Labour Economics 81 (2023) 102336

Contents lists available at ScienceDirect

Labour Economics

journal homepage: www.elsevier.com/locate/labeco

# Selective schooling and social mobility in England

Franz Buscha[a], Emma Gorman[b,*], Patrick Sturgis[c]

[a] University of Westminster, 35 Marylebone Road, London NW1 5LS, United Kingdom
[b] University of Westminster, 35 Marylebone Road, London NW1 5LS, United Kingdom, and IZA, Bonn, Germany
[c] London School of Economics, Houghton Street, London WC2A 2AE, United Kingdom

ARTICLE INFO

*JEL codes:*
I21
I24
I28
J18
J24

*Keywords:*
Social mobility
Selective schooling
Grammar schools
Intergenerational mobility

ABSTRACT

We assess whether changing from an academically selective to a comprehensive schooling system promotes social mobility, using England as a case study. Over a period of two decades, the share of pupils in academically selective schools in England declined sharply and differentially by area. Using a sample of census records matched to data on selective schooling, we exploit temporal and geographic variation in the proportion of pupils attending selective schools to estimate the effects of schooling system on intergenerational social mobility. Our results provide no support for the contention that the move from selective to comprehensive schooling had a notable effect on social mobility in England. The findings are robust to a battery of sensitivity and robustness checks.

## 1. Introduction

Social mobility addresses the link between family origins and later social and economic life outcomes (Chetty et al., 2014). The common normative interpretation is that higher levels of social mobility reflect a fairer society with more equality of opportunity, while the opposite is the case when life chances are strongly determined by circumstances of birth (Erikson and Goldthorpe, 1992). In political and public discourse, it is generally taken as self-evident that education promotes social mobility, because more educated individuals are more likely to attain better paid and higher status jobs (Heckman et al., 2018; Wolf, 2002). Thus, education is frequently presented as 'the great leveller', enabling children from all backgrounds to fulfil their potential, regardless of the constraints imposed by the material conditions of their economic origins. Academically selective schooling is often proposed as an effective system of education for achieving meritocratic advancement (Department for Education, 2016). This is because, in theory at least, able children from disadvantaged backgrounds can access the higher quality teaching, facilities, and positive peer influences that have historically been found in academically selective schools (Betts, 2011; Boliver and Swift, 2011). Conversely, proponents of comprehensive schools contend that academically selective systems will hinder social mobility due to inequality of access and resources across school types (Benn and Simon, 1971).

In this paper, we provide new evidence on the link between schooling systems and social mobility. We analyse the link between the extent of selective schooling in an area and the level of intergenerational social class mobility, for children born in England between 1956 and 1972. England provides an ideal case study for this purpose because it transitioned from fully selective secondary schooling to a predominantly mixed ability system over a short time frame (Bolton, 2020). Additionally, the timing of the transition from a selective to a mixed ability system occurred differentially by area. Under the selective system, pupils were allocated to an academically-focussed 'grammar' school, if they passed an ability test taken at 10 or 11 years, or to a 'secondary modern' school or technical college if they did not pass. This *'selective schooling system'* was gradually replaced by a mixed ability, or *'comprehensive schooling system'*, in which selection on ability for school admissions is prohibited. This shift from academic selection to a mixed ability system remains politically controversial, with proponents of selective schools contending that their decline resulted in social mobility, particularly of the 'long range' variety, grinding to a halt (Mansfield, 2019). Proponents of comprehensive schools, on the other hand, have made the opposite claim, if not quite as loudly; that comprehensivisation should increase upward mobility by providing a better quality education for less advantaged pupils (Benn and Simon, 1971; Gamoran, 2009; Gamoran and Berends, 1987; Oakes, 1985). As recently as 2017, the Conservative government was elected on a manifesto that pledged to overturn the legal ban on new grammar schools with the explicitly stated objective of increasing social mobility ("Britain the great meritocracy" Prime Minister's speech, 2016). And, although that pledge was

* Corresponding author.
*E-mail address:* e.gorman@westminster.ac.uk (E. Gorman).

https://doi.org/10.1016/j.labeco.2023.102336
Available online 25 January 2023

not implemented, the policy remains popular amongst many MPs and commentators.

We identify the effects of the schooling system on social mobility by exploiting the differential decline in selective system schools across English local areas, using data from the Office for National Statistics Longitudinal Study (LS). The LS is a 1% sample drawn from five decennial censuses in England and Wales spanning the period 1971 to 2011, which is also linked to administrative data on births, deaths and cancer registrations. These data allow us to construct measures of occupational social class mobility for a representative sample of over 90,000 people in England tracked over five decades. We link social mobility outcomes in the LS to an administrative dataset containing information on the proportion of pupils attending selective system schools in each of 145 Local Education Authorities (LEA) for the years 1967 to 1983. This enables us to relate the extent of selective system schooling to rates of intergenerational social mobility within these areas. To identify selective schooling effects, we use a two-way fixed effects specification with social mobility as the outcome; controls for area- and time-specific effects; and treatment variables which are a function of the share of pupils in selective system schooling in an area at a given time. A causal interpretation of the link between the share of pupils in selective system schooling and social mobility relies on the assumption that the variation in selective system schooling across LEAs is as good as random after accounting for LEA characteristics and time trends.

Our results show that individuals living in areas with a higher concentration of selective system schools had lower rates of absolute and relative social mobility over the period of observation, although these effects are small and become statistically indistinguishable from zero after adjusting for area and cohort fixed effects. We consider the plausibility of both the identification strategy and estimation assumptions, and show that our findings are robust to a range of sensitivity checks. Overall, our results indicate that, once local characteristics and secular changes to the macroeconomic environment are taken into account, there is little evidence that selective or comprehensive schooling systems shape aggregate social mobility outcomes. While unadjusted correlations do show small positive effects on social mobility from the shift to a comprehensive system, these effects are not statistically distinguishable from zero after adjustment for cohort trends. Although our central estimate of the effect of school system on social mobility is zero, we cannot rule out small effects due to limitations of sample size. However, even a change from 100% to 0% selectivity would be expected to have, at most, modest effects on social mobility based on our estimates.

Our analysis provides important advances in our understanding of how school selectivity is related to social mobility. Much of the current evidence on the effects of selective schooling uses discontinuity-based study designs to compare social and educational outcomes of pupils 'just passing' the test to enter an academically selective school with those who 'just miss out' (Abdulkadiroglu et al., 2014; Beuermann and Jackson, 2020). Evidence in the UK has found, at most, small positive effects on later test scores of attending a grammar school, and larger positive effects on years of completed education (Clark, 2010; Clark and Del Bono, 2016). These types of study design yield a causal effect of attending an academically selective school for the *marginal applicant*. However, selecting a subset of students for entry into academically selective schools modifies peer groups and school environments for all pupils, not only those attending academic schools. Hence, for policy purposes, the key question is how to design the broader assignment mechanism which matches pupils to schools, whether that be by ability, geography, ability-to-pay, and the consequences of that system for the full population of pupils (Dickson and Macmillan, 2020). Our study addresses this by estimating the net effects of the schooling system as a whole, rather than for those attending grammar school only. Most studies also consider the effect of school system on proximal outcomes such as test scores (Atkinson et al., 2004; Gorard and Siddiqui, 2018; Sullivan et al., 2014), university admission (Mansfield, 2019) and income inequality (Burgess et al., 2020), rather than social mobility itself. We directly estimate the association between the extent of school selectivity in an area and the degree of intergenerational social mobility of its residents.

Disentangling the effects of schooling systems from other factors which influence social mobility is challenging because of non-random selection of pupils into school types (Manning and Pischke, 2006). Existing studies of schooling in England have relied on cross-sectional variation to study the consequences of selective schooling (Atkinson et al., 2004; Boliver and Swift, 2011; Burgess et al., 2020; Galindo-Rueda and Vignoles, 2007). However, caution is warranted in interpreting such research designs which rely on between-pupil variation in the treatment only, as it is difficult to rule out bias from unobserved confounding. Our study builds on these existing studies by exploiting both cross-sectional and over-time variation in the extent of selective schooling, requiring less strong assumptions for a causal interpretation. The remainder of this paper is structured as follows. Section 2 defines key concepts and summarises the relevant literature. In Section 3, the institutional setting is described. Section 4 presents the data and summary statistics of our key variables. Section 5 sets out the empirical strategy. In Section 6, the results are presented and discussed, including a range of robustness checks and a discussion of the limitations of the results. Section 7 concludes with a consideration of the policy implications of our findings.

## 2. Related literature

This paper is about *intergenerational social mobility*, which involves a comparison of socio-economic status between parents and their children in adulthood. *Absolute mobility* is an unconditional comparison of parent and child status. *Absolute upward* and *downward mobility* are, respectively, the proportion of the population with a 'destination' status that is higher or lower than their parents. Relative mobility is a conditional comparison which adjusts for changes in the distribution of socio-economic outcomes across generations, to give the risk of upward and downward mobility for individuals in one origin category compared to another. This conditioning is important because absolute upward social class mobility can increase over time as a result of expansion or retraction of occupational groups, without any change in the *relative* chances of upward mobility for people from different social class backgrounds (Bukodi and Goldthorpe, 2018). Commonly examined dimensions of relative mobility include occupational social class, social status, and income, although some studies have also considered home ownership and education (Bell et al., 2022). Relative social class mobility is usually measured by the ratio of the odds of upward mobility amongst those from a high social class origin to the odds for those from low social class origins (Bukodi and Goldthorpe, 2018).

With regard to absolute social class mobility, early studies found that upward mobility increased substantially during the middle decades of the twentieth century as a result of the substantial expansion in 'white collar' and corollary retraction of 'blue collar' jobs that occurred at this time (Goldthorpe et al. 1987; Erikson and Goldthorpe, 1992). However, for the second half of the twentieth century, the evidence on trends in absolute mobility is less consistent (see Buscha and Sturgis (2018) for a detailed review). However, while studies differ in the timing and magnitude of changes in absolute occupational social class mobility in the twentieth century, they are in broad agreement on the overall pattern. Approximately 70–80% of people in the UK experienced some form of social class mobility, with the remaining 20–30% ending up in the same social class as their parents. Upward mobility was more common than downward during this period, with approximately 35–45% upwardly mobile and the remaining 25–35% downwardly mobile (Buscha and Sturgis, 2018). There is also evidence of slightly increasing downward and declining upward mobility in the later decades of the twentieth and the first decade of the twenty-first centuries (Bukodi et al., 2015). Analyses of *relative* social class mobility have found a steady increase in fluidity over the course of the twentieth century (e.g., Lambert et al., 2007), a static pattern of 'trendless fluctuation' in the post-war generations (Erikson and Goldthorpe, 1992; Goldthorpe and Mills, 2004), while others report a small increase in social fluidity in the post-war decades (Buscha and Sturgis, 2018; Bukodi and Goldthorpe, 2018).

