Do grammar schools increase or reduce inequality?[1]

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Abstract

There is much debate on both sides of the Atlantic concerning the merits of schooling systems that select students on the basis of academic ability. In the UK the debate concerns the existing inequality in access to high quality schools and whether a selective (grammar school) system is better at reducing inequality and promoting social mobility than a system where proximity determines access to schools. In the latter case (the comprehensive system), variation in school quality induces variation in local house-prices and this can act as a bar, preventing poorer students from accessing the higher quality schools. Proponents of the selective system – which sees the highest ability students attending the elite "grammar schools" – suggest that it is a pro-social mobility policy option, allowing bright students from poorer backgrounds to access the best schools. Unlike the existing literature, rather than focusing on the impact of grammar attendance (or not) on the marginal student who just passes (fails) the selection exam, this paper considers the impact of the grammar school system on the level of inequality in the whole of the earnings distribution later in life. We find that the wage distribution of individuals who grew up in areas operating a selective schooling system is significantly more unequal than that of individuals who grew up in areas with the comprehensive system.

JEL Classifications: I24, J31

Key words: selective schooling, inequality, wages

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1. Introduction

Grammar schools continue to be a prominent issue in policy proposals in England. More generally, the debate continues about inequality in access to high quality schools, and the best system for the assignment of students to schools to reduce the inequality. A grammar school system assigns students on the basis of a test score (in England this is taken at age 11) with passing students typically attending grammar schools while failing students typically attend secondary moderns. In non-grammar schools systems, there is no selection on test scores, with all students theoretically having the same choice set of schools.

The proponents of grammar schools argue that they enhance social mobility by offering poor, bright students a route to attend the best schools, although evidence on access to grammar schools suggests that children from deprived families are less likely to attend a grammar school, even conditional on attainment at age 11 (Cribbs et. al., 2013). Indeed, a quick look at the league tables of attainment gaps between Free School Meals (FSM)[2] and non-FSM children at age 16 (Key Stage 4 in the English schooling system) by Local Education Authority (LEA) confirms that those LEAs that are still selective today are commonly found to be poor performers in terms of educational inequality[3].

Alongside this debate on access to grammar schools, much of the previous research on this topic has considered the impact of attending a grammar school on the marginal student's attainment[4], focusing on the shorter and longer term benefits to those students who 'make it' to a grammar school. These types of analysis commonly use regression discontinuity designs to compare the outcomes of students just above and just below the entrance test threshold to access selective schools. However these studies say little about differences across selective and non-selective systems. Here we move away from the debate on fair access to grammar schools and considering the benefits to the marginal student to focus on a more systemic question: does a grammar school *system* increase or reduce inequality?

We use data from a large-scale household panel dataset to study students growing up in England, some in grammar schools areas, others not. This allows us to examine the effect of growing up in a selective schooling area on their earnings observed later in life. The richness

[2] Free school meal eligibility is based on parental resources and is a standard marker of disadvantage in the UK.

[3] https://docs.google.com/spreadsheet/ccc?key=0At6CC4x_yBnMdDRBUEM1UEFZOVptOHI0amRDaG1SQXc&usp=sharing#gid=3 (accessed 28th January, 12.08pm).

[4] Clarke (2010), Abdulkadiroglu, Angrist and Pathak (2011), Dobbie and Fryer (2011)

of the data means that we can control for the parental background of the individual. We compare the spread of the earnings distribution in middle age of those growing up in selective schooling areas and those growing up in very similar non-selective areas, controlling for parental background and the labour market characteristics of the local area where the individual now lives.

We find evidence that individuals who grew up in an area operating a selective schooling system have a more unequal wage distribution in later life, compared with individuals who grew up in areas without selection. 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. There are both winners and losers from the grammar system: the additional earnings differential between the 90th and 10th percentile in selective systems accounts for 12-15% of the total 90-10 earnings gap. This is in line with evidence from Atkinson, Gregg and McConnell (2006) who find that in selective LEAs grammar-educated pupils perform considerably better at age 16 than their non-selective LEA counterparts while non-grammar educated pupils do worse.

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

As noted, much of the previous literature on selective schools tends to focus on the benefit to the marginal student of attending a grammar school compared to not attending. In the UK, Clark (2010) uses access data from East Ridings (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 advanced courses and 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 in the 1950s. They find large effects on educational attainment, and for women there are long-run impacts 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 positive effects on the rigour of the types of courses taken.

Sullivan and Heath (2002) and Galindo-Rueda and Vignoles (2005) used the 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 LEA at the time, arguing that the political power of the constituency 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.

These studies say little about differences across selective and non-selective systems. Atkinson, Gregg and McConnell (2006) and Jesson (2000) use more recent NPD/PLASC data 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 grammar school system, assigning individuals to schools based on their performance on a test, is one way of assigning students to schools. In order to compare outcomes from this system to others we consider the main alternatives, namely neighbourhood schooling (each student goes to her local school) and school choice. In England, the latter involves students nominating a number of school preferences. 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[5] and one allocates pupils to schools based on proximity.

Why should one system imply greater inequality? Here is a very simple ideal-type framework for thinking about this:

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 quality, $\bar{a}$:

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

Later adult earnings depend on both the ability of the student and their 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

[5] 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.

system implies for $\bar{a}(a)$ and $q(a)$ – that is, how each system relates student ability to peer group ability and student ability to teacher quality.

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

Here we assume that the high quality schools are randomly distributed around an area. However, because of the proximity rule, families with high level of resources (high r) cluster around the good schools, so the quality of the school is related to 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: $y_i = a_i + \gamma . s_c(a_i)$.

