Do Selective School Systems Create A Lottery in Life Chances? Evidence on the Causal Effects of Attending the Elite Schools in a Selective School System[1]

Damon Clark
(Cornell University, NBER)

Emilia Del Bono
(University of Essex)

February, 2013

ABSTRACT

Between 1945 and 1990, the fraction of Western European children educated in selective school systems fell dramatically. One explanation for the declining popularity of selective school systems – and a criticism of the systems still in operation – is the perception that they create a lottery in life chances; that while a student's chance of being assigned to the elite schools in these systems can depend whether they slept well the night before the assignment test, being assigned to an elite school can have long-lasting effects on outcomes such as educational attainment and income. This paper tests this claim using data on several cohorts of students educated in the 1950s in a UK district operating a selective school system. First, we show that school assignment was indeed highly sensitive to the assignment procedure used, and would have been quite different had assignment tests been administered at slightly different ages or had teachers had a greater say in assignment. Second, we show that for men and especially for women, attending an elite school had large impacts on a range of long-run outcomes. Although our results paint nothing like a complete picture of selective school systems, they suggest there may have been some truth to the perception that these systems represented a lottery in life chances.

[1] We thank Professor David Leon for generous help with the Aberdeen data. We also thank the staff at the Aberdeen City Library for their hospitality and help with Education Committee minutes from the 1960s. Without them, we could not have deduced the rules used to assign students to different types of secondary schools and this paper would not have been possible. This work contains statistical data from ONS which is crown copyright and reproduced with the permission of the controller HMSO and Queen's Printer for Scotland. The use of the ONS statistical data in this work does not imply the endorsement of the ONS in relation to the interpretation or analysis of the statistical data. We would like to thank Annarosa Pesole for her help in analyzing this data. All mistakes are the authors' sole responsibility.

1. Introduction

As noted by Goldin and Katz (2008), in the mid twentieth century, the education systems in most Western European counties promoted "a cadre of extraordinary children" and gave the residual a "fair elementary education". These systems were typically built on tests taken by children at around age 11, the results of which determined whether they would attend an elite or a non-elite secondary school. Goldin and Katz (2008) contrast this with the "mass secondary schooling" provided in the US at that time.

These selective systems were highly controversial, and, over the next 20 years, most were replaced by US-style non-selective systems. Proponents of selective education argued that it was more efficient to tailor particular types of education to particular types of students. Critics argued that separate systems were inherently unequal, and that the elite schools within these systems were bound to attract a disproportionate share of resources: better students, better teachers and more funding. They also argued that the assignment procedures were unreliable, and generated assignment outcomes that were highly sensitive to the types of tests used, the age at which they were taken, and whether they were used alone or in combination with teacher recommendations. Hence, rather than generate a better match of talent to education type, they argued that selective systems created lotteries in life chances: lucky students assigned to elite schools would, for the rest of their lives, reap the returns to a well-resourced education; unlucky students assigned to non-elite schools would, for the rest of their lives, suffer the consequences of a poorly-resourced education.

In this paper we shed light on this debate. We do so by assembling the first dataset that includes, for a large number of individuals that passed through a selective school system, comprehensive information on the assignment process, an extensive set of background characteristics and a rich set of later-life outcomes including completed education, employment, income and marriage and fertility. We have assembled these data for several cohorts of students educated in a British school district (Aberdeen) in the 1950s and 1960s.

In the first part of the paper we assess the reliability of the procedure used to assign students to either the elite or the non-elite schools within this district. First, we use the assignment data – which includes scores from multiple administrations of the same test –

to assess the reliability of the test-based component of the assignment process. In particular, we estimate the reliability of these tests. Second, because the assignment procedure used in this district encompassed most of the assignment procedures used in Western Europe at this time, we assess the sensitivity of assignment outcomes to the precise assignment procedure used. For example, we compare assignment outcomes under test-based and teacher-based assignment. Third, because the background characteristics include age-7 and age-9 test scores, we assess the extent to which assignment outcomes were sensitive to the age at which the assignment occurred.

In the second part of the paper we estimate the long-run effects of being assigned to the elite schools within this district. Assuming these effects differ across different types of students, our estimates should be interpreted as effects for "borderline students", those that narrowly met or narrowly failed to meet the requirements for assignment to an elite school. To generate these estimates, we exploit specific features of the assignment procedure used in this district. In particular, since this procedure gave rise to an assignment probability that was zero for students with assignment scores below the 75th percentile (roughly), one for scores above the 85th percentile, and increasing from zero to one for scores between the 75th and 85th percentile, we instrument elite school attendance with dummy variables for scoring above the 75th percentile and the 85th percentile and interactions of these dummies with the assignment score. We provide several pieces of evidence that suggest that after controlling for smooth functions (i.e., low-order polynomials) of these assignment scores, these instruments are valid.

We note that while our estimates shed light on the effects of elite school attendance for students in one part of the ability distribution, we would ideally like to estimate effects for students in all parts of the distribution and hence on average. To do so, however, would require comparisons between students randomly assigned to different systems within the same country. In the absence of any such comparisons, and since this is the first study to estimate the sensitivity of elite school assignment or the long-run effects of elite school attendance we believe that it provides new important insights. To the extent that it speaks to questions surrounding the many selective systems that still remain, it complements recent studies of the short-run effects of attending elite schools in Trinidad and Tobago (Jackson, 2010), Romania (Pop-Eleches and Urquiola (2010)), Boston (Angrist et al, 2011) and New York City (Fryer et al, 2011). Although none of these

analyze assignment procedures or examine long-run outcomes, the last two provide compelling evidence that elite schools in these cities have no impact on a wide range of academic outcomes. As such, they caution us against simply assuming that elite schools affect long-run outcomes.

The first part of our analysis suggests that assignment outcomes were characterized by a high degree of chance. In particular, we show that the test component was unreliable, that the allocation was sensitive to the particular procedure used and especially sensitive to the age at which selection was conducted. For example, we estimate that a sizable fraction of the students allocated under a West German procedure applied at age 11 would have been allocated differently under an English procedure applied at age 9.

The second part of our analysis suggests that being assigned to an elite school had important effects on long-run outcomes. For both men and women, attending an elite school extended formal education by around one year, almost doubled the probability of receiving A levels and almost doubled the probability of attending University. For women, these effects appear to have generated large increases in annual income: effect sizes are comparable to those that would be predicted by the impacts on completed education (assuming returns to education in the range found in the literature). We argue that a key mechanism appears to be the occupations in which these women work, with elite school attendance increasing the probability that women work in professional occupations. Consistent with these effects on education and income, we find that elite school attendance increased the probability that women were childless, although it had no impact on their probability of marriage.

For men, income effects are statistically insignificant and we find no impacts on family outcomes. We suspect this is because men assigned to non-elite schools gained in apprenticeship training what they lost in formal training. Although our dataset does not contain apprenticeship information, evidence from other sources suggests this is a likely explanation. Consistent with the hypothesis that elite school affected the type but not necessarily the quantity of the education received by men, we find that elite school reduced the probability that men would work as managers.

Taken together, our analysis provides partial support for the claim that selective systems create lotteries in life chances. While the assignment procedure was characterized by a high degree of chance, and while women's life outcomes do appear to have been shaped by whether they were assigned to an elite school, the same cannot be said for men. Although these results paint nothing like a complete picture of the selective system, they support prominent criticisms of these selective school systems and can therefore help to explain why they were abolished.

2. The selective school system in the UK

Selective high schools in the UK, known as *Grammar Schools* in England and *Senior Secondary Schools* in Scotland, were established by the 1944 Education Act. Before the Act, these schools formed a class of private schools that offered some scholarships in exchange for some financial support from the local school district; after the Act, they received all of their funding from the district, were not allowed to charge fees and were required to admit students on the basis of academic potential assessed at the end of primary school. At its simplest, this involved all students in a district taking a test (the "11-plus"), with the elite school places going to the top-scoring students. The non-selective schools, known as *Modern Schools* in England and *Junior Secondary Schools* in Scotland, remained broadly unchanged, the important caveat being that where they previously educated all but those students that won scholarships to the elite schools, they now educated all students that "failed" the test.

The school system in Scotland was very similar to that operating in the rest of the UK in the 1960s. Education was compulsory for all children aged 5 to 15. After 7 years of primary school, at the age of about 12, children were transferred to secondary education. There were different types of secondary schools. *Junior Secondary Schools* provided 3-year courses of general education, mainly with a vocational content. These courses did not lead to any national external qualifications. *Senior Secondary Schools* provided 5 or 6-year courses and prepared their students for both the Lower and Higher grades of the Scottish Certificate of Education.

In 1962, the Ordinary grade replaced the Lower grade certification. The Ordinary grade was of a lower standard than its predecessor and it could be achieved after a 4-year course. The purpose of moving from the 5-year Lower certificate to the 4-year Ordinary

certificate was to encourage the top 30% of any age group to leave full-time education with a nationally recognized qualification (Hunter 1963). As a consequence of this change, many Junior Secondary Schools began to offer 4-year courses and adopted the new designation of *High School* in order to avoid any connotation of inferiority in relation to the Senior Secondary Schools.

The move towards Ordinary grades resulted in a system more closely comparable to that used in England and Wales. The Ordinary grade of the Scottish Certificate system was effectively equivalent to the English Ordinary Level, while the Higher grade was of a standard comparable to the English Advanced Level. Access to University education usually required at least three passes at the Higher grade. For those going to Colleges of Education the requirement was at least two passes at the Higher grade (Robbins 1963).

The type of secondary course attended was determined by tests and assessments that took place during the last year of primary school. The battery of tests usually included two intelligence tests, one attainment test in English, one attainment test in Arithmetic, a teachers' estimate in English and Arithmetic, and a primary school head teacher's recommendation regarding whether or not the student was suitable for a senior secondary school (suitable, doubtful or unsuitable). The teachers' estimate was then re-scaled against the corresponding objective attainment tests, and the four tests and the rescaled teachers' estimate were standardized such that they had mean 100 and standard deviation 15 and summed to give a total transfer score with mean 500.

The procedure determining the assignment to secondary school varied from district to district and was subject to changes over time. From the second half of the 1950s up to the academic year 1966/67, the Transfer Examination Board in Aberdeen adopted the following scheme: Pupils with total score of 580 or above were allocated to Senior Secondary Schools unless assessed by Head as "unsuitable"; pupils with total score between 560 and 579 and assessed by Head as "suitable" were allocated to Senior Secondary provided one of their intelligence test scores was at least 112; the names of the remaining pupils with total score between 540 and 579 were sent to the Heads of primary schools who had to arrange these pupils in order of merit and give a report on each, these names were then referred to the Appeals subcommittee which had powers to allocate them to the remaining places in the Senior Secondary Schools; pupils with a total

score below 540 were allocated to 3-year courses. At the end of this process, the allocation of pupils was publicly announced and parents could appeal.

As a result of this procedure, we expect that (i) no students with total score equal of below 539 should be observed in a Senior Secondary School; (ii) most students with total scores equal or above 560 should be observed attending a Senior Secondary School; (iii) the fraction of students attending senior secondary schools should be increasing in the test score among those in the range 540-559. As we show below, our data correspond closely to this prediction.

3. Data

Our data come from the Aberdeen Children of the 1950s study. The study cohort consists of 12,150 children who were born in Aberdeen between 1950 and 1956 and who participated in the Aberdeen Child Development Survey (Illsley and Wilson 1981, Batty et al. 2004). The target population was initially all students in primary school grades 3-7 in December 1962, roughly aged between 6 and 13 (because of a small amount of grade retention/promotion). In this, phase I of the study, all of these children were given a series of reading tests and asked to complete a form giving their own and their parents demographic details (address, date of birth). This information was used to link children to data from the Aberdeen Maternity and Neonatal Databank (match rate 86%), which included perinatal and social information collected throughout the course of their mother's pregnancy and their own birth. In phase II of the study (July 1963), medical records for these children were extracted. In phase III of the study (March 1964), sociometric and behavioral data were collected from teachers and children and a 20% sample of parents was conducted.

The dataset contains all of the transfer tests discussed above (two IQ, Arithmetic, English, Teacher Estimate). The exception is for students in Grade 3 in December 1962, since the assignment procedure for that cohort was based only on the IQ tests. For this reason, all of the analysis presented in this paper pertains to students in grades 4-7 in December 1962. In addition to these variables, the dataset contains the scores of tests taken at ages seven and nine. The test at age 7 was called the "Moray House Picture Intelligence Test" and was used to screen students for a mental handicap; the test at age

9 was the "Schonell and Adams Essential Intelligence Test" used to screen for poor readers.

Information on family background includes father's occupation in 1962 (as described by the survey child) and status of the area in which the family lived at the time of the 1962 survey based on data on age, ownership, building type and availability of domestic facilities. Because of the Maternity linkage, we also know father's occupation at birth of child and premarital occupation of child's mother.

In 1998 it was decided to gather new information from the original participants of the study using administrative records on pregnancies, hospital admissions and mortality, as well as a postal survey on current health and health behaviors. Names and dates of births of the original participants were matched to the NHS Central Register to ascertain vital status and area of residence for survivors. Over 97% of core population (N=11,727) were traced. Of these, 4% had died and 2.5% emigrated and 0.6% were in the armed forces (Batty et al. 2004). In 2001 the traced survivors were sent a sex-specific questionnaire to which the response rate was 63%.

Starting with the students matched to the Aberdeen Maternity Databank (N=12,150), we restrict the sample in a number of ways (see Appendix Table 1). First of all we exclude 358 individuals who had moved outside Aberdeen during the period 1962-1964, as we do not have complete information on their test results and a vast majority of these children went to secondary schools outside Aberdeen. We then exclude some children on the basis of the primary school attended. In particular, we do not consider: (i) individuals who attended either a private and/or faith primary school, as some of these did not take the "11-plus" test or even when they took the test they were less likely to attend an elite school; (ii) children who are in an elite secondary school during their primary school years, as these children are observed to attend an elite secondary school with probability one irrespective of their test results; (iii) children in schools for pupils with special needs; and (iv) a small number of children who appear to be in primary schools outside Aberdeen. Our sample is thus reduced to 10,544 observations.

As our analysis takes into account the school grade at the time of the first interview we restrict our sample to those children where grade information is available.[2] In order to specify the selection procedure we need to know the child's "11-plus" test score, and this leads us to exclude children for whom this information is not available. Since we will control for test scores at age 7 and 9, we also exclude observations with missing values on these variables. This brings our sample to 9,921 observations in total.