Recent research by Bell et al., (2022); Friedman & Macmillan (2017) and Buscha et al., (2021) also shows variation in absolute and relative mobility at regional and local authority levels, a pattern of lower level spatial heterogeneity that has also been observed in the United States (Chetty et al., 2014), Australia (Deutscher and Mazumder, 2020) and Canada (Corak, 2019). Several correlates of neighbourhood-level upward mobility were identified in Chetty et al., (2014), including: residential segregation, income inequality, school quality, social capital, and family stability. In sum, while there is variation in the exact pattern and timing of differences and trends, a robust body of evidence shows substantial heterogeneity in social mobility over time and place in a range of different contexts.

Turning to how school system is related to social mobility, a first strand of evidence focusses on the outcomes of attending an academically selective school, comparing those at the margins of the acceptance threshold. In the UK, Clark and Del Bono (2016) studied the effects of gaining a place at a grammar school in Scotland, finding positive effects on years of education completed for men and women, and positive effects on income and wages and reduced fertility amongst women in early adulthood. Clark (2010) used the same identification strategy to assess the effects of gaining admission to grammar school in one district in England, finding small positive effects on test scores at age 16, and higher university enrolment. The comparable international literature assessing the causal effects of gaining a place at an academically selective school has found little evidence of effects on short-run test scores (Abdulkadiroglu et al., 2014; Hoekstra et al., 2016), but often larger effects on longer run outcomes such as fertility, income, and mental health (Beuermann and Jackson, 2020).

While these studies provide compelling evidence on returns to attending a selective school for the marginal pupil, they are insensitive to the possibility of 'spill over' effects on pupils further away from the acceptance threshold. The marginal pupil at the admission threshold is also unlikely to be representative of the full cohort of pupils; for a given level of measured ability, pupils of higher socio-economic status are more likely to pass the admission test, again highlighting the importance of studying the system as a whole (Burgess et al., 2018). A further characteristic of much of the existing literature on selective schools and social mobility is that studies do not use social mobility outcomes directly, but focus on intervening variables such as test scores, university admission, and earnings. Positive effects of school type on education or labour market outcomes, while important, do not necessarily imply positive effects on social mobility, which also depend on the patterning of access to different status institutions and subject choices, amongst other factors. One exception is Pekkarinen et al., (2009) who study the consequences of moving from the academically-selective tracking system to comprehensive schooling in Finland, finding a substantial decrease in the intergenerational income elasticity amongst men.

A second branch of literature relevant to our concerns here evaluates changes in the design of schooling systems as a whole. A common finding is that shifting from an ability-tracking system to comprehensive schooling leads to positive educational impacts for pupils from lower socio-economic backgrounds, and either negative (Meghir and Palme, 2005) or null effects (Pekkala Kerr, Pekkarinen, and Uusitalo, 2013) for more advantaged pupils. Similar findings are documented in Matthewes (2021), for Germany, where between-state variation in tracking practices is used to identify the effect of early tracking on the lower-track students. Matthews finds negative effects of tracking on achievement, especially for students from lower socio-economic backgrounds.

Guyon et al., (2012) study the effects of a policy change in Northern Ireland which resulted in more students being admitted to the higher track, finding lower ability students experienced the largest educational benefit, with small or no losses amongst higher ability students. Burgess, Dickson, and Macmillan (2020) find cross-sectional variation in selective schooling across Local Education Authorities in England to be associated with significantly higher income inequality in areas with a predominantly selective schooling system. Around a fifth of the 90–10 earnings gap can be explained by differences in school systems. Boliver and Swift (2011) estimate social mobility outcomes for individuals attending different school types amongst a cohort of young people born in 1958 in Britain. Using a matching strategy to reduce confounding based on observed characteristics, they found small positive effects of attending a grammar compared to a comprehensive school. However, no difference in social mobility outcomes was observed when comparing those who attended any selective system school (either grammar or secondary modern) with those who attended a comprehensive school. This is because the small advantage accruing to individuals attending a grammar school was offset by the negative effects experienced by those attending a secondary modern.

## 3. Institutional context

Prior to the 1944 Education Act (the "Butler" Act), secondary schooling in England was fragmented across private, state and church providers, with access governed variously by aptitude or ability-to-pay. Post-war public support for social welfare motivated government support for common, and free, secondary schooling for all. This was formalised in the 1944 Education Act, through the so-called *tripartite system*. The tripartite system was intended to comprise 'grammar' schools, which selected pupils based on performance in an academic test taken at age 10/11 years, Technical schools, intended for 'scientifically-minded' pupils, and secondary moderns for the remainder. These school types were intended to be equal in esteem: 'the establishment of parity between all types of secondary school is a fundamental requirement' (Spens 1938, p. 376). In practice, however, the reality was a dual system where more academically able pupils were admitted to grammar schools and the remainder attended 'secondary moderns', with only a very small fraction attending Technical colleges (which, in any event, were not held in high esteem).

By the early 1960s, growing public dissatisfaction with secondary modern schools, damage to the self-esteem of those failing the 11+ test, and the logistical difficulties of managing a tripartite system in the face of population growth, led to waning political support for the tripartite system. The Labour government of 1964–1970 implemented Circular 10/65 in 1965 that requested (but did not mandate) that local education authorities "*…[reorganise] secondary education in their areas on comprehensive lines (DES 1965: par. 43)*". Further legislation passed by successive Conservative and Labour governments in the 1970s attempted to either strengthen or weaken the mandate towards comprehensive education, but none moved to the point of enforcing a complete ban on grammar, secondary modern or technical schools -collectively referred to collectively as the *'selective schooling system'*. The result was a steady decline in selective system schools over a period of approximately 15 years, driven not so much by central government but by general societal pressure and proactive Local Education Authorities (LEAs).

Fig. 1 shows the proportion of secondary school pupils taught in grammar schools in England between 1947 and 2016, declining from a high of 38% in 1947 to 5% in 2016 (Bolton, 2020). The total number of grammar schools in England peaked at almost 1300 in 1964 and by 2019 had fallen to 163, with the fastest decline occurring in the 1970s. Grammars that remain today are geographically dispersed across England, with all regions bar the North East containing at least one grammar school. However, the majority of schools are located in a minority of Local Education Authorities with 114 (75%) LEA's having no grammar schools at all.

The motivations for, and nature of comprehensivisation, were diverse, and not necessarily related to social mobility trajectories of

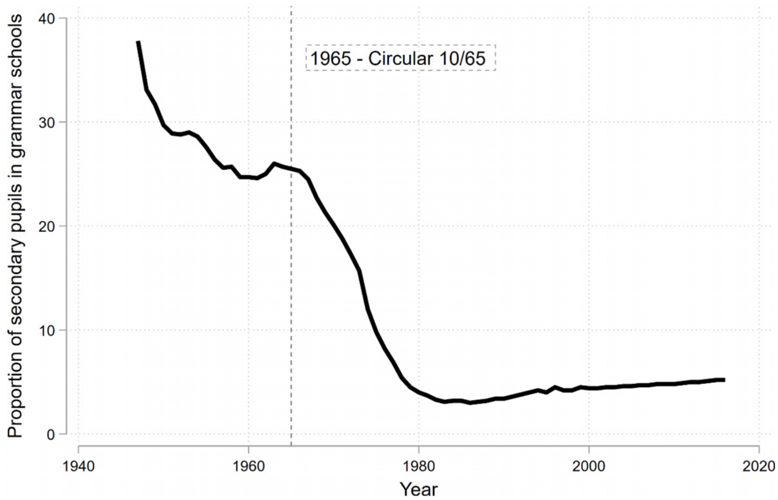


**Fig. 1.** Proportion of secondary school pupils taught in state-funded grammar schools, 1947 - 2016
Notes: Source: Bolton, P. (2020). This Figure plots the proportion of secondary school pupils taught in state-funded grammar schools in England, 1947 – 2016.

LEAs—but rather local resources, population growth, and parental preferences. For example, Mandler (2020) notes that many early-movers to comprehensive schooling were rural LEAs, which faced logistical difficulties in maintaining a bipartite system in the face of rapid population growth. A further motivation was not so much a demand for comprehensive schooling, but rather excess demand for grammar schools over secondary moderns—with new comprehensive schools seen by some as “grammars for all”. In other areas, rather than destroying or repurposing grammar schools, new comprehensives were built in areas with increasing populations such as suburban outposts and new towns. While the 1965 Circular was initiated by the Labour government, and many studies have noted a correlation between Labour control and comprehensivisation, the change was favoured across political lines. Therefore, the move to comprehensive schooling was not a sharp transition along political or economic lines following the 1965 Circular, but rather a gradual and heterogenous process motivated, for the most part, by local concerns.

## 4. Data

We use the Office for National Statistics Longitudinal Study (LS), a 1% sample of decennial censuses of the population of England and Wales spanning 1971 to 2011 (Shelton et al., 2019). The LS sample was selected from the 1971 census by identifying records for all individuals born on four (undisclosed) dates in the year. The study design is a continuous, multi-cohort study, where new samples are drawn in the subsequent 1981, 1991, 2001 and 2011 censuses by adding records for all persons meeting the day-of-birth criteria. These records are also linked to administrative data on births, deaths and cancer registrations. Study members enter via birth or immigration and can be lost to follow-up via nonresponse, linkage failure between censuses, death, or emigration. We limit our analysis to England because the administrative schooling data is not available for other parts of the UK.