Returning to how these systems impact on earnings, they are determined by: $y_i = a_i + \gamma . s_k(a_i)$ where k = g (grammar) or c (comprehensive).

What is $var_g\big(y(a_i)\big)$and $var_c\big(y(a_i)\big)$? The variance of a function of a, can be approximated by $var\big(f(a)\big) = \{f'\big(E(a)\big)\}^2 . \sigma_a^2$, hence the variances can be characterised as:

$var_k\big(y(a_i)\big) = \{\big(1 + \gamma s'(\mu_a)\big)\}^2 \sigma_a^2$, where k = g (grammar) or c (comprehensive).

Consequently, $var_g(y)$ < or > $var_c(y)$ depending on whether $\frac{\partial s_g(a)}{\partial a}$ < or > $\frac{\partial s_c(a)}{\partial a}$.

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 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 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 waves in our analysis.

Defining selectivity

We begin by defining LEAs at 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 Local Education Authority then the aggregated LEA data is used to calculate the percentage of children aged 13[6] in the LEA who had a place allocated by the selective system (grammar or secondary modern places)[7]. The time-series of 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 at the 1983 level henceforth.

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.

Matching

Having defined selectivity, we proceed by matching selective and non-selective LEAs 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]. We select the three nearest neighbour non-selective LEAs for each selective LEA and

[6] 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.

[7] 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 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 (for example in Understanding Society: mean of 0.66 percentage points and a median of 0.22 percentage points).

[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.

retain only matches that share common support. Individuals turned 13 in a number of different years in our data and hence the matching of LEAs is done separately for each year 1974 to 1996. Following the matching, we retain individuals who grew up in one of the selective or matched non-selective LEAs.

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 distance and in the majority of cases three or fewer)[11].

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.

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: families may cross-borders and therefore individuals may be educated in an alternative system and families may move areas between birth and the start of secondary school. With regards to the first issue, that some children cross LEA borders to attend schools in an alternative system to that in which they live, 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

[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.

boroughs are close together and there is therefore less distinction between boroughs. 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 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 systems 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 secondary moderns.

To consider the second issue, that families may move areas, we use data from two birth cohort studies, born in 1970 and 2000, and the National Pupil Database to investigate the extent to which we can observe families moving from birth to starting secondary school. The birth cohort studies provide information from birth to age 10 in the British Cohort Study (BCS) and from birth to age 7 in the Millennium Cohort Study (MCS), both at Government Office Region (GOR) level. The National Pupil Database 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 8.6 per cent in the BCS and 5.5 per cent in the MCS moving before the cohort member is 5.

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 57.1% of those growing up in selective LEAs moving while 43.5% of those growing up in non-selective LEAs move. 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.

Measuring earnings inequality

We use our individuals from selective and non-selective LEAs to compare their earnings distributions in adulthood. Hourly wages are 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[13] 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 that individual as a single observation. This averaging moves us towards a more permanent rather than transitory measure of individuals earnings. Sixty percent of the main estimation sample (1,469 of 2,455 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), our conditional model, we run an OLS regression controlling for a vector of family background characteristics, X_{ir}, including gender, ethnicity, parental occupational class, parental education, year of the survey (2009-2012) and also include fixed effects for the current local area, as well as the quadratic age*gender controls. Our aim by conditioning on these additional characteristics is

[13] Results are robust to the exclusion of the long-term sick and disabled.

to minimise the impact of any other factors that could account for differences in the variation of earnings by the selectivity of the school system in the area the individual was born.

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

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, adjusted for age and then additional factors. 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[14]. We use unconditional simultaneous quantile regressions (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.

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

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 are very similar across the two groups although slightly (insignificantly) higher amongst those from the selective areas (£8.50 versus £8.47). The variance of earnings is considerably higher for those growing up in selective areas (£36.21 versus £26.34). Figure 2 illustrates the impact of selective schooling across the entirety of the 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. Out of those who do make it to the top of the earnings distribution, individuals who grew up in areas operating a selective schooling system appear to do better than their non-selective counterparts. For those who find themselves at the bottom of the

[14] 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

earnings distribution, individuals who grew up in a selective area do worse than their non-selective counterparts. 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 70th percentile, at the top of the distribution (90th percentile) and towards the bottom of the distribution at the 20th percentile.

Figure 3, and Panel B of Table 5, replicate this analysis plotting the adjusted earnings distributions once we have additionally controlled for gender, ethnicity, parental occupational class (measured when the individual was 14 years old), parental education and current county of residence in addition to the quadratic in age and the selective schooling dummy. 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 remains a statistically significant positive effect of selective schooling at the 90th percentile while those at the distributions are also significantly different at the 80th percentile. These results are robust to including all observations (i.e. not averaging where an individual has two observations) or to including just a single observation per individual and to altering the definition of selective and non-selective areas – in each case the pattern and levels of significance remain essentially unchanged.

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 £2.25/hour larger than the 90-10 earnings gap of individuals from a non-selective system. This accounts for 14.5% percent of the overall 90-10 earnings gap in our sample. Focusing on the 75th-25th percentile earnings difference, the effect size is slightly smaller (12%) with the 75-25 earnings gap £0.81/hour more than that of individuals who grew up in a similar non-selective system. Panel B shows that when conditioning on additional background characteristics and the current county of residence, the difference at the 75th-25th percentiles are no longer significantly different. However, a significant difference remains when comparing the difference in earnings at the 90th percentile to the 10th percentile across the two systems with those from a selective system facing an earnings differential of £2.27/hour more than those from a similar non-selective system. This equates to 14.6% of the total inequality between the 90th and 10th percentile in the sample.

