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Franz Buscha
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Franz Buscha
University of Westminster

Emma Gorman
University of Westminster and IZA

Patrick Sturgis
London School of Economics

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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

Selective Schooling Has Not Promoted Social Mobility in England*

In this paper we use linked census data to assess whether an academically selective 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 administrative data on selective system schooling within local areas, we exploit temporal and geographic variation to estimate the effects of the selective schooling system on absolute and relative social class mobility. Our results provide no support for the contention that the selective schooling system increased social mobility in England, whether considered in absolute or relative terms. The findings are precisely estimated and robust to a comprehensive battery of robustness checks.

JEL Classification: I21, 124, 128, J18, J24

Keywords: social mobility, selective schooling, grammar schools

Corresponding author:
Emma Gorman
University of Westminster
35 Marylebone Road
London NW1 5LS
United Kingdom

E-mail: e.gorman@westminster.ac.uk

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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 significantly more likely to attain better paid and higher status jobs (Heckman, Humphries, and Veramendi 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 origin. 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).

In this paper, we provide new evidence on the link between schooling systems and social mobility. We analyse the association 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 academic 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 wide scale abolition resulted in social mobility, particularly of the 'long range' variety, grinding to a halt (Mansfield 2019). 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 never 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 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 significantly lower rates of absolute and relative social mobility over the period of observation, although these effects are substantively tiny and become statistically indistinguishable from zero after adjusting for area and cohort fixed effects. The coefficients are precisely estimated with narrow confidence intervals around zero, so we can rule out the possibility of modest effects of selective schooling systems on social mobility in either direction. 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.

Our analysis provides several 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, Angrist, and Pathak 2014; Jackson and Beuermann 2018). 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 gaining grammar school entry only. Most studies also consider the effect of school system on proximal outcomes such as test scores (Atkinson, Gregg, and McConnell 2004; Gorard and Siddiqui 2018; Sullivan et al. 2014), university admission (Mansfield 2019) and income inequality (Burgess, Dickson, and Macmillan 2020), rather than social mobility itself. We directly estimate the association between the extent of school selectivity in an area and the degree of 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 cross-sectional 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 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 discusses summary statistics. Section 5 delineates 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, ideally measured at approximately the same point(s) in the life course. *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, also termed 'social fluidity', is a conditional comparison, typically measured using the coefficient from a regression of child outcomes on parental background. This 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 is particularly important because absolute upward social class mobility can increase over time as a result of expansion of professional and service occupations, without any change in the *relative* chances of upward mobility for people in different social class groups (Bukodi and Goldthorpe 2018). Commonly examined dimensions of relative mobility include occupational social class, status, and income, although some studies have also considered home ownership and education (Bell, Blundell, and Machin 2019). Relative social class mobility is usually measured by means of odds ratios: the ratio of the odds of upward mobility among 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 class mobility, early studies found that upward mobility had increased significantly 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). 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 (the range depends on the number of categories included in the measure of social class). Upward mobility was more common than downward, with approximately 35-45% upwardly mobile and the remaining 25-35% downwardly mobile (Buscha and Sturgis 2018). There is also evidence of a trend 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 largely show a steady increase in fluidity over the twentieth century (e.g., Lambert, Prandy, and Bottero 2007) or, in some studies a static pattern (Erikson and Goldthorpe 1992; Goldthorpe and Mills 2004).

Recent studies by Bell et al. (2019), Friedman and Macmillan (2017) and Buscha, Gorman, and Sturgis (2021) also show significant 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 neighborhood-level upward mobility were identified in Chetty et al. (2014), including: lower residential segregation, lower income inequality, greater school quality, social capital, and family stability. In sum, while there is some inconsistency in the exact pattern and timing of differences and trends, there is a robust body of evidence showing substantial heterogeneity in social mobility over time and place.

In relation to school type, a first strand of relevant 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 among women in early adulthood. Clark (2010) used the same identification strategy to assess the effects of gaining admission to grammar schools 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 detected little evidence for effects on short-run test scores (Abdulkadiroglu et al. 2014; Hoekstra, Mouganie, and Wang 2016), but often larger effects on longer run outcomes such as fertility, income, and mental health (Jackson and Beuermann 2018).

While these studies provide compelling evidence on returns to attending selective schools for the marginal pupil, they neglect the possibility of 'spill over' effects on pupils further away from the acceptance threshold. The marginal pupil at the admission threshold is 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, Crawford, and Macmillan 2018). A further characteristic of much of the existing literature on selective schools and social mobility is that they do not use social mobility outcomes directly but focus on intervening variables such as test scores, university admission, and earnings. Positive effects of school choice on education or labour market outcomes, while important, do not necessarily imply positive effects on social mobility, which also depends on the patterning of access to different status institutions and subject choices, among other factors.

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. A consistent finding is documented in Matthewes (2020), for Germany, where between-state variation in tracking practices is used to identify the effect of early tracking on the lower-track students, finding negative effects on achievement, especially for students from lower socio-economic backgrounds.

Guyon, Maurin, and McNally (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 to no losses among higher ability students. Burgess, Dickson and Macmillan (2019) use cross-sectional variation in selective schooling across Local Education Authorities in England to identify significantly higher income inequality in areas with predominantly selective schooling systems. Around a fifth of the 90-10 earnings gap can be explained by differences in school systems. Boliver and Swift (2011) study individual social mobility outcomes for individuals attending different school types among 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 advantages accruing to individuals attending a grammar school were offset by the negative effects for those attending a secondary modern school. Key caveats in this analysis are that the findings rely on an assumption of selection-on-observables.

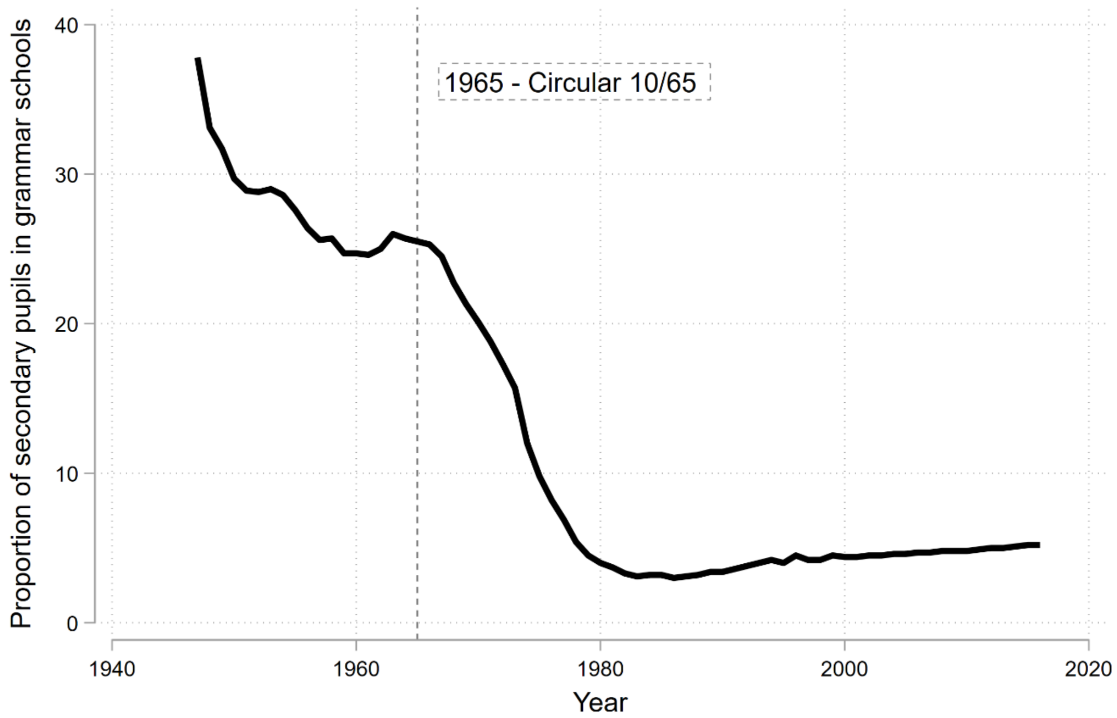
Additionally, the data follows only one cohort, born in 1958, who entering secondary school before many areas had begun the transition to a comprehensive system in earnest.