The LS is particularly well-suited to our research question for several reason. First, it has a sample size of over 500,000 at each census year, affording precise estimates of the association between selective schooling and intergenerational social mobility. Second, the LS does not have the high rates of non-response and attrition that characterise sample survey and cohort study data. Linkage rates of individuals between censuses are high, ranging from 91% in 1971 to 88% in 2001. Third, the LS includes data on people living in communal establishments, such as older adults and students, which are typically omitted from household surveys. Finally, the LS includes data on the other individuals who were enumerated in the study member’s household for the Census. This means we can identify the contemporaneous occupations of the parents of study members when they were children and do not need to rely on potentially erroneous recall data.[1]

### 4.1. Measures

#### 4.1.1. Social mobility

The occupations of study members, and linked household members, are coded to the National Statistics Socio-economic Classification (NS-SEC) (Rose et al., 2005), which comprises seven analytical groupings: Higher managerial and professional; Lower managerial and professional occupations; Intermediate occupations (clerical, sales, service); Small employers and own account workers; Lower supervisory and technical occupations; Semi-routine occupations; Routine occupations. To measure the social class of study members’ parents, we take the highest NS-SEC of either parent where they are different, a ‘quasi-dominance method’ (Erikson, 1984). We dichotomise the seven category NS-SEC to create a binary variable coded one for those in the managerial and professional categories (NS-SEC groups 1 and 2), and zero otherwise. This is done primarily to preserve sample size but also because our main interest is in mobility into the top social class groups rather than in movements between adjacent classes.

We consider two measures of social mobility, following the previous literature on occupational social class mobility using NS-SEC (Bukodi and Goldthorpe, 2018). First, we construct absolute (upward) mobility as a binary variable coded to one for study members whose origin class was NS-SEC groups 3 to 7 and whose destination class was NS-SEC groups 1 and 2, and zero otherwise. Relative mobility is measured using odds ratios derived from a logistic regression of study member NS-SEC on parental NS-SEC, estimated separately by LEA and school cohort. These coefficients give the odds of being in a high social class in adulthood given high parent class, divided by the odds of being in a high social class in adulthood given low parent social class, for each LEA and time point. Note that larger odds ratios indicate *lower* mobility because they show that the chances of an individual making it to NS-SEC classes 1 and 2 are greater for people whose parents were themselves in those groups compared to people with parents in classes 3 to 7. In robustness checks we consider alternative constructions of the outcome variables, for example based on linear probability models (see Appendix B).

#### 4.1.2. Selective schooling

While we would ideally observe selective/non-selective school status at the individual level, no such data is available. Instead, our ‘treatment’

[1] The standard approach to measuring parent social class in surveys is to ask respondents to report their parents’ occupations at age 14 which is prone to various kinds of recall and social desirability bias.

measure is of exposure to the predominant *system* in the local area at the time the individual was in secondary education, defined as the percentage of pupils attending schools in the selective system (grammar, secondary modern or technical) in each LEA.[2] This measure captures the combined effect of school system across all pupils in an area, rather than the individual effect of schooling on social mobility. In a policy context, this is arguably more informative because the system as a whole is the policy instrument that is intervened on. However, this also implies that there is likely to be measurement error in our treatment variable, to the extent that the measured value does not match the actual exposure of sample members to selective schooling. For example, a sample member may live in an LEA with no selective schools but actually attends a private (fee-paying) school. We therefore assess the sensitivity of our key estimates to measurement error in the selectivity variable. Fig. 2 displays the distribution of the percentage of pupils in selective system schools in each LEA from 1967 to 1983, after which the distribution remains similar (further detail on the LEA geography is reported in Appendix A).

Fig. 3 maps the selectivity data for three exemplar years spanning our period of observation, 1967, 1975, and 1983, demonstrating the substantial decline in selective schooling over the period, as well as its differential distribution by LEA. Figs. 2 and 3 together provide an intuition for our identification strategy; the variation in the rate of decline in selective schools across LEAs. The vast majority of LEAs reduced their rate of selective schooling from near 100% to near 0% but the speed of this transition varied substantially. Appendix A presents a graph showing how selectivity changed within each LEA.

*4.1.3. Sample construction*

We constructed the core sample as follows. First, we selected all study members who were aged 11 during the years 1967 to 1983 inclusive and assigned them a selectivity percentage based on their LEA at census enumeration and the year they entered secondary school (aged 11 years). Age 11 is the most common entry point into secondary education in the UK, mainly determined via the ‘eleven-plus’ test which determines entry into selective education. While the selectivity data is collected from the Annual Schools Census of 13 rather than 11-year-olds, it is an area-level variable which we should not expect to vary whether it is measured on 11- or 13-year-olds.[3]

This sample comprises study members who were born between 1953 and 1972, enumerated in either the 1971 or 1981 census and aged between 8 and 17 at first observation. We assign study members’ parents’ social class at first observation as the ‘origin’ class and their own social class twenty years later (at the 1991 or 2001 census) as the ‘destination’ social class. Age at destination therefore ranges from a minimum of 28 to a maximum of 37 years. We also measure social mobility outcomes 10 years later, yielding a destination age between 38 and 47 years to account for life-cycle effects (Haider and Solon, 2006). Results for this set of destination outcomes can be found in Appendix Fig. B4, the magnitude and significance is consistent with our main specification findings.

This procedure yields some small cell sizes, with approximately 10% of LEA-by-year combinations containing less than 20 observations. For the main analysis, we therefore group the data into two-year bands (henceforth termed ‘*cohort-bands*’), by taking the mean of the selectivity score across the LEAs in two consecutive years, computing social mobility measures which pool study members who were aged 11 in the LEA in either of the two years. This reduces the proportion of LEA-by-year combinations with cell sizes below 20 to 1%. Because there is an odd number of school cohorts (seventeen), we grouped the final three years (1981, 1982, and 1983) into a single cohort-band. These years were chosen because the year-on-year variation in selectivity is lowest at this

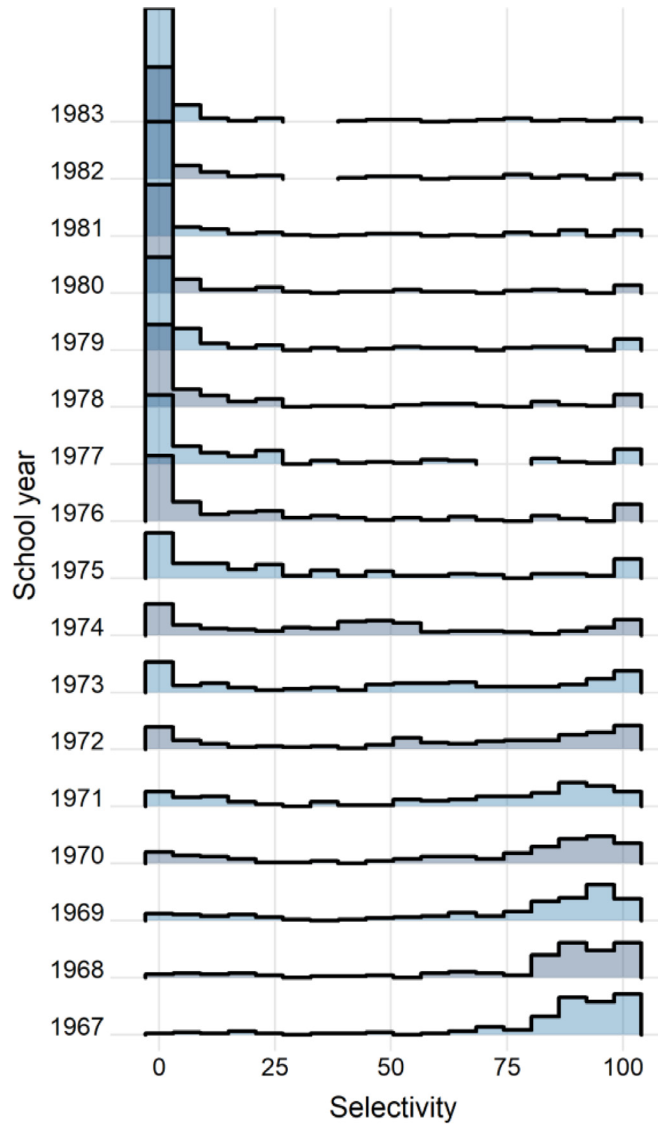


**Fig. 2.** Distributions of the percentage of 13-year-olds in selective system schools (Grammars, secondary moderns or Technical colleges) by school year Notes: Data source: Annual School’s Census.

[2] This dataset was compiled by Damon Clark from the Annual School Census (ASC), and we are grateful to him for making it available to us. The data is defined in pre-Local Government Reorganization (LGR) and starts in 1967 when publication of tables started. The data series stops in 1983 after comprehensive reorganisation remained relatively stable. The data was collected in several stages. From 1967-1973 the data come from published tables in the Statistics of Education: Schools series. This series continued beyond 1974 but LGR meant that LEA boundaries changed and the post-1973 series were not comparable to pre-1974 series. This problem was addressed in two steps: 1) for all schools in the “Form 7 data" in 1975 (i.e., the Annual Schools Census), the Database of Teacher Records was used to assign these schools to a pre-LGR LEA. The assigned pre-LGR LEA is the LEA in which most of the school’s teachers were working in 1974 (whether or not they were working in that particular school). This is done regardless of the number of teachers in the school or the fraction that were working in the assigned pre-LGR LEA. The rationale for this procedure is first that schools with few teachers are likely have fewer students hence mis-assignment is less of a concern. Analysis of the data suggests that where the number of teachers exceeds a reasonable number (e.g., 20), a very large proportion are observed to be working in the assigned pre-LGR LEA. 2) for all new schools that entered the "Form 7 data" between 1976 and 1983 and that survive until the 1990s (at which point we have postcode information for them), the pre-LGR LEA based on the location of the postcode in relation to the old LEA boundaries.

[3] We can also discount effects of study members attending selective schools outside their ‘home’ LEA as open enrolment was not allowed until the 1988 Education Act.