Differences by gender

Up until this point we have considered earnings inequality across schooling systems for males and females combined. 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. Table 7 and Figures 4 and 5 present the simultaneous quantile regressions based on the adjusted earnings from specification (2) for males (Panel A) and females (Panel B) while Table 8 presents the 90-10 and 75-25 earnings gaps from these models by gender.

While the results from Table 8 indicate that overall differences in inequality exist for both males and females in selective and non-selective systems with a similar magnitude to that seen in the pooled sample (16% of total 90-10 gap for males and 12% of total 90-10 gap for females), Table 7 illustrates that the distributions of earnings for selective and non-selective areas differ by gender. For males, although there are significant differences at the bottom and top of the distribution for the unconditional specification (not shown) in the conditional specification greater inequality in earnings for selective compared to non-selective males is driven by the top end of the earnings distribution – top-earning males from selective areas earn on average £2.32/hour more than their non-selective top-earning counterparts. For females, the picture at the top of the distribution is less pronounced, although there is still a significant advantage to growing up in a selective area for top earnings females. Unlike for males, the significant penalty to growing up in a selective area remains at the bottom of the distribution in the conditional specification – low-earning females from selective areas earn £0.80/hour less than their similar non-selective low-earning counterparts.

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 Table 6, excluding London as a larger proportion of individuals in London cross borders compared to elsewhere. 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. Table 9 presents the differences in the effect sizes found at the 90^{th} and 10^{th} percentile and 75^{th} and 25^{th} percentiles as seen in Table 6. Our results are robust to the exclusion of London: 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 significant difference in inequality of earnings between selective and non-selective educated individuals.

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 Table A1 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 £2.15) although the effects are slightly smaller at the bottom of the distribution and slightly larger at the top of the distribution. Figure A1 illustrates the results of the model with controls and comparison with Figure 3 provides visual confirmation of the robustness of the results.

6. Conclusions

Inequality in access to high quality schools raises concerns from both an equity and efficiency point of view. Understanding the best system for the assignment of students to schools to reduce inequality therefore remains a priority for policy. As such, in both the UK economics literature and policy debate, the issue of selective schooling continues to divide. This finds a parallel in the US where a similar literature concerns the merits of exam schools. In each case, the literature tends to focus on the impact of attending the elite (non-elite) school for the marginal students who just pass (fail) the exam or on whether there is fair access to elite schools. In general, the UK literature finds that access to grammar schools is socially graded, even when conditioning on prior attainment and there is little evidence to support a causal impact of grammar education on scholastic outcomes. However, to date this literature has not addressed the issue of the impact of selective school systems on the whole distribution of wages, rather than for the marginal student.

In this paper, we use data from a large household panel study to illustrate the extent to which selective schooling systems actually increase later wage inequality. Controlling for a range of background characteristics and the current labour market, the wage distribution for individuals who grew up in areas operating a selective schooling system shows significant differences to that for comparable areas that operated a comprehensive system. As one might expect, those making top earnings from the selective areas are earning significantly more than those making top earnings from comparable non-selective areas. For those at the bottom of the distribution of earnings, growing up in a selective system leads to significant penalties in terms of earnings compared to those growing up in non-selective systems. These results are robust to a number of specification checks and suggest that selective schooling systems have significant impacts across the whole distribution of earnings. The total effect sizes here are large: 16% of the total gap in earnings between the 90^{th} and 10^{th} percentile can be explained by the difference in the 90-10 gap between those growing up in selective compared to non-selective areas.

The descriptive framework suggests that the inequality in each system is driven by the mapping of ability into schooling outcomes, via the channels of peer groups and school (teacher) quality. While selective systems directly relate ability to peer groups and (arguably) teaching quality, in comprehensive systems this works through the mechanism of parental resources and the positive association between resources and ability. We might expect therefore that inequality is exacerbated by selective systems given these stronger peer group and teaching quality effects. The evidence in the UK literature on peer effects is mixed, which suggests that perhaps it is the teacher quality mechanism that leads to greater inequality in selective systems. The sorting of (possibly) the highest quality teachers to teach the highest ability students – and the implications for the quality match further down – may explain the differences seen at the top and bottom of the earnings distribution, though we leave this question for future research.

It remains a matter of opinion as to whether this evidential inequality from selective systems is a good or a bad thing: some would argue that if wages represent productivity and this is increasing at the top of the distribution then this will have positive effects on economic growth. On the other hand, the negative effects of inequality are well documented. If this inequality is coupled with unequal access to grammar schools then it seems likely that selective systems are likely to reinforce inequalities across generations rather than drive social mobility. While such questions remain for policy debate and future research, it is clear from this study that the idea that selective systems create no losers does not hold true.

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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

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	91.4	8.6
5-10	94.7	5.3
0-10	88.5	11.5
MCS		
0-3	96.5	3.5
3-5	98.0	2.0
5-7	98.5	1.5
0-7	94.1	5.9

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

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	53.9	Hampshire	48.0
Lancashire	70.4	Essex	49.2
Gloucestershire	41.6	Cambridgeshire	36.5
Buckinghamshire	62.6	Leicestershire	28.2
Dorset	50.0	Bedfordshire	50.0
Weighted average	57.1	Weighted average	43.5

Table 4: Raw earnings distribution by schooling system type

	Selective	Non-Selective
Hourly wage: mean	8.50	8.47
variance	36.21	26.34
N	1289	1166

Notes: hourly earnings in year 2000 £s

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	-0.917	0.668	-1.37	10	-0.862	0.323***	-2.67
20	-0.399	0.221	-1.81*	20	-0.250	0.262	-0.95
30	-0.235	0.177	-1.32	30	-0.141	0.217	-0.65
40	-0.163	0.196	-0.83	40	-0.227	0.229	-0.99
50	-0.327	0.214	-1.53	50	-0.180	0.195	-0.92
60	-0.350	0.240	-1.46	60	-0.142	0.225	-0.63
70	0.433	0.267	1.62*	70	0.122	0.270	0.45
80	0.548	0.390	1.41	80	0.715	0.345**	2.07
90	1.335	0.574	2.33**	90	1.412	0.515***	2.74
	N=2455				N=2455		

Notes: residuals from a regression of wage on a quadratic in age and a selective schooling area dummy (Panel A); and residuals from a regression of wage on a 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 county region and year of survey effects are removed via a regression.