We will be looking mainly at individual long-term outcomes, and in order to do so we will be using the information collected through the postal questionnaire in 2001. For this reason, our sample includes only those individuals who replied to the 2001 survey (60 percent). Moreover, as we will compare long-term outcomes of children who attended elite schools with those who did not attend elite schools, we restrict our analysis to pupils who attended state secondary schools (94.5 percent). Finally, as the assignment procedure described above changed after the year 1966/67, we exclude children who were in grade 3 in 1962 and for whom the new rules would have applied. Our final sample thus consists of 4,528 observations.

Appendix Table 1b provides some descriptive statistics for the sample of individuals with non-missing information on test scores, and for our final sample of observations As we can see, our final sample is more dominated by girls and slightly younger than the larger sample. These differences are due to the fact that our final sample excludes individuals who are in grade 3 in 1962 and those who attend a private secondary school (mainly boys). In terms of socio-economic characteristics, here represented by father's social class, the two samples are very remarkably close. Most importantly, there appear to be only minor differences the mean value of the test scores at age 7, 9 and 11. This would seem to suggest that differences in ability are not driving response rates to the 2001 questionnaire.

In the far right panel of Appendix Table 1b we compare the characteristics of individuals in our final sample according to whether they attended an elite secondary school or not. Non-elite school pupils appear to be more likely to be male than elite school pupils. This

[2] Grade information was recorded on "Form A" (the one filled out by the children at the time of the first interview), but it was not added to the dataset until 1964, when it was collected as part of the sociometric data (friends, etc.). Therefore, children with no sociometric data have no information on grades. Grade is recorded as a separate variable, and it is not based on date of birth, although the data suggest that there was not a lot of grade retention/promotion.

will be taken into account in our analysis as all our main models are estimated for men and women separately. There are instead no significant differences in terms of age, and correspondingly we see that the distribution of individuals by grade is very close in the two subsamples.

As we would expect, non-elite school individuals are very different from their elite school counterparts in respect of parental background. The former are more likely to come from families where the father is in an unskilled or semi-skilled manual occupation, while the latter are more likely to have a father in a professional or technical occupation.[3] These groups are also very different in relation to their ability, with the elite school individuals being generally more able than the non-elite schools children. These differences are evident at the age of 7 and 9, and become extremely marked with the "11-plus" test results.

4. Impacts of attending an elite school

4.1. Impact on educational achievement

Next we examine the effect of attending an elite school on educational outcomes by looking at the effect of test scores at age 11 on various measures of educational achievement. The latter is captured by the number of years of post-compulsory schooling as well as academic qualifications. We consider post-compulsory education, rather than years of schooling for two reasons. First, very few children leave school before the minimum schooling age (which at that time was 15 years); only 2.1 boys and 1.5% girls in our sample. Additionally, we expect the effect of being above the 560 test score threshold to be felt at the higher end of the ability distribution, i.e. among individuals who are all very likely to attend post-compulsory schooling in any circumstances. As before, the analysis is carried out using graphical tools and linear regression analysis.

Figure 3 illustrates the relationship between the "11-plus" test scores and the number of years in post-compulsory education. The figure presents results for men (top panel) and women (bottom panel) separately. The graphs to the left are obtained by running separate local polynomial regressions (solid line) of years of post-compulsory schooling on values of the "11-plus" test score for individuals who did not attend (to the left of

[3] Father social class is derived from the Registrar General's 'Classification of Occupations 1950.

560) or attended (to the right of 540) an elite school, as well as a local polynomial regression which uses the entire range of the test score (dotted line).

There is a clear positive relationship between test scores and years of education. What is more relevant to our purposes, however, is that there is a clear difference in mean attainment between treated and non-treated individuals even for individuals with the same tests scores, i.e. over the marginal area. This difference is close to two years of post-compulsory schooling for men and exceeds one year for women. As elite school attendance is not randomly assigned, the observed raw gap between treated and non-treated individuals in the marginal area could still be the result of selection bias. So, on the right-hand-side we plot the mean values of our attainment variable for treated and non-treated individuals as predicted by an Instrumental Variable regression of years of post-compulsory schooling on elite school attendance and a polynomial of fourth order in the "11-plus" test score. Instruments consist of a dummy for having a test score in the [540-559] interval, a dummy for having a test score in the interval [560-750] and their respective interactions with the test score values. As we can see, the gap in attainment between those who attend an elite school and those who don't which can be solely attributed to a difference in test scores is still large, although it has now reduced to about one year of post-compulsory education for men and about 0.7-0.8 years for women. This is much more robust evidence of a positive effect of "passing" the test at age 11 onto years of education.

In order to explore the size of this effect more precisely, and formally test our IV procedure, we perform some regression analysis. Table 2 reports "naïve" estimates of the effect of attending an elite school on years of post-compulsory education (panel A), as well as on different types of qualifications held by individuals in our sample (panels B-G) for men (columns 1 to 3), and women (columns 6 to 8). Each specification includes an elite school dummy, and gradually increases the vector of controls to include a polynomial of fourth degree in the test score at age 11 (columns 2 and 7) and a full set of background variables (columns 3 and 8) such as: School and grade in 1962, relative age within the school-grade, father's occupation, mother's premarital social class, and fourth degree polynomials in test scores at ages 7 and 9.

Taking columns 3 and 7 as the main specifications, we see that attending an elite school increases years of education by 1.42 for boys and 0.64 for girls. These are large effects, especially if compared to the results of the 1947 British school-leaving age reform, which extended compulsory schooling to age 15, and is estimated to have increased schooling by 0.5 years (Harmon and Walker, 1995). We expect that our treatment will impact mainly individuals with high levels of ability, so it is not surprising to see no effect of elite school attendance on the probability of obtaining no or very low educational qualifications. There is however a significant effect on the probability of achieving A-levels for both men and women, and on the probability of holding a degree for men. Once again the effects are large; for example the probability of achieving a degree level qualification is estimated to increase by 19 percentage points for men, which translates into an increase of more than 100 percentage points over the baseline.

Next we use the non-linear relationship between the test score at age 11 and the probability of attending an elite school to predict assignment to an elite secondary school and perform Instrumental Variable estimates using GMM (columns 4-5 for men and 9-10 for women). In accordance with the graphical evidence presented in Figure 3, we see large and significant effects of attending an elite school of the order of 1 year of schooling for men and 0.7 years of schooling for women (panel A). Looking at qualifications, the estimates reveal an increase of about 12.3 percentage points (over a baseline of 61.4 per cent) in the probability that men will obtain a level of qualification equivalent to O-levels, an increase of 16.1 percentage points on the probability that women will achieve an A-level qualification (over a baseline of 28.2 per cent), and an increase of 16.2 percentage points on the probability that men will achieve a degree (over a baseline of 15.8 per cent). In all these cases the over-identification test – here represented by the Hansen's J-statistic (p-value reported) – shows that the instruments are correctly excluded from the main equation. Although in some cases the IV estimates differ slightly from what the "naïve" models would seem to suggest (in particular when considering men's qualifications), we find robust evidence that an "11-plus" pass had a significant impact on the educational outcomes of the Aberdeen children of the 50s.

One of the ways in which selection bias could generate a problem for our analysis is if it introduced discontinuities in the observable background characteristics around the marginal area. We have some evidence that this is not a problem by comparing columns

4 and 5 (for men), and 9 and 10 (for women) in Table 2. Indeed, as we can see, the GMM estimates of the effect of attending an elite school are quite robust to the introduction of family background and other pre-determined characteristics of the individuals.

Another way we can check whether discontinuities in the observables is a problem for our analysis is to use these background characteristics to predict the dependent variable of interest and then analyze the relationship between this predicted value and the test score at age 11. Using similar tools as those employed in the previous analysis, Figure 4 shows that although there is a clear positive relationship between the "11-plus" test scores and the number of years of post-compulsory schooling predicted by our control variables, there is a very small gap between the predicted outcomes of the treated and the control group in the marginal area. This gap totally disappears (right hand side figures) once we use test score cut-off points to predict assignment to elite schools. The corresponding regression analysis results are shown in panel A of Appendix Table 2. Here we see clearly that while predicted years of post-compulsory schooling are different for the treated and non-treated groups when no other controls are included, the effects of treatment greatly diminishes when a non-linear function of test score at age 11 results is included and totally disappears when we implement our Instrumental Variable approach. Similar results are shown for A-level (panel B) and Degree-level (panel C) qualifications.

In Appendix Table 3 we ask whether there are significant differences in the returns to attending an elite school according to parental background. In order to do so, we split our sample according to whether the individual's father had either (i) a skilled manual occupation that required an apprenticeship, a non manual occupation, an intermediate or professional occupation (high SES) or (ii) held a lower skilled, semi-skilled, unskilled manual occupation or was not working (low SES).

As we can see, our causal estimate of the effect of attending an elite school are about 0.7 years of post-compulsory education for both groups (columns 5 and 10). While the point estimate is very close, the relative effects are different. Attending an elite school causes a 55.6 percentage point increase in the number of years of education of low SES individuals, and a 30.7 percentage point increase for high SES individuals. In would

therefore appear that the effect of attending an elite school is to reduce social inequalities.

The results by type of educational qualification show a more mixed picture. In the linear probability models we see some significant effect of attending an elite school on the probability of achieving an A-level qualification or a degree for both groups. Once we move onto the IV analysis, however, these effects largely disappear or become much less precisely estimated. In order to clarify this part of the analysis it would have been useful to disaggregate our sample according to gender – as during those years men and women had clearly different educational aspirations – but the number of observations at our disposal prevents us from doing so.

4.2. Impact on labor market outcomes

We now look at the impact of attending an elite school on labor market outcomes. These are recorded at the time of the 2001 postal questionnaire, and therefore refer to individuals between 46 and 51, with a mean age of 49. Among the outcomes, we consider employment and gross annual income. The 2001 postal survey was mainly focused on health and health behaviors, so we do not have a measure of labor earnings; income is recorded as "personal current gross income from all sources", including interests from dividends and benefits.[4] For this reason we complement the survey variable with a measure of earnings derived by using occupation-specific means of hourly gross wages from the *New Earnings Survey* (NES).[5] To complete the analysis, we also look at occupational status at the 1-digit level (major groups). We use graphical as well as regression tools and perform a separate analysis for men and women.

Figure 5 shows the relationship between log annual income and the "11-plus" test score. As before, in the left-side panels we plot the mean value of the dependent variable as calculated by separate local polynomial regressions for treated and un-treated individuals.

[4] Another drawback of the data is that income is recorded in 8 bands and a large group of individuals, especially men (about 20%), fall in the top interval. We conducted many types of checks and never found any effect of elite school on the probability of being in the top interval, however. We also experimented with different methods to account for the banded nature of the variable, including interval regression (Stewart 2983), but the results are very close to what we present in the current analysis.

[5] In order to compute occupation-specific earnings we take the period between 1997 and 2001 and restrict the sample to those working in Scotland and aged 45-55. The imputation of the earnings variables is based on 2-digit SOC 1990 classification. We would like to thank Annarosa Pesole for her help with the NES data.

The plots show very little evidence of an income effect for men, while for women there is some indication that the treated individuals experience about 3 percent higher levels of income than the untreated individuals in the marginal area. On the right-side of the figure we plot the same relationship but now add the predicted log annual income from an IV model in which the non-linear relationship between the test score at age 11 and the probability of attending an elite school is used to predict treatment status. The plots now show clearly no effects for men, and a positive gap for women, albeit this is now reduced to about 2 percentage points.

Table 3 reports the results of the corresponding regression analysis. Here we consider a variety of outcomes, including: (i) employment, (ii) log annual income, (iii) log annual income for working employees only, and (iv) NES-imputed log hourly wages for working employees only. As we can see from panel A, although there is a clear positive correlation between attending an elite school and being in employment in 2001 (columns 1 and 6), this effect disappears once we take into account a non-linear function of test scores; adding additional covariates does not change this results. GMM estimates confirm this result, showing no true effect of treatment on the probability of being in employment in the long-term.

Panel B presents the results of the analysis on log annual income. Focusing mainly on the GMM estimates, we see negative but not significant effects for men and a positive and significant impact of elite school status for women. The latter is in the order of 3 percentage points (column 10) and very similar to what estimated by the full OLS model (column 8).[6] This confirms the results shown in the graphical analysis. Restricting the sample to working employees in 2001 shows a slightly different picture. The positive impact on women becomes smaller in magnitude and not statistically significant, while the impact on men is now more clearly negative.[7] As the data does not really offer a good measure of earnings, we next consider NES-imputed wages (panel D). Here the results are very close to what we find in respect of income for all individuals, in that our causal estimates of the impact of attending an elite school are not statistically different from zero for men and positive for women.

[6] For falsification tests corresponding to those shown for years of schooling and type of qualification, see Appendix Table 2, panel D.

[7] Notice also that the over-identification test reject the validity of our instrument for the male regressions here.

In order to understand what is driving these results, and in particular why we never find a positive impact of attending an elite school on income or earnings for men despite its positive effect on years in education and degree-level qualifications, we look at occupational qualifications. Appendix Table 4 shows that the causal effect of attending an elite school reduces significantly the probability that a man will be found in a managerial position (column 5, panel A). As shown in Appendix Table 5, these are the positions with the highest income levels, but not necessarily the positions with the highest levels of qualifications. Indeed, as the table shows, the highest proportion of graduates are to be found among the professional and associate professional occupations, which however show mean income levels 16 per cent below the income levels of those in managerial occupations. So, while attending an elite school increased the level of qualification and schooling for some men, it did not always lead them to settle in occupations with the highest earnings. It is likely that in the absence of the treatment, these will have followed a different career-path and ended up in managerial positions instead. This would explain why on average we see insignificant or even small negative impacts of elite schooling on income.

For women the story is slightly different. Elite school attendance caused a significant increase in the probability of holding a level of qualification equivalent to A-levels for this group (Table 2). This resulted into an increase in the probability that these women would occupy a professional or associate professional occupation (column 10, Appendix Tabel 4). In the absence of treatment these women would have most likely settled in clerical and secretarial jobs, which are clearly paid much less, as Appendix Table 5 shows. In other words, for women a higher level of educational qualification went hand-in-hand with higher earnings.

4.3. Impact on fertility and marriage

We now turn our attention to the impact of elite school attendance on fertility and marriage. The survey at 2001 collects information from individuals aged 46 to 51, so we have information on completed fertility. The survey also asks questions about fertility timing and current marital status. Despite the fact that there is a very well-known and much studied negative empirical relationship between education and fertility, there are relatively few causal estimates of the impact of education on the number of children and

the timing of the fertility process (ref.). Our study thus represents one of the few contributions to this literature.