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*. This was intended to comprise three types of school: '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 as the remainder. These modes of education were planned 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 the more able were admitted to grammar schools and the remainder attended 'secondary moderns', with only a very small fraction attending Technical colleges—which were not held in high esteem.

By the early 1960s, growing public dissatisfaction with secondary modern schools, damage to the 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 school system'*. The result was a steady decline of 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). Notably, however, some LEAs, mainly but not exclusively in the South East of England, maintained the pre-1965 selective school system, whilst other areas saw a total conversion to comprehensive education. Figure 1 shows the proportion of secondary school pupils taught in grammar schools in England between 1947 and 2016 declining from a high of 37.8% in 1947 to 5.2% in 2016.

Figure 1 Proportion of secondary school pupils taught in state-funded grammar schools, 1947 - 2016



Notes: Source: Bolton, Paul (2016) 'Grammar school statistics' (SN01398). This Figure plots the proportion of secondary school pupils taught in state-funded grammar schools in England, 1947 – 2016.

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 equidistant (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 high rates of non-response and attrition that characterise sample survey and cohort studies. 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 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]

Measures

Social mobility

The occupations of study members and linked household members are coded to the National Statistics Socio-economic Classification (NS-SEC) (Rose, Pevalin, and O'Reilly 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] 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 bias.

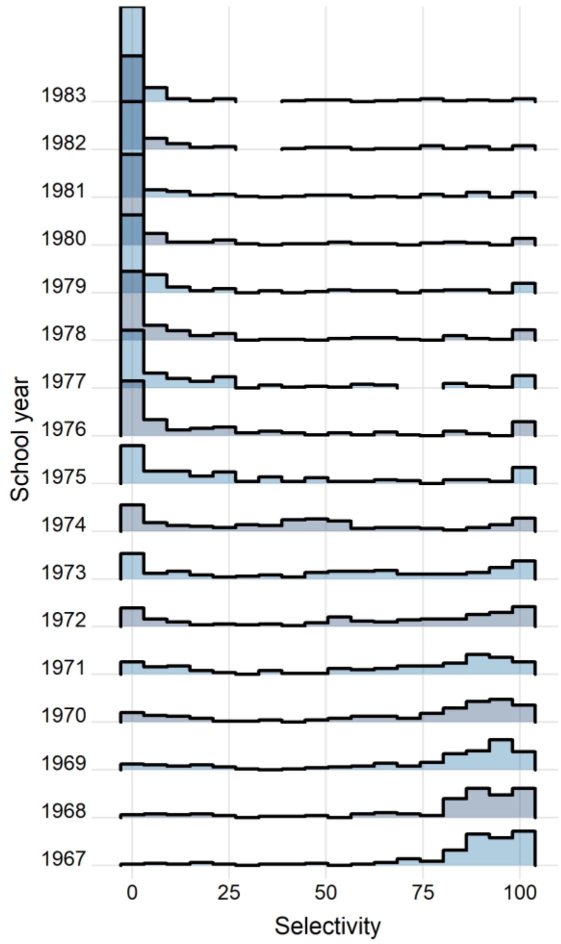
1 and 2, and zero otherwise. Relative mobility is measured using odds ratios derived from a logistic regression of study member NS-SEC on parental NS-SEC, estimated separately by LEA and school cohort. These odds ratios give the odds of being in a high social class in adulthood given high parent class, divided by the odds of being in a high social class in adulthood given low parent social class, for each LEA and time point. 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 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).

Selective schooling

To measure the extent of exposure to selective schooling, we use the percentage of pupils attending schools in the selective system (grammar, secondary modern or technical) in each LEA.[2] Figure 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.

[2] This data was compiled by Damon Clark from the Annual School Census (ASC), we are grateful to him for allowing us to access the data.

Figure 2 *Distributions of the percentage of 13-year-olds in selective system schools (Grammars, secondary moderns or Technical colleges) by school year*

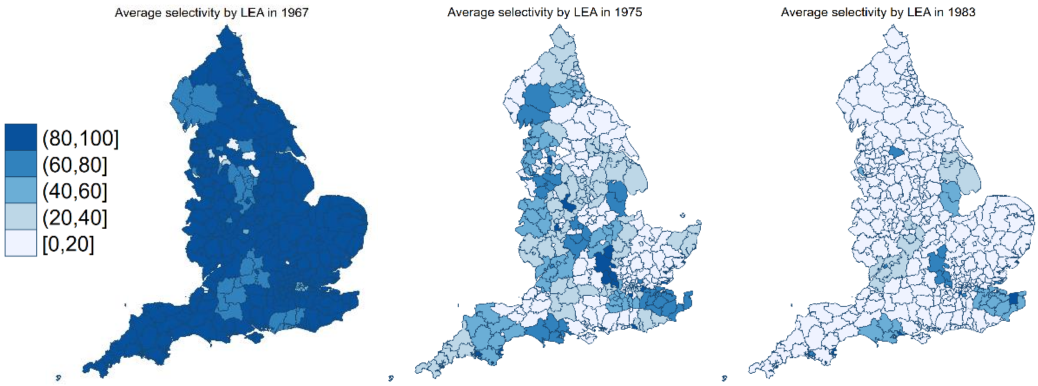


Notes: Data source: Annual School's Census.

When LEAs were first established in England they were based on the structure of local government in 1967, comprising two administrative units: county councils and county borough councils. There was subsequently substantial re-configuration of local government via the Local Government Act 1972 (LGA 1972) which re-organised England into a two-tier structure, with 45 county councils as upper-tier authorities and 366 district councils as the lower tier. The changes came into effect on 1 April 1974. We have therefore harmonised the set of LEAs in our analysis to be consistent over time in the following way. The 145 LEAs that existed in 1967 were matched to the subsequent Local Authority District geography available in the LS (detail on this matching is reported in Appendix A). This resulted in four pairs of pre-1972 LEAs which merged into only one 'new' Local Authority District. In these cases, we treat the pairs of LEAs as one geographic unit, and average their selectivity score for the new Local Authority District. This produced 141 time-harmonised LEAs in total.

