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Simon Burgess, Matt Dickson and Lindsey Macmillan
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Selective schooling systems increase inequality[1]

Simon Burgess[*], Matt Dickson[**], and Lindsey Macmillan[***]

[*] *Dept. of Economics and CMPO, University of Bristol, 2 Priory Rd, Bristol BS8 1TX, UK.*
[**] *Dept. of Social and Policy Sciences, University of Bath, BA2 7AY, UK.*
[***] *Dept. of Quantitative Social Science, Institute of Education, 20 Bedford Way, London WC1H 0AL, UK.*

Abstract

We investigate the impact on earnings inequality of a selective education system in which school assignment is based on initial test scores. We use a large, representative household panel survey to compare adult earnings inequality of those growing up under a selective education system with those educated under a comprehensive system. Controlling for a range of background characteristics and the current location, the wage distribution for individuals who grew up in selective schooling areas is quantitatively more unequal, and the difference is statistically significant. The total effect sizes are large: 14% of the raw 90-10 earnings gap and 18% of the conditional 90-10 earnings gap can be explained by differences across schooling systems.

JEL Classifications: I24, J31

Key words: selective schooling, inequality, wages, quantile regression

[#] Corresponding author:

Matt Dickson – e-mail: m.dickson@bath.ac.uk; tel: +44(0) 1225 386736

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1. Introduction

One of the key roles of any education system is to define the mechanism that assigns children to schools. The choice of mechanism is likely to affect the level and distribution of schooling outcomes and therefore later life outcomes. One such mechanism is to assign students based on test scores: those with high scores attend one school, those with lower scores go elsewhere. This is like tracking, but across schools rather than within school. In England this is known as the grammar school system, and was used to allocate children to schools from the time of a unified system of education in the 1940s through the 1980s; now only a few areas still use this as the main system. Nevertheless, grammar schools continue to be a prominent policy issue in England. There is a parallel debate in the US about elite or exam schools.

Much of the research on grammar schools has focussed on two important questions: who gets into grammar schools (is access 'fair'?), and what is the impact of attending a grammar school (is there a causal gain in attainment?). There is much less evidence focussing on the system as a whole, namely comparing the outcomes of one student assignment mechanism (by ability) with those of another (choice). That is the contribution of this paper: we examine the impact of a grammar school *system* on earnings inequality.

We use data from a large and representative household panel dataset and compare the spread of the earnings distribution in middle age. The richness of the data means that we can control for the parental background of the individual, as well as the current labour market status and location of the individual. We also know where the individual grew up and can map this back to the nature of the education system in that place at that time. This allows us to categorise respondents as grammar school system students or not, and to compare the earnings distribution they experience as adults.

We show that individuals who grew up in areas operating a selective schooling system have a more unequal wage distribution in later life. Those growing up in selective systems who make it to the top of the earnings distribution are significantly better off compared to their non-selective counterparts. For those at the bottom of the earnings distribution, those growing up in a selective system earn significantly less than their non-selective counterparts. These differences remain after controlling for a range of background characteristics and current local area. In summary, there are both winners and losers from the grammar system: the additional earnings differential between the 90^{th} and 10^{th} percentiles in selective systems accounts for 14% of the total raw 90-10 earnings gap and 18% of the conditional 90-10 earnings gap.

In the next section we review the related literature on the impact of selective systems on later outcomes before describing the framework for our analysis in section three. Our empirical approach and the data used are outlined in section four and our results are presented in section five. We end with some brief conclusions.

2. Related literature

Much of the previous literature on selective schools focuses on the benefit to the marginal student of attending a grammar school compared to not attending. In Great Britain, Clark (2010) uses access data from East Riding (a local government district in the north of England) to estimate the causal impact of attending a grammar school on attainment at 16, the types of course taken and university enrolment. He finds small effects of grammar schools on test scores at 16 but larger effects on longer-run outcomes such as taking more academic courses – which allow access to A-levels and university enrolment. Similarly, Clark and Del Bono (2014) implement a regression discontinuity design to assess the impact of attending a grammar school for a cohort of young people born in Aberdeen, Scotland, in the 1950s. They find large effects on educational attainment, and for women there are longer-term impacts on labour market outcomes and reduced fertility. For men there were no long-term impacts identified.

Abdulkadiroglu, Angrist and Pathak (2011) and Dobbie and Fryer (2011) assessed the impact of attending exam schools in Boston and New York on attainment and test scores. Both studies found limited impacts on student achievements from attending these selective schools, though Dobbie and Fryer (2011) found that these students were more likely to choose more academically rigorous subjects. Dustmann *et al.* (2014) similarly found little impact of the marginal student attending a more advanced track on their longer-term outcomes.

Sullivan and Heath (2002) and Galindo-Rueda and Vignoles (2005) used the National Child Development Study (NCDS) data from the UK to compare the outcomes of those attending grammar schools to comprehensive schools and secondary moderns. Both use a value-added approach alongside school-level controls to assess the impact of the different schools on educational attainment. In addition, Galindo-Rueda and Vignoles (2005) also instrument school type with the political power of the Local Education Authority (LEA) at the time, arguing that the political power of the LEA at the time of reform affected the speed at which the systems were switched from selective to mixed schooling. Both studies find significant positive effects on attainment of grammar education compared to comprehensives although Manning and Pischke (2006) use a falsification test of value-added from age 7 to 11 to show that these studies are still affected by selection bias.

A slightly different question is addressed by Guyon, Maurin and McNally (2012), who use data from Northern Ireland and exploit a policy change that compelled grammar schools to increase the number of children admitted each year. The change induced a discontinuous increase in the proportion of the school year group going to grammar schools, and this is used to identify the effect of school segregation by ability on the average performance in examinations taken at age 16, at age 18 and on university entrance rates. Rather than the impact on the marginal students who are shifted into the grammar school by the policy change, the estimates provide an assessment of the impact on the whole distribution. Guyon *et al.* find substantial positive impacts of the increased grammar attendance on average examination results and university entrance. However, as we might anticipate, disaggregating this into the impact on the grammar school results and the impact on the non-grammar school results, reveals a *negative* impact on the average results in the non-grammar schools as a consequence of the change in student composition induced by the policy.

While each of these approaches have clear strengths, and Guyon *et al.* in particular look at the distribution of results not just the effect on the marginal student, these studies say little about differences across selective and non-selective *systems*. Closer to our study are those of Atkinson, Gregg and McConnell (2006) and Jesson (2000), who use data from the more recent National Pupil Database (NPD) for England and Wales, to compare LEAs that are still selective now to non-selective LEAs. These studies are therefore more in line with our research, comparing the outcomes of pupils in systems as a whole rather than the outcomes of the marginal pupil who makes it into a grammar school. Both Jesson (2000) and Atkinson *et al.* (2006) use NPD data to compare value added attainment across selective and non-selective LEAs. While Jesson (2000) is open to the critique of Manning and Pischke (2006) that value-added alone does not remove selection bias, Atkinson, Gregg and McConnell (2006) match LEAs to attempt to control for this. They show that prior attainment when comparing selective LEAs to the comprehensive population as a whole is much higher in the selective LEAs but when comparing prior attainment in the matched LEAs, this is very similar. While neither study finds evidence of higher attainment across selective and non-selective systems as a whole, Atkinson, Gregg and McConnell (2006) find that grammar-educated children in selective LEAs outperform similar children in non-selective LEAs on average while non-grammar-educated children in selective LEAs underperform compared to similar children in non-selective LEAs. This is in line with our findings of greater inequality in earnings later in life for those from selective LEAs.

3. Framework

A selective school system, assigning individuals to schools based on their performance on a test, is one way of assigning students to schools. In England, the grammar school system assigns students to schools based on their performance on a test at age 11, commonly referred to as the ‘11+’ test. Typically in LEAs that operate a grammar system, students who achieve above a certain threshold are entitled to a place at a grammar school while students below the threshold are entitled to a place at a secondary modern school.

We compare the outcome of this system to the main alternative in England, namely school choice. In England, this involves families stating their preferred schools. However, given that the better schools quickly become over-subscribed and the criterion for assigning students in this case becomes proximity of the student's home to the school, school choice quickly reduces down to neighbourhood schooling. We therefore consider the differences in outcomes between two systems where, in their simplest form, one allocates pupils to schools based on ability[2] and one allocates pupils to schools based on proximity.

We present a very simple framework for thinking about the earnings inequality implied by each system.

Think of a population, where students have ability, *a*, and parental resources, *r.* These have distributions with variances σ^2_a and σ^2_r; they are positively correlated with covariance σ_{ar}.

The schooling outcome, s, for student *i* depends on ability, school quality, *q*, and peer group ability, $\bar{a}$:

$s_i=s(a_i, \bar{a}_i, q_i)$.

Later adult earnings depend on both the ability of the student and her schooling outcome:

$y_i = a_i + \gamma . s_i$

where γ is the relative weight on schooling.

To determine the relative impacts of the alternative schooling systems on earnings inequality, we must evaluate how each system translates ability into outcomes and therefore what each system implies for $\bar{a}(a)$ and $q(a)$ – that is, how each system relates student ability to peer group ability and to teacher quality.

The school assignment mechanism is different in the two systems. In a grammar school system, each student is assigned to the grammar school if a potentially noisy function of her ability is above some threshold (determined by the number of places in the grammar schools relative to the population). In a choice-based comprehensive system, admission depends on preferences and on priority. We could either assume random preferences or that all have preferences for high quality schooling; in either case, the driving force is priority. The most common priority rule in England is proximity: students living closest to the school are admitted. Under standard assumptions, the operation of the

[2] Of course there are issues concerning whether the tests used actually measure ability. Given the role of 'tutoring to the test', they are more likely to be measuring some mix of ability and attainment although this is not central to our analysis here.

housing market means that these nearby houses are valued more highly[3] and so the likelihood of admission to the higher performing schools depends on family resources, *r*.

Grammar system – assignment through selection on ability

By definition, grammar school systems sort pupils based on their ability: so $\bar{a}$(a) will be positive and very strong. Schools with high ability pupils are attractive to high ability teachers, hence we assume grammar schools attract and retain high quality teaching staff, hence *q(a)* will be positive and strong.

$s_i = s(a_i, \bar{a}_i(a_i), q_i(a_i)) = s_g(a_i)$ and earnings will be: $y_i = a_i + \gamma . s_g(a_i)$

Comprehensive system – assignment through residential proximity to school

We assume that the high quality schools are randomly distributed around an area. However, because of the proximity rule, the quality of the school attended depends on parental resources: *q(r)*. As a covariance exists between *r* and *a,* we can write this as *q(r(a))*. This also induces variation in peer groups, so $\bar{a}(a)$ again, but only through *r*. Therefore there is also a positive association between peer groups and ability and teaching quality and ability in this system, although these work through the correlation between *r* and *a* rather than directly as in the grammar system.

$s_i = s(a_i, \bar{a}_i(r(a_i)), q_i(r(a_i))) = s_c(a_i)$ and earnings will be: $y_i = a_i + \gamma . s_c(a_i)$.

Using these, we can express the variance of earnings in each system as:

$$var_k\big(y(a_i)\big) = \big\{\big(1 + \gamma s'(\mu_a)\big)\big\}^2 \sigma_a^2$$

where *k* = *g* (grammar) or *c* (comprehensive). Consequently, $var_g(y)$ < or > $var_c(y)$ depending on whether $\frac{dS_g(a)}{da}$ < or > $\frac{dS_c(a)}{da}$.

Therefore how the schooling system creates more equal or unequal wage distributions depends, among other things, on how the two systems translate individual ability into schooling outcomes. As we have seen, this will depend on how individual ability is related to peer group ability and how individual ability is related to school (teacher) quality in each system, both directly and indirectly via parental resources. These are empirical questions that we bring to the data.

4. Empirical analysis

To estimate the impact of selective systems compared to non-selective systems we would need to be living in an ideal world. Imagine two communities of identical families, growing up separately. One community has a grammar school system; the other has a comprehensive system (allocation by proximity). Following their education, both sets of individuals go on to work in the same labour market. A comparison of the distribution of wages amongst those who grew up in the selective

[3] This has repeatedly been shown to be the case for England, for a recent contribution see Gibbons, Machin and Silva (2013).

system with the distribution for those who grew up in the non-selective system, would tell us something about the impact of selective schooling on the whole distribution of wages.

Unfortunately such a thought experiment cannot be run in practice and we therefore have to use empirical methods to get as close to this ideal world as possible. In order to empirically test our model, we need to be able to compare the distribution of wages for individuals who grew up in LEAs[4] operating a selective mechanism for allocating students to schools, with the distribution amongst individuals who grew up in areas that were very similar along a number of relevant dimensions but that were operating the comprehensive system. This should ensure that we are not incorrectly attributing the effects of other area characteristics on later wages to the effect of growing up in a selective school area.

We use *Understanding Society* for our empirical analysis. This is a large longitudinal panel study following approximately 40,000 households in the UK, beginning in 2009. Information is collected from all individuals in the household aged 16 and over, on a wide range of topics, including parental background, labour market status and earnings. We make use of the special license release of the data, which includes the individual's age, current local authority of residence and crucially for our purposes, the local authority district where the individual was born. Each wave is collected over 24 months: the first was collected between January 2009 and January 2011, the second between January 2010 and January 2012 – we make use of both of these waves in our analysis. Given our sample requirements and matching process, our final analysis focuses on 2511 individuals who were born in 35 selective or similar non-selective LEAs from 1961-1983 (from these 35 LEAs we have observations from 152 LEA*years that were selective, and observations from 186 LEA*years that were non-selective). The birth year range means that the individuals in our sample are aged between 25 and 51 and so are of prime working age.