Figure 6 reports the results of a graphical analysis of the relationship between the total number of children and the "11-plus" test scores. For men, in the main range of the distribution we see an almost flat relationship between test scores and number of children, with most men having slightly less than 2 children on average. For women, by contrast, the relationship is negative, although the pattern is not very well defined. The plots on the left-hand side of the figure show no evidence of elite school effects for men, and rather large negative impacts for women in the marginal area. The IV analysis (right-side plots of Figure 6) seems to reveal small positive effects for men (about 0.2 children) and large negative effects for women in the order of about 0.5 children per woman.

Table 4 shows the results of our regression analysis, in which we look at different outcomes including: (i) whether the individual has any child, (ii) number of children, (iii) mean age at first child, (iv) whether the first child was before age 27, (v) currently married, and (vi) ever divorced.[8] We never find any significant effects for men, nor we find any effects on marriage or divorce, so we will focus our comments on the fertility results for women.

As we can see, in both the OLS (columns 6 to 8) and GMM estimates elite school attendance is found to decrease the probability that a woman will have children (panel A). The effects are quite large – a decrease of 14.8 percentage points over a baseline of 87.6 per cent – and interestingly the GMM estimates are in this case higher in magnitude than their "naïve" OLS counterparts. Similar results are found in the next panel, where we look at the total number of children. In this case our calculations show that women scoring just above the threshold have almost 0.62 fewer children than women who score just below the threshold (a decrease of about 33 percentage points over the baseline).

The fertility effects estimated for women are quite large, and as we will see are partly sensitive to the degree of the polynomial in test scores (Appendix Table 6). However, even if we took the OLS estimate on the number of children as a lower bound of the

[8] We analyse fertility timing by a means of dummy variables on whether the individual had a child before a certain age. Here we report results for age 27, as three quarters of women have had a child by then. We also looked at other ages, but always found no effects.

elite school effect, our results would imply that a year of schooling decreases fertility by about 0.46 children (effect of elite school on women is to increase schooling by 0.76 years), which is a very large number

5. Discussion

Elite school attendance has large effects on educational achievement and, especially for women, longer run outcomes. An obvious question is whether the longer run effects can be explained by the effects on post-compulsory education. To answer this question definitively requires that we know the causal effects of post-compulsory education on these outcomes. Although we do not, we instead analyze least squares estimates of the impacts of education on these outcomes (Table 3).

These tables suggest that there are large effects of education on income for women. These are almost large enough to explain all of the elite school effects through this channel, although we might expect these estimates to overstate the true causal effects of school on earnings. It may be that elite schools have other effects – for example by signaling ability, changing peer groups and so on. On the other hand, there are weak education effects on income for men. These add further weight to the view that we are not painting an accurate picture of the male income distribution. One explanation is the omission of work-based training from these data. It is possible that as many as 30% of the men in this sample completed an apprenticeship, which would generate a lot of error in the measurement of years of post-compulsory schooling. Another possibility are the effects of the North Sea Oil boom in Aberdeen, although one might expect these to equalize income among women if they equalize income among men.

6. Conclusions

In this paper we study a single educational intervention in a particular setting. The intervention is assignment to the elite high school in the selective secondary education system that existed in the UK until the late 1960s. The setting is a school district in which secondary school assignments were a rigid function of the scores obtained on a series of tests taken at age eleven (the "11-plus").

We find that assignment to the elite school has large effects on completed education and long-run outcomes including fertility and income. While it is interesting to note that such as "knife-edge" decision at age 11 had large impacts on these students' future life course, it would be interesting to know whether these effects can be attributed to the additional post-compulsory education experienced by these students or whether these schools impacted long-run outcomes directly. As we obtain data on more outcomes (including self-reported physical and mental health outcomes and behaviors), it may be possible to say more on this point

References

Altonji, J., Elder, T. and C. Taber (2005), Selection on Observed and Unobserved Variables: Assessing the Effectiveness of Catholic Schools, *Journal of Political Economy*, vol. 113(1): 151-184.

Batty, G., Morton, S., Campbell D. et al. (2004), The Aberdeen Children of the 1950s Cohort Study: Background, Methods, and Follow-up Information on a New Resource for the Study of Life-Course and Intergenerational Effects on Health, *Paediatric and Perinatal Epidemiology*, vol. 18:221-239.

Becker, G. (1960), An Economic Analysis of Fertility, in Demographic and Economic change in Developed Countries, Universities-National Bureau of Economic Research Conference Series 11. NBER: Princeton, NJ, 209-231

Black, S., Devereux, P. and K. Salvanes (2008), Staying in the Classroom and out of the Maternity Ward? The Effect of Compulsory Schooling Laws on Teenage Birth, forthcoming in *Economic Journal*.

Card, D. (2001), Estimating the Return to Schooling: Progress on Some Persistent Econometric Problems, *Econometrica*, vol. 69: 1127-60.

Cullen, J. and B. Jacob (2008), Is Gaining Access to a Selective Elementary School Gaining Ground? Evidence from Randomized Lotteries, forthcoming in J.Gruber (ed.) *An Economics Perspective on the Problems of Disadvantaged Youth*. Chicago, IL: University of Chicago Press.

Currie, J. and E. Moretti (2003), Mother's education and the intergenerational transmission of human capital: evidence from college openings, *Quarterly Journal of Economics*, vol. 118:1495-1532.

Hanushek, E. and L. Woessman (2006), Does Educational Trucking Affect Performance and Inequality? Differences-in-Differences Evidence Across Countries, *Economic Journal*, vol. 116: C63-C76.

Hunter, S. L. (1963), Scottish Education: Changes in the examination structure in secondary schools, *International Review of Education*, vol. 9 (3): 310-324.

Illsley, R. (2002), A City's Schools: from Inequality of Input to Inequality of Outcome, *Oxford Review of Education*, vol. 28: 427-445.

Illsley R, and Wilson F. (1981), Longitudinal Studies in Aberdeen, Scotland. C. The Aberdeen Child Development Survey. In: *Prospective Longitudinal Research. An Empirical Basis for the Primary Prevention of Psychosocial Disorders*. Editors: Mednick S, Baert A, Bachmann B. Oxford, UK: Oxford University Press, pp. 66–68.

Lee, D. (2008), Training, Wages, and Sample Selection: Estimating Sharp Bounds on Treatment Effects, forthcoming in *Review of Economic Studies*.

Leon, D, Lawlor, D., Clark, H. and S. Macintyre (2006), Cohort Profile: The Aberdeen Chilren of the 1950s Study, *International Journal of Epidemiology*, vol. 35: 549-552.

Lleras-Muney, A. (2005), The Relationship Between Education and Adult Mortality in the United States, *Review or Economic Studies* vol. 72(1): 155-189.

Manning, A. and S. Pischke (2006), Comprehensive versus Selective Schooling in England and Wales: What Do We Know?, IZA Discussion Paper n. 2072.

Oreopoulos, P. (2007), Do Dropouts Drop out Too Soon? Wealth, Health and Happiness from Compulsory Schooling, *Journal of Public Economics*, vol. 91(11-12): 2213-2229.

Robbins, C. (1963), *Higher education: report of the Committee appointed by the Prime Minister under the Chairmanship of Lord Robbins 1961-63*, Cmnd. 2154, London: HMSO.

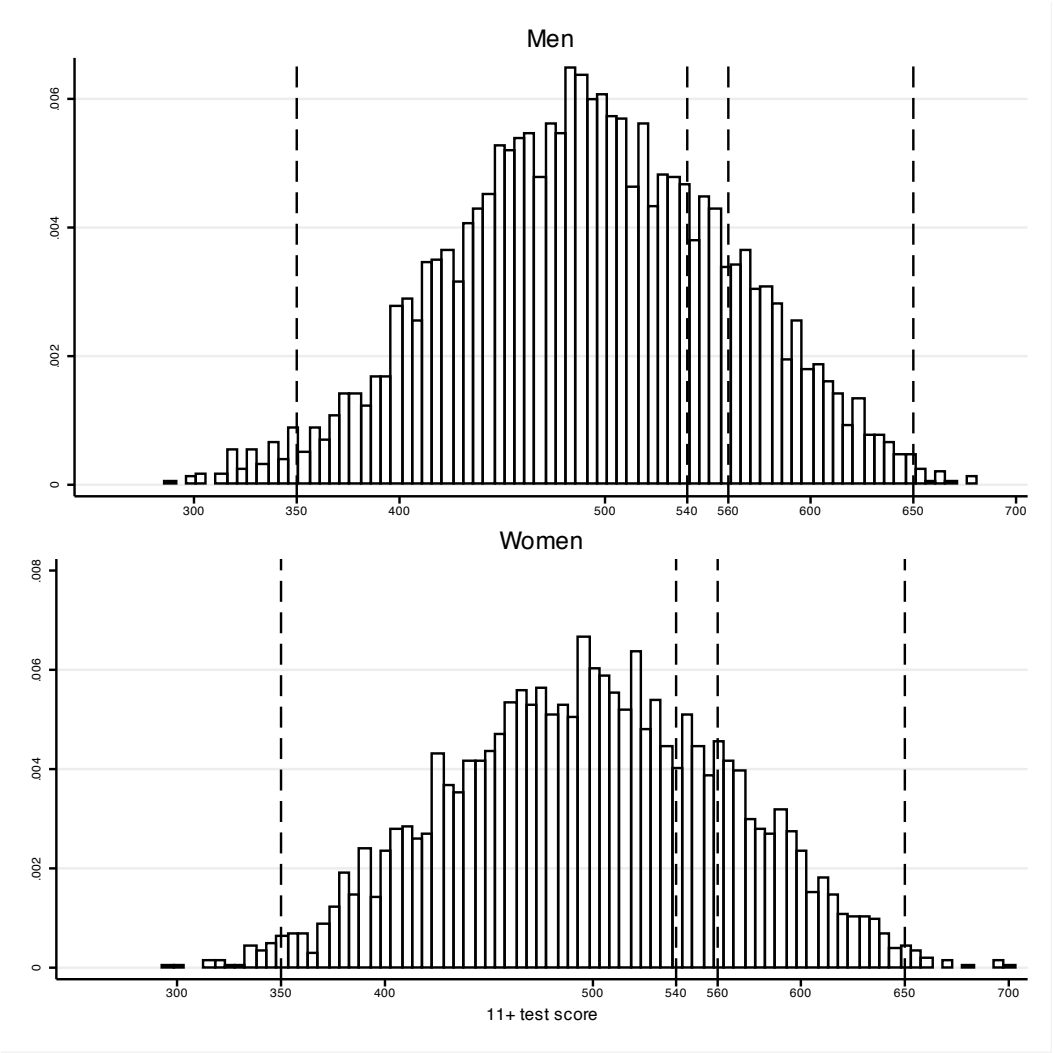
Sacerdote, B. (2001), Peer Effects With Random Assignment: Results For Dartmouth Roommates, *Quarterly Journal of Economics*, vol. 116(2): 681-704.

Stewart, M. (1983), On Least-Squares Estimation when the Dependent Variable is Grouped, *Review of Economic Studies*, vol. 50: 737-753.

Thistlethwaite, D. and D. Campbell (1960), Regression-Discontinuity Analysis: An alternative to the ex post facto experiment, *Journal of Educational Psychology*, vol. 51: 309–317.

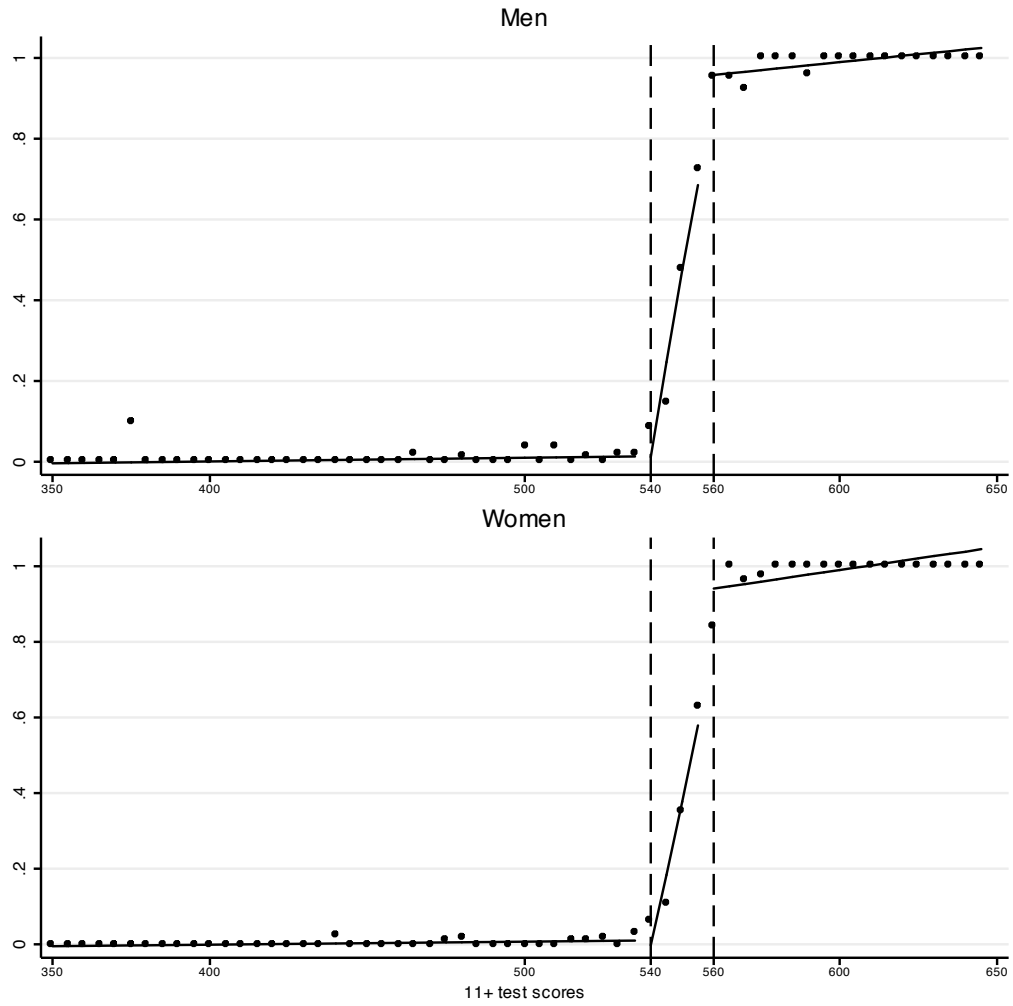
Willis, R. (1973), A New Approach to the Economic Theory of Fertility Behavior, *Journal of Political Economy*, vol. 81: 514-64.

