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No. 3993

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LABOUR ECONOMICS



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ABSTRACT

Does Education Raise Productivity or Just Reflect It?*

It is clear that education has an important effect on wages paid in the labour market. It is not clear, however, whether this is due to the role that education plays in raising the productivity of workers (the human capital explanation) or whether education simply reflects the ability of the worker (through a signalling role). In this Paper we describe and implement, using a variety of UK datasets, a number of tests from the existing literature for discriminating between the two explanations. We find little support for signalling ideas in these tests. We have, however, severe reservations about these results because of our doubts about the power of these tests and the appropriateness of the data. We propose an alternative test, based on the response of some individuals to a change in education incentives offered to other individuals caused by the changes in the minimum school leaving age in the 1970s. Using this idea, we find that data in the UK appears to strongly support the human capital explanation.

JEL Classification: I20 and J30

Keywords: human capital, screening and signalling

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1. Introduction

An important issue in the economics of education is that individual education may have an effect on wages paid in the labour market - not because of any effect on productivity, but because education may simply act as a signal of productivity (or some characteristics that employers value because they contribute to productivity which they cannot easily observe). Employers, believing that education is correlated with productivity, will screen their employees and pay higher wages to more educated workers. The employers' beliefs will be confirmed by their experience if it is the case that high productivity individuals signal this by choosing high levels of education. It will be optimal for individuals to behave in this way if the cost of acquiring education is less for high productivity individuals than it is for low productivity individuals. Thus, under reasonable conditions, the market will be characterised by a separating equilibrium where high productivity individuals choose high levels of education and earn high wages. The theory is largely due to Spence (1973, 1979) and the subsequent empirical literature has recently been reviewed by Groot and Oosterbeek (1994).

In contrast, the earlier human capital explanation, due to Becker (1962) and Schultz (1962) suggests that the correlation between education and wages is due to the education enhancing productivity. A recent UK example is Blundell *et al* (2002) which uses detailed education and later earnings information on a cohort of individuals born in 1958 to show that the returns to a degree (typically of 3-year duration) relative to graduating from high school at 18 (with 2 "A level" qualifications) is a 26% wage premium.

The fundamental difficulty in unravelling the extent to which education is a signal of existing productivity as opposed to enhancing productivity is that both human capital and signalling theories imply that there is a positive correlation between earnings and education. Indeed, Lazear (1977) in an early review stated that this "... makes it virtually impossible to come up with a valid test of the screening hypothesis" . Despite this pessimistic view, there have been many attempts to distinguish between the theories. Almost all of these attempts have been based on the presumption that signalling/screening is more prevalent for some types of individuals (say, workers in sectors where productivity is hard to measure) than others.

In this paper we implement several of the suggested methods for discriminating between the theories with UK data. We find the results of this unanimously in favour of the human capital explanation – but , in any event, we argue that these tests are weak since the

differences that they rely on could also be rationalised by either signalling of human capital theories. However, there is one test, originally suggested and implemented on cross state US data by Lang and Kroop (1986), that exploits differences in changes in education levels in response to a change in the minimum level of education. Since the UK has had an increase in the minimum school leaving age we explore how this has impacted on the school leaving age distribution. We find no evidence of signalling from this exercise. Thus, we feel that the large estimated effects of education on wages are rates of return on human capital investment.

2. Evidence

The UK Dearing Report (1997) made much of the difference between the correlation between wages and education and the productivity effect of education on wages and termed the difference between the two α . In the absence of information about the size of α the report included calculations for several values – with 20% and 40% being typically used.

In fact, there is very little evidence in the UK that pertains to α and here we provide some new evidence using methods that have typically been applied to discriminate between human capital and signalling models. We base our estimates on several datasets. We do not consider wider endogeneity issues that have been the concern of Blundell *et al* (2002) and Harmon and Walker (1995) for the UK, and of the review in Card (2000). Thus we do not control for any of the many selection effects that may be present in the data. So while each of our estimates are open to criticism we would argue that, since they all point in the same direction, they together provide useful evidence.

We begin with conventional estimates for prime age (25-59) individuals in England and Wales using the large Labour Force Survey data pooled from 1993 to 2001¹. We compute an hourly wage rate from the ratio of usual earnings to usual hours (from the respondent's main job). Figure 1 shows the coefficient on years of education² in each year of the data, for men and women separately, controlling for a quadratic in age, region, year, decade of birth, having a work-limiting health problem, non-white, union and marital status. The samples are

¹ We exclude Scotland to reduce as far as possible the distortions caused by the difference in the education system in that region. We also exclude those with zero or missing hours of work or earnings. See Walker and Zhu (2002) for more details.

² Measured as year of continuous full-time education. We deal with “gap” years by including controls for whether the years of education “matched” the qualifications recorded in the data. Including gap year controls made little difference to any of the results.

large (averaging more than 10,000 each year) and the estimates are very precise (t -values throughout exceed 40). The difference between men and women is highly significant and there are sizable year-to-year differences for both men and women but there is no significant time trend for either men or women.