Figure 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 region.

Figure 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.

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). 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. We then assigned study members' parents' social class at the time of their census enumeration 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 conducted robustness checks, reported in Appendix B, by measuring social mobility outcomes a further 10 years later, yielding a destination age between 38 and 47 years to account for life-cycle mobility (Haider and Solon, 2006).

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 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.

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	9,646
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		13		33	11,038
1969		1980	**1981 Census**	12	**2001 Census**	32	
1970	8	1981		11		31	14,903
1971		1982		10		30	
1972		1983		9		29	

Notes: Data source: ONS-LS.

Descriptive statistics

Table 2 shows summary statistics of selected political and economic characteristics by level of LEA selectivity in 1973. A political gradient is clearly identifiable; LEAs with low selectivity are significantly 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 higher share of owner occupiers and professional and managerial occupations.

These descriptives highlight differences in observable characteristics that are associated with school selectivity. As highlighted in section 3, the movement to comprehensive schooling was largely driven by local societal and political pressures, which in turn correlate with local socio-economic characteristics. Our estimates of the effect of school selectivity on social mobility must therefore account for this non-random selection into school system type.

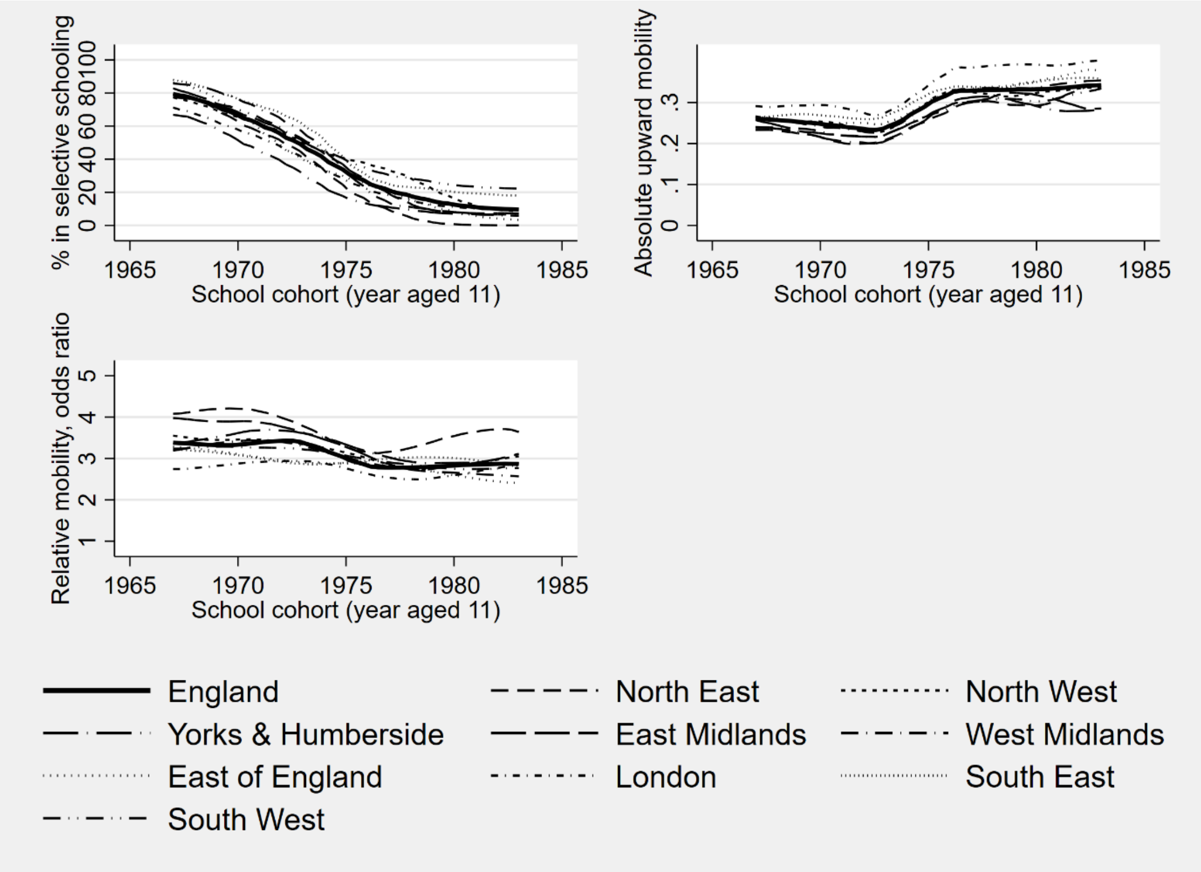
Table 2 Political and socio-economic characteristics by level of selectivity in an area (proportions)

	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 Democrat	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.

Figure 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 children in the South East continuing to attend selective 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.

Figure 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.

5. Empirical strategy

We begin by estimating the parameters of linear models of the form described in Equation 1 using ordinary least squares (OLS). $Y_{gt(i)}$ denotes the social mobility outcome for LEA g in cohort-band t, for individual i. We examine two outcomes. First, relative mobility which is measured using odds ratios computed at the area-by-cohort (gt) level (our relative mobility measure cannot be estimated at the individual-level). Second, absolute mobility which is a binary variable indicating upward mobility at the individual level (gti). α denotes the constant term, S_{gt} denotes the percentage 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, ε_{gti} is an individual-specific error term. $\boldsymbol{\beta}$ is the parameter of substantive interest, denoting the association between the level of selectivity and social mobility. Standard errors are clustered by LEA.

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

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 requires an assumption of homogenous treatment effects, which we relax in robustness checks (Goodman-Bacon 2021). Following this approach, we extend equation (1) and consider several sequential specifications adjusting for additional covariates, outlined in Equations 2a, 2b and 2c, where γ_g and δ_t denote LEA- and cohort-band-fixed effects, respectively, and T_t is a linear cohort-trend. In Equation 2a we add LEA fixed-effects to adjust for potential confounding from time-constant differences between LEAs. We know from Figure 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. 2019; Buscha and Sturgis 2018). As is evident in Figure 1, there was a simultaneous steep decline in the proportion of selective schools in England. To reduce the risk of wrongly attributing secular trends in social mobility to changes in selectivity, we add cohort-band fixed effects as specified in Equation 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 Equation 2c. This approach allows for unobserved time-varying LEA characteristics that may have led to differential mobility trajectories within LEAs.

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

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

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

Specifying the relationship between school selectivity and social mobility to be 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 may 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 allow 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 β_q (where q = 0, 1, 2, 3, 4) that define the selectivity quantiles. In each case a joint parameter test is conducted to determine whether these dummies are significantly different from zero. Finally, we include a dummy variable in Equation 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.