Figure 1: Distribution of test scores at age 11



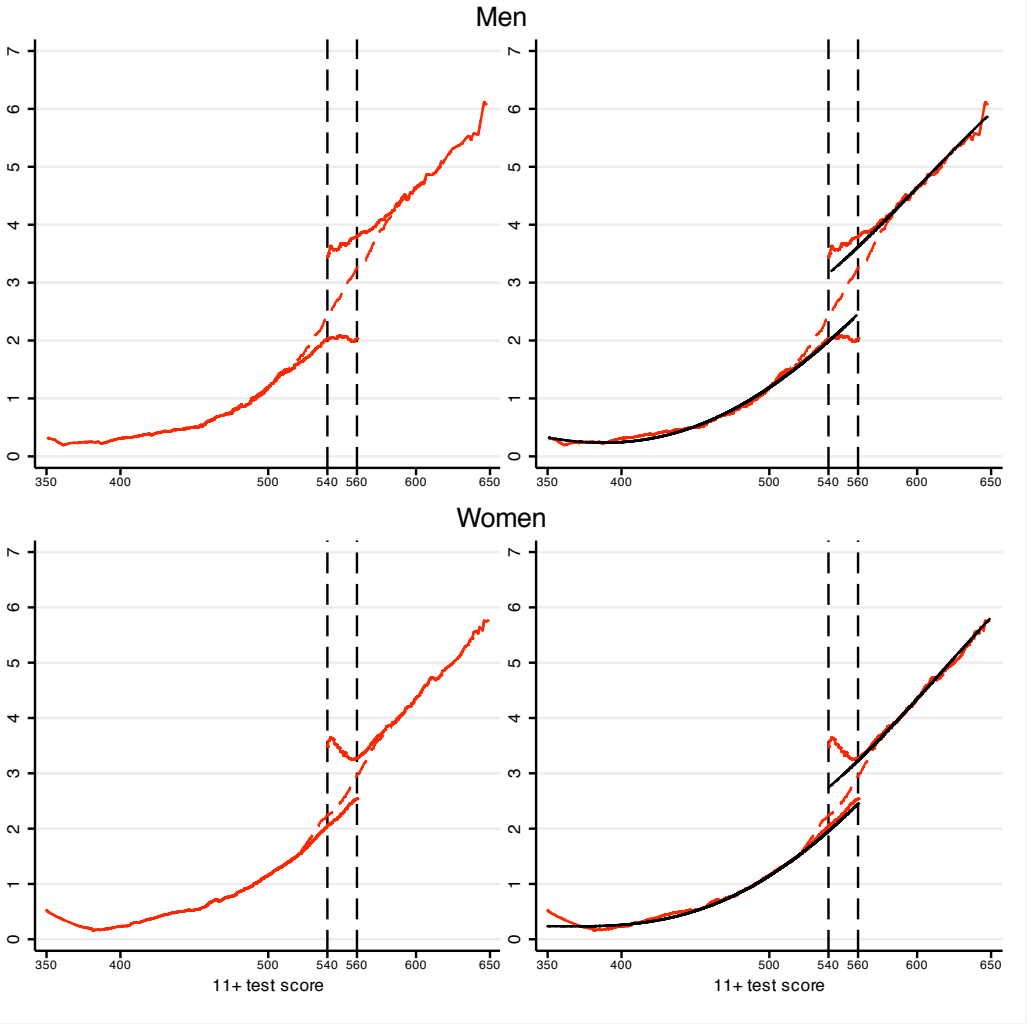
Note: Sample includes all children in grades 4-7 in 1962 with non-missing values of the test scores. Each bar is drawn over an interval defined by 5 values of the test score. Vertical lines plotted at the following values of the test score: [350-354], [540-544], [560-564], [650-654].

Figure 2: Relationship between test at age 11 score and probability to be in an elite school



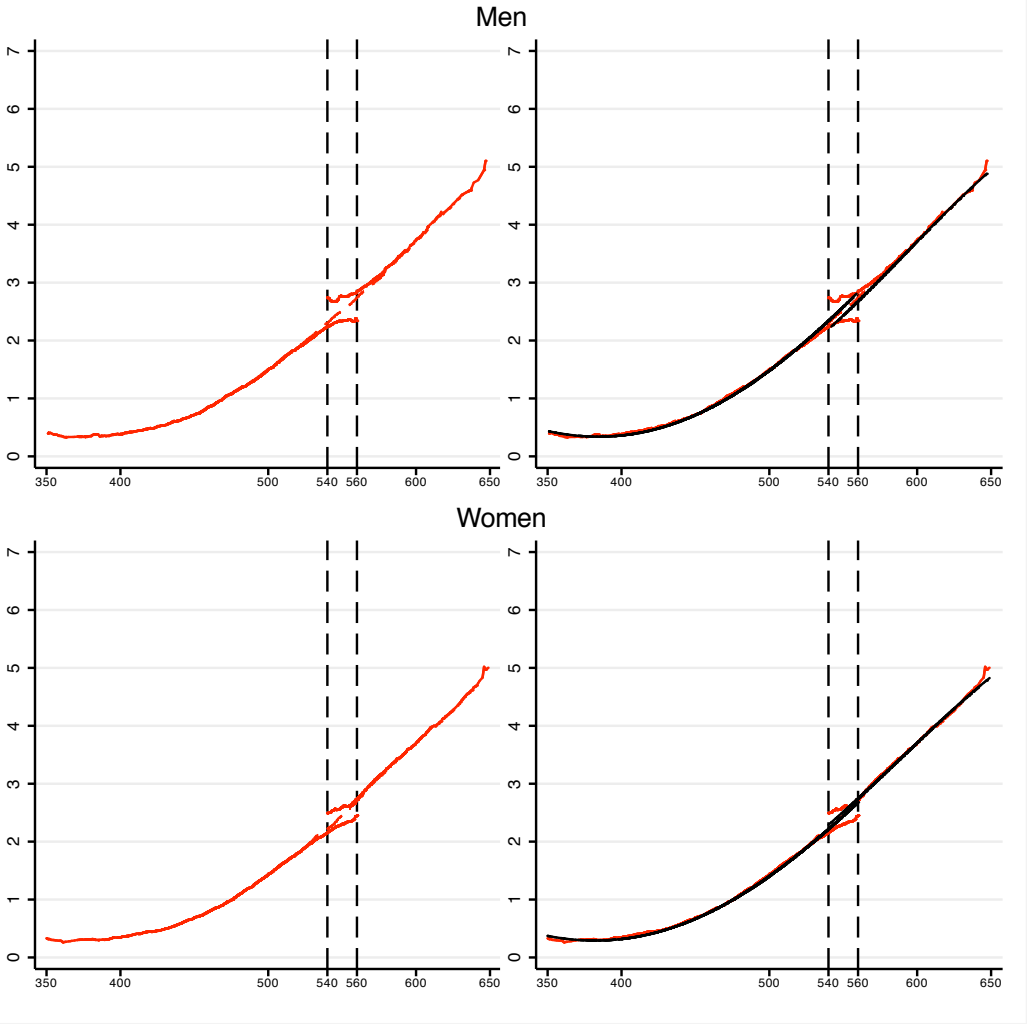
Notes: Sample includes all men and women in grades 4-7 in 1962 who reply to the 2001 survey and went to a non-private secondary school. Each circle represents the percentage of pupils in elite school in each cell. Cells are defined over 5 test scores values. The solid line represents the predicted probability of being in an elite school estimated as a function of the test scores at age 11 according to equation (xx).

Figure 3: Relationship between score at age 11 and years of post-compulsory schooling



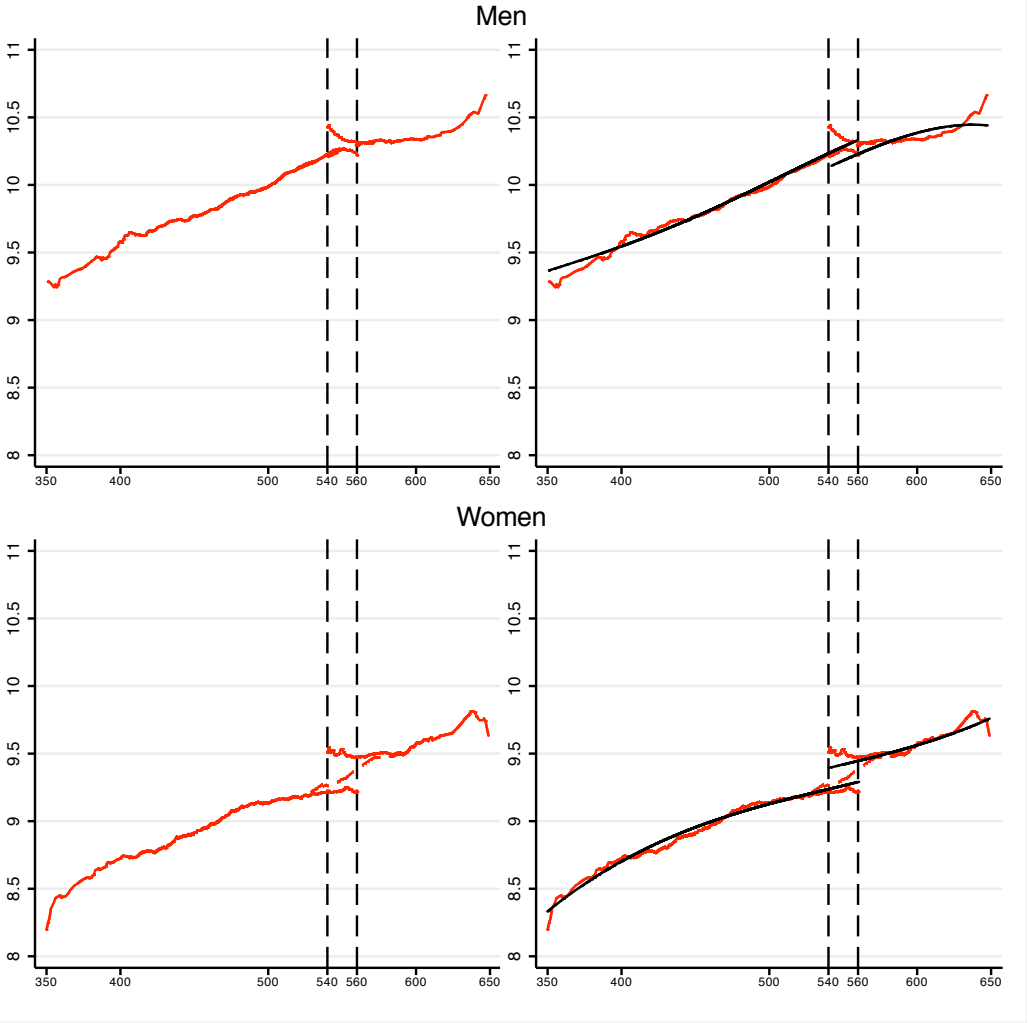
Notes: Sample includes all men and women in grades 4-7 in 1962 who reply to the 2001 survey and went to a non-private secondary school. The solid lighter lines are drawn using separate local polynomial regressions of years of post-compulsory schooling on values of the 11+ test score for individuals who did not attend (left of 560) or attended (right of 540) an elite secondary school. The dotted line is drawn from a local polynomial regression of years of post-compulsory schooling on the 11+ test score over the entire range of values of the latter. For these local polynomial regressions we use a 1st degree polynomial, a rectangular kernel and a bandwidth of 30.The solid darker lines (right-hand side figures) represent predicted values of the outcome variable obtained by running an Instrumental Variable regression of years of post-compulsory schooling on elite school attendance and a polynomial of fourth order in the 11+ test score. The instruments used are a dummy for having a test score in the interval [540-559] interval, a dummy for having a test score in the [560-650] interval, and their interactions with the test score values.

Figure 4: Relationship between score at age 11 and years of post-compulsory schooling, falsification tests



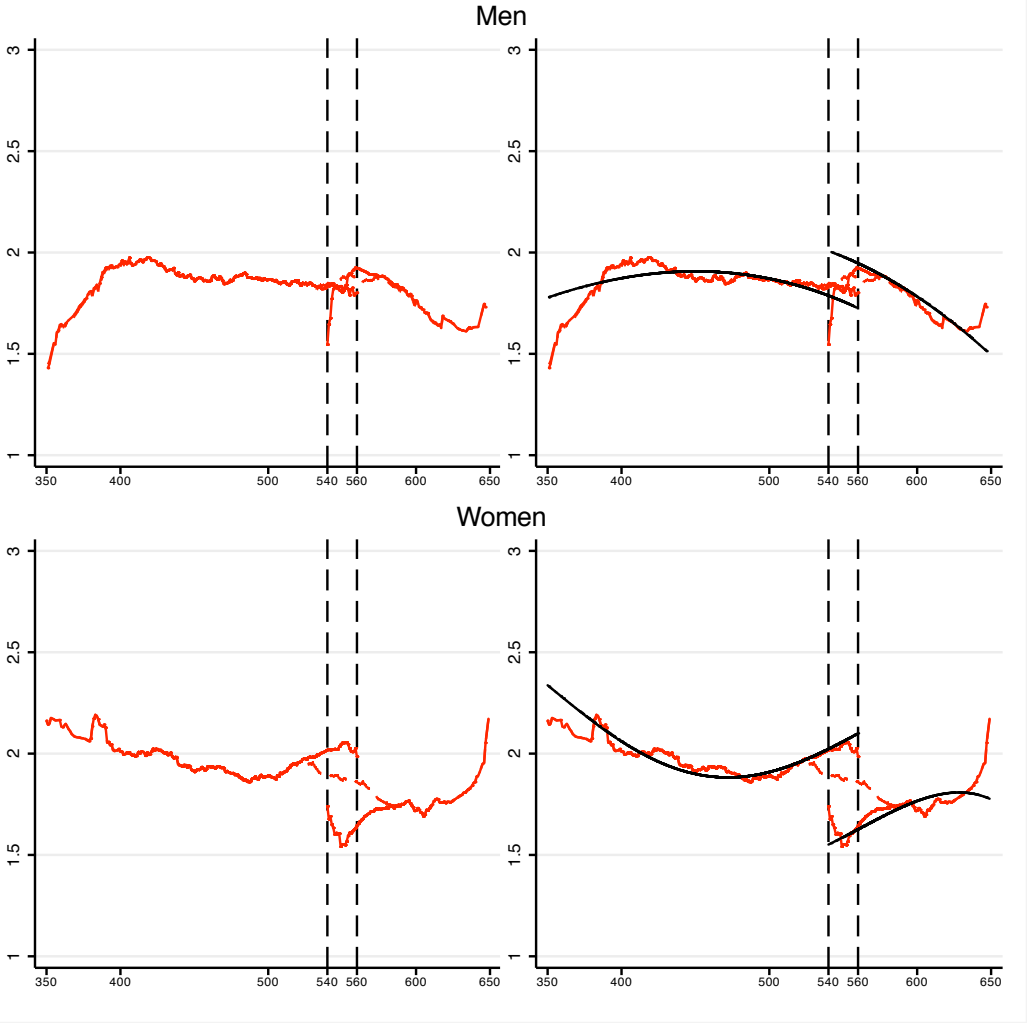
Notes: Sample includes all men and women in grades 4-7 in 1962 who reply to the 2001 survey and went to a non-private secondary school. The outcome variable is given by the year of post-compulsory schooling predicted from a regression of years of compulsory schooling on: dummies for father's occupation, mother's socio-economic status, school and grade attended in 1962, continuous variables capturing relative age within the school-grade, and fourth-degree polynomials in test scores at age 7 and 9. The solid lighter lines are drawn using separate local polynomial regressions of years of predicted post-compulsory schooling on values of the 11+ test score for individuals who did not attend (left of 560) or attended (right of 540) an elite secondary school. The dotted line is drawn from a local polynomial regression of predicted years of post-compulsory schooling on the 11+ test score over the entire range of values of the latter. For these local polynomial regressions we use a 1st degree polynomial, a rectangular kernel and a bandwidth of 30.The solid darker lines (right-hand side figures) represent predicted values of the outcome variable obtained by running an Instrumental Variable regression of predicted years of post-compulsory schooling on elite school attendance and a polynomial of fourth order in the 11+ test score. The instruments used are a dummy for having a test score in the interval [540-559] interval, a dummy for having a test score in the [560-650] interval, and their interactions with the test score values.