Defining selectivity

We begin by defining LEAs of birth as selective or non-selective. Selectivity of an area is calculated using school level data from the Annual Schools Census: schools are allocated to their LEA then the aggregated LEA data is used to calculate the percentage of children aged 13[5] in the LEA who had a place allocated by the selective system (grammar or secondary modern places).[6] The time-series of

[4] There are currently 152 local education authorities, or local authorities as they are now known, in the United Kingdom (Department for Education, http://www.education.gov.uk/). The average population in a Local Authority in 1998 was 140,000 individuals ranging from 25,000 to just over 1,000,000 individuals.

[5] The proportions were measured at age 13 rather than 11 or 12 because in some secondary schools (upper secondaries) children didn't start in the school until they were 13.

[6] We are extremely grateful to Damon Clark for providing this data. The figures for each LEA in each year are gender specific as there were/are a non-trivial proportion of single-sex schools in selective areas. For our purposes, we average the

data runs from 1967 to 1983, however post-1983 there has been very little further comprehensivisation (see Crook, 2013) and so we make the assumption that the proportion of selective school places within an LEA has remained similar to the 1983 level henceforth.[7] We do not model the process by which LEAs retained or abandoned selective schools. It is likely to have been influenced by fixed factors such as the size and geography of the area (population density and the like) as well as local political control. Our assumption is that the matching of LEAs, discussed below, takes account of most of the statistical force of these factors, and within the matched set, the retention of selection is as good as random.

We define an LEA as selective if more than 20% of children in the LEA were assigned their school place by selection. We define non-selective LEAs as those where less than 5% of 13-year old children were assigned by selection. As illustrated in Figure 1, given the distribution of levels of selectivity, these thresholds mark a clear delineation between what were selective and non-selective areas. Table 1 illustrates the distribution of selectivity in LEAs across the time period considered. 43% of LEA*time observations were 100% non-selective. Of those with any selectivity, 65% had greater than 20% selective schools within the LEA and 60% had greater than 30% selective schools. We consider whether our results are sensitive to these cut-offs at the end of the results section.

male and female figures to give us an average measure of selectivity for an LEA in a year. For the LEAs in our sample, the difference between the male and female figures is very small or zero (the mean is 0.66 percentage points and median is 0.22 percentage points).

[7] Despite the total number of grammar school places increasing as grammar schools have expanded, this phenomenon has also been witnessed to a similar extent in other schools, leaving the overall proportion of pupils in grammar schools increasing only 'very gradually' over the past 25 years, (see Figure 2: www.parliament.uk/briefing-papers/SN01398.pdf , accessed 12.51pm, 13th May 2014).

Figure 1: Distribution of selectivity across LEAs in 1983

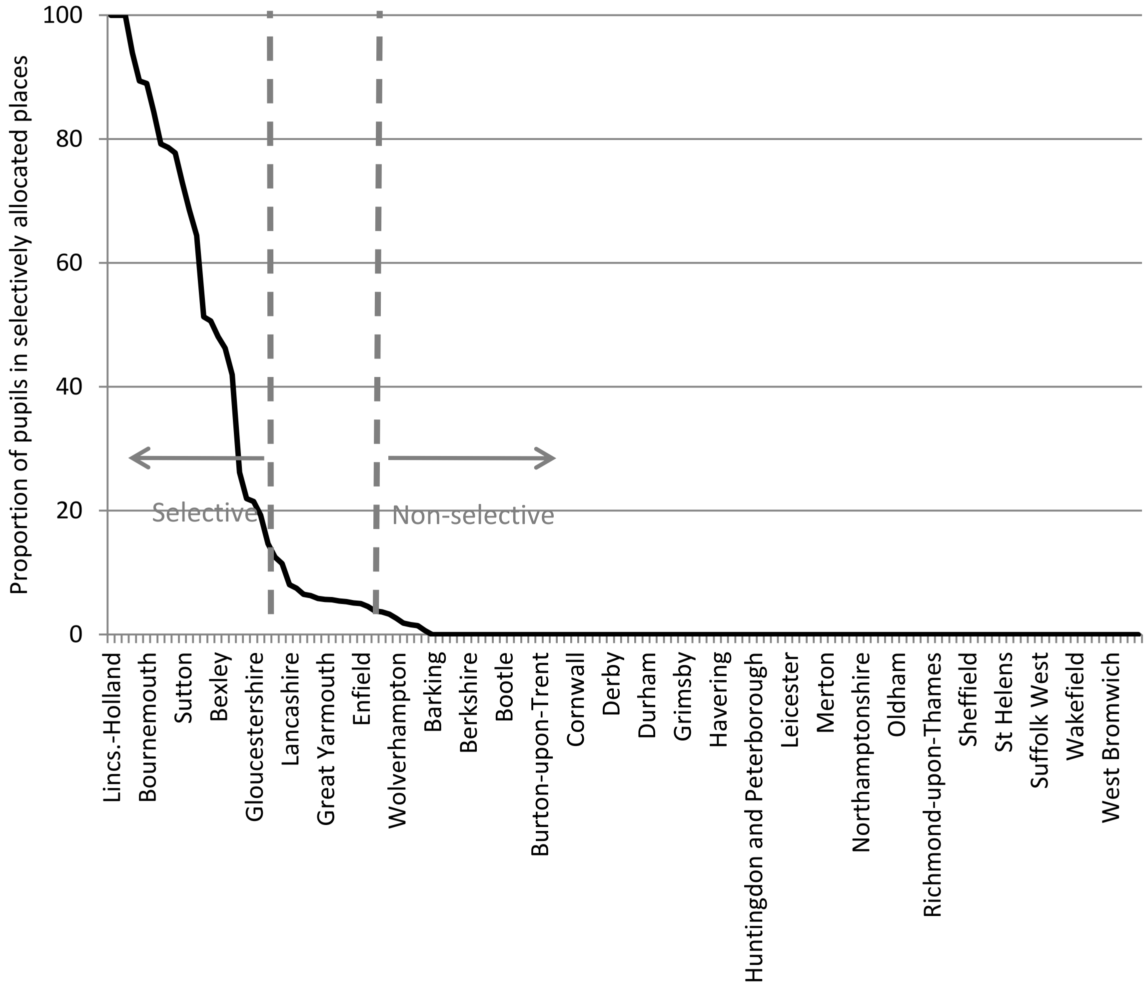


Table 1: Distribution of selectivity in LEAs across all time periods

	Selective %	Selective % conditional on >0
N	3915	2219
Mean	29.4	51.9
SD	38.6	38.2
10^{th}	0.0	3.1
25^{th}	0.0	9.5
50^{th}	3.8	56.3
75^{th}	68.6	90.3
90^{th}	94.9	99.2

Matching

Having defined selectivity, we proceed by matching selective and non-selective LEAs by LEA*year on the basis of labour market and school market characteristics: the local unemployment rate[8], the local male hourly wage rate[9] and the proportion of children who attend private schools in the area[10]. Individuals turned 13 in a number of different years in our data and hence the matching of LEAs is done separately for each year of our period of interest from 1974 to 1996. We select the three nearest neighbour non-selective LEAs for each selective LEA with replacement and retain only matches that share common support. Following the matching, we retain individuals who grew up in one of the selective or matched non-selective LEAs.

Appendix B contains summary statistics for the quality of the covariate balancing after the matching process in each year. These show that for every year and for each of the variables used in the matching, *t*-tests unanimously and comprehensively fail to reject the hypothesis that there is no difference in the means of the variables between the selective and non-selective areas (average *t*-stat of 0.434). Moreover, diagnostic statistics following regressions of the conditional 'treatment' probability on the matching covariates for the sample after matching show, as we would expect, that the matching covariates have little power to explain the variation in probability of being a selective area (for example, the average *p*-value on the test of joint insignificance of the covariates is 0.769). Hence the post-match test statistics suggest that selective and non-selective LEAs are well matched with respect to labour market and school market characteristics.

Data and methodological issues

Ideally the characteristics that we match on would all be measured at exactly the time that the individuals attended secondary school and for the majority of our data this is the case. However, due to the non-availability of some of this information – in part due to the restructuring of local authority organisation during the 1970s – there is some limit to the time-variation in the local unemployment data. In our data, only eight of the 23 years that we include in our analysis are affected. In these cases, we have to assign the value for the nearest available year (which is a maximum of five years

[8] Taken from the Employment Gazette, 1979 to 1998, county-level tables. Unemployment rates are matched to LEAs within counties with two LEAs in the same county taking the same unemployment rate.
[9] Taken from the New Earnings Survey, 1974 to 1996, region and sub-region tables. The specific earnings variable used to match is the average hourly earnings excluding the effect of overtime for full-time male workers over the age of 21 whose pay for the survey pay-period was not affected by absence.
[10] Compiled using the National Pupil Database 2002. Results are robust to the exclusion of private schools from the matching process, see the appendix Figures A2a and A3a and Tables A2a and A3a.

distance and in the majority of cases three or fewer).[11] Our results are robust to the exclusion of years in which the unemployment information has to be mapped from a nearby year (see Appendix Figures A2b and A3b, Tables A2b and A3b).

Information on the proportion of children attending private/independent schools is only available at the local authority level from 2002 and so there is no time-variation in this variable. However, given that the proportion of full-time pupils in private/independent schools in England and the proportion of English schools that are private/independent has changed very little between the time we have our measure of private school density (2002) and the relevant period for our data (1974 to 1996)[12], it is reasonable to assume that the local private school density has not changed too dramatically and thus our measure is relevant for matching.[13]

An obvious concern with our data is that we observe the LEA at birth rather than the LEA that the individual is enrolled into in secondary school. This raises two issues: children may attend a school across the LEA 'border' and so be educated under a different system; or families may move areas between the birth of the child and the start of secondary school. With regard to the first issue, we investigate the extent to which pupils cross borders in the NPD. On average around 11% of pupils attend a school in a different LEA from their LEA of residence. This is most likely to occur in London (over 20% cross-borders on average) where LEAs are small and close together. We test our results to see whether they are robust to the exclusion of London for this reason. We argue that if our results are robust to this exclusion, where border crossing is most relevant, then our results are not likely to be driven by border crossing elsewhere which will be much less prevalent.

We also argue that border crossing is likely to understate our findings to the extent to which border crossing across systems is made by 1) those that are the most able in non-selective systems crossing borders to attend grammar schools and 2) those who do not make it into grammars in the selective areas crossing borders to attend comprehensives rather than secondary moderns. In the first case, these individuals will push up the top end of the non-selective earnings distribution if grammars increase earnings relative to comprehensives and in the second case, these individuals will push up the bottom end of the selective earnings distribution if comprehensives increase earnings relative to

[11] In practice this means that for the years 1974 to 1978 each LEA has their 1979 level of unemployment and for the years 1994 to 1996 each LEA has their 1993 level of unemployment.

[12] See Ryan, C. and Sibieta, L. (2010) "Private schooling in the UK and Australia", IFS Briefing Note, no. 106.

[13] As a robustness test we perform the analysis when matching without private school proportion as a matching variable. Our results are robust to this test, see Appendix Figures A2a, A3a and Tables A2a and A3a.

secondary moderns. Both of these effects would lead us to *underestimate* the effects of the selective system at the top and the bottom of the earnings distribution.

To consider the second issue, that families may move areas, we use data from two birth cohort studies, the British Cohort Study (BCS) following children born in 1970, the Millennium Cohort Study (MCS) following children born in 2000, and the NPD to investigate the extent to which we can observe families moving from birth to starting secondary school. The birth cohort studies provide information on movements from birth to age 10 in the BCS and from birth to age 7 in the MCS, both at Government Office Region (GOR) level. The NPD provides information on moves from age 5-11 at the postcode level and Travel to Work Area (TTWA) level. As can be seen from Table 2, the vast majority of families do not move during childhood with 10 per cent moving to a different postcode in the NPD data and 1 per cent moving to a different travel to work area. The data from the cohort studies suggests that while more families move before children start school, the numbers moving are still small with 7.3 per cent in the BCS and 5.4 per cent in the MCS moving before the cohort member is 5[14].

Table 2: Proportion moving across different geographical areas during primary school

	Stay	Move
Postcode		
NPD 5-11	90.0	10.0
Travel to Work Area		
NPD 5-11	99.0	1.0
Government Office Region		
BCS		
0-5	92.7	7.3
5-10	95.5	4.5
0-10	89.4	10.6
MCS		
0-3	96.6	3.4
3-5	98.0	2.0
5-7	98.5	1.5
7-11	98.2	1.8
0-11	92.6	7.4

Notes: NPD figures from Allen, Burgess and Key (2010).

[14] Families that move are typically slightly more affluent than families that do not move in both cohort studies. For example, in the BCS, families that moved between birth and age 10 had an average family income of £1462 a month compared to an average of £1304 for families who did not move.

A final concern with our data is that we need individuals to move between school and when they are observed in the labour market as an adult in order to be able to separate out the effect of the schooling system from that of the local labour market. If everyone stayed where they went to school, our findings could be driven by the characteristics of the LEA that are related to labour market earnings and selection of the schooling system. For example, if selective LEAs were typically more unequal and individuals from selective LEAs stayed where they were from as adults, we would attribute the spurious association, or indeed reverse causation of inequality in selective areas, to selective areas causing inequality. Fortunately in our data, over 50% of the sample move LEAs between birth and adulthood. As illustrated in Table 3, this varies slightly by the type of system enrolled in with 56.9% of those growing up in selective LEAs moving while 43.8% of those growing up in non-selective LEAs move[15]. We therefore argue that we have enough variation in our data to be able to separate the effect of the school system from the effect of the LEAs' labour market characteristics.