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

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

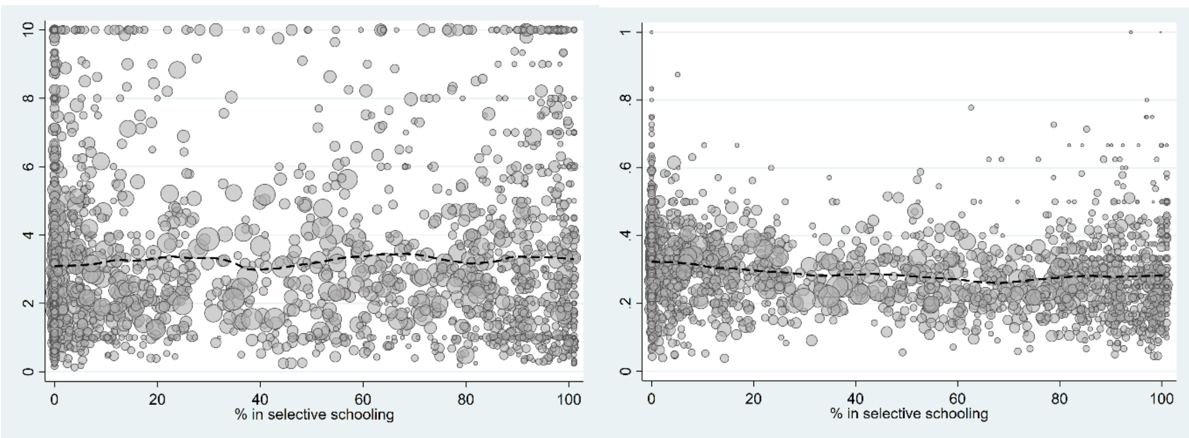
A recent literature has highlighted the potential for the TWFE coefficient to depart from the ATT in the presence of heterogenous treatment effects. For example, Chaisemartin and D'Haultfoeuille (2020) show that the TWFE estimator retrieves a weighted average of the treatment effects in each group and time period, and that these weights can be negative. In the presence of negative weights, the direction of bias of the TWFE coefficient away from the ATT tends to be downward (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), which estimates an ATT which is robust to these concerns, with results reported in Appendix Table B3.

6. Results

Figure 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 raw comparison of any notable association between the level of school selectivity in a local area and the degree of social mobility experienced by its inhabitants. Indeed, 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 small downward trend. This suggests that more school selectivity in an area is associated with *lower* rates upward mobility from working class origins to middle class destinations in adulthood.

Figure 5 *Scatterplots of social mobility and selectivity*

(a) Relative mobility *(b) Absolute upward mobility*



Notes: Data source: Annual School's Census and ONS-LS.

Regression analysis

These scatterplots suggest, at most, a slightly negative unconditional correlation between selectivity and social mobility, we next consider these relationships using regression. Table 3 reports the results of linear regressions which relate the absolute and relative social mobility measures to selectivity, with no covariate adjustment. The selectivity variable ranges from zero to one-hundred percent in increments of one percent. Assuming linearity, the coefficient of -0.000373 therefore implies that a 10 percentage point increase in the share of pupils in selective schools in an LEA is associated with a 0.00373 point decline in the proportion of people from working-class origins moving to a middle class occupation in adulthood. Putting this in context, a 20-point reduction in selectivity from the mean level of selectivity in England of 19.2 (pooled over all time periods) yields an expected increase of less than one tenth of one percent in the rate of upward social mobility. While the large sample size of the LS enables us to reject the null hypothesis that this quantity is not significantly different from zero, it is nonetheless negligible in terms of the substantive impact on the lives of the cohort members.

Turning to the estimates for relative mobility, column (2) of Table 3 reports the coefficient from the unadjusted model relating LEA by cohort-band odds ratios to the continuous selectivity index. Again, assuming linearity, a 10-point increase in the proportion of pupils in selective schools in an LEA is associated with a 0.0412 increase in the ratio of the odds of a child from working class origins making it to a middle-class destination relative to a child from middle class origins. As with absolute upward mobility, this suggests that higher rates of school selectivity in an area are associated with *less* social fluidity for its inhabitants. Again however, although significantly different from zero, this coefficient is very small in magnitude when considered relative to the mean of the odds ratios over all LEAS of 3.08. The extremely small magnitude of the coefficients for both absolute and relative mobility is also evident in the standardized coefficients (not shown in table) which show that a one standard deviation change in selectivity reduces absolute mobility by 0.085 standard deviations and relative mobility by 0.034 standard deviations.

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

	Absolute upward mobility	Relative mobility
	(1)	(2)
Coefficient	-0.000373***	0.00412**
(s.e)	(0.0000495)	(0.00145)
[95% CI]	[-0.000471, -0.000275]	[0.00126, 0.00698]
Outcome mean	*19.2*	*3.08*
N	90,894	90,894
R^2	0.003	0.008

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

Table 4 reports the results of models which sequentially add covariate adjustments. Models (1) and (4) add controls for gender and parental age and LEA fixed effects, with the coefficients for both absolute and relative mobility remaining statistically significant. This specification avoids the potential problem of LEA-level confounding factors by using only within-LEA variation, however it may still be confounded by spurious correlation with secular trends in social mobility. To address this issue, we add cohort-band fixed effects in models (3) and (6). For both absolute and relative mobility, the coefficients 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. The coefficients for both absolute and relative mobility remain very small and are not significantly different from zero.

Taken together, these results show a small negative correlation between the share of pupils in selective system schools and the degree of absolute and relative social mobility, which is not explained by time-invariant area-level characteristics. Rather, the chief confounding factor appears to be correlated time trends; the small increase in social mobility in Britain during this period mirrors the decline in selective schooling but is not caused by it. While these are null results, they are precisely estimated, so we can reasonably rule out the possibility of even moderate size effects of selective schooling on social mobility in either a positive or a negative direction.

Table 4 *Linear regression of LEA social mobility on selectivity index*

	Absolute upward mobility			Relative mobility		
	(1)	(2)	(3)	(4)	(5)	(6)
Coefficient	-0.000497***	-0.0000104	-0.0000892	0.00587**	-0.00432	-0.00475
(s.e)	(0.0000505)	(0.0000697)	(0.0001000)	(0.00179)	(0.00300)	(0.00418)
[95% CI, LL	[-0.000596	[-0.000148	[-0.000287	[0.00233	[-0.0102	[-0.0130
95% CI, UL]	-0.000397]	0.000127]	0.000108]	0.00941]	0.00161]	0.00352]
Controls						
Individual	✓	✓	✓	✓	✓	✓
LEA FE	✓	✓	✓	✓	✓	✓
Cohort FE		✓	✓		✓	✓
LEA*Cohort			✓			✓
Outcome mean	*19.2*	*19.2*	*19.2*	*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.

A linear specification for the selectivity variable is quite restrictive, and, as noted earlier, there are theoretical reasons to consider that a non-linear specification may better capture the relationship. In Table 5 we therefore allow the association between selectivity and social mobility to be non-linear by replacing the selectivity variable with the categorical variable which takes five values, zero and then 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.[3]

In Table 5 the negative coefficients for selectivity, for absolute and relative mobility, 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 fixed effects. In these models the variables are not statistically different from zero, either individually or jointly.