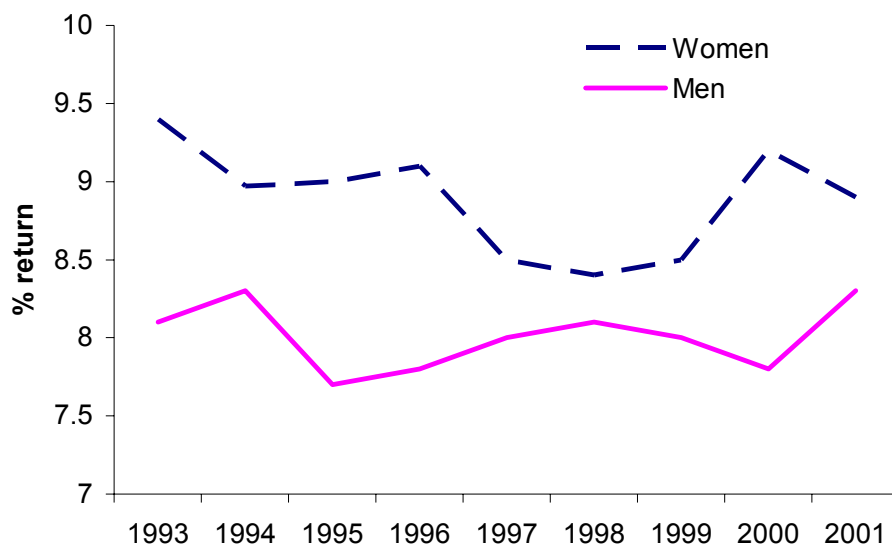


Figure 1 *Percentage Effect of Additional Year of Education on Wages - MEN and WOMEN LFS 1993-2001*

The specification behind Figure 1 is extremely simple and assumes linearity in years of education. One way of introducing greater flexibility is to control for qualifications rather than years of education. Figures 2a and 2b shows the coefficients on selected qualification levels over time. There are no significant differences over time and no significant gender differences in the effects of O-Levels (5 GCSE grade A-C, CSE grade 1, and GCE grade 1-6) relative to no qualifications, or (first) degree relative to no qualifications. The returns to 2+ A-Levels relative to no-qualifications and relative to O-Levels are significantly higher for men than women³.

³ The effect of 1 A-level, not reported here, is somewhat higher for women than men. Other qualifications not reported are Masters degrees, Doctorates and other higher educational qualifications which are largely post-degree teaching qualifications.

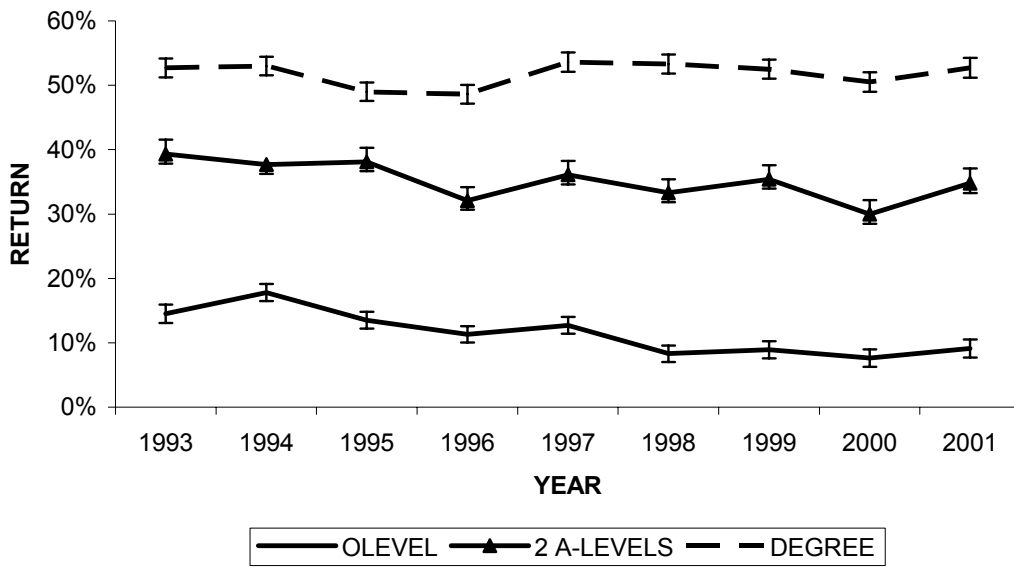


Figure 2a *Percentage Effect of Educational Qualifications on Wages - MEN LFS 1993-2001*