Table 3: Proportion of people who move between birth and adulthood from the five largest selective and non-selective LEAs

Selective LEA	Proportion move	Non-Selective LEA	Proportion move
Kent	53.3%	Hampshire	46.4%
Lancashire	71.0%	Essex	48.9%
Gloucestershire	41.8%	Cambridgeshire	42.0%
Buckinghamshire	61.2%	Leicestershire	27.5%
Dorset	49.4%	Bedfordshire	49.0%
Weighted average	56.9%	Weighted average	43.8%

Measuring earnings inequality

To compare earnings distributions in adulthood, we use hourly wages calculated from the recorded usual gross monthly pay including overtime, usual weekly hours and overtime hours, deflated to year 2000 £s. Zero earnings are included for individuals who are unemployed or long-term sick or disabled at the time of the survey[16] as these are viewed as valid labour market outcomes. Given two waves of data, each individual has either one or two observations. Rather than discarding information, where we have two wage observations for an individual we average them and include

[15] Note that selective schooling systems can affect adult wages in a number of ways including potentially increasing the likelihood of students moving away from home after school to attend university and enter the labour market. This is all part of the effect that we are trying to measure.
[16] Results are robust to the exclusion of the long-term sick and disabled, see Appendix Figures A2b and A3b and Tables A2b and A3b.

that individual as a single observation. This averaging moves us towards a more permanent rather than transitory measure of individuals' earnings. Sixty-five per cent of the main estimation sample (1,621 of 2,511 individuals) have two wage observations. Prior to the averaging, an initial regression is run to remove any year of survey effects from wages.

We begin by estimating an OLS wage regression (1) where y_{ir} is the average hourly wage of individual *i* in LEA *r*, $selective_r$, is a dichotomous variable equal to 1 if the individual was born in a selective LEA and 0 if they were born in a matched non-selective LEA and $a * g_{ir}$ is a gender specific quadratic in age. This ensures that in our baseline specification we are comparing the earnings of similarly aged males and similarly aged females.

$$y_{ir} = \alpha + \beta selective_r + \gamma a * g_{ir} + \delta a * g_{ir}^2 + u_{ir} \qquad (1)$$

In addition to the effects of age and gender, there are other factors – unrelated to schooling – that may affect current wages. In our second specification (2), the conditional model, we control for personal characteristics (gender, ethnicity, plus the quadratic in age interacted with gender), controls for the individual's parental background (parental occupational class and parental education measured when the individual was 14 years old), dummies for the current local labour market (county), and dummies for the year of the survey (2009-2012).[17]

$$y_{ir} = \alpha + \beta selective_r + \gamma a * g_{ir} + \delta a * g_{ir}^2 + \zeta' X_{ir} + u_{ir} \qquad (2)$$

In both specifications, we recover the residuals from our wage regressions and compare the distribution of earnings for those growing up in selective and non-selective systems. As we are interested in the *relative* distributions rather than the effects on the average, we remove the global mean from the residual before calculating the deciles of the distribution.[18] We use unconditional simultaneous quantile regressions (equation 3), regressing adjusted earnings on the dichotomous selection variable to estimate whether growing up in a selective system has a significant effect on earnings at each decile (d) of the distribution of earnings.[19]

$$Q_d(\hat{y}_{ir}) = \alpha + \beta_d selective_r \qquad \text{where } d = [1, 2, \dots 9] \qquad (3)$$

[17] Appendix Table A4 contains the coefficient estimates for the main estimation sample conditional specification. Robust standard errors are obtained in all regressions, clustering at the individual level in cases where more than one observation is used per person.

[18] As we are removing a constant the results hold for non-mean-adjusted earnings. Note the average earnings are not significantly different across groups indicating a good match.

[19] We implement the sqreg command in Stata, which provides bootstrapped standard errors.

Finally, we perform tests on linear combinations at the 90th and 10th percentiles and 75th and 25th percentiles to test whether there are significant differences in the effect of selective systems on earnings inequality.

5. Results

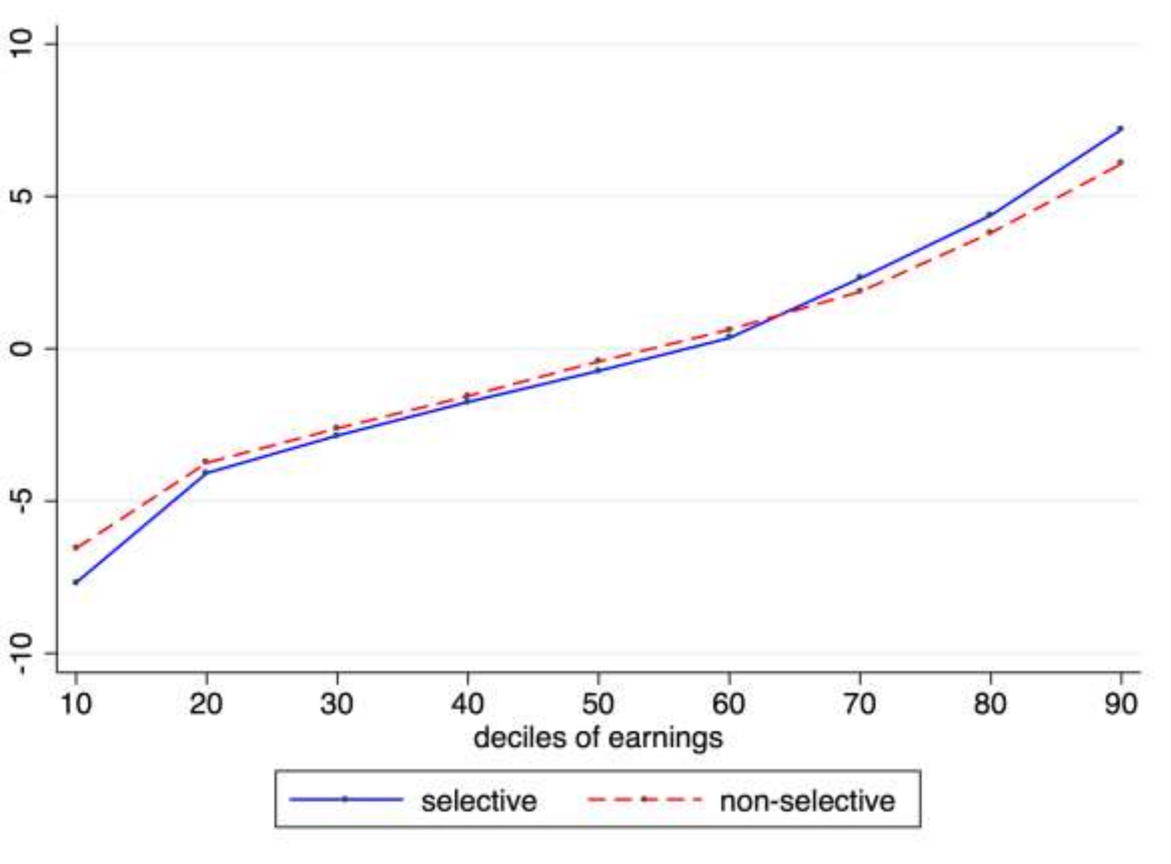
Table 4 shows the raw mean and variance statistics for the selective versus non-selective areas: overall, average hourly earnings 2009-2012 are very similar across the two groups although slightly (insignificantly) higher amongst those from the non-selective areas (£8.61 versus £8.59). The variance of earnings is considerably higher for selective areas (£35.13 versus £27.71). Figure 2 illustrates the impact of selective schooling across the whole distribution, plotting the deciles of age*gender adjusted hourly earnings for each system. As can be seen in this figure, the impact of the selective system has a positive effect on earnings at the top of the distribution and a negative effect on earnings at the lower end of the distribution. For those at the top of the earnings distribution, individuals who grew up in selective schooling areas earn more than their non-selective counterparts. At the bottom of the earnings distribution, this is reversed.

Table 4: Raw earnings distribution by schooling system type

	Selective	Non-Selective
Hourly wage: mean	8.59	8.61
variance	35.13	27.71
N	1319	1192

Notes: hourly earnings in year 2000 £s

Figure 2: Deciles of the raw earnings distribution by schooling system type



Notes: residuals from a regression of wage on a gender specific quadratic in age and a selective schooling area dummy, with the global mean of the residual removed. Before averaging wages for individuals with two wage observations, year of survey effects are removed via a regression. Source: Understanding Society

Panel A of Table 5 presents the simultaneous quantile regression estimates corresponding to Figure 2. These estimates show that the differences between the distributions are statistically significant at the 10th percentile, the 50th percentile, the 75th percentile and the 90th percentile.

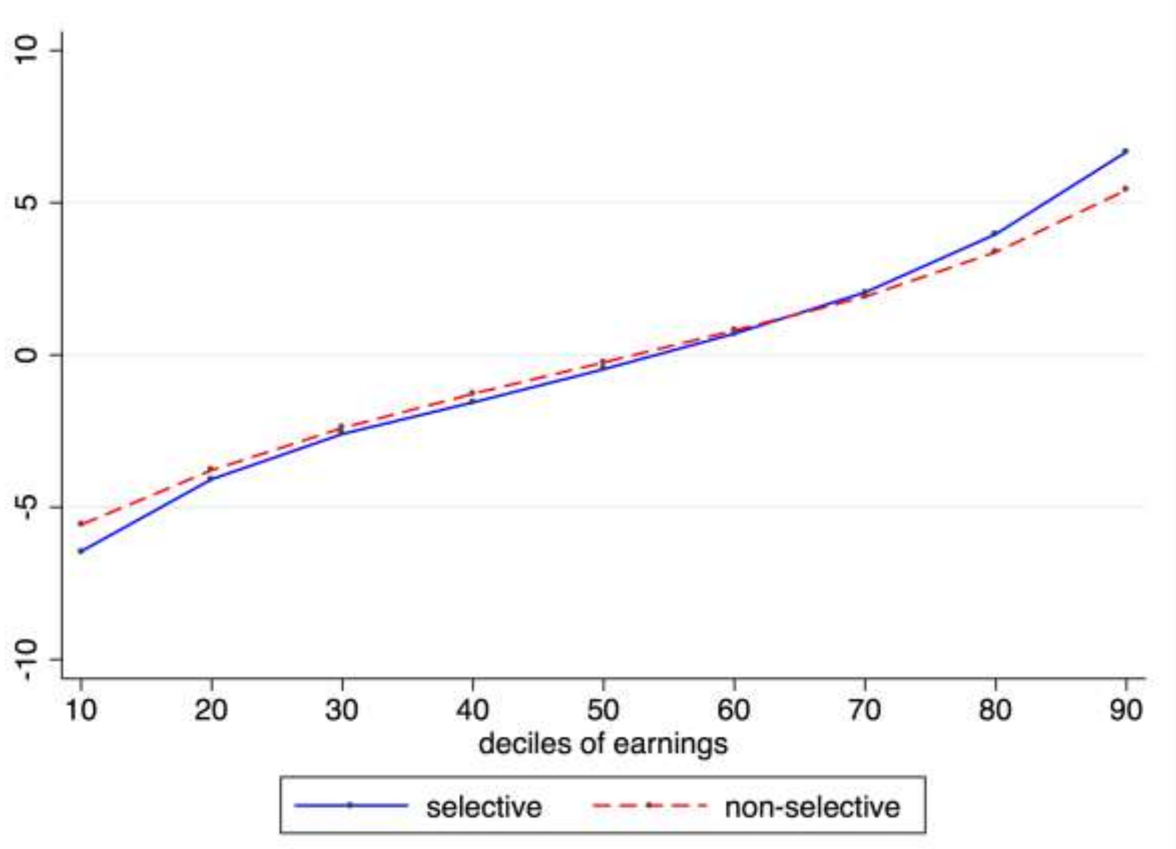
Figure 3, and Panel B of Table 5, present the results using the conditional earnings residuals. The qualitative nature of the results remains largely unchanged: at the lower end of the distribution, individuals born in a selective schooling area earn less than those from the matched non-selective areas, while this reverses for the top deciles. The distributions are significantly different at the 10th percentile. At the top of the distribution there is a statistically significant positive effect of selective schooling at the 90th percentile and the 80th percentile.

Table 5: Quantile Regression estimates of selective schooling effect on wages

	A: Without controls				B: With controls		
	coeff.	*std. error*	*t*		*coeff.*	*std. error*	*t*
10	-1.143	0.605	-1.89*	10	-0.897	0.383	-2.35**
20	-0.336	0.229	-1.47	20	-0.295	0.267	-1.10
25	-0.224	0.215	-1.04	25	-0.068	0.242	-0.28
30	-0.237	0.219	-1.08	30	-0.199	0.248	-0.80
40	-0.196	0.198	-0.99	40	-0.267	0.243	-1.13
50	-0.310	0.189	-1.65*	50	-0.237	0.215	-1.10
60	-0.275	0.260	-1.06	60	-0.106	0.251	-0.42
70	0.439	0.280	1.57	70	0.144	0.260	0.55
75	0.748	0.373	2.00**	75	0.239	0.330	0.72
80	0.584	0.387	1.51	80	0.595	0.280	2.13**
90	1.136	0.500	2.27**	90	1.308	0.449	2.91***
	N=2511				N=2511		

Notes: residuals from a regression of wage on a gender specific quadratic in age and a selective schooling area dummy (Panel A); and residuals from a regression of wage on a gender specific quadratic in age, a selective schooling area dummy, gender, ethnicity, parental occupational class when the individual was 14, parental education and current county of residence (Panel B). Global means of the residual removed. Before averaging wages for individuals with two wage observations the year of survey effects are removed via a regression.

Figure 3: Deciles of the conditional earnings distribution by schooling system type



Notes: residuals from a regression of wage on a gender specific quadratic in age, gender, ethnicity, parental occupational class when the individual was 14, parental education, current county of residence and a selective schooling area dummy with the global mean of the residual removed. Before averaging wages for individuals with two wage observations, year of survey effects are removed via a regression. Source: Understanding Society.