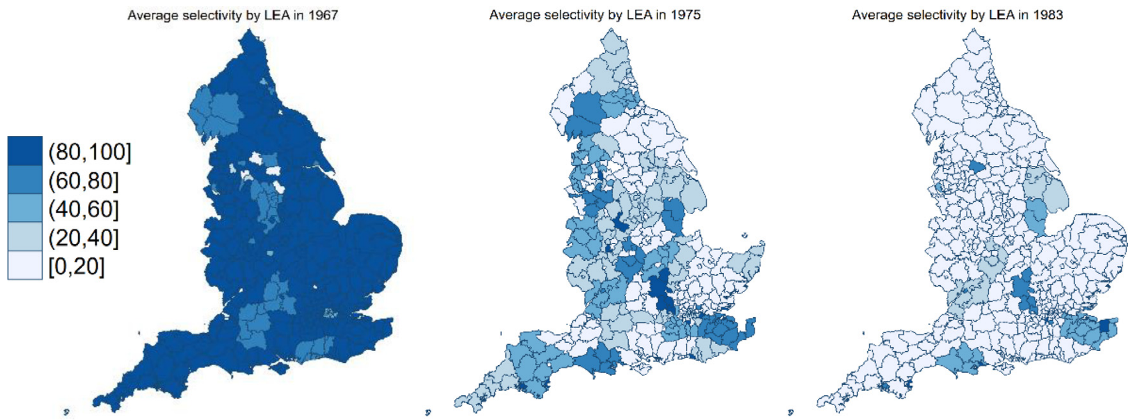


**Fig. 3.** Percentage of 13-year-olds in the selective schooling system by LEA in 1967, 1975 and 1983
Notes: Data source: Annual School's Census. The map borders are Local Authority Districts, with the matched LEAs filled in colour.

**Table 1**
Core analytical sample description.

| | | Selectivity assignment | Social mobility | | | | |
|---|---|---|---|---|---|---|---|
| Year of birth | Cohort band | Year age 11 | Age at origin | | Age at destination | | N |
| 1956 | 1 | 1967 | | 15 | | 35 | 9646 |
| 1957 | | 1968 | | 14 | | 34 | |
| 1958 | 2 | 1969 | | 13 | | 33 | 10,504 |
| 1959 | | 1970 | | 12 | | 32 | |
| 1960 | 3 | 1971 | **1971 Census** | 11 | **1991 Census** | 31 | 10,552 |
| 1961 | | 1972 | | 10 | | 30 | |
| 1962 | 4 | 1973 | | 9 | | 29 | 11,686 |
| 1963 | | 1974 | | 8 | | 28 | |
| 1964 | 5 | 1975 | | 17 | | 37 | 11,331 |
| 1965 | | 1976 | | 16 | | 36 | |
| 1966 | 6 | 1977 | | 15 | | 35 | 11,234 |
| 1967 | | 1978 | | 14 | | 34 | |
| 1968 | 7 | 1979 | **1981 Census** | 13 | **2001 Census** | 33 | 11,038 |
| 1969 | | 1980 | | 12 | | 32 | |
| 1970 | | 1981 | | 11 | | 31 | |
| 1971 | 8 | 1982 | | 10 | | 30 | 14,903 |
| 1972 | | 1983 | | 9 | | 29 | |

*Notes:* Data source: ONS-LS.

point. Therefore, in our core analysis sample, we have data comprising measures of social mobility and selectivity for 141 LEAs, for study members in eight groups defined by the year in which they were aged 11 ("cohort bands"). This data structure is summarised in Table 1. A description of alternative analytical samples used for robustness checks is provided in Appendix Table A1.

#### *4.1.4. Descriptive statistics*

Table 2 shows summary statistics of selected political and economic characteristics by level of LEA selectivity in 1973. A political gradient is apparent; LEAs with low selectivity are more likely to be under Labour control while areas with higher selectivity are more likely to be controlled by the Conservatives. High selectivity areas also tend to have higher socio-economic characteristics, such as a more owner occupiers and professional and managerial occupations. These summary statistics highlight differences in observable characteristics that are associated with school selectivity. Clearly, estimates of the effect of school selectivity on social mobility must account for potential non-random selection into school system type.

Fig. 4 panel (a) shows the proportion of children in selective system schools across the major regions in England, with all regions experiencing a steep decline over the period of observation. However, it is not until the late 1970s that a stable floor of below 20% is reached. Even then, there are significant differences by region, with over 30% of pupils

**Table 2**
Political and socio-economic characteristics by level of selectivity in an area (proportions).

| | *Selectivity distribution in 1973 (%)* | | | |
|---|---|---|---|---|
| | 0 | [0 – 40) | [40 – 90) | [90+) |
| *Political control of Local Authority*[a] | | | | |
| Conservative | 0.13 | 0.15 | 0.26 | 0.30 |
| Independent | 0.07 | 0.09 | 0.13 | 0.06 |
| Liberal | 0.01 | 0.00 | 0.00 | 0.04 |
| Labour | 0.61 | 0.52 | 0.34 | 0.29 |
| No Overall Control | 0.18 | 0.23 | 0.27 | 0.32 |
| *Earnings and employment*[b] | | | | |
| Female FT hourly earnings (£) | 0.66 | 0.67 | 0.67 | 0.66 |
| Male FT hourly earnings (£) | 1.00 | 1.02 | 1.01 | 0.99 |
| Manual occupation | 0.58 | 0.56 | 0.57 | 0.58 |
| Public sector | 0.29 | 0.30 | 0.30 | 0.29 |
| *Socio-economic variables*[c] | | | | |
| NS-SEC Class 1 or 2 | 0.23 | 0.29 | 0.29 | 0.27 |
| Owner occupier | 0.36 | 0.47 | 0.55 | 0.54 |

*Notes: a:* Political control of Local Authority: The local political control variable was constructed from local government elections data compiled by Michael Thrasher and Colin Rallings, downloaded from the Elections Centre website, available from 1973 onward. *b*: Nominal hourly earnings in £GBP, and employment, data are derived from the New Earnings Survey 1974, measured at the regional level (9 regions). *c*: variables derived from the 1971 ONS-LS for study members aged 16 to 64 years inclusive.

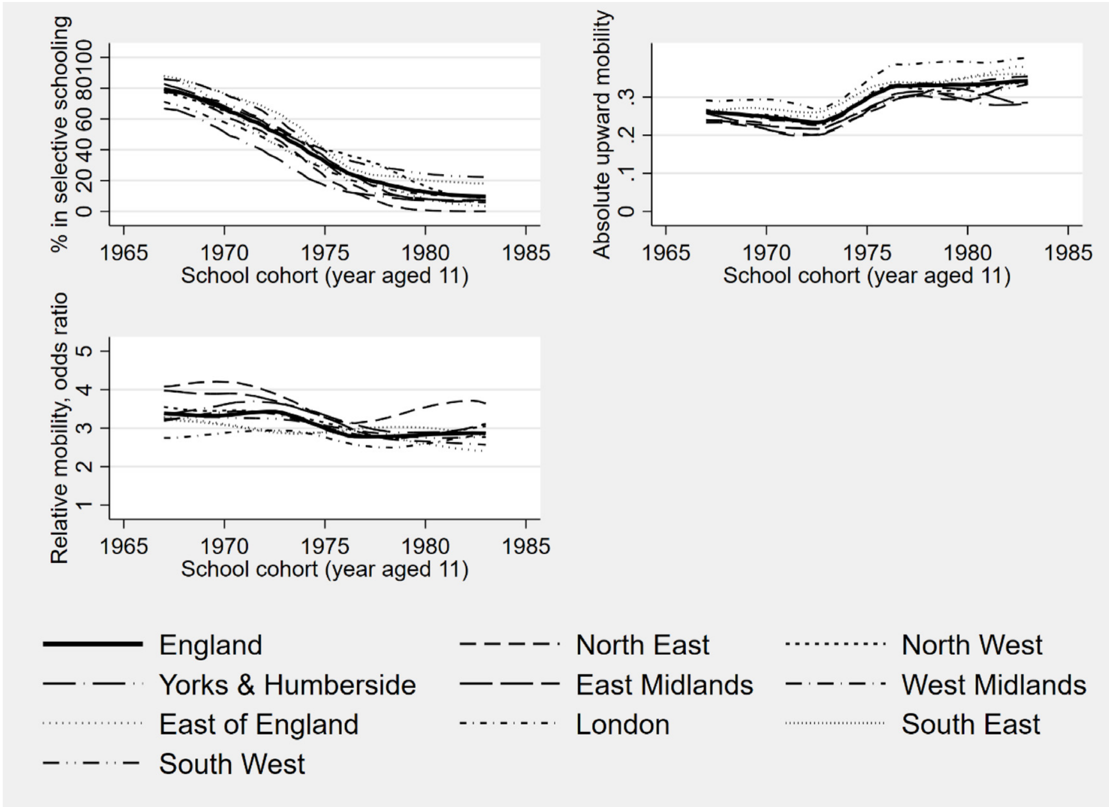


**Fig. 4.** Trends in share of pupils in selective system schools and social mobility for school cohorts 1967–1983
Notes: Data source: Annual School's Census and ONS-LS.

in the South East continuing to attend selective system schools. During the same period, upward absolute mobility (panel b) followed an upward trend while relative mobility also increased somewhat (panel c), although less steeply compared to absolute mobility (recall that higher odds ratios indicate lower relative mobility). These trends are salient for our analysis, because a naïve comparison might erroneously attribute changes in social mobility to the decline in selective schooling.

## 5. Empirical strategy

We begin by estimating the parameters of linear models of the form described in Eq. (1) using ordinary least squares (OLS),

$$Y_{gt(i)} = \alpha + \beta S_{gt} + X_{gti} + \varepsilon_{gti} \tag{1}$$

$Y_{gt(i)}$ denotes the social mobility outcome for LEA $g$ in cohort-band $t$, for individual $i$. We examine two outcomes. First, relative mobility at the area-by-cohort (*gt*) level (our relative mobility measure cannot be estimated at the individual-level, because it is a regression coefficient). Second, absolute mobility which is a binary variable indicating upward mobility at the individual level (*gti*). $\alpha$ denotes the constant term, $S_{gt}$ denotes the proportion of pupils in selective system schools in LEA $g$ in cohort-band $t$, $X_{gti}$ denotes a vector of individual characteristics that includes gender and parental age and parental age squared, $\varepsilon_{gti}$ is an individual-specific error term. $\beta$ is the parameter of substantive interest, denoting the association between the level of selectivity and social mobility. Standard errors are clustered by LEA.