Finally, 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 at all 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 relatively mobility the effect becomes statistically non-significant once LEA-cohort specific trends are included. Irrespective of statistical significance, the substantive magnitude of the effect on absolute mobility remains tiny in comparison to the average level of absolute mobility. The effect on relative mobility is of greater magnitude, with odds-ratios increasing by approximately half a point in all three models. However, the standardized coefficient shows the effect size to be 0.122, similar to the estimate of 0.085 in Table 3. Overall, then, the magnitude of these effects is, at most, tiny.

[3] 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).

Table 5 *Linear regression of LEA social mobility on categorical selectivity index*

	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)
	-0.0360***	-0.00722	-0.00579	0.648***	0.334	0.424
Q 2	(0.00542)	(0.00624)	(0.00826)	(0.185)	(0.223)	(0.319)
(s.e)	-0.0535***	-0.00490	-0.00611	0.749***	0.0703	0.193
	(0.00516)	(0.00673)	(0.00979)	(0.177)	(0.267)	(0.404)
Q 3	-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)
	-0.0207***	-0.00457	-0.00686	0.639***	0.453*	0.562
Q 4	(0.00447)	(0.00459)	(0.00736)	(0.187)	(0.188)	(0.290)
(s.e)	-0.0360***	-0.00722	-0.00579	0.648***	0.334	0.424
F-test p-value (Null: Quartiles are jointly equal to zero	0.000	0.761	0.917	0.000146	0.0593	0.187
	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	*19.2*	*19.2*	*19.2*	*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.

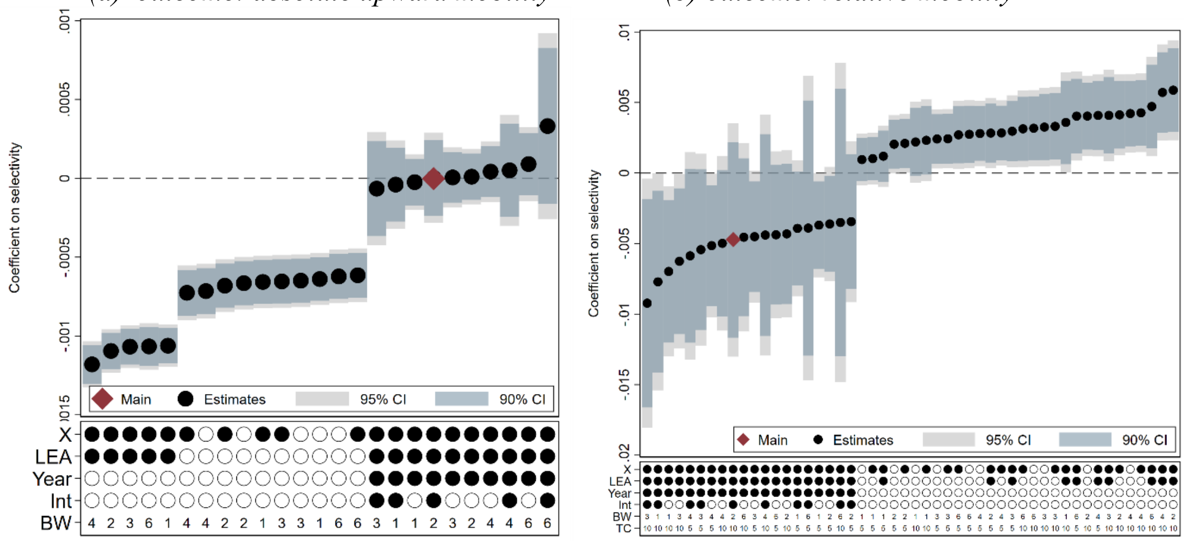
Sensitivity analyses

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 the results to a range of data and model specification choices. We report the outcome of these investigations in specification curves in Figure 6, which plots the estimates for a range of different control variables, bandwidth and top-coding choices. In each plot we highlight our primary specification which uses grouped data based on two-year cohort bands, two-way fixed effects controls with LEA interactions and top coding at 10 for the odds ratio outcome.[4]

Results show that coding and bandwidth choices matter less than control choices. The estimates which adjust only for LEA fixed effects and individual controls, suggest that higher school selectivity is associated with lower social mobility (both absolute and relative), albeit of a very small magnitude. Finally, the small effects sizes across all specifications demonstrate that a major change in admission school policy across England led to no detectable change in local social class mobility.

Figure 6 *Sensitivity analyses to alternative modelling and data choices*

(a) outcome: absolute upward mobility *(b) outcome: relative mobility*



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.

Further robustness checks

By adjusting for LEA fixed effects and time trends, we aim to adjust for other changes occurring concurrently with the decline in selectivity which could also influence social mobility outcomes. However, over and above these controls, offsetting responses or secular changes in the school sector occurring concurrently with the decline in selective schooling systems could represent an omitted confounder. One potential change in the school sector is the supply of private (fee-paying) schools—if families are unsatisfied with the comprehensive

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

system, the other option from securing a place in a grammar school is to choose a private provider. However, during our time period of interest, the proportion of pupils attending private schools remained relatively constant, at between 6-7% of pupils (Green et al. 2012).

Another possible factor influencing our results is the potential for pupils to attend schools outside their LEA. For example, pupils may live in an LEA with zero selectivity but attend a grammar school in a neighbouring LEA. If grammar schools do promote social mobility, this would lead us to under-estimate the effect because such pupils would be incorrectly coded as having zero exposure to selective schooling. However, this is unlikely to represent a significant issue for our analyses because our data spans school years 1967 to 1983, and the right to apply to schools outside your LEA of residence was not enacted until the 1988 Education Reform Act. This Act introduced open-enrolment, which extended nationwide the scope of parental preference to schools beyond the boundaries of their home LEA. The exception is London, where this provision had existed since 1965. In our main analyses we group the inner London boroughs as one LEA (based on the historic Inner London Education Authority), such that any cross-border mobility within this area is already accounted for. In robustness checks we have ensured that grouping inner and outer London into one area group does not affect the results.

We also address the issue of the age at which to measure destination social class, which in our main results is between 28 and 37 years. We have also produced estimates which extend the destination age to between 38 and 47 years. Results for this set of destination outcomes can be found in Appendix Figure B3, where the findings are presented in a similar fashion to Figure 7. The pattern is consistent with our main specification findings; the majority of estimates of the association between school selectivity and social mobility are tiny and statistically non-significant.

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 -.0000407 for absolute mobility and -.006204 for relative mobility, both statistically non-significant at the 95% level of confidence based on block bootstrapped standard errors with 50 replications. These results are reassuring as they are consistent with our main findings, although as expected the magnitude of the DID_M effect sizes are larger than the TWFE coefficients. The full results of this robustness check are reported in Appendix Table B3.

7. Conclusion

There has for some time now been a settled view amongst politicians and media commentators that the UK is characterised by low and declining levels of social mobility (Goldthorpe 2013). While the contention that social mobility is in decline 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, Heckman, and Vytlacil 2011; Dolton and Sandi 2017). A small but prominent part of the debate over how education policy can promote social mobility relates to academically selective schooling, with proponents arguing that selection into secondary/high school 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. While the existing evidence base provides little support for the contention that selective schools promote social mobility, it remains a popular policy amongst conservatives.