Figure 5: Relationship between score at age 11 and log annual income



Notes: Sample includes all men and women in grades 4-7 in 1962 who reply to the 2001 survey and went to a non-private secondary school. The solid lighter lines are drawn using separate local polynomial regressions of log annual income (for all) on values of the 11+ test score for individuals who did not attend (left of 560) or attended (right of 540) an elite secondary school. The dotted line is drawn from a local polynomial regression of log annual income on the 11+ test score over the entire range of values of the latter. For these local polynomial regressions we use a 1st degree polynomial, a rectangular kernel and a bandwidth of 30.The solid darker lines (right-hand side figures) represent predicted values of the outcome variable obtained by running an Instrumental Variable regression of log annual income on elite school attendance and a polynomial of fourth order in the 11+ test score. The instruments used are a dummy for having a test score in the interval [540-559] interval, a dummy for having a test score in the [560-650] interval, and their interactions with the test score values.

Figure 6: Relationship between score at age 11 and number of children



Notes: Sample includes all men and women in grades 4-7 in 1962 who reply to the 2001 survey and went to a non-private secondary school. The solid lighter lines are drawn using separate local polynomial regressions of number of children on values of the 11+ test score for individuals who did not attend (left of 560) or attended (right of 540) an elite secondary school. The dotted line is drawn from a local polynomial regression of number of children on the 11+ test score over the entire range of values of the latter. For these local polynomial regressions we use a 1st degree polynomial, a rectangular kernel and a bandwidth of 30.The solid darker lines (right-hand side figures) represent predicted values of the outcome variable obtained by running an Instrumental Variable regression of number of children on elite school attendance and a polynomial of fourth order in the 11+ test score. The instruments used are a dummy for having a test score in the [540-559] interval, a dummy for having a test score in the [560-650] interval, and their interactions with the test score values.

Table 1: Probability of being in an elite school as a function of the test score at age 11

	MEN				
	(1)	(2)	(3)	(4)	(5)
Dummy [540-559]	0.822	0.810	0.826	0.821	0.823
	(0.054)	(0.055)	(0.056)	(0.056)	(0.058)
Dummy [540-559] * Test11	0.045	0.045	0.045	0.045	0.045
	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)
Dummy [560-649]	0.941	0.930	0.939	0.930	0.941
	(0.014)	(0.017)	(0.016)	(0.021)	(0.021)
Dummy [560-649] * Test11	0.001	0.000	0.002	0.002	0.002
	(0.000)	(0.000)	(0.001)	(0.001)	(0.001)
Test11	Y	Y	Y	Y	Y
Test11^2		Y	Y	Y	Y
Test11^3			Y	Y	Y
Test11^4				Y	Y
Xs					Y
Observations	2,072	2,072	2,072	2,072	2,072
R^2	0.852	0.852	0.852	0.852	0.858
F-test	9562	7053	8076	3295	730.6
	WOMEN				
	(1)	(2)	(3)	(4)	(5)
Dummy [540-559]	0.678	0.677	0.709	0.684	0.681
	(0.056)	(0.057)	(0.059)	(0.059)	(0.058)
Dummy [540-559] * Test11	0.038	0.038	0.039	0.039	0.038
	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)
Dummy [560-649]	0.927	0.926	0.941	0.903	0.899
	(0.019)	(0.019)	(0.016)	(0.023)	(0.023)
Dummy [560-649] * Test11	0.001	0.001	0.004	0.004	0.004
	(0.000)	(0.001)	(0.001)	(0.001)	(0.001)
Test11	Y	Y	Y	Y	Y
Test11^2		Y	Y	Y	Y
Test11^3			Y	Y	Y
Test11^4				Y	Y
Xs					Y
Observations	2,397	2,397	2,397	2,397	2,397
R^2	0.872	0.872	0.872	0.872	0.876
F-test	17880	15020	24914	11021	1370

Notes: Sample includes all children in grades 4-7 in 1962 who reply to the 2001 survey and went to a non-private secondary school. Probability of being in an elite school is modelled as a function of the test score at age 11, dummies for the marginal area [540-559] and the area to the right of the second cutoff point [560-649] and their interactions with the test score. Estimation method is via linear probability models. Standard errors clustered according to values of the test score at age 11. Specifications (2) to (4) include higher order polynomials of the test score at age 11, specification (5) also includes controls for school in 1962, grade in 1962, occupation of the father, socio-economic status of the mother, and two polynomials of grade 4 for the tests at age 7 and at age 9. The F-test reported represents the value of the F-statistics when the coefficients shown are excluded from the model.

Table 2: Impact of attending an elite school on years of post-compulsory education and highest educational qualification attained

	MEN					WOMEN				
	Linear Probability Model			GMM estimates		Linear Probability Model			GMM estimates	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Panel A: Years of postcompulsory education										
Elite school	3.228	1.527	1.421	0.839	0.981	2.914	0.801	0.639	0.830	0.760
	(0.151)	(0.261)	(0.251)	(0.314)	(0.304)	(0.135)	(0.181)	(0.181)	(0.264)	(0.259)
F(Test11)		Y	Y	Y	Y		Y	Y	Y	Y
Control variables			Y		Y			Y		Y
Dep. variable	1.652	1.652	1.652	1.652	1.652	1.781	1.781	1.781	1.781	1.781
Observations	2,070	2,070	2,070	2,070	2,070	2,396	2,396	2,396	2,396	2,396
Overid. test				0.412	0.587				0.217	0.217
Panel B: No qualifications										
Elite school	-0.274	-0.06	-0.038	-0.014	-0.014	-0.271	0.004	0.019	0.016	0.022
	(0.017)	(0.032)	(0.031)	(0.038)	(0.042)	(0.020)	(0.027)	(0.027)	(0.045)	(0.045)
F(Test11)		Y	Y	Y	Y		Y	Y	Y	Y
Control variables			Y		Y			Y		Y
Dep. variable	0.232	0.232	0.232	0.232	0.232	0.246	0.246	0.246	0.246	0.246
Observations	2,070	2,070	2,070	2,070	2,070	2,396	2,396	2,396	2,396	2,396
Overid. test				0.875	0.848				0.325	0.414
Panel C: Ungraded CSE, school leaving cert., other										
Elite school	0.057	0.038	0.040	0.104	0.101	-0.028	-0.069	-0.081	-0.075	-0.076
	(0.024)	(0.044)	(0.044)	(0.063)	(0.061)	(0.023)	(0.050)	(0.052)	(0.068)	(0.067)
F(Test11)		Y	Y	Y	Y		Y	Y	Y	Y
Control variables			Y		Y			Y		Y
Dep. variable	0.176	0.176	0.176	0.176	0.176	0.309	0.309	0.309	0.309	0.309
Observations	2,070	2,070	2,070	2,070	2,070	2,396	2,396	2,396	2,396	2,396
Overid. test				0.385	0.468				0.710	0.749
Panel D: O level										
Elite school	0.342	0.097	0.088	0.110	0.123	0.316	0.018	0.002	-0.041	-0.053
	(0.026)	(0.046)	(0.046)	(0.064)	(0.065)	(0.024)	(0.037)	(0.038)	(0.065)	(0.065)
F(Test11)		Y	Y	Y	Y		Y	Y	Y	Y
Control variables			Y		Y			Y		Y
Dep. variable	0.614	0.614	0.614	0.614	0.614	0.605	0.605	0.605	0.605	0.605
Observations	2,070	2,070	2,070	2,070	2,070	2,396	2,396	2,396	2,396	2,396
Overid. test				0.0885	0.0885				0.389	0.448
Panel E: A levels										
Elite school	0.522	0.220	0.202	0.066	0.093	0.601	0.240	0.218	0.168	0.161
	(0.027)	(0.051)	(0.048)	(0.076)	(0.070)	(0.025)	(0.045)	(0.042)	(0.062)	(0.060)
F(Test11)		Y	Y	Y	Y		Y	Y	Y	Y
Control variables			Y		Y			Y		Y
Dep. variable	0.370	0.370	0.370	0.370	0.370	0.282	0.282	0.282	0.282	0.282
Observations	2,070	2,070	2,070	2,070	2,070	2,396	2,396	2,396	2,396	2,396
Overid. test				0.495	0.522				0.0285	0.0235
Panel F: HNC, teaching, etc.										
Elite school	0.076	0.040	0.028	-0.042	-0.023	0.268	0.035	0.001	0.081	0.054
	(0.027)	(0.054)	(0.053)	(0.083)	(0.083)	(0.024)	(0.040)	(0.041)	(0.060)	(0.056)
F(Test11)		Y	Y	Y	Y		Y	Y	Y	Y
Control variables			Y		Y			Y		Y
Dep. variable	0.267	0.267	0.267	0.267	0.267	0.203	0.203	0.203	0.203	0.203
Observations	2,070	2,070	2,070	2,070	2,070	2,396	2,396	2,396	2,396	2,396
Overid. test				0.540	0.602				0.179	0.191
Panel G: Degree										
Elite school	0.425	0.199	0.190	0.149	0.162	0.325	0.055	0.044	0.051	0.046
	(0.028)	(0.047)	(0.044)	(0.059)	(0.054)	(0.025)	(0.037)	(0.035)	(0.046)	(0.047)
F(Test11)		Y	Y	Y	Y		Y	Y	Y	Y
Control variables			Y		Y			Y		Y
Dep. variable	0.158	0.158	0.158	0.158	0.158	0.131	0.131	0.131	0.131	0.131
Observations	2,070	2,070	2,070	2,070	2,070	2,396	2,396	2,396	2,396	2,396
Overid. test				0.330	0.425				0.358	0.312

Notes: Sample includes all men and women in grades 4-7 in 1962 who reply to the 2001 survey and went to a non-private secondary school. Columns (2)-(5) and (7)-(10) control for a 4th order polynomial in test scores at age 11; columns (3), (5), (8) and (10) additionally control for socio-demographic characteristics of the individual and past test scores (4th order polynomials). GMM estimates are obtained instrumenting the elite school dummy using dummies for the marginal area [540-559] and the area to the right of the second cutoff point [560-650] and their interactions with the test score. Standard errors clustered according to values of the test score at age 11.

Table 3: Impact of attending an elite school on labour market outcomes

	MEN					WOMEN				
	Linear Probability Model			GMM estimates		Linear Probability Model			GMM estimates	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Panel A: Employment										
Elite school	0.055	0.015	0.013	0.016	0.025	0.053	0.008	0.018	0.045	0.062
	(0.014)	(0.029)	(0.027)	(0.045)	(0.043)	(0.016)	(0.033)	(0.034)	(0.051)	(0.054)
F(Test11)		Y	Y	Y	Y		Y	Y	Y	Y
Control variables			Y		Y			Y		Y
Dep. variable	0.899	0.899	0.899	0.899	0.899	0.811	0.811	0.811	0.811	0.811
Observations	2,066	2,066	2,066	2,066	2,066	2,387	2,387	2,387	2,387	2,387
Overid. test				0.698	0.488				0.612	0.745
Panel B: Log annual income (all)										
Elite school	0.420	0.032	0.007	-0.145	-0.137	0.489	0.283	0.296	0.287	0.325
	(0.032)	(0.067)	(0.065)	(0.083)	(0.081)	(0.042)	(0.093)	(0.096)	(0.123)	(0.126)
F(Test11)		Y	Y	Y	Y		Y	Y	Y	Y
Control variables			Y		Y			Y		Y
Dep. variable	10.00	10.00	10.00	10.00	10.00	9.172	9.172	9.172	9.172	9.172
Observations	2,029	2,029	2,029	2,029	2,029	2,335	2,335	2,335	2,335	2,335
Overid. test				0.290	0.564				0.809	0.728
Panel C: Log annual income (working employees only)										
Elite school	0.335	0.038	0.025	-0.167	-0.165	0.472	0.154	0.137	0.111	0.119
	(0.026)	(0.053)	(0.053)	(0.080)	(0.075)	(0.040)	(0.080)	(0.081)	(0.124)	(0.122)
F(Test11)		Y	Y	Y	Y		Y	Y	Y	Y
Control variables			Y		Y			Y		Y
Dep. variable	10.14	10.14	10.14	10.14	10.14	9.361	9.361	9.361	9.361	9.361
Observations	1,552	1,552	1,552	1,552	1,552	1,785	1,785	1,785	1,785	1,785
Overid. test				0.0315	0.0288				0.465	0.319
Panel D: NES-inputed log hourly wage (working employees only)										
Elite school	0.356	0.050	0.048	-0.094	-0.073	0.354	0.073	0.055	0.140	0.129
	(0.023)	(0.046)	(0.045)	(0.060)	(0.061)	(0.024)	(0.041)	(0.041)	(0.053)	(0.051)
F(Test11)		Y	Y	Y	Y		Y	Y	Y	Y
Control variables			Y		Y			Y		Y
Dep. variable	2.184	2.184	2.184	2.184	2.184	1.829	1.829	1.829	1.829	1.829
Observations	1,858	1,858	1,858	1,858	1,858	1,905	1,905	1,905	1,905	1,905
Overid. test				0.111	0.303				0.495	0.296