Table 6: Estimated effects sizes

	Sample wage gap	A: Without controls			B: With controls		
		coeff.	*std. error*	*Effect size*	*coeff.*	*std. error*	*Effect size*
90-10	15.58	2.252	0.743***	14.5	2.274	0.594***	14.6
75-25	6.55	0.813	0.346**	12.4	0.320	0.330	4.9

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.

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	-0.424	0.566	-0.75	10	-0.804	0.300***	-2.68
20	0.152	0.477	0.32	20	-0.668	0.395*	-1.69
30	0.470	0.331	1.42	30	-0.571	0.279*	-2.04
40	0.415	0.303	1.37	40	-0.409	0.281	-1.46
50	0.547	0.286*	1.92	50	-0.551	0.281**	-1.96
60	1.070	0.264***	4.04	60	-0.430	0.274	-1.57
70	0.916	0.427**	2.15	70	-0.458	0.323	-1.42
80	1.205	0.447***	2.69	80	-0.218	0.461	-0.47
90	2.324	0.589***	3.95	90	0.869	0.477*	1.82
	1068				1387		

Notes: residuals from a regression of wage on a quadratic in age and a selective schooling area dummy (Panel A); and residuals from a regression of wage on a quadratic in age, a selective schooling area dummy, 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 county region and 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	16.93	2.749	0.757***	16.2	14.19	1.673	0.495***	11.8
75-25	6.96	0.592	0.628	8.5	5.66	-0.079	0.380	-3.7

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.

Table 9: Estimated effects sizes excluding London

	Sample wage gap	A: Without controls			B: With controls		
		coeff.	*std. error*	*Effect size*	*coeff.*	*std. error*	*Effect size*
90-10	15.53	2.346	0.797***	15.1	2.136	0.560***	13.8
75-25	6.48	0.774	0.357**	11.9	0.386	0.355	6.0

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 1: Distribution of selectivity across LEAs in 1983