In this study we have used census data and linked administrative records on school selectivity within Local Education Authorities to examine the question of whether selective schooling does in fact boost social mobility, using England as a case study. We assessed whether the extent of selective schooling in an area is causally related to the social mobility outcomes of its residents measured in adulthood. Our results confirm the findings of existing studies which find little or no evidence to support the contention that selective schools have a beneficial effect on social mobility. Adjusting for both area characteristics and time trends we find small, non-significant correlations between exposure to selective schooling and absolute and relative mobility including linear and non-linear specifications. The dataset we have used for this analysis is very large and yields sufficiently precise estimates for us to rule out even modest effects of selective schooling systems on social mobility in either a positive or negative direction. 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 evidence than has previously been available.

Much of the appeal of academically selective schools has derived from the positive and often florid individual accounts of 'long range mobility' from 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 selective schooling on social mobility, it is necessary to consider the outcomes for all affected individuals, not the beneficiaries only. Of course, a corollary conclusion to the one we draw here is that 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 grammars. 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 any notable effect on intergenerational social class mobility.

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 (Sturgis and Buscha 2015, Bukodi and Goldthorpe 2018). Grammar schools are known to have a range of negative consequences including social segregation of schools and local areas (Gorard and Siddiqui 2017), 'skimming off' of high ability students from non-selective schools, 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 promoted social mobility in England.

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Online Appendix A

Data and variables

Geography

The data we use on share of pupils in selective system schools is collated at the Local Education Authority (LEA) level. The LEA geography used is based on the structure that was in place in 1967. At this time, there were two administrative units of local government: county councils and county borough councils. The term LEA was introduced by the Education Act 1902 (2 Edw.7, c. 42). The Act designated each local authority; either county council or county borough council; would set up a committee known as a local education authority (LEA). There has since been substantial local government re-organisation via the Local Government Act 1972. This Act re-organised England and Wales into a two-tier structure, with 45 county councils as upper-tier authorities and 366 district councils as the lower tier. The changes came into effect on 1 April 1974. In the LS we have data on the 1991 Census Local Authority District of each study member, of which there are 366. Using the 1972 LGA legislation, we identified which post-1972 districts match (or nest within) the pre-1972 country council or country boroughs. This is generally a one-to-one match, however for 16 out of the 366 post 1972 districts there was a partial match, where a post-1972 district was created from small areas located across two pre-1972 LEAs. In these cases, we assign the post-1972 district to the LEA where it took the majority of areas (e.g. urban or rural district) from as identified from the 1972 LGA. There were four pairs of pre-1972 LEAs which each merged into one 'new' Local Authority District. In these cases we treat these pairs as one geographic unit, and average the selectivity data from the pairs of LEAs to assign to the new Local Authority District. This leaves us with 141 LEAs.

Appendix Table A1 Analytical sample description including samples used in robustness checks to level of aggregation

						Selectivity assignment	Social mobility										
Year of birth	Cohort band					Year age 11		Age at origin		Age at destination 1		Age at destination 2	Number of observations				
	1yr	2yr	3yr	4yr	6yr								1yr	2yr	3yr	4yr	6yr
1956	1	1	1	1	1	1967	**1971 Census**	15	**1991 Census**	35	**2001 Census**	45	4616	9,646	14746	20150	30702
1957	2					1968		14		34		44	5030				
1958	3	2				1969		13		33		43	5100	10,504			
1959	4		2			1970		12		32		42	5404		15956		
1960	5	3		2		1971		11		31		41	5094	10,552		22238	
1961	6					1972		10		30		40	5458				
1962	7	4	3		2	1973		9		29		39	5631	11,686	17368		34251
1963	8					1974		8		28		38	6055				
1964	9	5		3		1975	**1981 Census**	17	**2001 Census**	37	**2011 Census**	47	5682	11,331		22565	
1965	10		4			1976		16		36		46	5649		16883		
1966	11	6				1977		15		35		45	5706	11,234			
1967	12					1978		14		34		44	5528				
1968	13	7	5	4	3	1979		13		33		43	5472	11,038	16242	25941	25941
1969	14					1980		12		32		42	5566				
1970	15	8				1981		11		31		41	5204	14,903			
1971	16		6			1982		10		30		40	4964		9699		
1972	17					1983		9		29		39	4735				

Notes: Data source: ONS-LS.