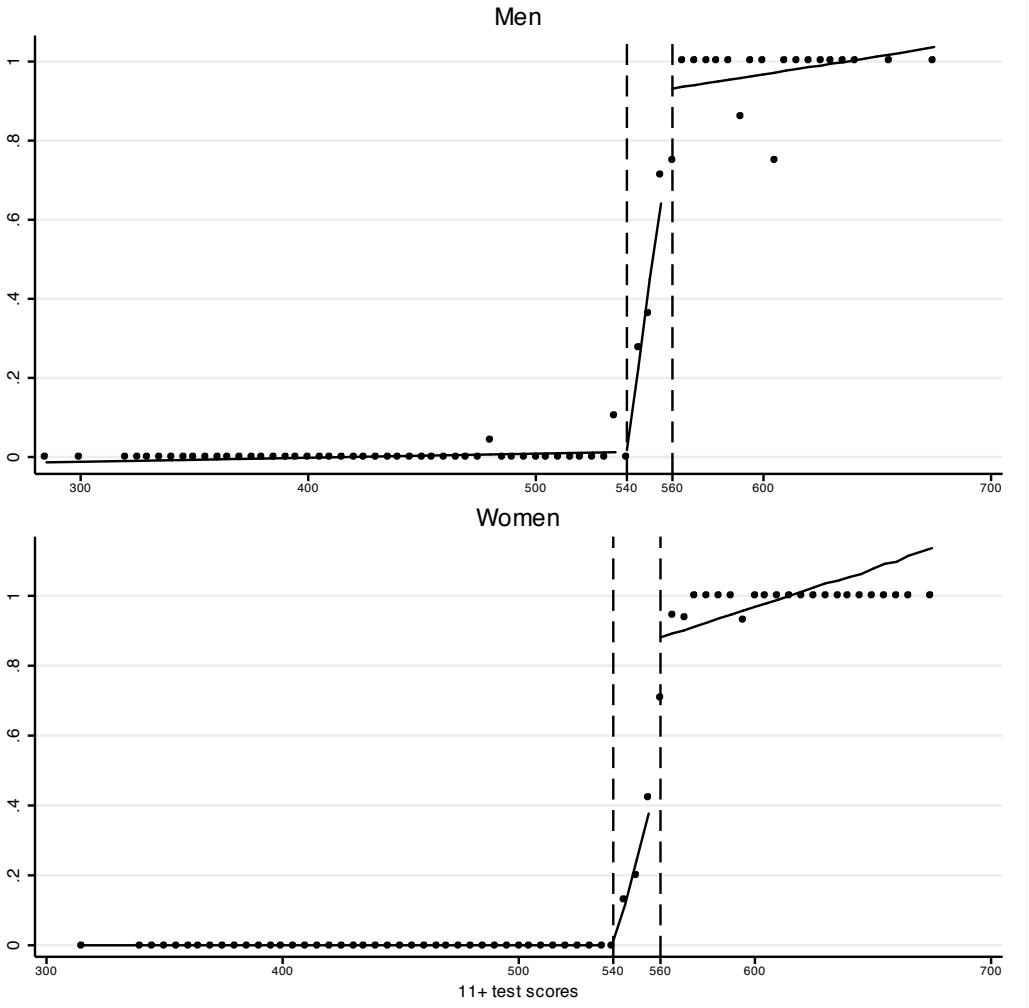
Notes: Sample includes all men and women in grades 4-7 in 1962 who reply to the 2001 survey and who went to a non-private school. Columns (2)-(5) and (7)-(10) control for a 4th order polynomial in test scores at age 11; columns (3), (5), (8) and (10) additionally control for socio-demographic characteristics of the individual and past test scores (4th order polynomials). GMM estimates are obtained instrumenting the elite school dummy using dummies for the marginal area [540-559] and the area to the right of the second cutoff point [560-650] and their interactions with the test score. Standard errors clustered according to values of the test score at age 11.

Table 4: Impact of attending an elite school on fertility and marriage

	MEN					WOMEN				
	Linear Probability Model			GMM estimates		Linear Probability Model			GMM estimates	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Panel A: Any child										
Elite school	-0.019	-0.015	-0.001	-0.037	-0.017	-0.069	-0.084	-0.072	-0.157	-0.148
	(0.021)	(0.043)	(0.042)	(0.064)	(0.065)	(0.017)	(0.036)	(0.036)	(0.049)	(0.049)
F(Test11)		Y	Y	Y	Y		Y	Y	Y	Y
Control variables			Y		Y			Y		Y
Dep. variable	0.846	0.846	0.846	0.846	0.846	0.876	0.876	0.876	0.876	0.876
Observations	2,068	2,068	2,068	2,068	2,068	2,394	2,394	2,394	2,394	2,394
Overid. test				0.720	0.703				0.695	0.717
Panel B: Number of children										
Elite school	-0.030	0.084	0.135	0.125	0.148	-0.214	-0.392	-0.349	-0.654	-0.623
	(0.060)	(0.117)	(0.114)	(0.188)	(0.181)	(0.050)	(0.120)	(0.121)	(0.137)	(0.132)
F(Test11)		Y	Y	Y	Y		Y	Y	Y	Y
Control variables			Y		Y			Y		Y
Dep. variable	1.865	1.865	1.865	1.865	1.865	1.892	1.892	1.892	1.892	1.892
Observations	2,068	2,068	2,068	2,068	2,068	2,394	2,394	2,394	2,394	2,394
Overid. test				0.935	0.851				0.689	0.747
Panel C: Age at first child										
Elite school	2.785	0.808	0.586	0.476	0.457	2.587	0.732	0.301	0.212	-0.005
	(0.311)	(0.590)	(0.583)	(0.909)	(0.795)	(0.259)	(0.566)	(0.567)	(0.798)	(0.819)
F(Test11)		Y	Y	Y	Y		Y	Y	Y	Y
Control variables			Y		Y			Y		Y
Dep. variable	26.08	26.08	26.08	26.08	26.08	23.78	23.78	23.78	23.78	23.78
Observations	1,722	1,722	1,722	1,722	1,722	2,085	2,085	2,085	2,085	2,085
Overid. test				0.625	0.880				0.843	0.930
Panel D: First child before age 27										
Elite school	-0.229	-0.103	-0.079	-0.067	-0.039	-0.189	-0.051	-0.022	-0.030	-0.016
	(0.031)	(0.056)	(0.056)	(0.086)	(0.076)	(0.024)	(0.041)	(0.042)	(0.066)	(0.066)
F(Test11)		Y	Y	Y	Y			Y	Y	Y
Control variables			Y		Y			Y		Y
Dep. variable	0.666	0.666	0.666	0.666	0.666	0.792	0.792	0.792	0.792	0.792
Observations	1,722	1,722	1,722	1,722	1,722	2,085	2,085	2,085	2,085	2,085
Overid. test				0.358	0.662				0.997	0.967
Panel E: Currently married										
Elite school	0.012	-0.049	-0.038	-0.057	-0.056	0.008	-0.004	-0.010	0.025	0.034
	(0.027)	(0.051)	(0.051)	(0.077)	(0.075)	(0.019)	(0.042)	(0.043)	(0.061)	(0.063)
F(Test11)		Y	Y	Y	Y		Y	Y	Y	Y
Control variables			Y		Y			Y		Y
Dep. variable	0.752	0.752	0.752	0.752	0.752	0.729	0.729	0.729	0.729	0.729
Observations	2,063	2,063	2,063	2,063	2,063	2,389	2,389	2,389	2,389	2,389
Overid. test				0.157	0.160				0.657	0.718
Panel F: Ever divorced										
Elite school	-0.050	0.037	0.046	0.090	0.098	-0.031	-0.036	-0.018	-0.122	-0.118
	(0.024)	(0.044)	(0.047)	(0.077)	(0.077)	(0.025)	(0.053)	(0.053)	(0.076)	(0.076)
F(Test11)		Y	Y	Y	Y		Y	Y	Y	Y
Control variables			Y		Y			Y		Y
Dep. variable	0.318	0.318	0.318	0.318	0.318	0.369	0.369	0.369	0.369	0.369
Observations	1,862	1,862	1,862	1,862	1,862	2,222	2,222	2,222	2,222	2,222
Overid. test				0.118	0.0932				0.371	0.468

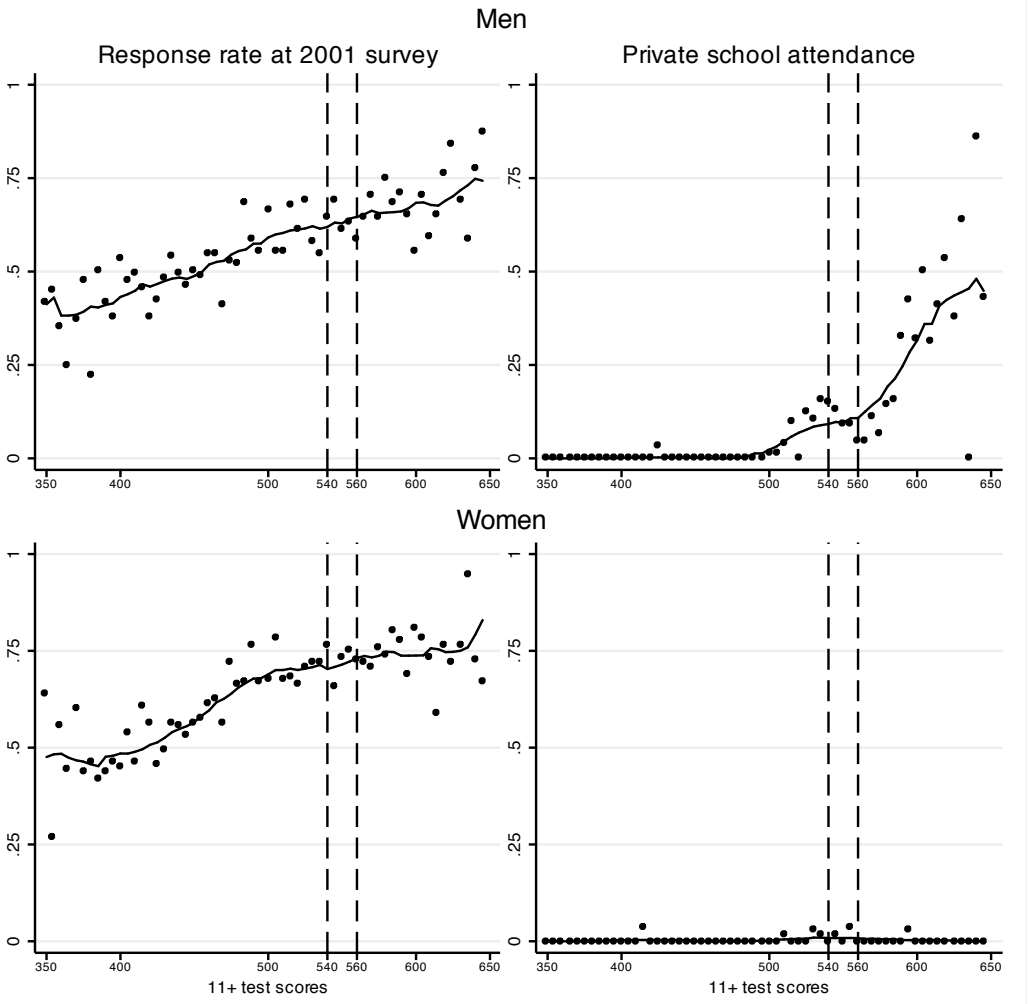
Notes: Sample includes all men and women in grades 4-7 in 1962 who reply to the 2001 survey and who went to a non-private school. Columns (2)-(5) control for a 4th order polynomial in test scores at age 11; columns (3), (5) additionally control for socio-demographic characteristics of the individual and past test scores (4th order polynomials). GMM estimates are obtained instrumenting the elite school dummy using dummies for the marginal area [540-559] and the area to the right of the second cutoff point [560-649] and their interactions with the test score. Standard errors clustered according to values of the test score at age 11.

Appendix Figure 1: Relationship between test at age 11 score and probability to be in an elite school in 1964



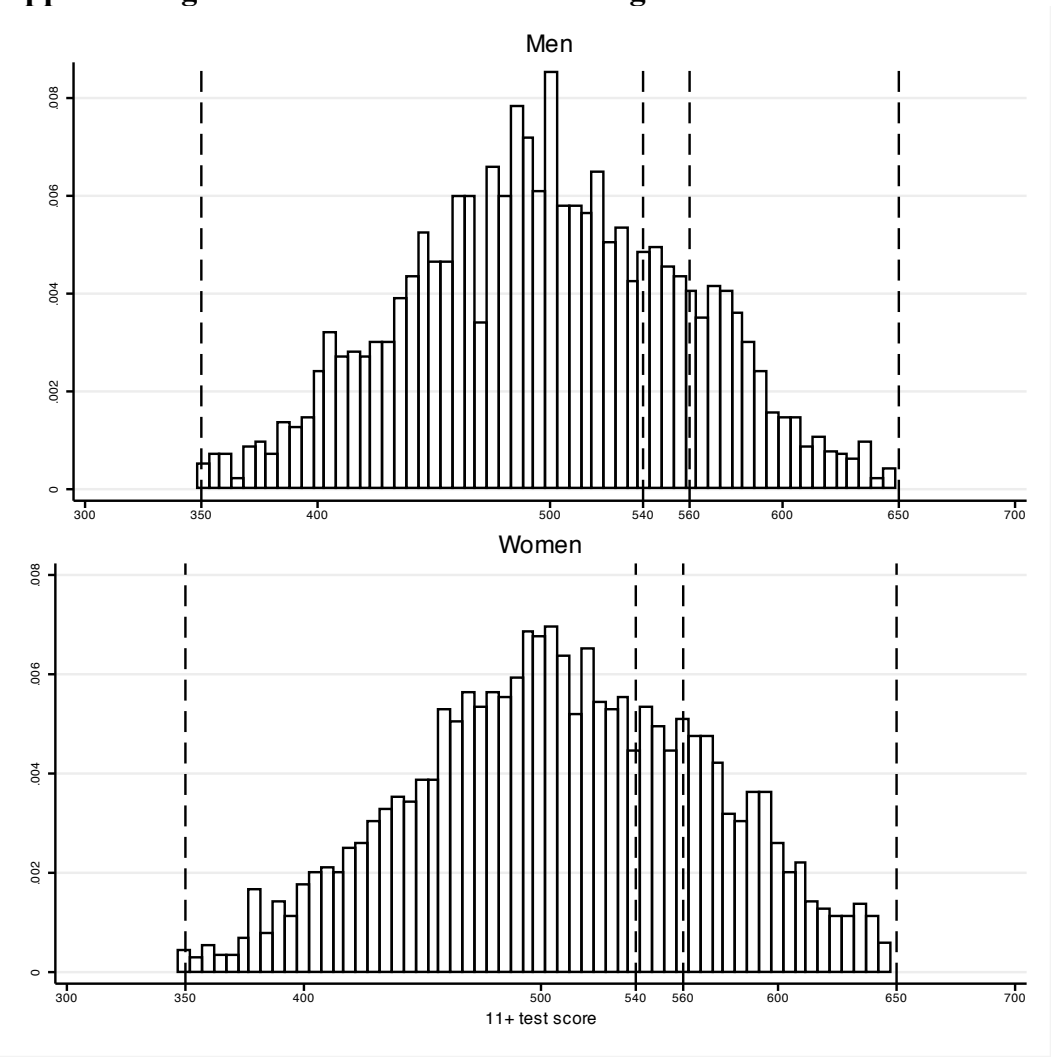
Notes: Sample includes only men and women in grade 7 in 1962 who are observed in secondary school in 1964. Each circle represents the percentage of pupils in elite school in each cell. Cells are defined over 5 test scores values. The solid line represents the predicted probability of being in an elite school estimated as a function of the test scores at age 11 according to equation (xx).