Linear regression with group (LEA) and time (cohort-band) fixed effects (two-way fixed effects, TWFE) is commonly used with panel data with the aim of estimating an average treatment effect on the treated (ATT) by adjusting for both group- and time-specific confounding. This approach assumes homogenous treatment effects, which we relax in robustness checks (Goodman-Bacon, 2021). Following this approach, we extend Eq. (1) to consider sequential specifications adjusting for additional covariates, outlined in Eqs. (2a), 2b and 2c, where $\gamma_g$ and $\delta_t$ denote LEA- and cohort-band-fixed effects, respectively, and $T_t$ is a linear cohort-trend. In Eq. (2a) we add LEA fixed-effects to adjust for potential confounding from time-constant differences between LEAs. The inclusion of LEA fixed effects removes potential unobserved confounders at that level from our analysis. Moreover, we know from Fig. 4 and existing studies of the LS that both absolute and relative social mobility increased for the census cohorts we are considering here (Bell et al., 2022; Buscha and Sturgis, 2018). To reduce the risk of wrongly attributing secular trends in social mobility to correlated changes in selectivity, we add cohort-band fixed effects as specified in Eq. (2b). Finally, we include an interaction between linear cohort-band trends and the LEA fixed effects to allow the cohort trend to vary by LEA, as in Eq. (2c). This approach allows for unobserved time-varying LEA characteristics that may have led to differential mobility trajectories for each LEA.

$$Y_{gt(i)} = \alpha + \beta S_{gt} + X_{gti} + \gamma_g + \varepsilon_{gti} \tag{2a}$$

$$Y_{gt(i)} = \alpha + \beta S_{gt} + X_{gti} + \gamma_g + \delta_t + \varepsilon_{gti} \tag{2b}$$

$$Y_{gt(i)} = \alpha + \beta S_{gt} + X_{gti} + \gamma_g + \delta_t + \theta_g T_t + \varepsilon_{gti} \tag{2c}$$

Specifying the relationship between school selectivity and social mobility as linear is a strong assumption. It may be the case, for example, that the pedagogic benefits of a schooling system do not accrue incrementally but exhibit a 'step-change' at a particular threshold. This may, indeed, be the case for comprehensive schools which are thought to be adversely affected by the co-presence of grammar schools in the local area which 'cream skim' the most able students, dampening positive peer effects. We check for such non-linearities by replacing the continuous selectivity variable with a categorical indicator with five values: zero selectivity as the base category (23% of cells) and then each quartile of the positive selectivity distribution. The parameters of interest now are $\beta_q$ (where $q = 0, 1, 2, 3, 4$) that define the selectivity quantiles.

(a) Relative mobility (b) Absolute upward mobility

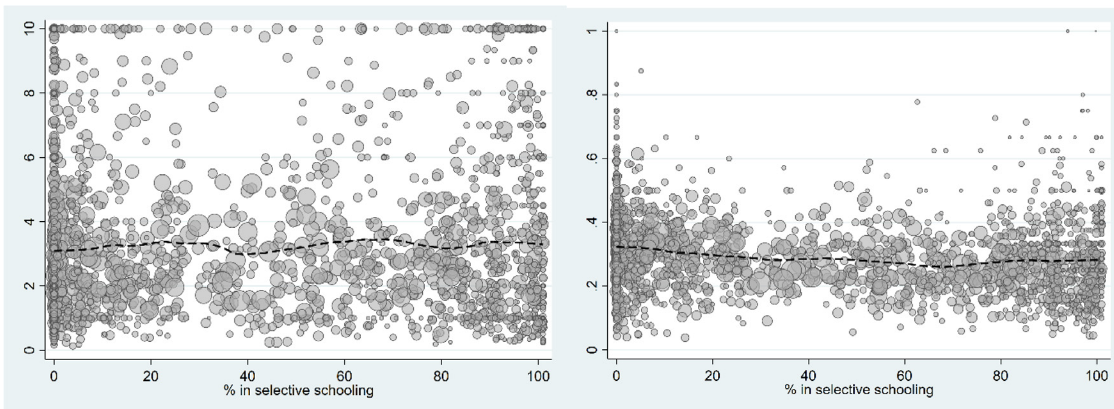


**Fig. 5.** Scatterplots of social mobility and selectivity
Notes: Data source: Annual School's Census and ONS-LS.

In each case a joint parameter test is conducted to determine whether the dummy coefficients are significantly different from zero. Finally, we include a dummy variable in Eq. (3b) that codes the linear selectivity variable into a binary indicator ($D$) where zero selectivity is the base category, and *any* selectivity is set to one.

$$Y_{gt(i)} = \alpha + \sum_{q=0}^{4} \beta_q S_{gtq} + X_{gti} + \gamma_g + \delta_t + \varepsilon_{gti} \tag{3a}$$

$$Y_{gt(i)} = \alpha + \beta D_{gt} + X_{gti} + \gamma_g + \delta_t + \varepsilon_{gti} \tag{3b}$$

A recent literature has highlighted the potential for the TWFE coefficient to depart from the ATT in the presence of heterogenous treatment effects. For example, Chaisemartin and D'Haultfoeuille (2020) show that TWFE retrieves a weighted average of the treatment effects in each group and time period, and that these weights can be negative. In the presence of negative weights, there tends to be downward bias in the TWFE coefficient away from the ATT (Callaway and Sant'Anna 2020; Chaisemartin and D'Haultfoeuille 2020; Goodman-Bacon 2021). To ensure our results are not driven by choice of estimation method, we employ the alternative estimator developed in Chaisemartin and D'Haultfoeuille (2020). This estimates an average treatment effect on the treated which is robust to these concerns, with results reported in Appendix Table B4.

## 6. Results

Fig. 5 shows the bivariate relationship between relative (panel a) and absolute (panel b) mobility and the continuous measure of selectivity at the LEA by cohort-band level. The size of the data points is proportional to the sample size in each LEA by cohort-band cell. There is no evidence in this comparison of any notable association between the level of school selectivity in a local area and the social mobility experienced by its inhabitants. Insofar as any trend is apparent, there is some oscillation around the horizontal for relative mobility whilst the corresponding plot for absolute upward mobility shows a slight downward trend. This suggests that more selective schooling in an area was associated with lower rates of upward mobility. We next consider these relationships using regression.

### 6.1. Regression models

Table 3 reports the results of models fitted using Eqs. (2a), (2b) and (2c). Models (1) and (4) control for LEA fixed effects, with the coefficients for both absolute and relative mobility both statistically significant.[4] Moving from a fully comprehensive to fully selective system would be expected to reduce absolute mobility by -0.0497, from a mean of 0.192. This implies that the probability of moving from a low to a high social class would decrease by approximately 25%. For relative mobility, the odds-ratio would increase by 0.587 from a mean of 3.08 if an LEA school system is switched from fully comprehensive to fully selective. This implies an increase of 19% in the odds-ratio. These results suggest a negative effect on social mobility of moving from a comprehensive to a selective system controlling for LEAs. However, while this specification avoids the problem of LEA-level confounding, it may be driven by spurious correlation with secular time trends in social mobility. To address this, we add cohort fixed effects in models (2) and (5). For both absolute and relative mobility, the coefficients are reduced and are now no longer statistically significant. Models (3) and (6) add an interaction between LEA fixed effects and a linear cohort trend to allow time trends to vary by LEA.[5] The gradual transition of some LEAs to non-selective schooling makes identification of selectivity independent of time effects challenging. By including time trends for each LEA these models allow us to control for time trends in a more granular way, the trade-off being larger standard errors. The coefficient for absolute mobility increases from -0.00104 in Model (2) to -0.00892 in Model (3). This estimate remains statistically non-significant. For relative mobility, the estimates also remain statistically non-significant and of similar magnitudes in Model (3).

These estimates suggest that a transition from no selectivity to full selectivity would reduce absolute upward mobility by 0.00892, from a mean of 0.192. To put this into context, the upward mobility rate for an LEA at the 90th percentile is 0.278, compared to an LEA at the 10th percentile which is 0.112. Similarly, for relative mobility, a transition from no selectivity to full selectivity would reduce the odds-ratio by 0.475, from a mean of 3.08. The level of relative mobility for an LEA at the 90th percentile is 6.40, compared to an LEA at the 10th percentile which is 1.00. Taken together, these results show a non-significant relationship between the share of pupils in selective system schools and the degree of absolute and relative social mobility. The change from significant to non-significant effects is not driven by time-invariant area-level characteristics. Rather, the chief confounding factor appears to be correlated time trends. The increase in social mobility in Britain during this period happens to mirror the decline in selective schooling but is not caused by it.

[4] Note that absolute mobility is measured between 0 and 1 whilst relative mobility is measured > 1.

[5] Tests with quadratic trends to not appear to add any further modelling improvements.

**Table 3**
Linear regression of LEA social mobility on selectivity index.

| | Absolute upward mobility | | | Relative mobility | | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| Coefficient[†] | -0.0497*** | -0.00104 | -0.00892 | 0.587** | -0.432 | -0.475 |
| (s.e) | (0.00505) | (0.00697) | (0.01000) | (0.179) | (0.300) | (0.418) |
| *Controls* | | | | | | |
| Individual | √ | √ | √ | √ | √ | √ |
| LEA FE | √ | √ | √ | √ | √ | √ |
| Cohort FE | | √ | √ | | √ | √ |
| LEA*Cohort | | | √ | | | √ |
| *Outcome mean* | *0.192* | *0.192* | *0.192* | *3.08* | *3.08* | *3.08* |
| N | 90,894 | 90,894 | 90,894 | 90,894 | 90,894 | 90,894 |
| $R^2$ | 0.007 | 0.008 | 0.010 | 0.158 | 0.177 | 0.285 |

*Notes:* Annual School's Census and ONS-LS. Standard errors are clustered by LEA. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. [†] The treatment variable (% selectivity) is rescaled to a proportion so the coefficient can be interpreted as the change in social mobility expected from a change from 100% comprehensive to 100% selective schools.