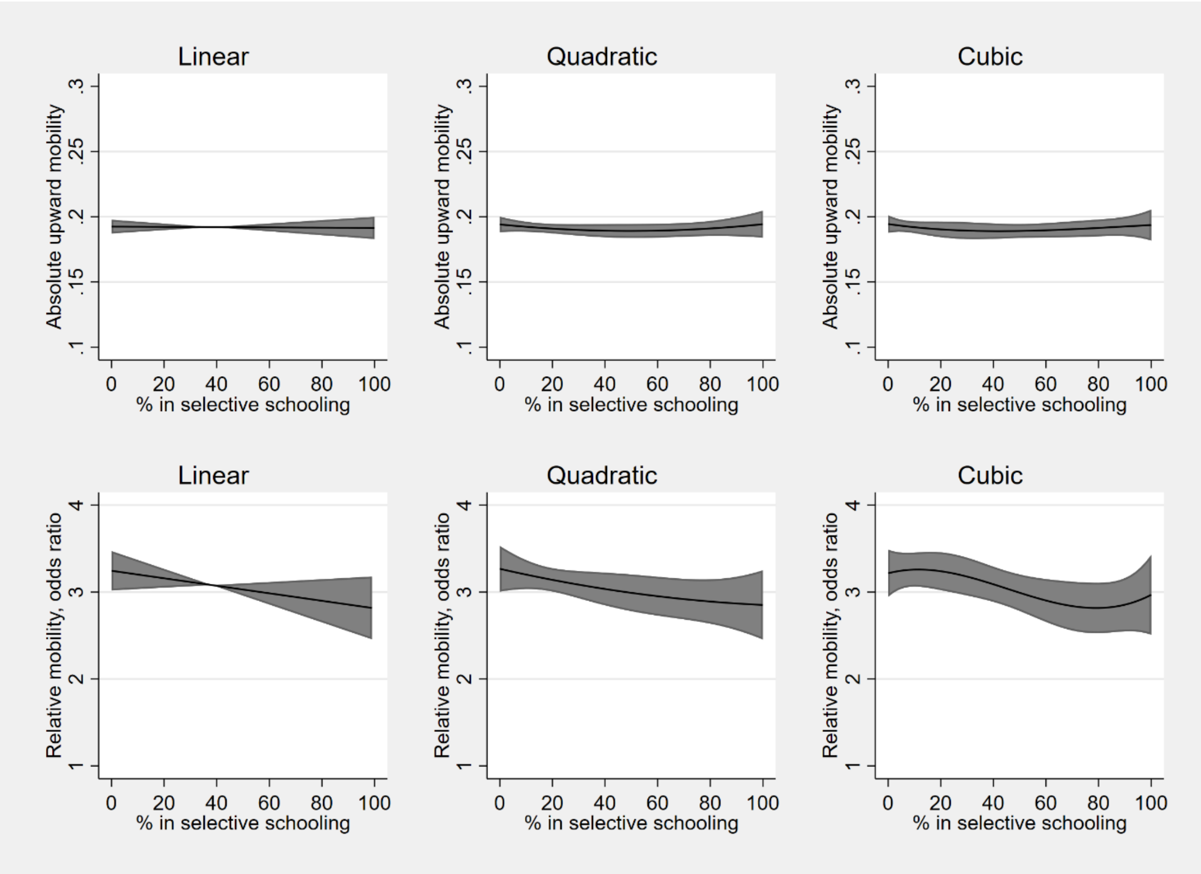
Online Appendix B

We explored the functional form of the link between selectivity and social mobility by including quadratic and cubic specification as denoted in Equation B1. The predicted values of social mobility from Equation B1 are plotted in Figure B1. In Equation B1, the parameters of interest are the relevant polynomials $\boldsymbol{\beta}_p$ (where p = 1, 2, 3).

$$Y_{gt} = \alpha + \sum_{p=o}^{3} \boldsymbol{\beta}_p \, S_{gt}^{p} + X_{gti} + \gamma_g + \delta_t + \varepsilon_{gt} \qquad \text{(B1)}$$

Appendix Figure B1 suggests that an approximately linear treatment of the relationship between selectivity and *absolute* mobility is a reasonable characterisation. Joint tests of higher-order polynomial terms indicated statistical insignificance. The results for *relative* social mobility are visually more supportive of a non-linear relationship. The cubic relationship, for example, suggests two possible inflections points in the relationship between social mobility and selective schooling. However, the confidence intervals are wide and joint tests of polynomial terms do not reveal any statistical significance. All results remain statistically insignificantly different from the null.

Appendix Figure B1 *Non-linear functional form predictions*

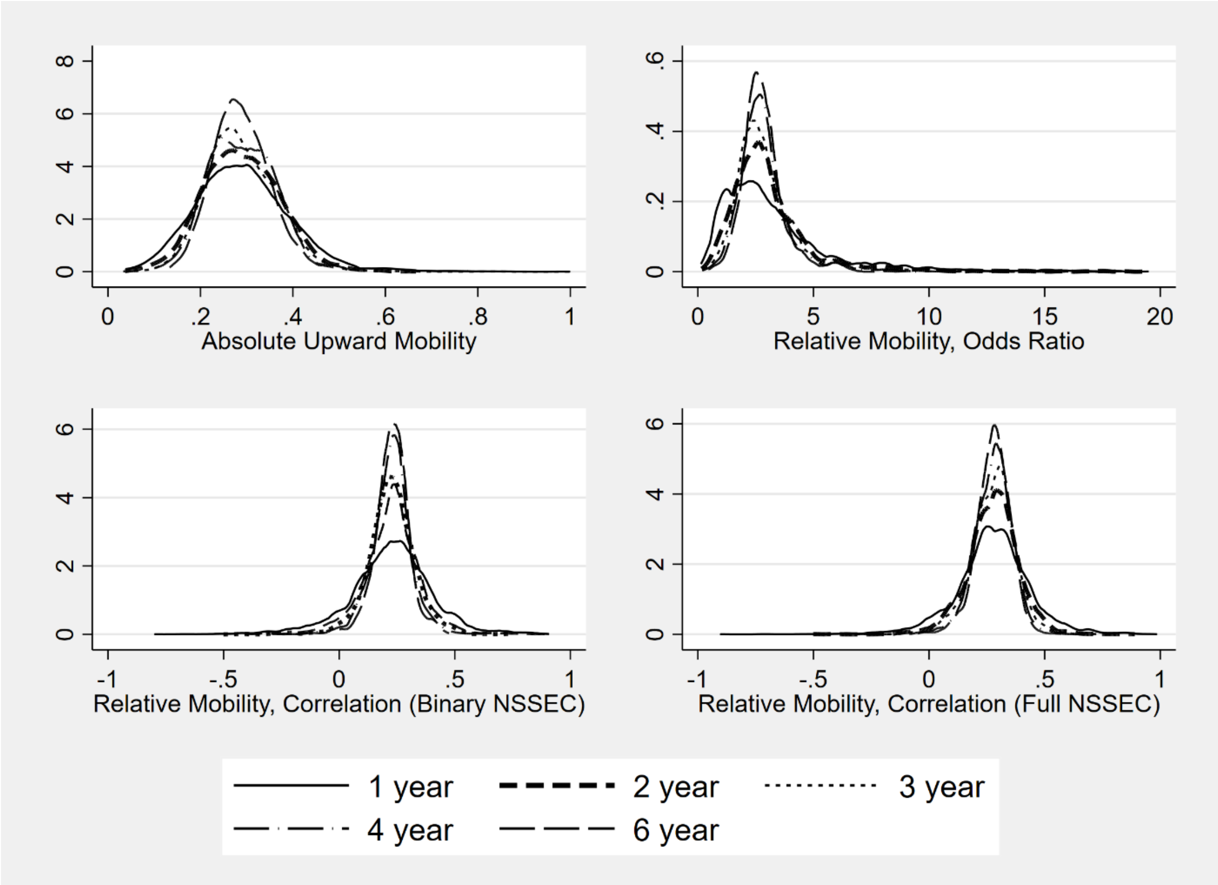


Notes: Annual School's Census and ONS-LS, 95% confidence interval plotted. These charts show the predicted values of social mobility from regression specifications based on linear, quadratic and cubic specifications of the continuous selectivity variable. The top panel reports results based on regressions with absolute mobility as the outcome, and the bottom panel reports results from regressions with relative mobility as the outcome.

Level of aggregation of the treatment and relative mobility variables

We also investigate varying *cohort-bandwidths* including bands of 1, 3, 4 and 6 years, in addition to the 2-year banding which is the main specification. The distribution of these estimates is shown in Figure B2, which highlights the impact that higher cohort-banding has. In all cases the 1-year cohort-band is an outlying distribution with wider variation and fatter tails. Higher banding results in a more compact distributional properties with fewer outliers.

Appendix Figure B2 Kernel density plots of mobility estimates using different sample cohort-bands.



Notes: Data source: Annual School's Census and ONS-LS.

