.134***	-0.057***	-0.292***	-0.136***	-0.081***
	(0.008)	(0.006)	(0.012)	(0.003)	(0.002)	(0.003)
Two or more fixed-term exclusions	-0.480***	-0.213***	-0.106***	-0.489***	-0.209***	-0.126***
	(0.006)	(0.006)	(0.013)	(0.004)	(0.002)	(0.003)
Permanent Exclusion						
One or more permanent exclusions	-0.567***	-0.230***	-0.074	-0.560***	-0.239***	-0.135***
	(0.022)	(0.025)	(0.040)	(0.013)	(0.009)	(0.011)
Observed Covariates	N	Y	Y	N	Y	Y
Cohort Fixed Effects	N	Y	Y	N	Y	Y
School Fixed Effects	N	Y	Y	N	Y	Y
Sibling Fixed Effects	N	N	Y	N	N	Y
Observations	70,860	70,860	70,860	839,614	839,614	839,614
R-squared	0.076	0.480	0.811	0.085	0.471	0.762
			Number of GCSEs at Grades A-G at KS4 Percentile Rank*			
Fixed-Term Exclusion						
One fixed-term exclusion	-17.68***	-7.899***	-3.239***	-18.17***	-8.369***	-4.490***
	(0.495)	(0.345)	(0.469)	(0.209)	(0.140)	(0.154)
Two or more fixed-term exclusions	-32.28***	-15.19***	-8.033***	-33.58***	-15.42***	-9.675***
	(0.483)	(0.406)	(0.689)	(0.322)	(0.215)	(0.200)
Permanent Exclusion						
One or more permanent exclusions	-43.25***	-19.13***	-10.34***	-43.20***	-19.62***	-12.13***
	(1.591)	(1.436)	(2.373)	(1.107)	(0.699)	(0.653)
Observed Covariates	N	Y	Y	N	Y	Y
Cohort Fixed Effects	N	Y	Y	N	Y	Y
School Fixed Effects	N	Y	Y	N	Y	Y
Sibling Fixed Effects	N	N	Y	N	N	Y
Observations	70,860	70,860	70,860	839,614	839,614	839,614
R-squared	0.091	0.516	0.891	0.105	0.511	0.822
			GCSE Points Score at KS4 Percentile Rank			
Fixed-Term Exclusion						
One fixed-term exclusion	-13.38***	-6.841***	-2.996***	-12.95***	-6.333***	-3.677***
	(0.422)	(0.419)	(0.810)	(0.212)	(0.158)	(0.235)
Two or more fixed-term exclusions	-23.47***	-11.66***	-5.190***	-24.20***	-11.67***	-7.375***
	(0.553)	(0.526)	(1.070)	(0.327)	(0.158)	(0.251)
Permanent Exclusion						
One or more permanent exclusions	-31.81***	-11.11***	-3.624	-34.41***	-13.83***	-8.480***
	(2.832)	(2.225)	(3.617)	(1.683)	(0.583)	(0.794)
Observed Covariates	N	Y	Y	N	Y	Y
Cohort Fixed Effects	N	Y	Y	N	Y	Y
School Fixed Effects	N	Y	Y	N	Y	Y
Sibling Fixed Effects	N	N	Y	N	N	Y
Observations	70,860	70,860	70,860	839,614	839,614	839,614
R-squared	0.050	0.279	0.695	0.056	0.266	0.646

Notes: Standard errors clustered at the school level. ***, **, * indicates statistical significance at the 99.9%, 99% and 95% levels respectively.