These results are robust to including all observations (i.e. not averaging where an individual has two observations), to including just a single observation per individual, to including only the observations of individuals with two observations, and to altering the definition of selective and non-selective areas – in each case the pattern and levels of significance remain essentially unchanged.[20]

Table 6 presents estimates of the difference in the effect sizes found at the 90th and 10th percentile and 75th and 25th percentiles for both the unconditional (Panel A) and conditional (Panel B) models. Focusing first on Panel A, the 90-10 earnings gap of individuals growing up in a selective LEA is £2.28/hour larger than the 90-10 earnings gap of individuals from a non-selective system. This accounts for 14.5% of the overall 90-10 earnings gap in our sample, and the test against zero has a *p*-value of 0.004. Focusing on the 75th-25th percentile earnings difference, the gap is 15.0% of the total raw gap, with a *p*-value of 0.012 for the test against zero.

Panel B shows that in the conditional model, there is a quantitatively and statistically significant difference in the 90-10 earnings gap between the two education systems. This is £2.21/hour, or 18.0% of the total conditional 90-10 gap in the sample, with a *p*-value of below 0.001. However the difference at the 75th-25th percentiles is smaller and no longer significantly different.

Table 6: Estimated effects sizes

	Sample wage gap	A: Without controls			Sample wage gap	B: With controls		
		coeff.	*std. error*	*Effect size*		*coeff.*	*std. error*	*Effect size*
90-10	15.73	2.279	0.790***	14.49	12.25	2.205	0.588***	18.00
75-25	6.48	0.972	0.388**	14.99	5.82	0.307	0.335	5.27

Notes: earnings differentials estimated by testing the linear combination from the simultaneous quantile regressions. The effect size is calculated as the estimated difference divided by the total earnings differential in the sample.

Differences by gender

While there is no *a priori* reason to think that schooling systems will have differential effects on inequality by gender according to our descriptive framework, it is interesting to consider this question for males and females separately. Tables 7 and 8 and Figures 4 and 5 present the results by gender (showing the conditional model only, for each gender the unconditional model results follow

[20] See Appendix Figures A2a, A2b, A3a and A3b plus Tables A2a, A2b, A3a and A3b.

the same pattern and significance of the pooled results). Table 8 shows that the differences in inequality for both males and females in the conditional model also mirror those seen in the pooled sample (19.6% of total 90-10 gap for males and 13.2% of total 90-10 gap for females). However, the detail in Table 7 and the figures show a slightly more complex picture: for males, the difference is concentrated at the top of the distribution, whereas for females, the gap is really particularly evident at the bottom of the distribution in the conditional specification. It may well be that this is because there was a significant gender difference in school assignment in selective areas. That is, the grammar school era was a time when boys typically outperformed girls at school, and being in a selective area meant that female students disproportionately went to secondary modern schools and male students disproportionately went to grammar schools.

Table 7: Quantile Regression estimates of selective schooling effect on wages, by gender

	A: Males				B: Females		
	coeff.	*std. error*	*t*		*coeff.*	*std. error*	*t*
10	-0.451	0.699	-0.65	10	-0.874	0.320	-2.73***
20	0.001	0.400	0.00	20	-0.519	0.310	-1.67*
25	0.217	0.412	0.53	25	-0.682	0.264	-2.59**
30	0.522	0.367	1.42	30	-0.709	0.233	-3.04***
40	0.606	0.293	2.07**	40	-0.592	0.279	-2.13**
50	0.572	0.340	1.68*	50	-0.532	0.250	-2.13**
60	0.988	0.366	2.70***	60	-0.502	0.231	-2.18**
70	0.962	0.443	2.17**	70	-0.512	0.351	-1.46
75	1.041	0.500	2.08**	75	-0.452	0.409	-1.10
80	0.929	0.470	1.98**	80	-0.152	0.416	-0.37
90	2.247	0.738	3.05***	90	0.596	0.551	1.08
	1102				1409		

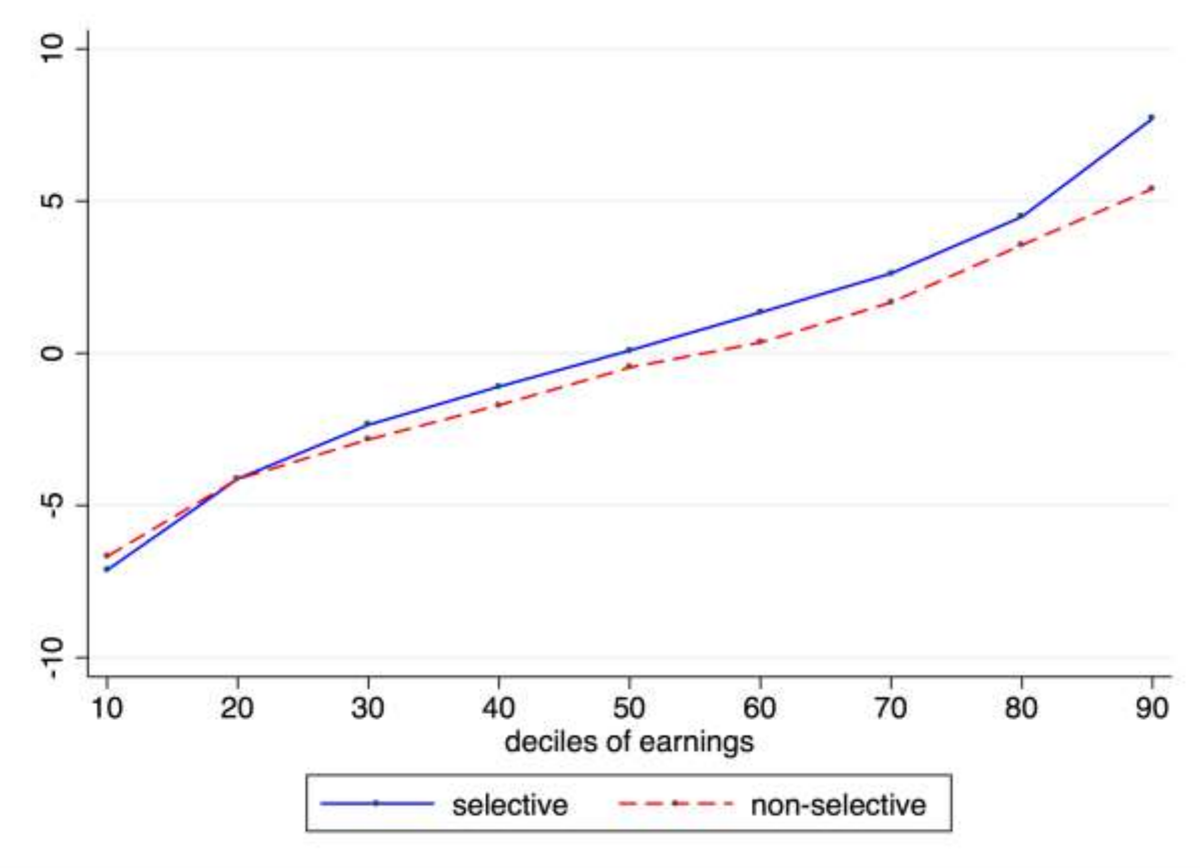
Notes: residuals from a regression of wage on a gender specific quadratic in age, a selective schooling area dummy, ethnicity, parental occupational class when the individual was 14, parental education and current county of residence. Men only (Panel A) and Women only (Panel B). Global means of the residual removed. Before averaging wages for individuals with two wage observations the year of survey effects are removed via a regression.

Table 8: Estimated effects sizes for conditional specification by gender

	Sample wage gap	A: Males			Sample wage gap	B: Females		
		coeff.	*std. error*	*Effect size*		*coeff.*	*std. error*	*Effect size*
90-10	13.77	2.697	1.061***	19.60	11.12	1.470	0.577**	13.22
75-25	6.23	0.824	0.489*	13.22	5.14	0.230	0.402	4.48

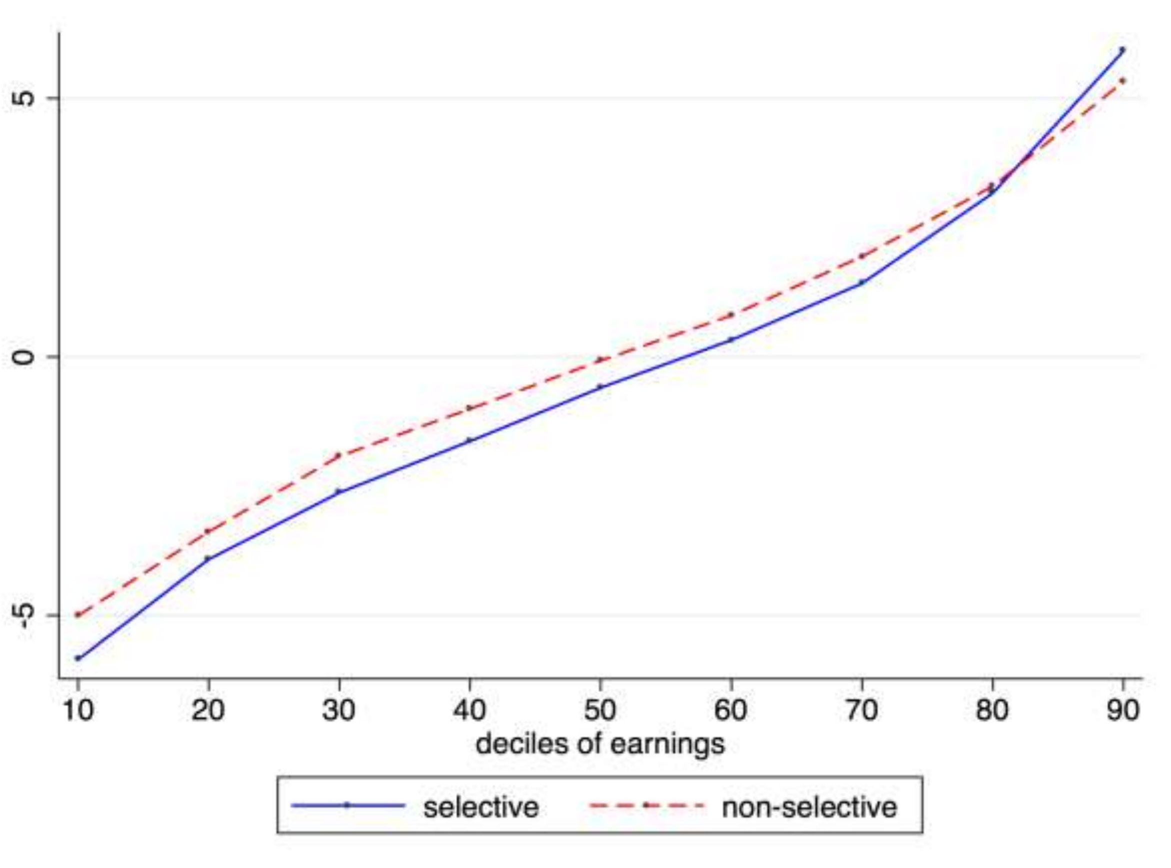
Notes: earnings differentials estimated by testing the linear combination from the simultaneous quantile regressions. The effect size is calculated as the estimated difference divided by the total earnings differential in the sample.

Figure 4: Deciles of the conditional earnings distribution by schooling system type, males



Notes: residuals from a regression of wage on a gender specific quadratic in age, ethnicity, parental occupational class when the individual was 14, parental education, current county of residence and a selective schooling area dummy with the global mean of the residual removed. Before averaging wages for individuals with two wage observations, year of survey effects are removed via a regression. Source: Understanding Society

Figure 5: Deciles of the conditional earnings distribution by schooling system type, females



Notes: residuals from a regression of wage on a gender specific quadratic in age, ethnicity, parental occupational class when the individual was 14, parental education, current county of residence and a selective schooling area dummy with the global mean of the residual removed. Before averaging wages for individuals with two wage observations, year of survey effects are removed via a regression. Source: Understanding Society

Robustness

Given that we only observe the LEA that individuals lived in at birth, rather than the LEA that they attended school in, we repeat our analysis from Tables 5 and 6 excluding London. We argue that if our results are robust to the exclusion of London from the analysis, it is unlikely that our results are driven by children crossing borders into selective systems when we classify them as non-selective and vice versa. Figure 6 replicates Figure 3, our conditional model, for this more restrictive sample (full results reported in Appendix Table A2a). Table 9 presents the differences in the effect sizes found at the 90th and 10th percentile and 75th and 25th percentiles as seen in Table 6. The results are robust: Figures 3 and 6 are very similar and the total 90-10 and 75-25 earnings gaps found in Tables 6 and 9 are almost identical, suggesting that London is not driving the result.[21]