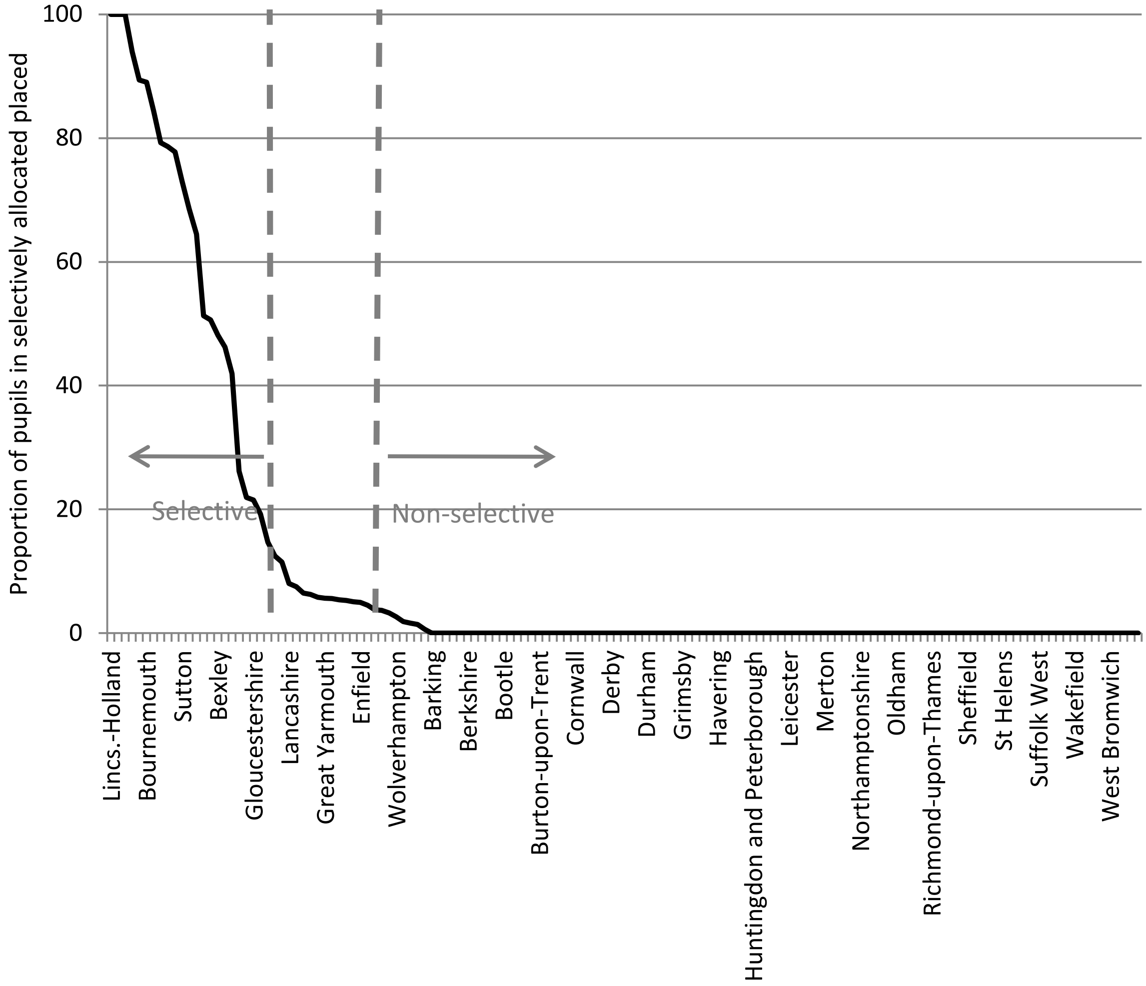
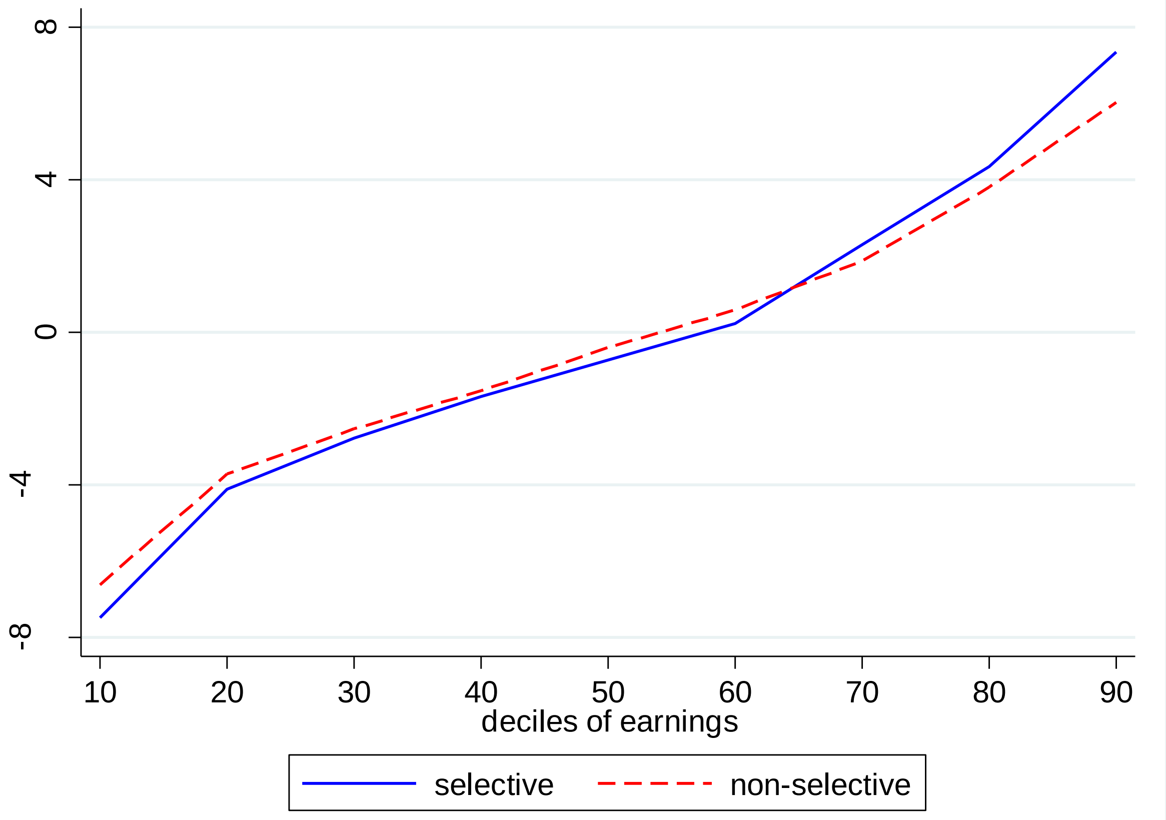
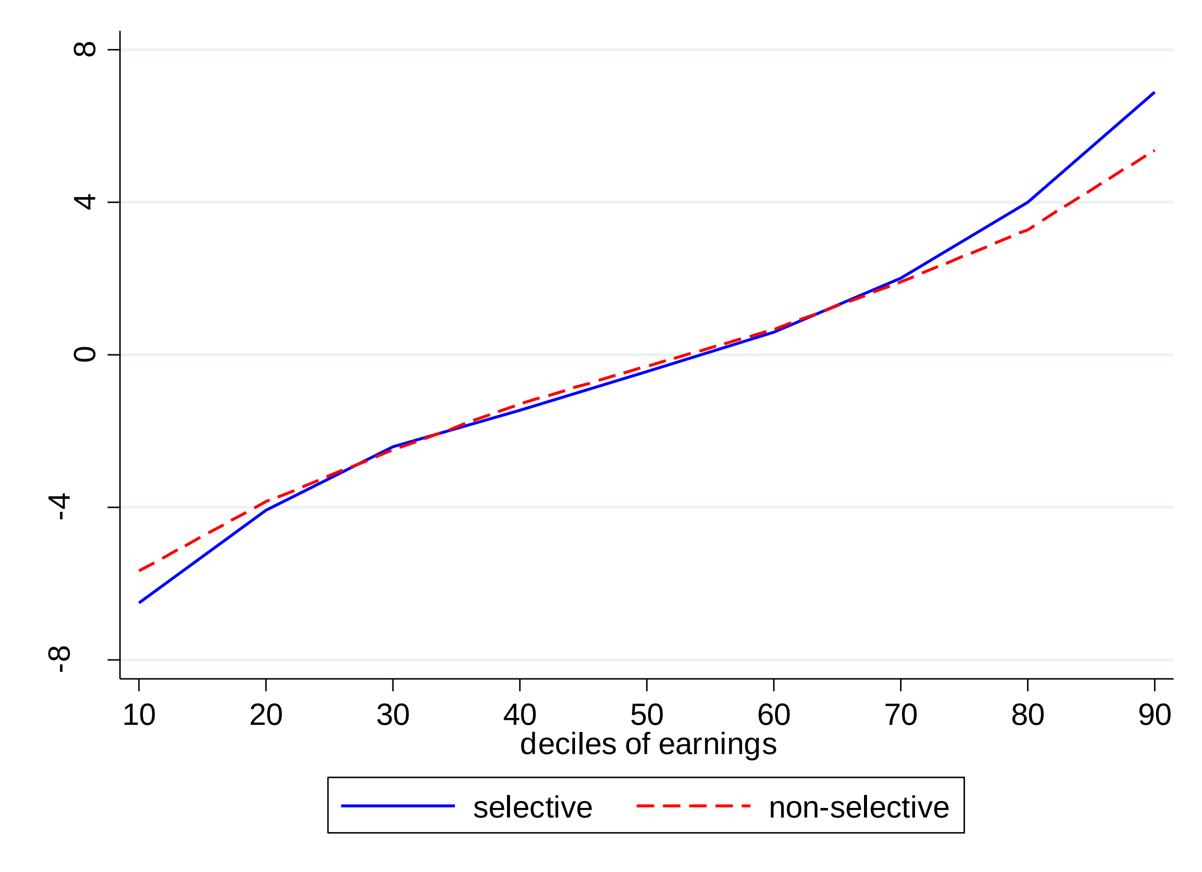