Appendix Figure 2: Relationship between test at age 11 score and response rate at 2001 (left) or probability to be in private school (right)



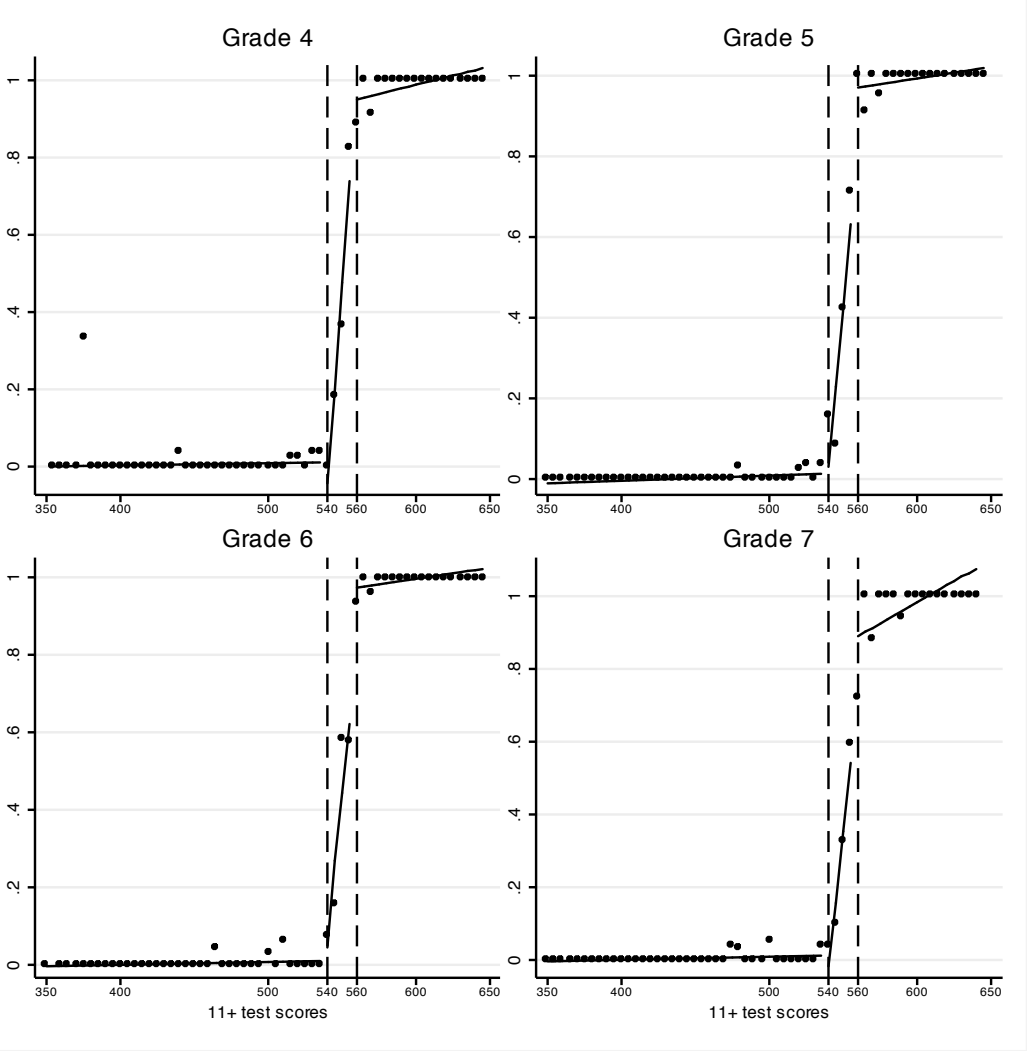
Notes: Sample includes all men and women in grades 4-7 in 1962. Each circle represents the percentage of pupils who respond to the 2001 survey in each cell (figures to the left) or who attend a private secondary school (figures to the right). Cells are defined over 5 test scores values. Vertical lines plotted at the [540-544] and [560-564] values of the test score. Solid lines represent local polynomial regressions performed separately for the 3 separate intervals identified by the following values of the test score at age 11: [350-539], [540-559] and [560-650]. The interval [540-559] represents the marginal area. For these local polynomial regressions we use a 1st degree polynomial, a rectangular kernel and a bandwidth of 30 for the areas to the right and the left of the interval [540-559] and a bandwidth of 15 for the interval [540-559].

Appendix Figure 3: Distribution of test at age 11 scores after selections



Notes: Sample includes all men and women in grades 4-7 in 1962 who reply to the 2001 survey and went to a non-private secondary school. Each bar is drawn over an interval defined by 5 values of the test score. Vertical lines plotted at the following values of the test score: [350-354], [540-544], [560-564], [650-654].

Appendix Figure 4: Relationship between test at age 11 score and probability to be in an elite school by grade in 1962



Notes: Sample include all children in grades 4-7 in 1962 who reply to the 2001 survey and went to a non-private secondary school. Each circle represents the percentage of pupils in elite school in each cell. Cells are defined over 5 test scores values. The solid line represents the predicted probability of being in an elite school estimated as a function of the test scores at age 11 according to equation (xx).

Appendix Table 1a: Sample selection and definition of subsamples

	Number of observations	
Main sample	12150	Original sample
Moved 1962-64 (358)	11792	May have left Aberdeen
Not Private school (255)		Some of these didn't take the 11+
Not RC school (229)		Most of them took the 11+, although they stay in a RC school
Not RC Private school (81)		Same arguments as RC and Private
Not Elite school already (504)		These pupils stay in the school with probability 1 whatever the results of the test at age 11
Not Special school (167)		Some of these didn't take the 11+
Not Outside school (12)	10544	Might go to secondary school outside Aberdeen
With grade info	10497	Because we will want to split by grade
With 11+ info	10219	Because we will need 11+ scores
With test at 7, test at 9 info	9921	Because it will be important to regression-adjust for these variables
Replied to 2001 survey	5970	Because we will be looking at outcomes of respondents
Went to state secondary	5646	Because we will want to focus on elite vs. non-elite
Grade 4-7 in 1962	4528	Because assignment procedure changed

Notes: Details of sample selection procedure.

Appendix Table 1b: Descriptive statistics

	Sample with test at 7 and test at 9 information		Grade 4-7 in 1962		Grade 4-7 in 1962			
					Non-elite		Elite	
	mean	N	mean	N	mean	N	mean	N
Male	0.518	9921	0.464	4528	0.480	3489	0.412	1039
Age (Dec 1962)	9.727	9921	10.203	4528	10.216	3489	10.162	1039
	(1.436)		(1.158)		(1.160)		(1.151)	
Father's social class		9921		4528		3489		1039
Other (unemployed, disabled, etc.)	0.051		0.048		0.052		0.035	
Unskilled manual	0.170		0.161		0.184		0.085	
Semi-skilled manual	0.149		0.146		0.164		0.085	
Skilled manual, other	0.208		0.211		0.226		0.163	
Skilled manual, requiring apprent.	0.251		0.260		0.243		0.316	
Other non manual	0.109		0.115		0.093		0.189	
Intermediate/Technical	0.051		0.049		0.033		0.103	
Professional	0.011		0.010		0.005		0.026	
Grade in 1962		9921		4528		3489		1039
Grade 3	0.208		0.000		0.000		0.000	
Grade 4	0.205		0.260		0.261		0.253	
Grade 5	0.204		0.251		0.252		0.246	
Grade 6	0.195		0.247		0.242		0.266	
Grade 7	0.196		0.242		0.245		0.235	
Test7	106.735	9921	108.023	4528	104.073	3489	121.284	1039
	(15.172)		(14.552)		(13.068)		(11.011)	
Test9	110.612	9921	112.258	4528	107.305	3489	128.888	1039
	(16.214)		(15.358)		(12.868)		(10.632)	
Test11	496.046	7929	503.303	4528	479.078	3489	584.650	1039
	(66.433)		(62.825)		(48.085)		(29.123)	
Replied to 2001 survey	0.602	9921	1	4528	1	3489	1	1039
Went to private secondary school	0.040	5878	0	4528	0	3489	0	1039
Elite School	0.225	5646	0.230	4528	0	3489	1	1039

Notes: Mean and standard deviation (only for continuous variables) shown for selected variables and different samples. See Appendix Table 1a for definition of sample.

Appendix Table 2: Impact of attending an elite school on main outcomes, falsification tests

	MEN			WOMEN		
	Linear Probability Model		GMM estimates	Linear Probability Model		GMM estimates
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Years of postcompulsory education						
Elite school	2.038	0.282	-0.078	2.132	0.221	0.127
	(0.097)	(0.119)	(0.140)	(0.093)	(0.098)	(0.142)
F(Test11)		Y	Y		Y	Y
Dep. variable	1.652	1.652	1.652	1.781	1.781	1.781
Observations	2,070	2,070	2,070	2,396	2,396	2,396
Overid. test			0.444			0.083
Panel B: A level						
Elite school	0.333	0.038	-0.023	0.421	0.046	0.027
	(0.015)	(0.019)	(0.022)	(0.017)	(0.019)	(0.029)
F(Test11)		Y	Y		Y	Y
Dep. variable	0.512	0.512	0.512	0.371	0.371	0.371
Observations	2,070	2,070	2,070	2,396	2,396	2,396
Overid. test			0.480			0.183
Panel C: Degree or equivalent						
Elite school	0.260	0.034	-0.006	0.248	0.016	0.004
	(0.014)	(0.016)	(0.017)	(0.012)	(0.012)	(0.016)
F(Test11)		Y	Y		Y	Y
Dep. variable	0.159	0.159	0.159	0.132	0.132	0.132
Observations	2,070	2,070	2,070	2,396	2,396	2,396
Overid. test			0.706			0.193
Panel D: Log annual income (all)						
Elite school	0.333	0.038	-0.006	0.386	0.006	-0.002
	(0.016)	(0.020)	(0.022)	(0.019)	(0.018)	(0.032)
F(Test11)					Y	Y
Dep. variable	9.996	9.996	9.996	9.168	9.168	9.168
Observations	2,070	2,070	2,070	2,396	2,396	2,396
Overid. test			0.294			0.139
Panel E: Any child						
Elite school	-0.034	-0.012	-0.016	-0.041	-0.015	-0.010
	(0.004)	(0.007)	(0.011)	(0.003)	(0.006)	(0.009)
F(Test11)		Y	Y		Y	Y
Dep. variable	0.846	0.846	0.846	0.876	0.876	0.876
Observations	2,070	2,070	2,070	2,396	2,396	2,396
Overid. test			0.811			0.456
Panel F: Number of children						
Elite school	-0.097	-0.048	-0.025	-0.100	-0.058	-0.052
	(0.013)	(0.020)	(0.029)	(0.010)	(0.019)	(0.030)
F(Test11)		Y	Y		Y	Y
Dep. variable	1.864	1.864	1.864	1.893	1.893	1.893
Observations	2,070	2,070	2,070	2,396	2,396	2,396
Overid. test			0.571			0.799

Notes: Sample includes all men and women in grades 4-7 in 1962 who reply to the 2001 survey and went to a non-private secondary school. The outcome variable is the predicted value of the dependent variable obtained from a regression of the variable shown on: dummies for father's occupation, mother's socio-economic status, school and grade attended in 1962, continuous variables capturing relative age within the school-grade, and fourth-degree polynomials in test scores at age 7 and 9. Columns (2)-(3) and (5)-(6) control for a 4th order polynomial in test scores at age 11. GMM estimates are obtained instrumenting the elite school dummy using dummies for the marginal area [540-559] and the area to the right of the second cutoff point [560-649] and their interactions with the test score. Standard errors clustered according to values of the test score at age 11.