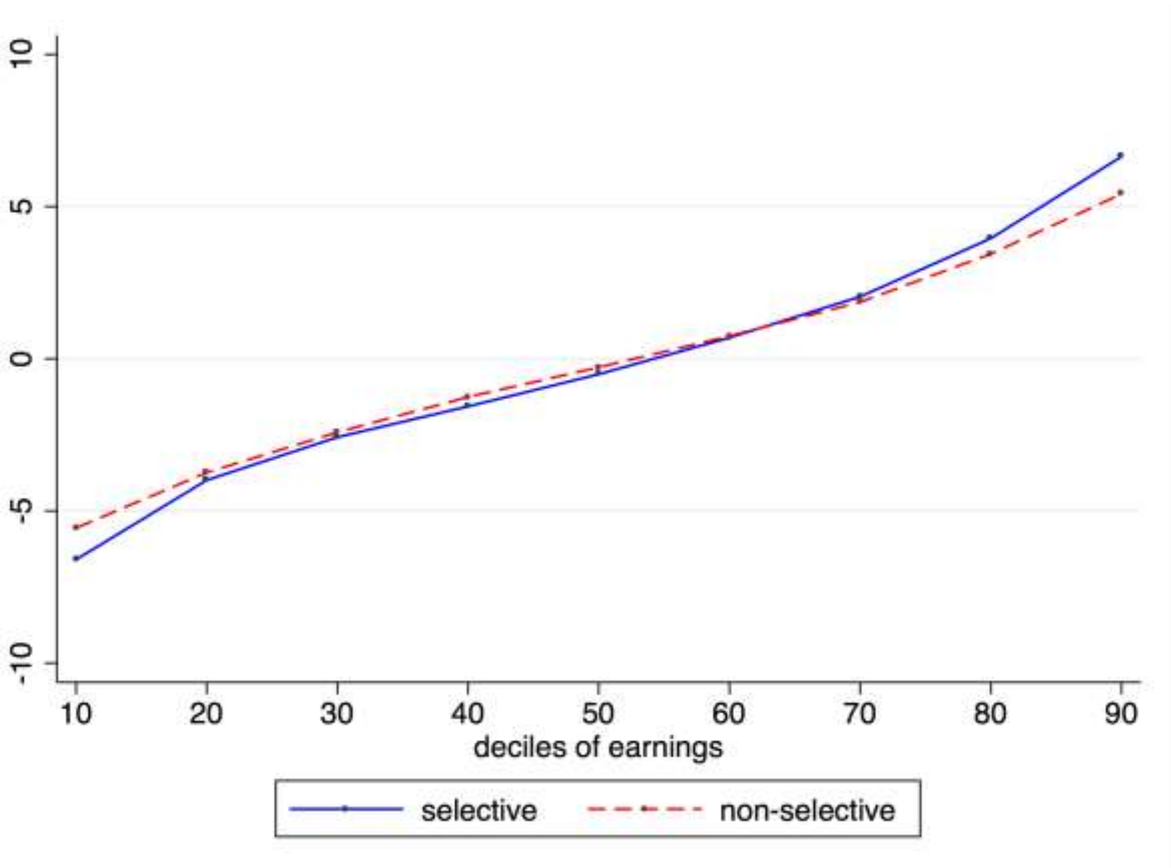
[21] Tables A2a and A3a in the appendix contain the full regression results for the raw and conditional models excluding London, showing how they are very similar to Table 5 both qualitatively and quantitatively. These tables and Figures A2a and A3a show that our results are robust to excluding London and simultaneously changing the definition of selectivity

Table 9: Estimated effects sizes excluding London

	Sample wage gap	A: Without controls			Sample wage gap	B: With controls		
		coeff.	*std. error*	*Effect size*		*coeff.*	*std. error*	*Effect size*
90-10	15.61	2.203	0.868**	14.11	12.17	2.244	0.625***	18.43
75-25	6.42	0.846	0.338**	13.18	5.76	0.297	0.293	5.15

Notes: earnings differentials estimated by testing the linear combination from the simultaneous quantile regressions. The effect size is calculated as the estimated difference divided by the total earnings differential in the sample.

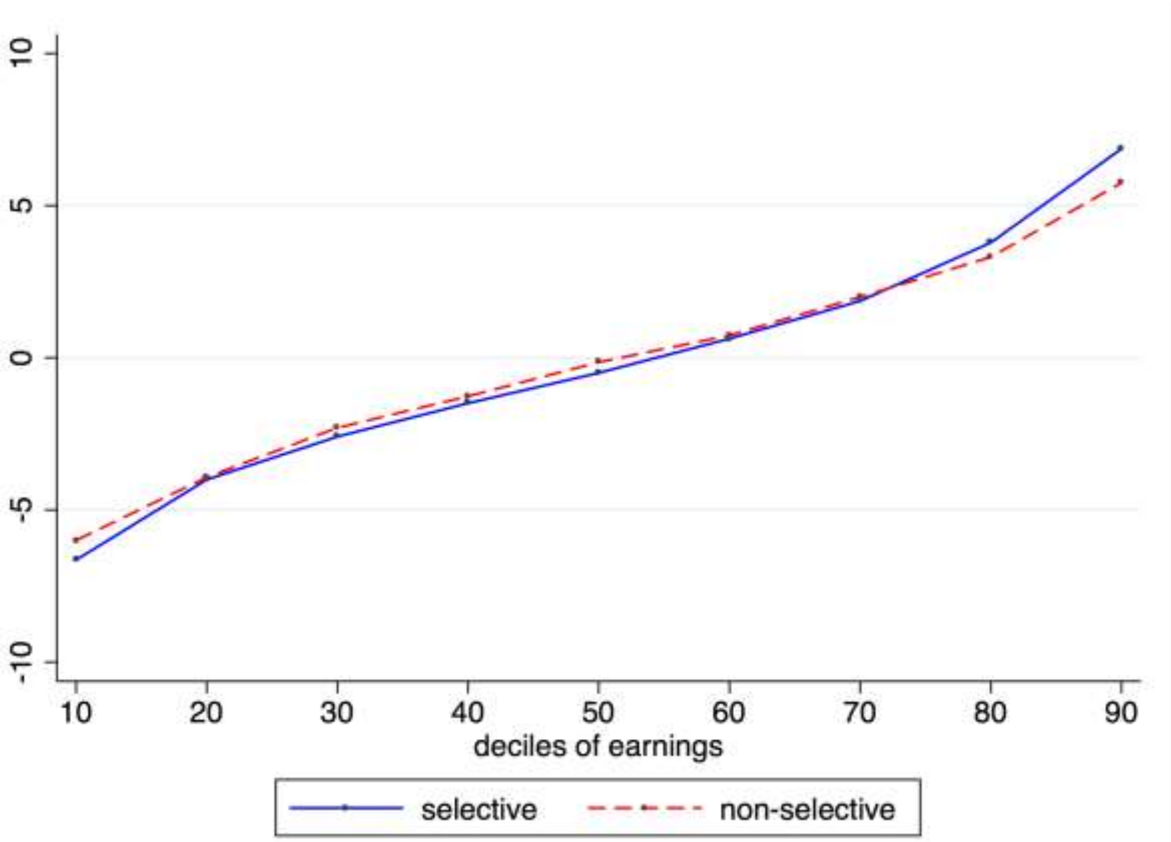
Figure 6: Deciles of the conditional earnings distribution by schooling system type, excluding London



Notes: residuals from a regression of wage on a gender specific quadratic in age, gender, ethnicity, parental occupational class when the individual was 14, parental education, current county of residence and a selective schooling area dummy with the global mean of the residual removed. Before averaging wages for individuals with two wage observations, year of survey effects are removed via a regression. . Source: Understanding Society

To test whether our results are robust to changes in the definition of selective and non-selective areas we redefine selective LEAs as those assigning more than 30% of places by selection whilst retaining the definition of non-selective as those that assign less than 5% by this method. Appendix Tables A2a and A3a shows the quantile regressions for the models with and without controls. The results are qualitatively and quantitatively similar to the corresponding figures in Table 5 (the 90-10 gap in the conditional results is £1.76). Figure 7 illustrates the results of the model with controls and comparison with Figure 3 provides visual confirmation of the robustness of the results. Further robustness tests are illustrated in Figures A2a and A2b (both for the conditional model) and A3a and A3b (raw model), in which the selectivity definition, inclusion/exclusion of London, the inclusion/exclusion of private school percentage from the matching, the treatment of multiple observations for an individual, the treatment of long-term sick/disabled and the restriction of sample years are all tested. The figures all illustrate the same pattern of results and the corresponding quantile regression results in Tables A2a, A2b, A3a and A3b confirm the robustness of our results.

Figure 7: Deciles of the conditional earnings distribution by schooling system type, selective defined as >30% assigned by selection, non-selective <5% assigned by selection



Notes: residuals from a regression of wage on a gender specific quadratic in age, gender, ethnicity, parental occupational class when the individual was 14, parental education, current county of residence and a selective schooling area dummy with the global mean of the residual removed. Before averaging wages for individuals with two wage observations, year of survey effects are removed via a regression . Source: Understanding Society.

As one further robustness test, we take our main estimation sample and re-assign at random the indicator for growing up in an area operating the selective system, though maintaining the same proportion of the sample in each system. We then carry out the same exercise in comparing the distribution on earnings for individuals 'growing up' in each system. As can be seen visually in Appendix A Figure A1, whether the conditional or raw models are used, there is now no significant difference between the distributions at any point – in contrast to our main estimates, which are reproduced to allow comparison. Similarly, Appendix A Table A1 shows the corresponding coefficient estimates, all bar one of which are not significant.[22]

6. Conclusions

In this paper we have investigated the impact on earnings inequality of a selective education system in which school assignment is based on initial test scores. In England, this was the system in place until the 1970s, when the comprehensive system became the norm. Despite this ever-receding historical background, the role of grammar schools continues to be a lively and contentious issue in the current education policy debate. This finds a parallel in the US where a similar literature concerns the merits of exam schools.

We have used a large and representative household panel survey with information on each respondent's childhood to compare adult earnings inequality of those growing up under a selective education system with those educated under a comprehensive system. Controlling for a range of background characteristics and the current labour market, the wage distribution for individuals who grew up in selective schooling areas is quantitatively more unequal, with this difference being statistically significant. The total effect sizes are large: 14% of the raw 90-10 earnings gap and 18% of the conditional 90-10 earnings gap can be explained by schooling system. These results are robust to a wide range of specification checks. In terms of magnitude, we can compare the proportion of the 90-10 earnings gap that is explained by the schooling system, to the general increase in wage inequality in the first part of this century. Between 1997 and 2009, the 90-10 earnings gap increased by almost 60% at a rate of 15 percentage points every three years.[23] Thus our results suggest that the schooling system can explain the equivalent of approximately three to four years of inequality growth, during a time of rapidly growing inequality in hourly earnings (see Gregg, Machin and Salgado, 2014). Given this context, it is clear that the schooling system is responsible for a sizeable share of wage inequality.

[22] One coefficient is significant at the 10% level, which might be expected even in random samples, when estimating 24 coefficients.

[23] Authors' calculations using the Annual Survey of Hours and Earnings, 2013. Percentile values of hourly earnings for 1997-2013 available in Table 5 of 'ASHE 1997 to 2014 selected estimates (Excel sheet 408Kb)' see: http://www.ons.gov.uk/ons/rel/ashe/annual-survey-of-hours-and-earnings/2013-revised-results/index.html

Our modelling framework highlighted the roles of peer groups and school (teacher) quality in magnifying inequality in ability in a selective education system. The evidence on peer effects is mixed, whereas the UK evidence on the wide variation of teacher effectiveness mirrors that in the US (Slater, Burgess and Davies, 2012). It seems likely therefore that the main mechanism generating greater inequality is the sorting of the more effective teachers to the highest ability students. Unfortunately, there is no historical data available to test this, and a comparison of the few contemporary grammar schools in England may not be that relevant to this study.

We have shown that cohorts of students growing up in areas with a selective education system experience greater earnings inequality once in the labour market. If higher earnings inequality is coupled with socially graded access to grammar schools then it seems likely that selective systems will also reinforce inequalities across generations. Previous analysis of the impact of the grammar system as a whole, by Atkinson, Gregg and McConnell (2006), suggested that this is indeed the case. They found that access to grammar schools is heavily skewed towards more affluent families even after taking into consideration prior attainment. High ability children from low-income families are approximately half as likely to go to grammar school as children of comparable ability from better off families (32% versus 60%). This finding has been replicated more recently by Cribb, Sibieta and Vignoles (2013), who find 66% of high achieving children from better-off families attend grammar schools, while only 40% of similarly able but lower income children do. Atkinson *et al* also found that grammar-educated children in selective LEAs outperform similar children in non-selective LEAs on average while non-grammar-educated children in selective LEAs *underperform* compared to similar children in non-selective LEAs. Our findings resonate with this, suggesting that these exam performance disparities carry through into the labour market and are reflected in subsequent earnings. Importantly for the debate around the merits of grammar schools for promoting social mobility, the findings of Atkinson *et al*, taken with our findings, imply that while children from poorer families who make it to the grammar school do well – in exam and earnings terms – those who grow up in selective areas but do not attend the grammar schools do worse than they would have done, had they not been schooled in a selective area. Given the disproportionate numbers of low-income families whose children *do not* make it to the grammar school, the implication is that the selective system is in fact regressive with respect to social mobility.

There may, in theory, be positive effects of earnings inequality that may off-set some of the negative effects identified here – a social welfare function may accept higher earnings inequality as a cost worth paying for generally higher earnings, for example. However, looking at the mean earnings of those growing up in selective versus non-selective areas (which are almost identical) there is no evidence of higher inequality being accompanied by increased average earnings in this case.

Moreover, though it is mostly only for the lower percentiles of the distribution that the negative effect of the selective schooling system is statistically significant, the point estimates are generally negative throughout the distribution up to the 60th or 70th percentile. Thus these findings suggest that the benefits of earnings inequality are narrowly focused on those at the very top of the distribution. Setting up a formal model to weigh the positive and negative effects of earnings inequality is beyond the ambition of this paper. Our contribution is to add a new fact to the debate on grammar schools: selective schooling systems increase inequality.

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Appendix A

Figure A1: Falsification test estimates – randomly assigning selective area status to individuals within the matched main estimation sample. With controls (top) and without (bottom). Notes see Figure 2.