Figure 2: Understanding Society, deciles of the raw earnings distribution by schooling system type



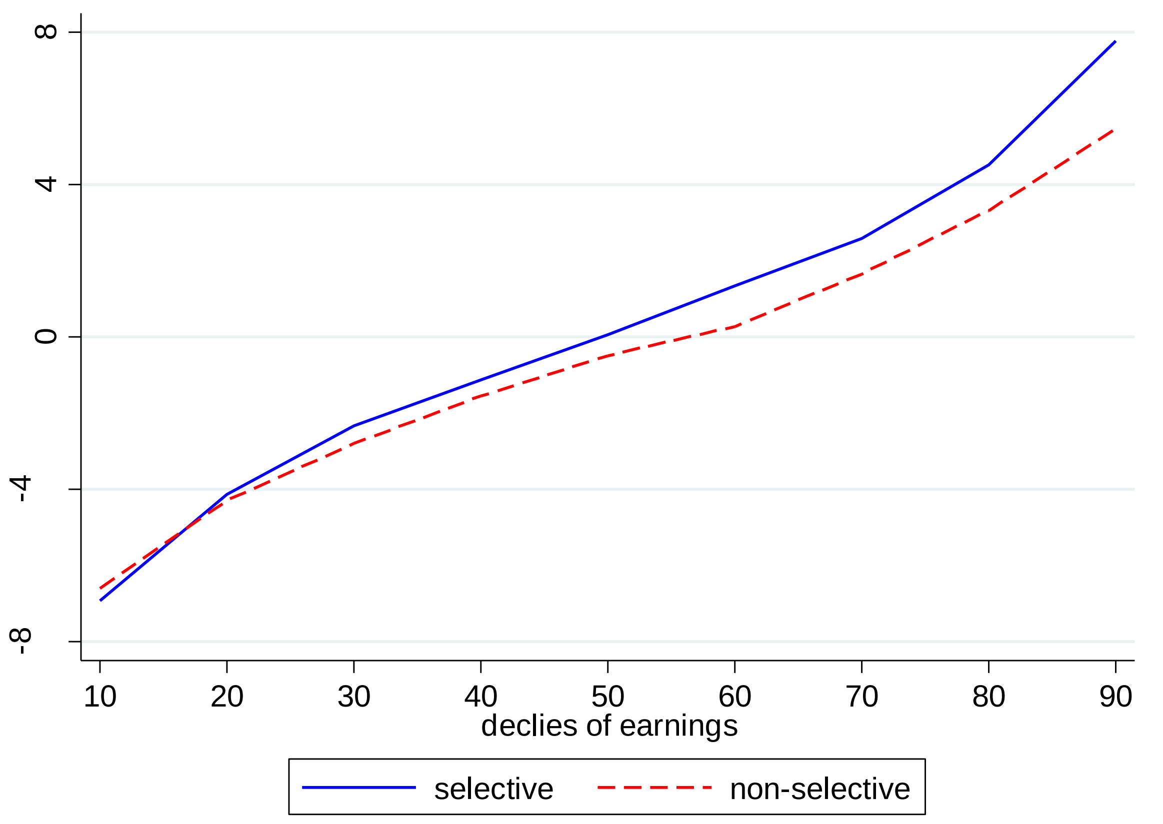
Notes: residuals from a regression of wage on a quadratic in age and a selective schooling area dummy, with the global mean of the residual removed. Before averaging wages for individuals with two wage observations the county region and year of survey effects are removed via a regression.