Appendix Table B1 Sensitivity of main results to the level of aggregation of treatment and outcome in years

	Level of aggregation of the treatment and outcome				
	[1]	[2 – main]	[3]	[4]	[6]
			Absolute mobility		
Coefficient	-0.0000362	-0.0000892	-0.0000620	-0.0000440	0.000123
(s.e)	(0.0000931)	(0.0001000)	(0.000121)	(0.000122)	(0.000211)
			Relative mobility (odds ratios)		
Coefficient	-0.00771*	-0.00475	-0.00923*	-0.00440	-0.00351
(s.e)	(0.00389)	(0.00418)	(0.00446)	(0.00432)	(0.00572)
N	90894	90894	90894	90894	90894

Notes: Data sources: Annual School's Census and ONS-LS. This table reports the coefficient on the continuous selectivity variable and associated standard error, clustered by LEA. Each column is based on a different level of aggregation of the treatment and relative mobility outcome variable (which is a regression coefficient estimated at the area-by-time level, and is thus sensitive to the level of aggregation). The treatment variable was originally collected at the year-by-LEA level (column 1). In our preferred specification, we use the outcome and selectivity aggregated over two-year groups to increase cell size (2 – main). In the columns 3,4 and 6, we report the results based on aggregating the outcome and selectivity variable over 3, 4- and 6-year groupings respectively. The controls included are those from our preferred specification: age, age squared, parental age, parental age squared, sex (individual level); LEA fixed effects, cohort fixed effects, interaction between LEA fixed effects and a linear cohort variable.

Alternative dependent variables

Our main analyses use two social mobility outcomes, absolute upward mobility and relative mobility. We examine two alternative ways of constructing the relative mobility outcome variables, both using the coefficient from a linear probability model relating child to parent status, rather than odds ratios. 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 (while acknowledging that a linear equidistant relationship between the corresponding NS-SEC categories is an approximation only). Results are presented in Table B2 which repeats our previous linear (panel A), categorical (panel B) and binary (panel C) analysis of the selectivity regressor, with these two alternative dependent variables. Overall, the results confirm our previous pattern found using odds ratios; there are several statistically significant coefficients in the models which adjust only for LEA fixed effects, but our favoured specifications which add cohort controls, (2) (3) (5) and (6), generally show no statistical significance.

Appendix Table B2 Linear regression of LEA social mobility on categorical selectivity index

	Relative Mobility, Correlation (2 class)			Relative Mobility, Correlation (7 class)		
Selectivity	(1)	(2)	(3)	(4)	(5)	(6)
			Panel A: Linear treatment variable			
Coefficient	0.0000944	-0.000416*	-0.000511	0.000331*	-0.000161	-0.000326
(s.e)	(0.000127)	(0.000200)	(0.000277)	(0.000125)	(0.000187)	(0.000232)
			Panel B: Categorical treatment variable			
Ref (zero)	-	-	-	-	-	-
Q 1	0.0317*	0.0264	0.0287	0.0391**	0.0323*	0.0136
(s.e)	(0.0131)	(0.0143)	(0.0209)	(0.0120)	(0.0129)	(0.0194)
Q 2	0.0343*	0.0263	0.0288	0.0383**	0.0301	0.0138
(s.e)	(0.0144)	(0.0180)	(0.0245)	(0.0131)	(0.0163)	(0.0219)
Q 3	0.0241	-0.00209	-0.00271	0.0512***	0.0217	0.00133
(s.e)	(0.0134)	(0.0206)	(0.0301)	(0.0123)	(0.0179)	(0.0259)
Q 4	0.0257	-0.0117	-0.0197	0.0455***	0.0103	-0.0178
(s.e)	(0.0151)	(0.0240)	(0.0344)	(0.0132)	(0.0196)	(0.0287)
F-test p-value (Null: Quartiles are jointly equal to zero	0.140	0.0396	0.105	0.00106	0.122	0.423
			Panel C: Binary treatment variable			
Coefficient	0.0289*	0.0267	0.0313	0.0433***	0.0320*	0.0149
(s.e)	(0.0116)	(0.0139)	(0.0210)	(0.0103)	(0.0129)	(0.0193)
Controls						
Individual	✓	✓	✓	✓	✓	✓
LEA FE	✓	✓	✓	✓	✓	✓
Cohort lin.*		✓	✓		✓	✓
LEA			✓			✓
Outcome mean	*0.226*	*0.226*	*0.226*	*0.277*	*0.277*	*0.277*
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. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

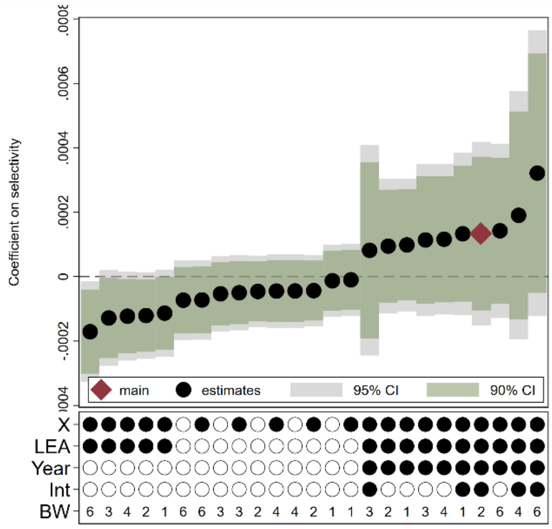
Appendix Table B3 Sensitivity to TWFE modelling approach using the alternative estimator developed in Chaisemartin and D'Haultfoeuille (2020)

	Absolute upward mobility	Relative mobility (odds-ratio)
TWFE coefficient	-0.0000104	-0.00432
(s.e)	(0.0000697)	(0.00300)
[CI LB	[-0.000148	[-0.0102
CI UB]	0.000127]	0.00161]
$ATT\ DID_M$	0.0000407	-0.0062041
(s.e)	(.0004427)	(0.0143113)
[CI LB	[-.0008269	[-.0342542
CI UB]	.0009083]	.0218461]
N	50,117	50,117

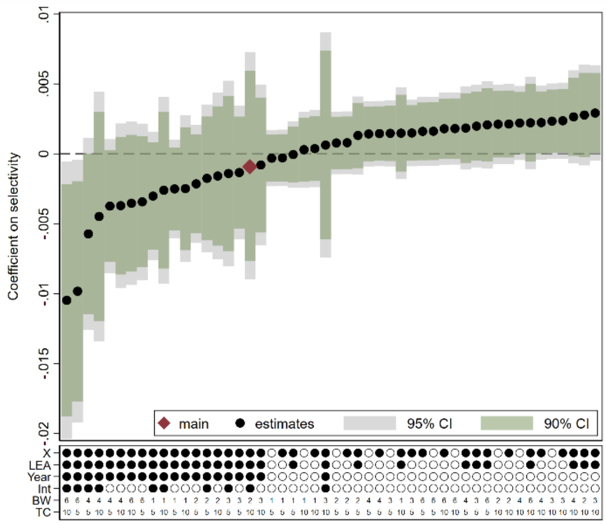
Notes: Data source: Annual School's Census and ONS-LS. This Table reports the Average Treatment Effect on the Treated estimate, and standard error, from the DID_M estimator developed in Chaisemartin and D'Haultfoeuille (2020) which provides a robust check on the two-way fixed effects regression coefficient.

Appendix Figure B3 Sensitivity analyses to alternative modelling and data choices for social mobility outcomes measured 40 years after social origin time point

(a) Absolute upward mobility



(b) Relative mobility



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. Dependent variable in each sub-figure: *(a) Absolute mobility (proportion experiencing upward mobility); (b) Relative mobility (as 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.