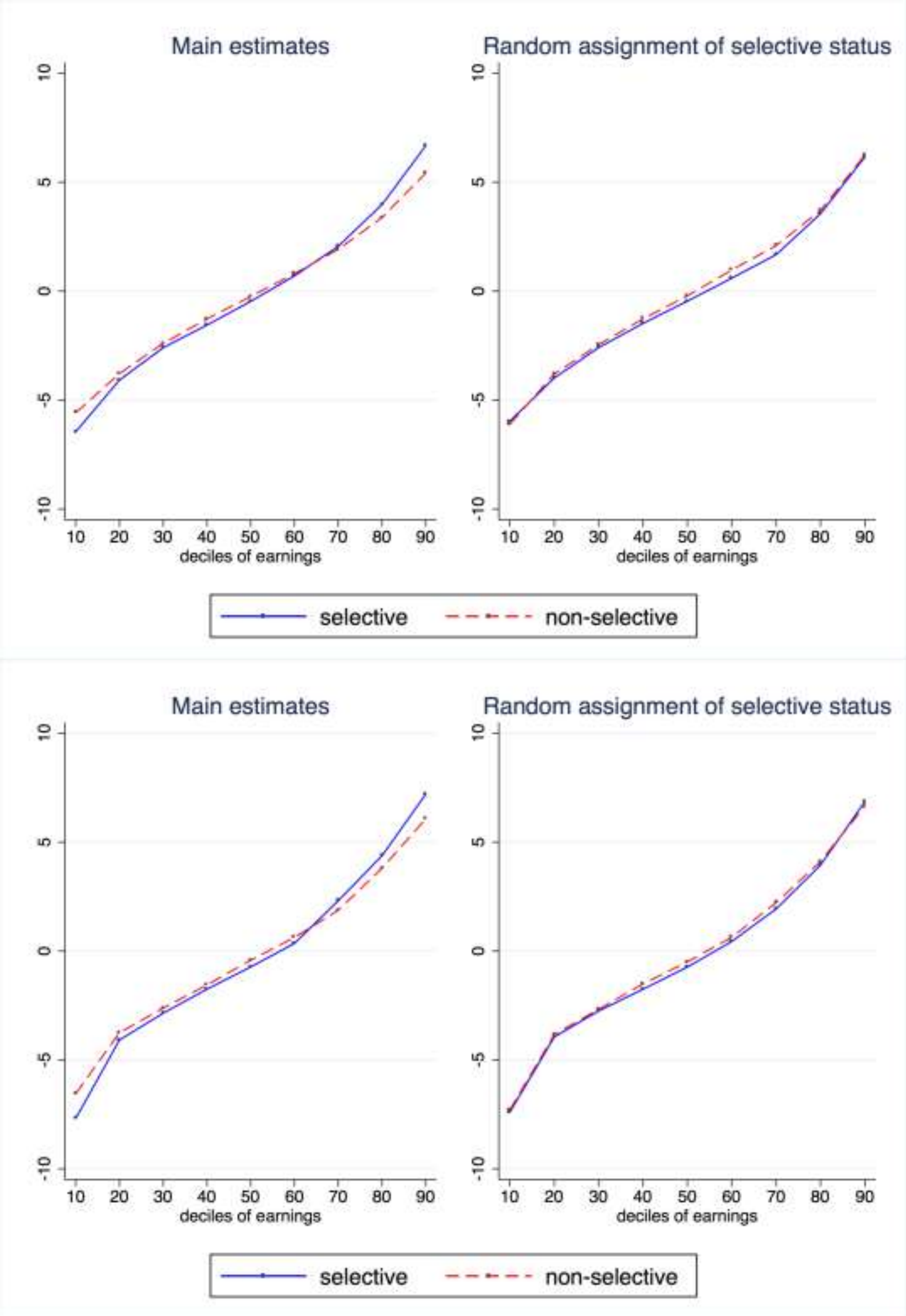
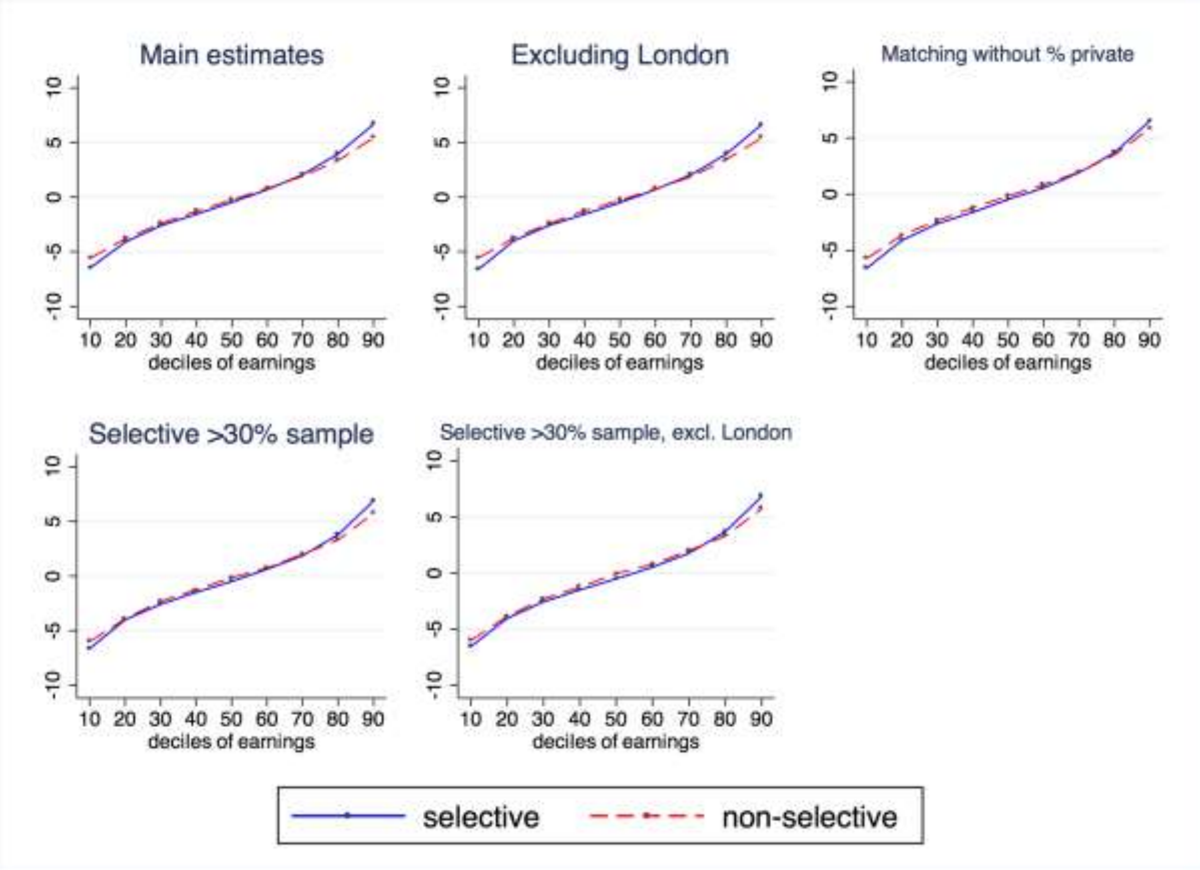
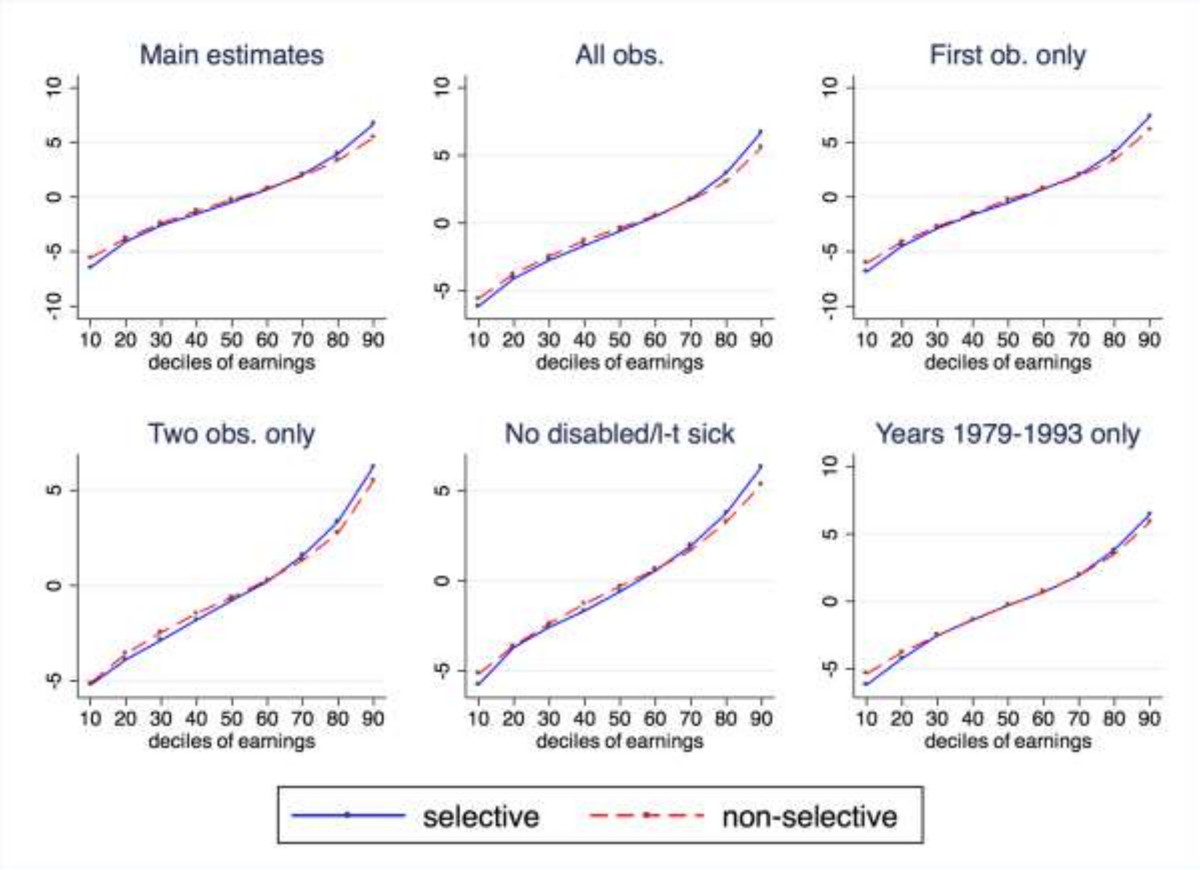


Figure A2a: Deciles of the conditional earnings distribution by schooling system type, robustness analysis



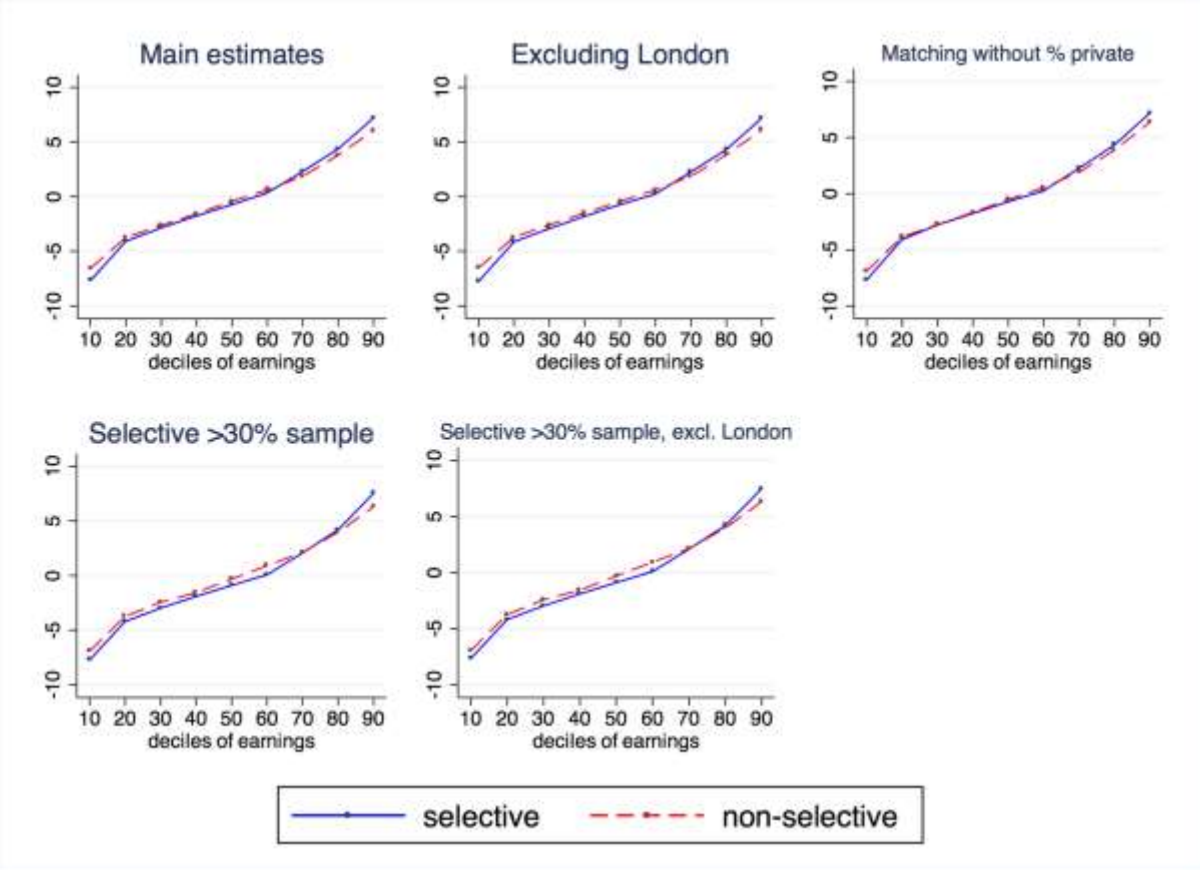
Notes: residuals from a regression of wage on a gender specific quadratic in age, gender, ethnicity, parental occupational class when the individual was 14, parental education, current county of residence and a selective schooling area dummy with the global mean of the residual removed. Before averaging wages for individuals with two wage observations, year of survey effects are removed via a regression. Source: Understanding Society. From top left: “Main estimates” (see Figure 2), “Excluding London” excludes from matching all London LEAs, “Matching without % private” excludes % of private schools from the matching criteria, “Selective >30% sample” defines an area as selective is 30% or more of places are assigned via selection (non-selective if fewer than 5% are), “Selective >30% sample, excl. London” as previous only excluding London LEAs from the matching.