Figure 3: Understanding Society, deciles of the conditional earnings distribution by schooling system type



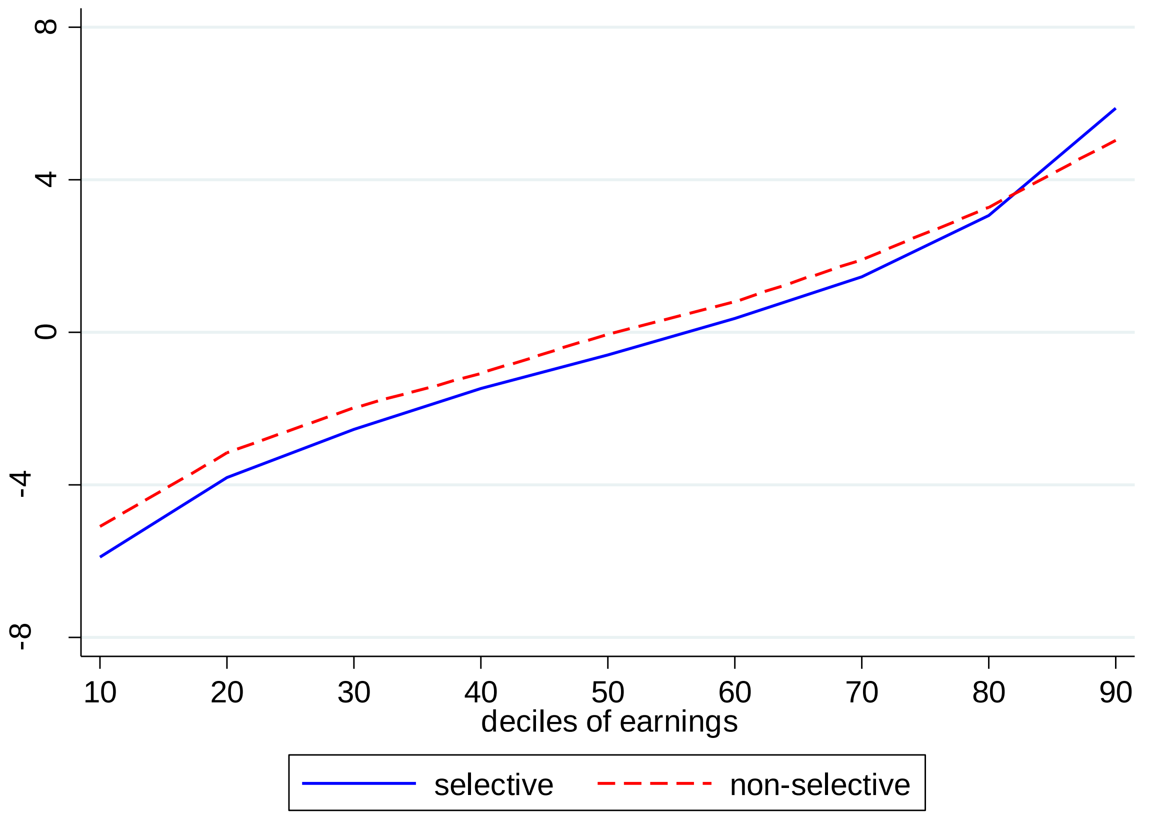
Notes: residuals from a regression of wage on a quadratic in age, gender, ethnicity, parental occupational class when the individual was 14, parental education, current county of residence and a selective schooling area dummy with the global mean of the residual removed. Before averaging wages for individuals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

Figure 4: Understanding Society, deciles of the conditional earnings distribution by schooling system type, males only



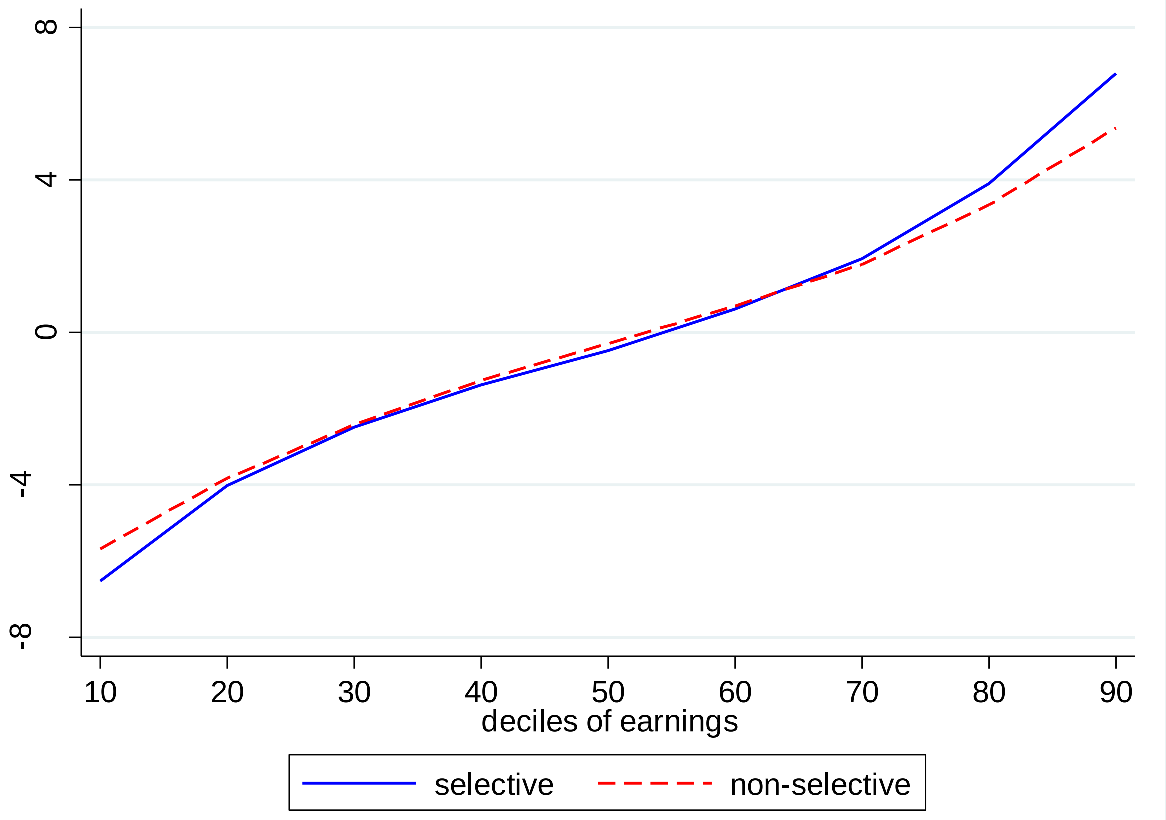
Notes: residuals from a regression of wage on a quadratic in age, ethnicity, parental occupational class when the individual was 14, parental education, current county of residence and a selective schooling area dummy with the global mean of the residual removed. Before averaging wages for individuals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

Figure 4: Understanding Society, deciles of the conditional earnings distribution by schooling system type, females only



Notes: residuals from a regression of wage on a 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 the county region and year of survey effects are removed via a regression.