Appendix Table 3: Impact of attending an elite school on years of post-compulsory education and highest educational qualification attained

	LOW SES					HIGH SES				
	Linear Probability Model			GMM estimates		Linear Probability Model			GMM estimates	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Panel A: Years of postcompulsory education										
Elite school	2.825	0.929	0.891	0.651	0.703	2.983	1.211	1.081	0.859	0.711
	(0.156)	(0.247)	(0.233)	(0.281)	(0.292)	(0.129)	(0.234)	(0.238)	(0.325)	(0.320)
F(Test11)		Y	Y	Y	Y		Y	Y	Y	Y
Control variables			Y		Y			Y		Y
Dep. variable	1.265	1.265	1.265	1.265	1.265	2.313	2.313	2.313	2.313	2.313
Observations	2,521	2,521	2,521	2,521	2,521	1945	1945	1945	1945	1945
Overid. test				0.343	0.386				0.541	0.645
Panel B: No qualifications										
Elite school	-0.305	-0.033	0.001	-0.021	0.018	-0.207	-0.005	0.009	0.062	0.069
	(0.021)	(0.034)	(0.034)	(0.047)	(0.047)	(0.017)	(0.026)	(0.026)	(0.046)	(0.045)
F(Test11)		Y	Y	Y	Y		Y	Y	Y	Y
Control variables			Y		Y			Y		Y
Dep. variable	0.297	0.297	0.297	0.297	0.297	0.165	0.165	0.165	0.165	0.165
Observations	2,521	2,521	2,521	2,521	2,521	1,945	1,945	1,945	1,945	1,945
Overid. test				0.866	0.693				0.250	0.264
Panel C: Ungraded CSE, school leaving cert., other										
Elite school	0.007	-0.041	-0.038	-0.010	-0.008	0.015	-0.023	-0.041	0.005	-0.050
	(0.026)	(0.042)	(0.043)	(0.065)	(0.064)	(0.021)	(0.043)	(0.044)	(0.070)	(0.071)
F(Test11)		Y	Y	Y	Y		Y	Y	Y	Y
Control variables			Y		Y			Y		Y
Dep. variable	0.237	0.237	0.237	0.237	0.237	0.261	0.261	0.261	0.261	0.261
Observations	2,521	2,521	2,521	2,521	2,521	1,945	1,945	1,945	1,945	1,945
Overid. test				0.789	0.829				0.011	0.009
Panel D: O level										
Elite school	0.360	0.090	0.065	0.020	0.002	0.265	0.014	0.004	-0.008	0.002
	(0.026)	(0.052)	(0.054)	(0.066)	(0.065)	(0.024)	(0.038)	(0.038)	(0.067)	(0.066)
F(Test11)		Y	Y	Y	Y		Y	Y	Y	Y
Control variables			Y		Y			Y		Y
Dep. variable	0.550	0.550	0.550	0.550	0.550	0.685	0.685	0.685	0.685	0.685
Observations	2,521	2,521	2,521	2,521	2,521	1,945	1,945	1,945	1,945	1,945
Overid. test				0.282	0.410				0.155	0.122
Panel E: A levels										
Elite school	0.538	0.192	0.165	0.109	0.090	0.534	0.266	0.251	0.126	0.137
	(0.029)	(0.049)	(0.048)	(0.061)	(0.064)	(0.024)	(0.045)	(0.045)	(0.067)	(0.069)
F(Test11)		Y	Y	Y	Y		Y	Y	Y	Y
Control variables			Y		Y			Y		Y
Dep. variable	0.238	0.238	0.238	0.238	0.238	0.432	0.432	0.432	0.432	0.432
Observations	2,521	2,521	2,521	2,521	2,521	1,945	1,945	1,945	1,945	1,945
Overid. test				0.241	0.186				0.088	0.138
Panel F: HNC, teaching, etc.										
Elite school	0.167	-0.006	-0.028	-0.006	-0.029	0.172	0.075	0.053	0.027	0.000
	(0.028)	(0.048)	(0.048)	(0.061)	(0.059)	(0.023)	(0.046)	(0.048)	(0.066)	(0.065)
F(Test11)		Y	Y	Y	Y		Y	Y	Y	Y
Control variables			Y		Y			Y		Y
Dep. variable	0.200	0.200	0.200	0.200	0.200	0.276	0.276	0.276	0.276	0.276
Observations	2,521	2,521	2,521	2,521	2,521	1,945	1,945	1,945	1,945	1,945
Overid. test				0.147	0.123				0.837	0.719
Panel G: Degree										
Elite school	0.331	0.102	0.0922	0.0778	0.0803	0.372	0.138	0.118	0.0853	0.0725
	(0.026)	(0.039)	(0.038)	(0.044)	(0.045)	(0.024)	(0.036)	(0.035)	(0.056)	(0.055)
F(Test11)		Y	Y	Y	Y		Y	Y	Y	Y
Control variables			Y		Y			Y		Y
Dep. variable	0.0972	0.0972	0.0972	0.0972	0.0972	0.204	0.204	0.204	0.204	0.204
Observations	2,521	2,521	2,521	2,521	2,521	1,945	1,945	1,945	1,945	1,945
Overid. test				0.277	0.348				0.625	0.782

Notes: Sample includes all men and women in grades 4-7 in 1962 who reply to the 2001 survey and went to a non-private secondary school. Columns (2)-(5) and (7)-(10) control for a 4th order polynomial in test scores at age 11; columns (3), (5), (8) and (10) additionally control for socio-demographic characteristics of the individual and past test scores (4th order polynomials). GMM estimates are obtained instrumenting the elite school dummy using dummies for the marginal area [540-559] and the area to the right of the second cutoff point [560-649] and their interactions with the test score. Standard errors clustered according to values of the test score at age 11.

Appendix Table 4: Impact of attending an elite school on occupation

	MEN					WOMEN				
	Linear Probability Model			GMM estimates		Linear Probability Model			GMM estimates	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Panel A: Managers										
Elite school	0.118	-0.038	-0.037	-0.220	-0.201	0.056	0.029	0.027	-0.057	-0.054
	(0.027)	(0.058)	(0.058)	(0.078)	(0.077)	(0.018)	(0.041)	(0.041)	(0.048)	(0.050)
F(Test11)		Y	Y	Y	Y		Y	Y	Y	Y
Control variables			Y		Y			Y		Y
Dep. variable	0.252	0.252	0.252	0.252	0.252	0.096	0.096	0.096	0.096	0.096
Observations	1,826	1,826	1,826	1,826	1,826	1,926	1,926	1,926	1,926	1,926
Overid. test				0.226	0.337				0.522	0.436
Panel B: Professionals and associate professionals										
Elite school	0.281	0.140	0.130	0.077	0.078	0.306	0.052	0.036	0.140	0.128
	(0.031)	(0.061)	(0.060)	(0.080)	(0.084)	(0.025)	(0.042)	(0.043)	(0.063)	(0.063)
F(Test11)		Y	Y	Y	Y		Y	Y	Y	Y
Control variables			Y		Y			Y		Y
Dep. variable	0.211	0.211	0.211	0.211	0.211	0.227	0.227	0.227	0.227	0.227
Observations	1,826	1,826	1,826	1,826	1,826	1,926	1,926	1,926	1,926	1,926
Overid. test				0.869	0.838				0.922	0.859
Panel C: Clerical and secretarial										
Elite school	-0.026	-0.033	-0.029	-0.010	-0.007	-0.056	-0.046	-0.038	-0.053	-0.023
	(0.012)	(0.028)	(0.028)	(0.037)	(0.036)	(0.026)	(0.051)	(0.053)	(0.079)	(0.082)
F(Test11)		Y	Y	Y	Y		Y	Y	Y	Y
Control variables			Y		Y			Y		Y
Dep. variable	0.066	0.066	0.066	0.066	0.066	0.322	0.322	0.322	0.322	0.322
Observations	1,826	1,826	1,826	1,826	1,826	1,926	1,926	1,926	1,926	1,926
Overid. test				0.271	0.191				0.643	0.532
Panel D: Craft and related										
Elite school	-0.190	-0.064	-0.066	0.044	0.031	-0.010	0.004	0.001	0.005	0.003
	(0.016)	(0.029)	(0.030)	(0.043)	(0.044)	(0.005)	(0.010)	(0.010)	(0.009)	(0.010)
F(Test11)		Y	Y	Y	Y		Y	Y	Y	Y
Control variables			Y		Y			Y		Y
Dep. variable	0.193	0.193	0.193	0.193	0.193	0.014	0.014	0.014	0.014	0.014
Observations	1,826	1,826	1,826	1,826	1,826	1,926	1,926	1,926	1,926	1,926
Overid. test				0.0194	0.0413				0.566	0.438
Panel E: Personal and sales										
Elite school	-0.025	0.024	0.026	0.073	0.065	-0.177	0.005	0.011	0.062	0.057
	(0.017)	(0.036)	(0.036)	(0.050)	(0.049)	(0.018)	(0.041)	(0.042)	(0.053)	(0.056)
F(Test11)		Y	Y	Y	Y		Y	Y	Y	Y
Control variables			Y		Y			Y		Y
Dep. variable	0.089	0.089	0.089	0.089	0.089	0.240	0.240	0.240	0.240	0.240
Observations	1,826	1,826	1,826	1,826	1,826	1,926	1,926	1,926	1,926	1,926
Overid. test				0.922	0.843				0.886	0.693
Panel F: Other										
Elite school	-0.158	-0.029	-0.024	0.042	0.048	-0.117	-0.044	-0.037	-0.035	-0.049
	(0.018)	(0.033)	(0.033)	(0.057)	(0.057)	(0.012)	(0.024)	(0.025)	(0.035)	(0.040)
F(Test11)		Y	Y	Y	Y		Y	Y	Y	Y
Control variables			Y		Y			Y		Y
Dep. variable	0.188	0.188	0.188	0.188	0.188	0.101	0.101	0.101	0.101	0.101
Observations	1,826	1,826	1,826	1,826	1,826	1,926	1,926	1,926	1,926	1,926
Overid. test				0.523	0.540				0.348	0.359

Notes: Sample includes all men and women in grades 4-7 in 1962 who reply to the 2001 survey, went to a non-private secondary school and are employed in 2001. Columns (2)-(5) and (7)-(10) control for a 4th order polynomial in test scores at age 11; columns (3), (5), (8) and (10) additionally control for socio-demographic characteristics of the individual and past test scores (4th order polynomials). GMM estimates are obtained instrumenting the elite school dummy using dummies for the marginal area [540-559] and the area to the right of the second cutoff point [560-649] and their interactions with the test score. Standard errors clustered according to values of the test score at age 11.

Appendix Table 5: Percentage holding a degree or an A-level qualification, and mean income by occupation

	% degree	% A-levels	Mean annual income	Mean log annual income
		MEN		
Managers	0.230	0.510	38,445	10.498
Prof. and associate prof.	0.456	0.671	33,311	10.341
Clerical and secretarial	0.033	0.248	20,691	9.835
Craft and related	0.014	0.256	23,724	9.991
Personal and sales	0.025	0.278	22,003	9.904
Other	0.029	0.142	20,519	9.809
Observations	1,826	1,826	1,826	1,826
		WOMEN		
Managers	0.227	0.427	24,743	9.954
Prof. and associate prof.	0.434	0.632	21,576	9.842
Clerical and secretarial	0.042	0.234	13,681	9.396
Craft and related	0.038	0.077	9,846	9.118
Personal and sales	0.028	0.130	9,210	8.933
Other	0.005	0.036	7,112	8.728
Observations	1,926	1,926	1,926	1,926

Notes: Sample includes all men and women in grades 4-7 in 1962 who reply to the 2001 survey, went to a non-private secondary school, are employed in 2001 and have non-missing occupational code. Percentage of individuals holding a degree-level qualification or an A-level qualification or equivalent by occupation shown. Also shown, mean values of annual income and log annual income in each occupational category.

Appendix Table 6: Impact of attending an elite school on main outcomes, sensitivity checks

	MEN			WOMEN		
	GMM estimates			GMM estimates		
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Years of postcompulsory education						
Elite school	1.133	1.204	0.981	0.825	0.829	0.760
	(0.264)	(0.270)	(0.304)	(0.221)	(0.228)	(0.259)
F(Test11) polynomial	2nd degree	3rd degree	4th degree	2nd degree	3rd degree	4th degree
Dep. variable	1.652	1.652	1.652	1.781	1.781	1.781
Observations	2,070	2,070	2,070	2,396	2,396	2,396
Overid. test	0.277	0.581	0.587	0.107	0.102	0.103
Panel B: A level						
Elite school	0.158	0.128	0.093	0.214	0.234	0.161
	(0.054)	(0.055)	(0.070)	(0.048)	(0.049)	(0.060)
F(Test11) polynomial	2nd degree	3rd degree	4th degree	2nd degree	3rd degree	4th degree
Dep. variable	0.512	0.512	0.512	0.371	0.371	0.371
Observations	2,070	2,070	2,070	2,396	2,396	2,396
Overid. test	0.0269	0.642	0.522	0.0108	0.0192	0.0235
Panel C: Degree or equivalent						
Elite school	0.157	0.149	0.162	0.076	0.041	0.046
	(0.046)	(0.047)	(0.054)	(0.039)	(0.040)	(0.047)
F(Test11) polynomial	2nd degree	3rd degree	4th degree	2nd degree	3rd degree	4th degree
Dep. variable	0.159	0.159	0.159	0.132	0.132	0.132
Observations	2,070	2,070	2,070	2,396	2,396	2,396
Overid. test	0.312	0.399	0.425	0.00517	0.336	0.312
Panel D: Log annual income (all)						
Elite school	-0.023	-0.062	-0.137	0.189	0.194	0.325
	(0.063)	(0.065)	(0.081)	(0.100)	(0.098)	(0.126)
F(Test11) polynomial	2nd degree	3rd degree	4th degree	2nd degree	3rd degree	4th degree
Dep. variable	10.008	10.008	10.008	9.172	9.172	9.172
Observations	2,029	2,029	2,029	2,335	2,335	2,335
Overid. test	0.226	0.328	0.564	0.590	0.791	0.728
Panel E: Any child						
Elite school	0.028	0.025	-0.017	-0.092	-0.091	-0.148
	(0.053)	(0.053)	(0.065)	(0.041)	(0.041)	(0.049)
F(Test11) polynomial	2nd degree	3rd degree	4th degree	2nd degree	3rd degree	4th degree
Dep. variable	0.846	0.846	0.846	0.876	0.876	0.876
Observations	2,068	2,068	2,068	2,394	2,394	2,394
Overid. test	0.498	0.549	0.703	0.221	0.404	0.717
Panel F: Number of children						
Elite school	0.346	0.300	0.148	-0.450	-0.473	-0.623
	(0.152)	(0.148)	(0.181)	(0.109)	(0.108)	(0.132)
F(Test11) polynomial	2nd degree	3rd degree	4th degree	2nd degree	3rd degree	4th degree
Dep. variable	1.865	1.865	1.865	1.892	1.892	1.892
Observations	2,068	1,750	1,750	2,394	2,394	2,394
Overid. test	0.838	0.577	0.851	0.270	0.360	0.747

Notes: Sample includes all men and women in grades 4-7 in 1962 who reply to the 2001 survey and went to a non-private secondary school. GMM estimates are obtained instrumenting the elite school dummy using dummies for the marginal area [540-559] and the area to the right of the second cutoff point [560-649] and their interactions with the test score. Columns (1) and (4) control for a polynomial of 2nd order in test score at age 11, columns (2) and (5) include a polynomial of 3rd order, while columns (3) and (6) include a polynomial of 4th order, as shown in our main specifications. Standard errors clustered according to values of the test score at age 11.