Figure A2b: Deciles of the conditional earnings distribution by schooling system type, further robustness analysis



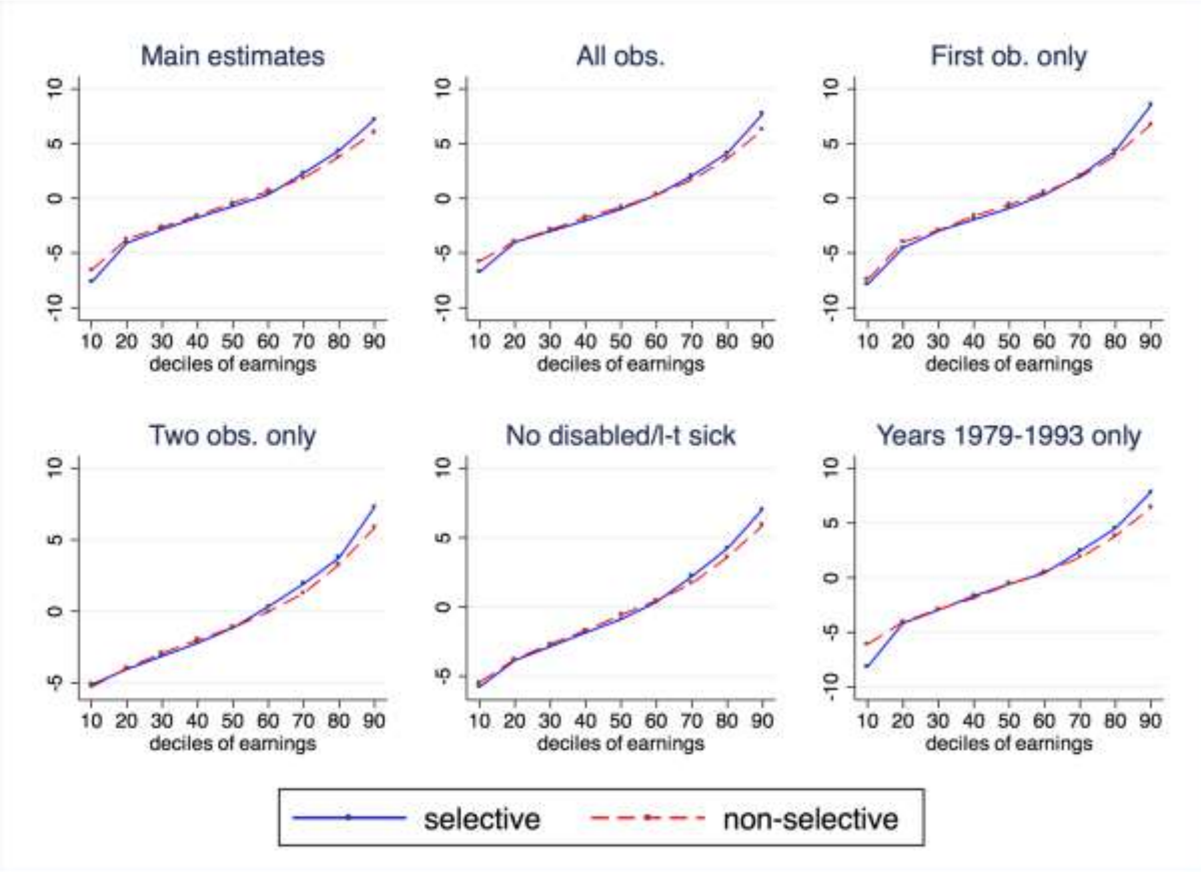
Notes: residuals from a regression of wage on a gender specific quadratic in age, gender, ethnicity, parental occupational class when the individual was 14, parental education, current county of residence and a selective schooling area dummy with the global mean of the residual removed. Before averaging wages for individuals with two wage observations, year of survey effects are removed via a regression. . Source: Understanding Society. From top left: "Main estimates" (see Figure 2), "All obs." includes all of an individuals wage observations if they have more than one (max 2); "First ob. only" includes just the first wage observation of an individual; "Two obs. only" includes only individuals who have two wage observation; "No disabled/l-t sick" excludes the disabled and long-term sick from the non-earner category; "Years 1979-1993 only" only includes individuals turning 13 in these years.

Figure A3a: Deciles of the raw earnings distribution by schooling system type, robustness analysis



Notes: residuals from a regression of wage on a gender specific quadratic in age and a selective schooling area dummy, with the global mean of the residual removed. Before averaging wages for individuals with two wage observations, year of survey effects are removed via a regression. Source: Understanding Society. From top left: “Main estimates” (see Figure 2), “Excluding London” excludes from matching all London LEAs, “Matching without % private” excludes % of private schools from the matching criteria, “Selective >30% sample” defines an area as selective is 30% or more of places are assigned via selection (non-selective if fewer than 5% are), “Selective >30% sample, excl. London” as previous only excluding London LEAs from the matching.

Figure A3b: Deciles of the raw earnings distribution by schooling system type, further robustness analysis



Notes: residuals from a regression of wage on a gender specific quadratic in age and a selective schooling area dummy, with the global mean of the residual removed. Before averaging wages for individuals with two wage observations, year of survey effects are removed via a regression. Source: Understanding Society. From top left: "Main estimates" (see Figure 2), "All obs." includes all of an individuals wage observations if they have more than one (max 2); "First ob. only" includes just the first wage observation of an individual; "Two obs. only" includes only individuals who have two wage observation; "No disabled/l-t sick" excludes the disabled and long-term sick from the non-earner category; "Years 1979-1993 only" only includes individuals turning 13 in these years.

Table A1: Quantile Regression estimates of selective schooling effect on wages, selective defined as >20% assigned by selection, non-selective <5% assigned by selection, falsification test estimates – randomly assigning selective area status to individuals within the matched main estimation sample

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	With Controls			Without controls		
Percentile	*coeff.*	*std. err.*	*t*	*coeff.*	*std. err.*	*t*
10	0.132	0.420	0.31	-0.067	0.523	-0.13
20	-0.188	0.305	-0.62	-0.101	0.279	-0.36
25	-0.046	0.296	-0.15	-0.172	0.215	-0.80
30	-0.145	0.261	-0.55	-0.083	0.229	-0.36
40	-0.217	0.204	-1.06	-0.249	0.227	-1.10
50	-0.239	0.217	-1.10	-0.234	0.219	-1.07
60	-0.361	0.231	-1.56	-0.189	0.293	-0.64
70	-0.409*	0.231	-1.77	-0.284	0.308	-0.92
75	-0.214	0.286	-0.75	-0.374	0.379	-0.99
80	-0.150	0.289	-0.52	-0.164	0.368	-0.45
90	-0.089	0.411	-0.22	0.195	0.514	0.38
	N=2511			N=2511		

Table A2a: Quantile Regression estimates of selective schooling effect on wages, various robustness checks, conditional specification in all columns, selective defined as >20% assigned by selection, non-selective <5% assigned by selection (except columns 4 and 5)

	(1) Main		(2) Excluding London		(3) Matching without % private		(4) Selective >30%		(5) Selective >30%, excluding London	
	coeff.	*std. err.*	*coeff.*	*std. err.*	*coeff.*	*std. err.*	*coeff.*	*std. err.*	*coeff.*	*std. err.*
10	-0.897**	0.383	-1.041***	0.341	-0.829**	0.333	-0.642	0.414	-0.544*	0.330
20	-0.295	0.267	-0.244	0.251	-0.453*	0.258	-0.070	0.300	-0.188	0.285
25	-0.068	0.242	-0.067	0.210	-0.186	0.248	-0.186	0.352	-0.238	0.309
30	-0.199	0.248	-0.160	0.226	-0.303*	0.168	-0.293	0.336	-0.260	0.295
40	-0.267	0.243	-0.308	0.200	-0.386*	0.204	-0.228	0.247	-0.250	0.232
50	-0.237	0.215	-0.223	0.212	-0.277*	0.160	-0.402	0.288	-0.457	0.301
60	-0.106	0.251	-0.058	0.226	-0.294	0.199	-0.107	0.281	-0.250	0.285
70	0.144	0.260	0.130	0.281	-0.036	0.241	-0.166	0.333	-0.237	0.347
75	0.239	0.330	0.230	0.296	0.000	0.238	-0.075	0.329	-0.023	0.347
80	0.595**	0.280	0.493*	0.272	0.177	0.254	0.473	0.367	0.389	0.426
90	1.308***	0.449	1.204**	0.537	0.615	0.411	1.121*	0.599	1.061*	0.625
N	2511		2434		2863		1735		1722	

Notes: residuals from a regression of wage on a gender specific quadratic in age, gender, ethnicity, parental occupational class when the individual was 14, parental education, current county of residence and a selective schooling area dummy with the global mean of the residual removed. In all cases, year of survey effects are removed via an initial regression. Source: Understanding Society. Column (1) main estimation results from Table 5; (2) “Excluding London” excludes from matching all London LEAs; (3) “Matching without % private” excludes % of private schools from the matching criteria; (4) “Selective >30%” defines an area as selective is 30% or more of places are assigned via selection (non-selective if fewer than 5% are); (5) “Selective >30%, excluding London” as column (4) but also excludes from matching all London LEAs.

Table A2b: Quantile Regression estimates of selective schooling effect on wages, various robustness checks, conditional specification in all columns, selective defined as >20% assigned by selection, non-selective <5% assigned by selection

	(1) Main		(2) All observations included per individual		(3) Just first observation per individual		(4) Just individuals with two observations		(5) Disabled/long-term sick not in non-earner category		(6) Years 1979-1993 only included	
	coeff.	*std. err.*	*coeff.*	*std. err.*	*coeff.*	*std. err.*	*coeff.*	*std. err.*	*coeff.*	*std. err.*	*coeff.*	*std. err.*
10	-0.897**	0.383	-0.555**	0.283	-0.788***	0.263	-0.102	0.199	-0.596*	0.363	-0.838**	0.424
20	-0.295	0.267	-0.348*	0.189	-0.393	0.267	-0.312*	0.168	-0.083	0.224	-0.465	0.355
25	-0.068	0.242	-0.304*	0.162	-0.288	0.245	-0.232*	0.142	-0.068	0.204	-0.039	0.388
30	-0.199	0.248	-0.288*	0.161	-0.172	0.208	-0.398**	0.188	-0.188	0.218	0.035	0.284
40	-0.267	0.243	-0.364**	0.162	-0.124	0.237	-0.344**	0.171	-0.401**	0.196	-0.044	0.326
50	-0.237	0.215	-0.234	0.149	-0.298*	0.179	-0.158	0.162	-0.288	0.220	0.011	0.278
60	-0.106	0.251	-0.071	0.151	-0.067	0.192	-0.084	0.182	-0.067	0.205	-0.020	0.327
70	0.144	0.260	0.072	0.194	0.135	0.207	0.254	0.219	0.231	0.293	-0.030	0.381
75	0.239	0.330	0.314	0.220	0.321	0.364	0.441**	0.211	0.179	0.289	0.120	0.433
80	0.595**	0.280	0.652***	0.229	0.666**	0.289	0.601*	0.348	0.486*	0.294	0.334	0.374
90	1.308***	0.449	1.134***	0.417	1.326**	0.524	0.751	0.506	0.949**	0.481	0.612	0.661
N	2511		4132		2511		3233		2403		1373	

Notes: residuals from a regression of wage on a gender specific quadratic in age, gender, ethnicity, parental occupational class when the individual was 14, parental education, current county of residence and a selective schooling area dummy with the global mean of the residual removed. In all cases, year of survey effects are removed via an initial regression. Source: Understanding Society. Column (1) main estimation results from Table 5; (2) “All observations included per individual” includes two observations for those with two observations, clustering at the individual level; (3) “Just first observation per individual”, includes one observation per individual, their first; (4) “Just individuals with two observations, includes only those individuals who have two observations; (5) “Disabled/long-term sick not in non-earner category” excludes the disables/long-term sick from the estimations; (6) “Years 1979-1993 only included” uses only 1979-1993 as year individuals are age 13 since these are the years when the local wages and unemployment rate data are exactly contemporary, note this reduces the sample size substantially.

Table A3a: Quantile Regression estimates of selective schooling effect on wages, various robustness checks, basic specification in all columns, selective defined as >20% assigned by selection, non-selective <5% assigned by selection (except columns 4 and 5)

	(1) Main		(2) Excluding London		(3) Matching without % private		(4) Selective >30%		(5) Selective >30%, excluding London	
	coeff.	*std. err.*	*coeff.*	*std. err.*	*coeff.*	*std. err.*	*coeff.*	*std. err.*	*coeff.*	*std. err.*
10	-1.143*	0.605	-1.136	0.714	-0.823*	0.516	-0.832	0.777	-0.723	0.859
20	-0.336	0.229	-0.425**	0.212	-0.215	0.201	-0.444*	0.267	-0.454	0.318
25	-0.224	0.215	-0.302	0.209	-0.031	0.192	-0.423**	0.200	-0.428*	0.251
30	-0.237	0.219	-0.332*	0.198	-0.039	0.176	-0.516**	0.213	-0.522**	0.260
40	-0.196	0.198	-0.277	0.189	-0.089	0.213	-0.348*	0.208	-0.342	0.251
50	-0.310*	0.189	-0.299	0.194	-0.182	0.21	-0.571**	0.250	-0.579**	0.242
60	-0.275	0.260	-0.372	0.255	-0.290	0.296	-0.822**	0.338	-0.841**	0.332
70	0.439	0.280	-0.350	0.331	0.356	0.325	-0.031	0.397	-0.036	0.456
75	0.748**	0.373	0.544	0.364	0.410	0.346	-0.044	0.404	-0.115	0.435
80	0.584	0.387	0.481	0.378	0.457	0.352	0.206	0.407	0.215	0.401
90	1.136**	0.500	1.067**	0.469	0.816*	0.489	1.241*	0.651	1.109**	0.578
N	2511		2434		2863		1735		1722	

Notes: residuals from a regression of wage on a gender specific quadratic in age, gender, ethnicity, parental occupational class when the individual was 14, parental education, current county of residence and a selective schooling area dummy with the global mean of the residual removed. In all cases, year of survey effects are removed via an initial regression. Source: Understanding Society. Column (1) main estimation results from Table 5; (2) "Excluding London" excludes from matching all London LEAs; (3) "Matching without % private" excludes % of private schools from the matching criteria; (4) "Selective >30%" defines an area as selective is 30% or more of places are assigned via selection (non-selective if fewer than 5% are); (5) "Selective >30%, excluding London" as column (4) but also excludes from matching all London LEAs.