**Table 4**
Linear regression of LEA social mobility on categorical selectivity variable.

| | Absolute upward mobility | | | Relative mobility | | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| *Panel A: Categorical treatment variable* | | | | | | |
| *Ref (zero)* | – | – | – | – | – | – |
| Q 1 | -0.0207*** | -0.00457 | -0.00686 | 0.639*** | 0.453* | 0.562 |
| (s.e) | (0.00447) | (0.00459) | (0.00736) | (0.187) | (0.188) | (0.290) |
| Q 2 | -0.0360*** | -0.00722 | -0.00579 | 0.648*** | 0.334 | 0.424 |
| (s.e) | (0.00542) | (0.00624) | (0.00826) | (0.185) | (0.223) | (0.319) |
| Q 3 | -0.0535*** | -0.00490 | -0.00611 | 0.749*** | 0.0703 | 0.193 |
| (s.e) | (0.00516) | (0.00673) | (0.00979) | (0.177) | (0.267) | (0.404) |
| Q 4 | -0.0516*** | -0.00307 | -0.00851 | 0.863*** | 0.0535 | 0.127 |
| (s.e) | (0.00521) | (0.00833) | (0.0123) | (0.198) | (0.315) | (0.476) |
| *F-test p*-value | 0.000 | 0.761 | 0.917 | 0.000 | 0.059 | 0.187 |
| (Null: Quartiles are jointly equal to zero) | | | | | | |
| *Panel B: Binary treatment variable* | | | | | | |
| Binary | -0.0390*** | -0.00519 | -0.00662 | 0.727*** | 0.428* | 0.558 |
| (s.e.) | (0.00385) | (0.00468) | (0.00721) | (0.154) | (0.179) | (0.284) |
| *Controls* | | | | | | |
| Individual | √ | √ | √ | √ | √ | √ |
| LEA FE | √ | √ | √ | √ | √ | √ |
| Cohort FE | | √ | √ | | √ | √ |
| LEA*Cohort | | | √ | | | √ |
| *Outcome mean* | *0.192* | *0.192* | *0.192* | *3.08* | *3.08* | *3.08* |
| N | 90,894 | 90,894 | 90,894 | 90,894 | 90,894 | 90,894 |

*Notes: Data sources:* Annual School's Census and ONS-LS. Standard errors are clustered by LEA. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

### 6.2. Functional form

A linear specification for the selectivity variable is restrictive, and, as noted earlier, there are theoretical reasons to consider that a non-linear specification may better capture the relationship. In Table 4 we therefore replace the selectivity variable with indicators for each quartile of the positive selectivity distribution. In Panel B, we use the binary variable which takes the value one for any score above zero for selectivity and zero otherwise.[6] In Table 4 the negative coefficients for selectivity are larger at the higher end of the selectivity distribution. The *p*-values from an *F*-test indicate that the four categories are jointly statistically different from zero for Models (1) and (4). However, both these models include individual controls and LEA fixed effects only. Our favoured specifications are models (2) and (3) for absolute mobility and models (5) and (6) for relative mobility, as these additionally control for cohort time effects. In these models the variables are not statistically different from zero, either individually or jointly.

The results for the binary indicator in Panel B (which tests zero selectivity vs *any* selectivity) show the same pattern; adjusting for LEA fixed effects, having *any* selective schooling compared with none in an LEA is associated with lower absolute upward and relative mobility. However, for absolute mobility the effect is not statistically significant once cohort fixed effects are added, while for relative mobility the effect becomes statistically non-significant once LEA-cohort specific trends are included. Irrespective of statistical significance, the magnitude of the point estimate for absolute mobility is very small, though given the larger standard error that comes with the addition of cohort fixed effects, we cannot rule out modest effect sizes at the 95% level of confidence.

### 6.3. Sensitivity analyses

Our results show small, statistically non-significant, correlations between school selectivity and social mobility when cohort trends are controlled for. In this section we explore the sensitivity of these results to a

[6] We have also explored parametric modelling of non-linearities in selectivity using linear, quadratic- and cubic-polynomial specifications of the selectivity percentage (see Appendix Figure B1).

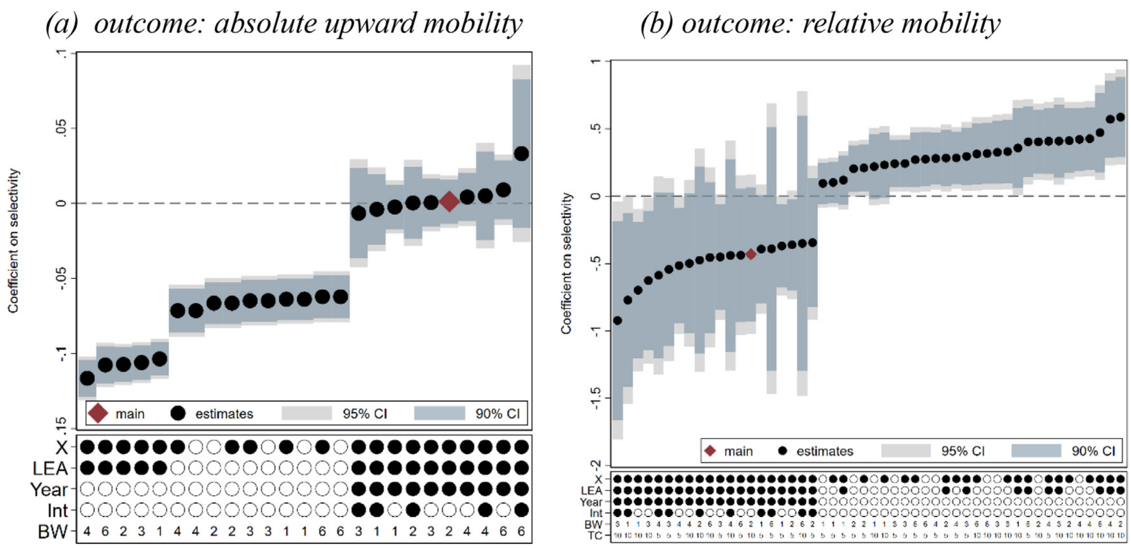


**Fig. 6.** Sensitivity analyses to alternative modelling and data choices
Notes: Data source: Annual School's Census and ONS-LS. Each data point on these charts is the value of the coefficient from a linear regression, with varying specifications. Dependent variable in each sub-figure: (a) Absolute mobility (proportion experiencing upward mobility); (b) Relative mobility (odds ratio). The shaded bands are confidence intervals, with the darker shaded areas the 90% confidence interval and the lighter shaded area is the 95% confidence interval. The panel below the chart indicates the nature of the data and model specification which generated each coefficient. $X$=individual-level controls included; LEA = LEA fixed effects included; Year = cohort-band fixed effects included; Int = interaction between linear cohort trends and LEA dummies included; BW = level of aggregation of cohort groups (in years); TC, indicates whether top-coding of the odds ratio outcome at 10 has been imposed.

range of data and model specification choices. We report the outcome of these investigations in specification curves in Fig. 6, which plots the estimates for a range of different control variables, bandwidth and top-coding choices. In each plot we highlight our favoured specification, which uses grouped data based on two-year cohort bands, two-way fixed effects controls and top coding at 10 for the odds ratio (relative mobility) outcome.[7] Results show that coding and bandwidth choices matter less than control choices. The estimates which adjust only for LEA fixed effects and individual characteristics are more precisely estimated. They suggest that more school selectivity is associated with lower social mobility (both absolute and relative). However, when cohort controls are added (denoted Year), estimates for absolute mobility move towards zero, while for relative mobility they move towards negative, but both are statistically non-significant.

We have also examined several alternative ways of constructing the social mobility outcome. The first alternative outcome is the correlation coefficient from a linear regression of the child's binary NS-SEC on parent binary NS-SEC; and the second uses the seven-class NS-SEC. These results confirm the pattern using odds ratios; there are several statistically significant coefficients in the models which adjust for LEA fixed effects only, but our favoured specifications which include cohort controls show no statistical significance. We also estimated social mobility correlations using standardised occupational rankings in the form of the Cambridge Social Interaction and Stratification (CAMSIS) scores (see Sturgis and Buscha, 2015). Results are presented in Appendix Table B3. These results also confirm the pattern of null effects on social mobility from school selectivity using social class.

### 6.4. Two-way fixed effects estimation robustness checks

Using the estimator for the Average Treatment Effect on the Treated developed in Chaisemartin and D'Haultfoeuille (2020) ($DID_M$), we find a treatment effect of -0.00407 for absolute mobility and -0.6204 for relative mobility, both statistically non-significant at the 95% level of confidence based on block bootstrapped standard errors with 100 replications. These results are reassuring as they are consistent with our main findings, although the magnitude of the $DID_M$ effect sizes are slightly larger than the TWFE coefficients. The full results of this robustness check are reported in Appendix Table B3.

### 6.5. Measurement error

One potential source of bias in our estimates is measurement error in the treatment variable, which, in the classical errors-in-variables framework (Griliches, 1986), biases the estimates toward zero. Measurement error might arise due to our assigning to pupils a single value of their local school system selectivity, based on the year they entered school. This decision is motivated by the idea that the predominant assignment mechanism when the pupil is selected for secondary school is the most relevant concept to address the research question of this paper. However, if a pupil enters a school which has just switched to be non-selective (comprehensive) from selective (say, a grammar school), the higher year groups would have been selected based on academic criteria. This school may well be distinct from a school which has been comprehensive over a long period, such that the pupil mix – as well as teacher quality, resourcing and so on - would have adjusted to the comprehensive system. In this situation, there is potential for error in the treatment variable, as the selectivity proportion would reflect any contamination effects from the nature of the school in the immediately preceding years. This would not apply in all cases, because in some instances new schools were built that had no prior history of selectivity, or grammars and secondary moderns combined, rather than a grammar school switching to a comprehensive in-take. Nonetheless, the potential for error in the selectivity treatment remains. More generally, the gradual change from a selective to comprehensive system makes it difficult to disentangle the year-on-year treatment effect from time effects or a lagged effect.

We therefore implement robustness checks that assess the extent to which our estimates are affected by the choice of time point at which to allocate the selectivity measure. First, we construct a treatment variable which is the average of the selectivity variable over the current period and previous four periods. This aims to capture any residual effects of the previous system which may still exert an effect post-transition. Sec-

[7] We also report a complete Table of results based on varying data aggregation levels in Appendix Table A1.