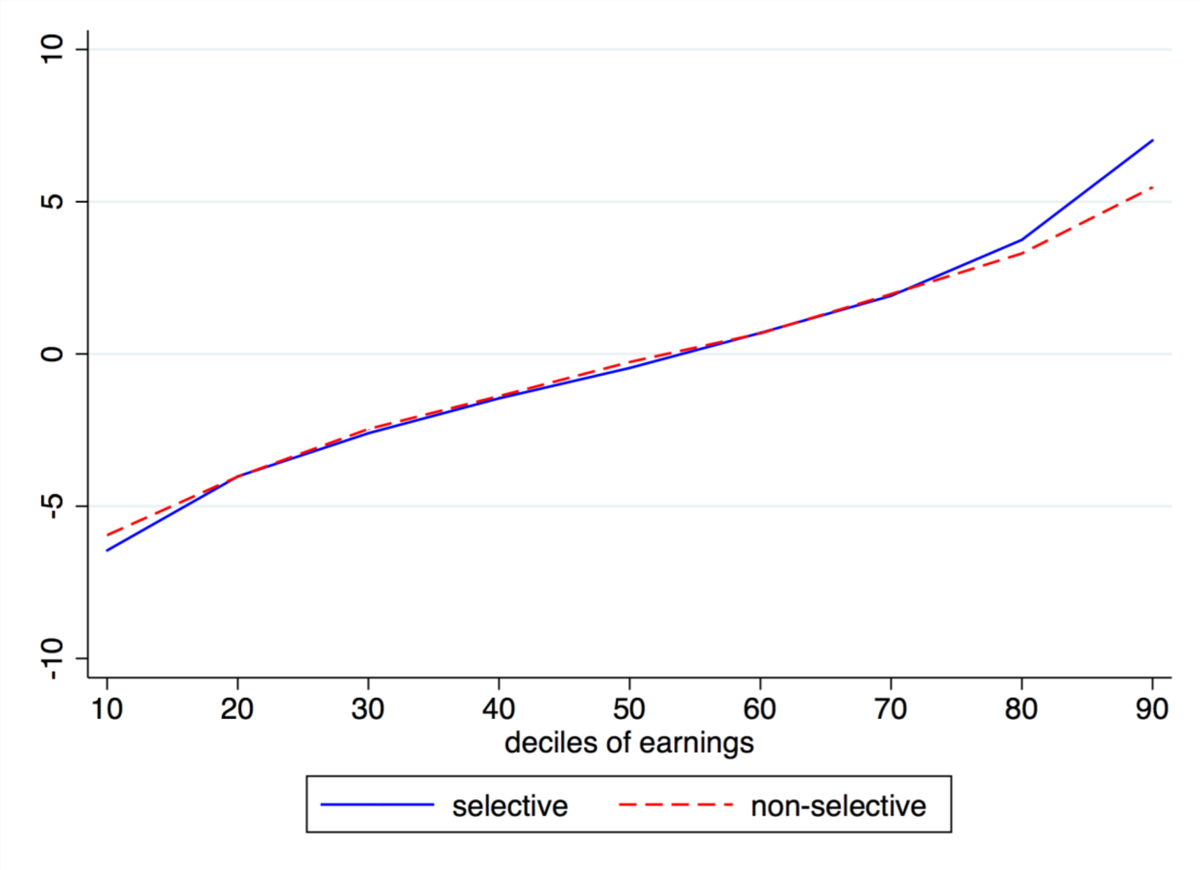
Figure 6: Understanding Society, deciles of the conditional earnings distribution by schooling system type, excluding London



Notes: residuals from a regression of wage on a 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 the county region and year of survey effects are removed via a regression.

Appendix

Figure A1: Understanding Society, 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 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 the county region and year of survey effects are removed via a regression.

Table A1: Quantile Regression estimates of selective schooling effect on wages, Selective defined as >30% assigned by selection, non-selective <5% assigned by selection

	A: Without controls			B: With controls		
	coeff.	*std. error*	*t*	*coeff.*	*std. error*	*t*
10	-0.443	0.891	-0.50	-0.551	0.458	-1.20
20	-0.363	0.305	-1.19	0.002	0.344	0.01
30	-0.443*	0.239	-1.86	-0.174	0.266	-0.66
40	-0.273	0.212	-1.28	-0.108	0.240	-0.45
50	-0.458*	0.242	-1.89	-0.202	0.198	-1.02
60	-0.794**	0.358	-2.22	0.001	0.219	0.01
70	-0.019	0.421	-0.04	-0.042	0.234	-0.18
80	0.320	0.465	0.69	0.479	0.377	1.27
90	1.475**	0.648	2.28	1.602***	0.532	3.01
	1689			1689		

Notes: residuals from a regression of wage on a quadratic in age and a selective schooling area dummy (Panel A); and residuals from a regression of wage on a 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 county region and year of survey effects are removed via a regression.