Table A3b: Quantile Regression estimates of selective schooling effect on wages, various robustness checks, basic specification in all columns, selective defined as >20% assigned by selection, non-selective <5% assigned by selection (except column 1)

	(1) Main		(2) All observations included per individual		(3) Just first observation per individual		(4) Just individuals with two observations		(5) Disabled/long-term sick not in non-earner category		(6) Years 1979-1993 only included	
	coeff.	*std. err.*	*coeff.*	*std. err.*	*coeff.*	*std. err.*	*coeff.*	*std. err.*	*coeff.*	*std. err.*	*coeff.*	*std. err.*
10	-0.723	0.859	-0.989*	0.541	-0.433	0.396	0.166	0.221	-0.326	0.521	-2.064***	0.776
20	-0.454	0.318	-0.064	0.152	-0.517*	0.320	-0.088	0.125	-0.075	0.161	-0.120	0.333
25	-0.428*	0.251	-0.120	0.133	-0.163	0.186	-0.139	0.156	-0.133	0.173	0.046	0.309
30	-0.522**	0.260	-0.135	0.166	-0.064	0.203	-0.222	0.162	-0.181	0.173	-0.056	0.366
40	-0.342	0.251	-0.306*	0.162	-0.333	0.215	-0.212	0.160	-0.151	0.179	0.179	0.285
50	-0.579	0.242	-0.167	0.147	-0.309	0.212	-0.012	0.233	-0.300*	0.184	0.021	0.306
60	-0.841**	0.332	0.005	0.231	-0.237	0.278	0.358	0.248	-0.109	0.259	-0.117	0.347
70	-0.036	0.456	0.372*	0.228	0.153	0.309	0.656**	0.268	0.496**	0.232	0.588	0.504
75	-0.115	0.435	0.592**	0.235	0.495	0.337	0.632**	0.335	0.635**	0.320	0.617	0.496
80	0.215	0.401	0.502*	0.273	0.425	0.443	0.447	0.284	0.624*	0.330	0.678	0.503
90	1.109*	0.578	1.463***	0.401	1.820***	0.569	1.465***	0.434	1.136**	0.562	1.438*	0.832
N	1722		4132		2511		3233		2403		1373	

Notes: residuals from a regression of wage on a gender specific quadratic in age, gender, ethnicity, parental occupational class when the individual was 14, parental education, current county of residence and a selective schooling area dummy with the global mean of the residual removed. In all cases, year of survey effects are removed via an initial regression. Source: Understanding Society. Column (1) main estimation results from Table 5; (2) “All observations included per individual” includes two observations for those with two observations, clustering at the individual level; (3) “Just first observation per individual”, includes one observation per individual, their first; (4) “Just individuals with two observations, includes only those individuals who have two observations; (5) “Disabled/long-term sick not in non-earner category” excludes the disables/long-term sick from the estimations; (6) “Years 1979-1993 only included” uses only 1979-1993 as year individuals are age 13 since these are the years when the local wages and unemployment rate data are exactly contemporary, note this reduces the sample size substantially.

Table A4: Regression of Wage on Covariates for Conditional Specification

Dependent variable: hourly wage	*Coefficient.*	*Std. Err.*	*t*
born in grammar area dummy	0.029	0.273	0.11
age	1.082	0.297	3.65***
age^2	-0.013	0.004	-3.30***
female dummy	5.939	7.408	0.80
age*female dummy	-0.319	0.402	-0.79
age^2*female dummy	0.003	0.005	0.61
ethnicity: white	0.298	0.471	0.63
father occupational class group 1	2.258	0.507	4.46***
father occupational class group 2	1.375	0.555	2.48**
father occupational class group 3	1.458	0.542	2.69***
father occupational class group 4	2.382	0.657	3.62***
father occupational class group 5	0.683	0.447	1.53
father occupational class group 6	0.057	1.098	0.05
father occupational class group 7	0.114	0.766	0.15
father occupational class group 8	0.296	0.481	0.62
father occupational class group 10	-0.663	0.487	-1.36
mother occupational class group 1	0.561	0.586	0.96
mother occupational class group 2	1.356	0.475	2.85***
mother occupational class group 3	1.294	0.716	1.81*
mother occupational class group 4	1.270	0.415	3.06***
mother occupational class group 5	-0.671	0.665	-1.01
mother occupational class group 6	0.448	0.485	0.92
mother occupational class group 7	0.257	0.485	0.53
mother occupational class group 8	0.627	0.630	1.00
mother occupational class group 10	0.129	0.348	0.37
father’s education: some qualifications	-0.101	0.320	-0.32
father’s education: university	0.565	0.532	1.06
father’s education: unknown	-1.234	0.493	-2.50**
father’s education: missing	3.130	1.921	1.63
mother’s education: some qualifications	0.569	0.312	1.82*
mother’s education: university	1.591	0.601	2.65***
mother’s education: unknown	-0.908	0.553	-1.64
mother’s education: missing	-2.756	1.917	-1.44
constant	-18.268	7.621	-2.40**
N	2511		
R^2	0.1675		

Notes: also included dummies (84) for current county region. Parental occupation class groups refer to when the individual was aged 14 and the categories are: 1 “managers or administrators”, 2”professional occupations”, 3 “associate professional or technical occupations”, 4”clerical/secretarial occupations”, 5”craft and related occupations”, 6 ”personal/protective services”, 7 “sales occupations”, 8 “plant and machine operatives”, 9 “other unskilled”, 10 “missing”. Omitted categories: born in comprehensive area, male, non-white, father and mother occupational class “other unskilled”, father and mother education “no qualifications”. An initial regression is run to remove the year of survey effects.

Appendix B

Table B1: Indicators of covariate balancing following matching, summary statistics over the 23 sets of tests derived from the 23 separate by year matches

	absolute value of *t*-statistic				*p*-value			
	mean	min	median	max	mean	min	median	max
Private school attendance proportion	0.439	0.010	0.410	1.280	0.688	0.209	0.694	0.994
Local unemployment rate	0.417	0.050	0.330	1.100	0.698	0.280	0.748	0.961
Local male hourly wage rate	0.445	0.040	0.370	1.390	0.684	0.176	0.719	0.972

For each row the reported statistics refer to the test of the null hypothesis that the mean of the variable is the same in the selective and non-selective areas. These are the mean, min, median and max of the values over the 23 years of data.

Table B2: Indicators of covariate balancing following matching, by year, Treated (selective), Control (non-selective)

	Mean				Mean			
1974	Treated	Control	t-test	p-value	1975 Treated	Control	t-test	p-value
Private school attendance proportion	7.15	8.04	-0.62	0.538	7.11	8.88	-1.28	0.209
Local unemployment rate	4.91	4.70	0.57	0.575	4.76	4.42	1.10	0.280
Local male hourly wage rate	103.57	105.53	-1.05	0.302	130.28	133.28	-1.39	0.176

1976	Treated	Control	t-test	p-value	1977 Treated	Control	t-test	p-value
Private school attendance proportion	7.21	7.20	0.01	0.994	6.82	7.99	-0.70	0.496
Local unemployment rate	4.55	4.45	0.38	0.710	4.54	4.28	0.69	0.504
Local male hourly wage rate	158.96	156.39	0.73	0.469	172.14	171.50	0.16	0.876

1978	Treated	Control	t-test	p-value	1979 Treated	Control	t-test	p-value
Private school attendance proportion	7.26	7.48	-0.15	0.886	7.34	8.28	-0.52	0.614
Local unemployment rate	4.75	4.43	0.70	0.492	4.76	4.42	0.79	0.444
Local male hourly wage rate	194.81	193.38	0.36	0.724	221.54	219.65	0.37	0.719

1980	Treated	Control	t-test	p-value
Private school attendance proportion	8.12	8.64	-0.33	0.749
Local unemployment rate	6.72	6.41	0.32	0.755
Local male hourly wage rate	276.78	273.07	0.32	0.757

1981	Treated	Control	t-test	p-value
Private school attendance proportion	8.87	7.55	0.62	0.559
Local unemployment rate	9.73	10.98	-0.61	0.562
Local male hourly wage rate	318.93	313.36	0.29	0.783

1982	Treated	Control	t-test	p-value
Private school attendance proportion	8.87	9.01	-0.19	0.852
Local unemployment rate	10.18	9.23	0.85	0.427
Local male hourly wage rate	352.78	355.93	-0.36	0.730

1983	Treated	Control	t-test	p-value
Private school attendance proportion	8.87	8.69	0.13	0.903
Local unemployment rate	10.40	10.48	-0.05	0.961
Local male hourly wage rate	379.98	386.27	-0.47	0.657

1984	Treated	Control	t-test	p-value
Private school attendance proportion	8.55	8.63	-0.09	0.927
Local unemployment rate	10.92	10.65	0.20	0.848
Local male hourly wage rate	400.82	400.27	0.04	0.972

1985	Treated	Control	t-test	p-value
Private school attendance proportion	8.55	8.28	0.46	0.658
Local unemployment rate	10.90	10.81	0.08	0.936
Local male hourly wage rate	431.66	438.32	-0.47	0.654

1986	Treated	Control	t-test	p-value
Private school attendance proportion	8.55	9.67	-0.79	0.450
Local unemployment rate	10.32	10.05	0.19	0.853
Local male hourly wage rate	473.92	465.05	0.52	0.619

1987	Treated	Control	t-test	p-value
Private school attendance proportion	8.55	9.00	-0.51	0.622
Local unemployment rate	7.78	7.52	0.29	0.778
Local male hourly wage rate	511.74	502.69	0.49	0.640

1988	Treated	Control	t-test	p-value
Private school attendance proportion	8.55	8.95	-0.22	0.828
Local unemployment rate	5.20	5.05	0.20	0.846
Local male hourly wage rate	560.76	545.13	0.50	0.631

1989	Treated	Control	t-test	p-value
Private school attendance proportion	8.55	7.80	0.59	0.572
Local unemployment rate	3.66	3.55	0.17	0.871
Local male hourly wage rate	612.30	604.87	0.20	0.847

1990	Treated	Control	t-test	p-value
Private school attendance proportion	8.55	8.47	0.06	0.956
Local unemployment rate	4.34	4.24	0.19	0.857
Local male hourly wage rate	682.92	657.21	0.69	0.513

1991	Treated	Control	t-test	p-value
Private school attendance proportion	8.55	9.53	-0.80	0.447
Local unemployment rate	8.26	7.95	0.33	0.748
Local male hourly wage rate	748.84	740.51	0.23	0.825

1992	Treated	Control	t-test	p-value
Private school attendance proportion	8.55	8.96	-0.23	0.827
Local unemployment rate	10.42	10.07	0.27	0.794
Local male hourly wage rate	788.80	791.13	-0.04	0.967

1993	Treated	Control	t-test	p-value
	8.55	8.23	0.31	0.766
	10.16	10.00	0.15	0.884
	819.20	798.93	0.47	0.651

1994	Treated	Control	t-test	p-value
Private school attendance proportion	8.55	7.25	0.95	0.369
Local unemployment rate	10.16	9.59	0.55	0.596
Local male hourly wage rate	834.20	798.53	0.77	0.462

1995	Treated	Control	t-test	p-value
	8.55	7.69	0.41	0.694
	10.16	9.75	0.37	0.722
	874.00	866.07	0.17	0.866

1996	Treated	Control	t-test	p-value
Private school attendance proportion	8.55	8.84	-0.12	0.909
Local unemployment rate	10.16	9.53	0.54	0.602
Local male hourly wage rate	935.40	928.40	0.15	0.887

Table B3: Indicators of covariate balancing for selective and non-selective areas following matching, by year

	Pseudo-R2	LR chi2	p>chi2
1974	0.027	1.40	0.706
1975	0.058	2.59	0.460
1976	0.041	1.60	0.659
1977	0.056	1.25	0.741
1978	0.044	0.98	0.806
1979	0.047	0.92	0.821
1980	0.102	1.42	0.701
1981	0.121	1.35	0.718
1982	0.091	1.01	0.800
1983	0.066	0.73	0.865
1984	0.021	0.30	0.961
1985	0.083	1.15	0.765
1986	0.087	1.20	0.752
1987	0.105	1.45	0.693
1988	0.117	1.63	0.653
1989	0.064	0.89	0.827
1990	0.160	2.22	0.528
1991	0.081	1.12	0.773
1992	0.010	0.14	0.986
1993	0.027	0.38	0.945
1994	0.109	1.51	0.679
1995	0.022	0.31	0.958
1996	0.047	0.65	0.885
Average	0.069	1.139	0.769

Pseudo R2 from a regression of the conditional treatment probability (propsensity score) on covariates after matching. Likelihood Ratio chi2 test statistic and corresponding p-value for test of the null hypothesis that the covariates are jointly insignificant.