**Table 5**
Standard, long difference and. 5-year average specifications.

| | Absolute upward mobility | | | Relative mobility | | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| *Standard* | | | | | | |
| Coefficient† | -0.0497*** | -0.00104 | -0.00892 | 0.587** | -0.432 | -0.475 |
| (s.e) | (0.00505) | (0.00697) | (0.010) | (0.179) | (0.300) | (0.418) |
| *Long difference* | | | | | | |
| Coefficient† | -0.0356*** | 0.0195 | n/a | 0.525 | -0.535 | n/a |
| (s.e) | (0.00933) | (0.0202) | n/a | (0.279) | (0.807) | n/a |
| *5 yr average* | | | | | | |
| Coefficient† | -0.0741*** | -0.00494 | -0.00776 | 0.818*** | -0.0954 | -0.0447 |
| (s.e) | (0.00518) | (0.0103) | (0.0258) | (0.215) | (0.447) | (0.930) |
| *Standard* | | | | | | |
| *Outcome mean* | *0.192* | *0.192* | *0.192* | *3.08* | *3.08* | *3.08* |
| N | 90,894 | 90,894 | 90,894 | 90,894 | 90,894 | 90,894 |
| $R^2$ | 0.007 | 0.008 | 0.01 | 0.158 | 0.177 | 0.285 |
| *Long difference* | | | | | | |
| Outcome mean | *0.203* | *0.203* | *n/a* | *3.08* | *3.08* | *n/a* |
| N | *24,549* | *24,549* | *n/a* | *24,549* | *24,549* | *n/a* |
| $R^2$ | *0.009* | *0.01* | *n/a* | *0.511* | *0.519* | *n/a* |
| *5 yr average* | | | | | | |
| Outcome mean | 0.195 | 0.195 | 0.195 | 3.01 | 3.01 | 3.01 |
| N | *70,744* | *70,744* | *70,744* | *70,744* | *70,744* | *70,744* |
| $R^2$ | *0.008* | *0.009* | *0.011* | *0.190* | *0.205* | *0.366* |
| *Controls* | | | | | | |
| Individual | √ | √ | √ | √ | √ | √ |
| LEA FE | √ | √ | √ | √ | √ | √ |
| Cohort FE | | √ | √ | | √ | √ |
| LEA*Cohort | | | √ | | | √ |

*Notes: Data sources:* Annual School's Census and ONS-LS. Standard errors are clustered by LEA. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. †The treatment variable (selectivity) is a proportional variable that ranges from 0 to 1.

ond, we estimate a 'long difference' specification, which uses only the first and last cohorts (this is similar to Donohue III and Levitt, 2001) in order to overcome the issue of the gradually changing treatment variable. Table 5 presents these results.

The results show that the long difference estimates are similar in size, sign, and significance to our main estimates. Standard errors are higher due to the lower sample size but the pattern of point estimates is quite similar. Specifications (3) and (6), which contain individual LEA cohort trends, cannot be estimated as the models have only two time points. Estimates using average selectivity over a 5-year band are also similar to the main estimates; our preferred specifications which include LEA and cohort effects remain statistically non-significant. Effect sizes for the 5-year band remain small at less than 5% changes from the mean, in absolute and relative mobility, when moving from a fully comprehensive system to fully selective.

Another way to assess the potential impact of measurement error in the treatment variable on our key estimate is to consider its approximate magnitude under an extreme assumption about the ratio of true score variance to random error variance, for example, that the school selectivity variable contains equal parts true score and random error. Under classical errors in variables assumptions of additive zero-mean random error, if we were to completely correct for the measurement component, this would result, ceteris paribus, in a doubling of the point estimate. Under our favoured specification for absolute mobility and relative mobility, this would produce coefficients of -0.01784 and -0.95, respectively, which are both within the current 95% confidence intervals. Overall, these robustness checks suggest our main estimates are unlikely to be driven by issues of measurement, variable construction, and analysis specification choices.

## 7. Conclusion

There has for some time now been a settled view amongst politicians and media commentators alike that the UK is characterised by low and declining levels of social mobility (Goldthorpe, 2013). While this is not entirely consistent with the empirical record (Bukodi and Goldthorpe, 2018), it is undeniably the case that where you end up in life is strongly conditioned by the economic circumstances into which you were born (Buscha and Sturgis, 2018). While the diagnosis is uncontentious, the cure is less clear; how can life chances be equalised through social and economic reform? Policymakers commonly turn to education as a means of reducing inequalities in life chances, an intuitively appealing policy response given the strong association between educational attainment and positive human capital and labour market outcomes (Carneiro et al., 2011; Dolton and Sandi, 2017). A small but prominent part of the debate over how education policy can promote social mobility relates to schooling systems, with proponents of academically selective education arguing that selection on the basis of academic achievement enables able and motivated young people to achieve their full potential, irrespective of the economic circumstances of their early lives. Conversely, advocates of comprehensive educational systems argue that non-selective schools will produce better social mobility outcomes while also reducing the psychological scarring resulting from categorisation as an academic failure at a young age.

In this study we have used census data linked to administrative records on school selectivity within Local Education Authorities in England to examine the question of whether or not the choice of schooling systems affects social mobility. We assessed whether the extent of selective schooling in an area is causally related to the social mobility outcomes of the children who were resident there during a period of transition from entirely selective to mostly comprehensive schooling. Our results provide little or no support for contentions that either selective or comprehensive school systems have a beneficial effect on social mobility. Adjusting for both area characteristics and time trends we find small and non-significant correlations between exposure to selective schooling and social mobility, of both the absolute and relative kind. Our findings add to the existing evidence base in two important ways. First, we consider the effects of selective schools for all children in a cohort rather than those attending grammar schools only and, second, we exploit both cross-sectional and longitudinal variation in the schooling system to offer more robust causal evidence than has previously been available.

Overall, our results indicate that once local area characteristics and secular changes in the economy are taken into account, there is little evidence to support the contention that selective or comprehensive schooling improves aggregate social mobility outcomes. While our central estimate of the effect of school system on social mobility is zero, sample size limitations mean we cannot rule out small effects in either direction. However, we can reject the large effects which are often mooted on both sides of this debate and note that our null findings are consistent with existing studies of the effect of selective schools in England (Boliver and Swift, 2011; Burgess et al., 2020). Our findings are robust to a comprehensive set of alternative measurement strategies and model specifications as well as to extreme assumptions about measurement error in our treatment variable.

Two unobserved aspects of school choice that we are unable to account for empirically, but which might threaten the validity of our findings are sample members studying in private (fee-paying) schools or attending schools outside their 'home' LEA. However, during our time period of interest, the proportion of pupils attending private schools remained effectively constant, at between 6 and 7% of pupils (Green et al., 2012). So, there is no evidence of offset towards private schooling to compensate for the loss of selective schooling. And while it is now common for pupils to attend selective schools in neighbouring LEAs, this is unlikely to represent a significant issue for our estimates because our data spans the school years 1967 to 1983, and the right to apply to schools outside the LEA of residence was not introduced until 1988. A further consideration is the speed of adjustment of changes in the school system. Many of the mechanisms through which the effects of schooling system could exert themselves – such as teacher sorting, peer effects, school management and resources – are likely to take time to manifest. Whilst our data follows the full transition from selective to non-selective schooling, we are unable to look at children of the late 1980s and 1990s because these cohorts have not reached labour market maturity by the time of the 2011 census. It may be that long-run effects from the transition to a comprehensive system are only now becoming apparent.

Much of the appeal of academically selective schools derives from the positive and often florid individual accounts of 'long range mobility' from humble working-class origins to professional and managerial destinations. Indeed, high profile proponents of grammar schools often point to their own experiences of upward mobility facilitated, as they see it, by gaining a place at the local grammar school. Our findings do not contradict these anecdotal experiences. Indeed, long range mobility of this kind was no doubt facilitated for some individuals from disadvantaged backgrounds by attending a grammar school (Boliver and Swift, 2011; Clark and Del Bono, 2016). However, we hear much less often from the corresponding group of people who did less well in a secondary modern than they would otherwise have done in a comprehensive school. And to properly assess the effect of a schooling system on social mobility, it is necessary to consider the outcomes for all affected individuals, not the beneficiaries only. Of course, a corollary conclusion is that the introduction of comprehensives did not increase social mobility either, albeit this has never been as key to the benefits claimed for them by their advocates as is the case for selective schools. It is also true that the full benefits of a comprehensive system cannot be realised while a significant minority of academically high achieving pupils are 'creamed off' into the selective system. In any event, we find no evidence that either type of schooling system had a notable effect on intergenerational social class mobility in the context we have focused on here.

This conclusion casts doubt on the idea that education policy can be a 'silver bullet' solution to the larger problems of widening economic inequality and low social mobility (Bukodi and Goldthorpe, 2018; Sturgis and Buscha, 2015). Grammar schools are known to have a range of negative consequences for individuals and society, including social segregation of schools and local areas (Gorard and Siddiqui, 2018), and psychological and emotional scarring of pupils who fail the entrance exam (Gorard and See, 2013). The burden of proof for the mooted benefits of selective schools must therefore be high, and, in the case of social mobility, the evidential threshold is not met: selective schooling has not improved social mobility in England.

## Data availability

The authors do not have permission to share data.

## Acknowledgements

We are grateful for funding from the ESRC via ES/R00627X/1. The authors do not have any conflicts of interest to disclose. The permission of the Office for National Statistics to use the Longitudinal Study is gratefully acknowledged, as is the assistance provided by staff of the Centre for Longitudinal Study Information & User Support (CeLSIUS). CeLSIUS is supported by the ESRC under project ES/V003488/1. The authors alone are responsible for the interpretation of the data. This work contains statistical data from ONS which is Crown Copyright. The use of the ONS statistical data in this work does not imply the endorsement of the ONS in relation to the interpretation or analysis of the statistical data. This work uses research datasets which may not exactly reproduce National Statistics aggregates. We are also grateful to Damon Clark (University of California) for providing us with school selectivity data. *Data statement:* The datasets used in this paper cannot be shared publicly due to confidentiality requirements. Researchers can apply to access the Office for National Statistics (ONS) Longitudinal Study via the ONS Research Accreditation Service (https://www.ons.gov.uk/aboutus/whatwedo/statistics/requesting statistics/approvedresearcherscheme#research-project-accreditation), and the data can then be accessed via the ONS Secure Research Server (ONS SRS).

## Supplementary materials

Supplementary material associated with this article can be found, in the online version, at doi:10.1016/j.labeco.2023.102336.

## References

Abdulkadiroglu, A., Angrist, J.D., Pathak, P.A., 2014. The elite illusion: achievement effects at Boston and New York Exam Schools. Econometrica 82 (1), 137–196. doi:10.3982/ECTA10266.

Atkinson, A., Gregg, P., McConnell, B., 2004. The Result of 11 Plus selection; An Investigation Into Equity and Efficiency of Outcome for Pupils. Selective LEAs.

Bell, B., Blundell, J., Machin, S., 2022. Where is the Land of Hope and Glory? The geography of intergenerational mobility in England and Wales. Scand. J. Econ..

Benn, C., Simon, B., 1971. Half-way there: report on the British comprehensive school reform. Br. J. Educ. Stud. 19 (1).

Betts, J.R., 2011. The economics of tracking in education. In: Handbook of the Economics of Education, 3. Elsevier, pp. 341–381.

Beuermann, D.W., Jackson, C.K., 2020. The short and long-run effects of attending the schools that parents prefer. J. Hum. Resour. 1019–10535R1. doi:10.3368/jhr.57.3.1019-10535r1.

Boliver, V., Swift, A., 2011. Do comprehensive schools reduce social mobility? Br. J. Sociol. 62 (1), 89–110. doi:10.1111/j.1468-4446.2010.01346.x.

Bolton, P., 2020. Grammar school statistics. In: House of Commons Briefing Papers. Briefing Paper, p. 1398. doi:10.1093/nq/s1-IV.105.345-b.

"Britain the great meritocracy" Prime Minister's speech. (2016). "Britain, the great meritocracy" Prime Minister's speech. Retrieved June 16, 2021, from https://www.gov.uk/government/speeches/britain-the-great-meritocracy-prime-ministers-speech

Bukodi, E., Goldthorpe, J.H., 2018. Social Mobility and Education in Britain: Research, Politics and Policy. Cambridge University Press, Cambridge.

Bukodi, E., Goldthorpe, J.H., Waller, L., Kuha, J., 2015. The mobility problem in Britain: new findings from the analysis of birth cohort data. Br. J. Sociol. 66 (1), 93–117. doi:10.1111/1468-4446.12096.

Burgess, S., Crawford, C., Macmillan, L., 2018. Access to grammar schools by socio-economic status. Environ. Plan. A. 50 (7), 1381–1385. doi:10.1177/0308518x18787820.

Burgess, S., Dickson, M., Macmillan, L., 2020. Do selective schooling systems increase inequality? Oxf. Econ. Pap. 72 (1), 1–24. doi:10.1093/oep/gpz028.

Burgess, S., Dickson, M., Macmillan, L., 2020. Do selective schooling systems increase inequality? Oxf. Econ. Pap. 72 (1), 1–24. doi:10.1093/oep/gpz028.

Buscha, F., Gorman, E., Sturgis, P., 2021. Spatial and social mobility in England and Wales: a sub-national analysis of differences and trends over time. Br. J. Sociol. (5) 72.
Buscha, F., Sturgis, P., 2018. Declining social mobility? Evidence from five linked censuses in England and Wales 1971–2011. Br. J. Sociol. 69 (1), 154–182. doi:10.1111/1468-4446.12275.
Callaway, B., Sant'Anna, P.H.C., 2020. Difference-in-differences with multiple time periods. J. Econom..
Carneiro, P., Heckman, J.J., Vytlacil, E., 2011. Estimating Marginal Returns to Education. Am. Econ. Rev. 101 (6), 2754–2781. doi:10.1257/aer.101.6.2754.
Chaisemartin, C.de, D'Haultfoeuille, X, 2020. Two-way fixed effects estimators with heterogeneous treatment effects. Am. Econ. Rev. 110 (9), 2964–2996.
Chetty, R., Hendren, N., Kline, P., Saez, E., 2014. Where is the land of opportunity? The geography of intergenerational mobility in the United States. Q. J. Econ. 129 (4), 1553–1623. doi:10.1093/qje/qju022.
Clark, D., 2010. Selective schools and academic achievement. BE J. Econ. Analy. Policy 10 (1).
Clark, D., Del Bono, E, 2016. The long-run effects of attending an elite school: evidence from the United Kingdom. Am. Econ. J.: Appl. Econ. 8 (1), 150–176.
Corak, M., 2019. The Canadian geography of intergenerational income mobility. Econ. J. doi:10.1093/ej/uez019.
Department for Education, 2016. Schools That Work For everyone: Government consultation. DfE.
Deutscher, N., Mazumder, B., 2020. Intergenerational mobility across Australia and the stability of regional estimates. Labour Econ., 101861.
Department of Education and Skills, "*The Organisation of Secondary Education*" (Circular 10/65), July 12, 1965.
Dickson, M., & Macmillan, L. (2020). *Social Mobility and Higher Education: are grammar schools the answer?*
Dolton, P., Sandi, M., 2017. Returning to returns: revisiting the British education evidence. Labour Econ. 48, 87–104. doi:10.1016/j.labeco.2017.07.001.
Donohue III, J.J., Levitt, S.D, 2001. The impact of legalized abortion on crime. Q. J. Econ. 116 (2), 379–420.
Erikson, R., 1984. Social class of men, women and families. Sociology 18 (4), 500–514.
Erikson, R., Goldthorpe, J.H., 1992. The Constant flux: a Study of Class Mobility in Industrial Societies. Clarendon Press, Oxford.
Friedman, S., Macmillan, L., 2017. Is London really the engine-room? Migration, opportunity hoarding and regional social mobility in the UK. Natl. Inst. Econ. Rev. 240 (1), R58–R72. doi:10.1177/002795011724000114.
Galindo-Rueda, F., Vignoles, A., 2007. The heterogeneous effect of selection in UK secondary schools. School. Equal Opport. Probl. 103–128.
Gamoran, A., 2009. Tracking and inequality: New directions For Research and Practice. Routledge.
Gamoran, A., Berends, M., 1987. The effects of stratification in secondary schools: synthesis of survey and ethnographic research. Rev. Educ. Res. 57 (4), 415–435.
Goldthorpe, J.H., 2013. Understanding–and misunderstanding–social mobility in Britain: the entry of the economists, the confusion of politicians and the limits of educational policy. J. Soc. Policy 42 (3), 431–450.
Goldthorpe, J.H., Llewellyn, C., Payne, C., 1987. Social Mobility and Class Structure in Modern Britain. Clarendon Press.
Goldthorpe, J.H., Mills, C., 2004. Trends in Intergenerational Class Mobility in Britain in the Late Twentieth Century. Social Mobility in Europe, pp. 195–224.
Goodman-Bacon, A., 2021. Difference-in-differences with variation in treatment timing. J. Econom. doi:10.1016/j.jeconom.2021.03.014.
Gorard, S., See, B., 2013. Overcoming Disadvantage in Education. Routledge.
Gorard, S., Siddiqui, N., 2018. Grammar schools in England: a new analysis of social segregation and academic outcomes. Br. J. Sociol. Educ. 39 (7), 909–924. doi:10.1080/01425692.2018.1443432.
Green, F., Machin, S., Murphy, R., Zhu, Y., 2012. The changing economic advantage from private schools. Economica 79 (316), 658–679.
Griliches, Z., 1986. Economic data issues. Handb. Econometr. 3, 1465–1514.
Guyon, N., Maurin, E., McNally, S., 2012. The effect of tracking students by ability into different schools a natural experiment. J. Hum. Resour. 47 (3), 684–721.
Heckman, J.J., Humphries, J.E., Veramendi, G., 2018. Returns to education: the causal effects of education on earnings, health, and smoking. J. Polit. Econ. 126 (S1), S197–S246.
Hoekstra, M., Mouganie, P., Wang, Y., 2016. Peer quality and the academic benefits to attending better schools. J. Labor Econ. 36 (4), 881–884. doi:10.3386/w22337.
Haider, S., Solon, G., 2006. Life-cycle variation in the association between current and lifetime earnings. Am. Econ. Rev. 96 (4), 1308–1320.
Lambert, P., Prandy, K., Bottero, W., 2007. By slow degrees: two centuries of social reproduction and mobility in Britain. Sociolog. Res. Online 12 (1), 37–62.
Mandler, P., 2020. The Crisis of the Meritocracy: Britain's Transition to Mass Education Since the Second World War. Oxford University Press.
Manning, A., & Pischke, J.-S. (2006). Comprehensive Versus Selective Schooling in England & Wales : what Do We Know ?
Mansfield, I., 2019. The Impact of Selective Secondary Education On Progression to Higher Education. Higher Education Policy Institute.
Matthewes, S.H., 2021. Better together? Heterogeneous effects of tracking on student achievement. Econ. J. 131 (635), 1269–1307. doi:10.1093/ej/ueaa106.
Meghir, C., Palme, M., 2005. Educational reform, ability, and family background. Am. Econ. Rev. 95 (1), 414–424.
Oakes, J. (1985). Keeping Track: how Schools Structure Inequality.
Pekkala Kerr, S., Pekkarinen, T., Uusitalo, R, 2013. School tracking and development of cognitive skills. J. Labor Econ. 31 (3), 577–602. doi:10.1086/669493.
Pekkarinen, T., Uusitalo, R., Kerr, S., 2009. School tracking and intergenerational income mobility: evidence from the Finnish comprehensive school reform. J. Public Econ. 93 (7–8), 965–973.
Rose, D., Pevalin, D.J., O'Reilly, K, 2005. The National Statistics Socio-economic Classification: origins, Development and Use. Palgrave Macmillan Basingstoke.
Shelton, N., Marshall, C.E., Stuchbury, R., Grundy, E., Dennett, A., Tomlinson, J., Xun, W., 2019. Cohort profile: the Office for National Statistics Longitudinal Study (the LS). Int. J. Epidemiol. 48 (2), 383–384. doi:10.1093/ije/dyy243.
Spens, H., 1938. Secondary Education With Special Reference to Grammar Schools and Technical High Schools Report of the Consultative Committee. HMSO, London.
Sturgis, P., Buscha, F., 2015. Increasing Inter-generational social mobility: is educational expansion the answer? Br. J. Sociol. 66 (3), 512–533.
Sullivan, A., Parsons, S., Wiggins, R., Heath, A., Green, F., 2014. Social origins, school type and higher education destinations. Oxf. Rev. Educ. 40 (6), 739–763.
Wolf, A., 2002. Does Education matter?: Myths about Education and Economic Growth. Penguin UK.