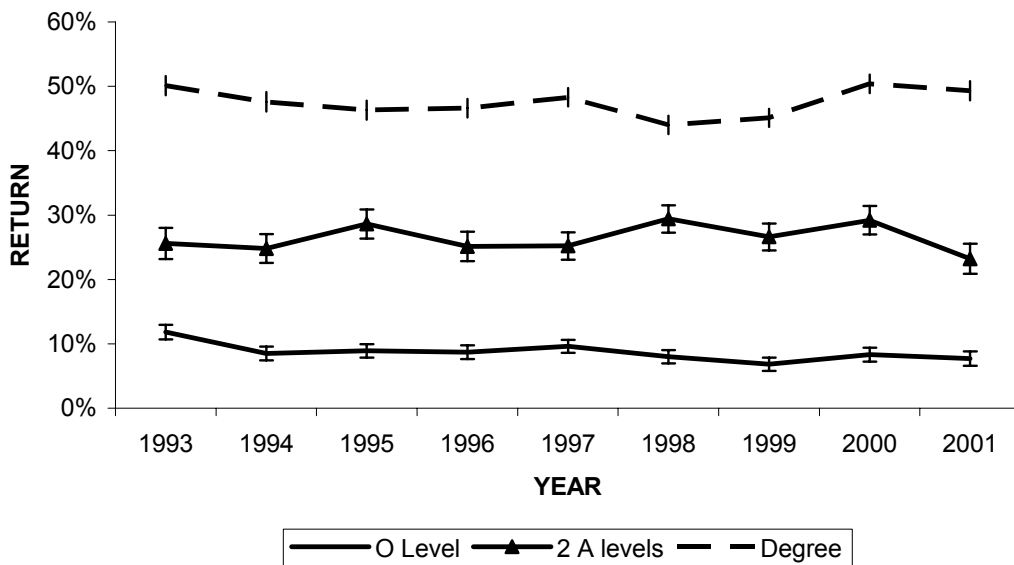


Figure 2b *Percentage Effect of Educational Qualifications on Wages - WOMEN LFS 1993-2001*

The participation rate in higher education increased dramatically in the mid to late 1960's (due to the so-called "Robbins" expansion) and again in the 1990's (when institutions were given strong financial incentives to admit more students). While there are no significant time trends in the effects of education years or qualifications on wages it may still be possible that more recent cohorts experience lower rates of return because of the rapid expansion of post-compulsory participation and participation in higher education⁴ if it is the case that young workers are not a good substitute for older ones.

To explore this issue, that returns may have fallen across cohorts, we present estimates of OLS and quantile regressions of the coefficient on years of education in Table 1 for a number of specific birth cohorts. The idea behind looking at quantile regressions is that it gives us a feel for how returns vary across the ability distribution. On average, less able individuals will be concentrated in the bottom of the distribution and the more able towards the top. If it is the case that the expansion of education has been at the expense of a reduction in ability we might expect there to be a fall in returns at the bottom of the distribution as an increasing number of less-able individuals move into higher levels of education. There does seem to have been a fall in the coefficient on years of education for the most recent cohort although the fall has not been disproportionately large for the bottom quantile.

Table 2 presents OLS and quantile regression estimates of the coefficient on degree for those with at least two A-levels. The returns to a degree for women does seem to have fallen at the bottom quantile. However, the returns to a degree for men, although lower, seem to have risen for the latest cohort across all quantiles. There seems to be no support here for the idea that the latest cohort is not benefiting from a degree any less than earlier cohorts⁵. These results also suggest little evidence for the idea that returns to children from lower social classes, who we might also expect to be more heavily represented at the bottom of the distribution, have higher returns (perhaps because of credit rationing).

⁴ Card and Lemieux (2001) explore this issue using GHS data for the UK and comparable US and Canadian data.

⁵ Appendix Table A1 presents estimates based on LFS data of the return to education for the birth cohorts in a specification that includes dummy variables for each schooling level. This specification seems to support the finding of a reduction in the returns to leaving at 17 and 18 for men and women but the return to having a degree for recent cohorts has not changed. The return to age gets very large but this could be because of the relative scarcity of well educated older individuals people in the most recent cohort.

Table 1
Return to Year of Education – Quantile Regressions (LFS Men & Women 1993-2001 by birth cohort)

| | Born 1933-46 | Born 1947-57 | Born 1958-68 | Born 1969-77 |
|-----------------------------------|----------------------|----------------------|----------------------|----------------------|
| WOMEN: | | | | |
| <i>OLS</i> | 8.85 <i>50.72</i> | 8.73 <i>76.64</i> | 8.91 <i>75.27</i> | 5.65 <i>33.19</i> |
| QUANTILE REGRESSIONS | | | | |
| 25th Percentile | 8.59 <i>47.42</i> | 8.32 <i>64.89</i> | 8.46 <i>66.42</i> | 5.63 <i>32.23</i> |
| 50th Percentile | 9.69 <i>47.64</i> | 9.24 <i>73.25</i> | 9.28 <i>68.45</i> | 5.41 <i>28.82</i> |
| 75th Percentile | 9.82 <i>40.28</i> | 9.70 <i>52.73</i> | 9.36 <i>57.39</i> | 5.80 <i>24.73</i> |
| No. of Observations | 16264 | 29808 | 31915 | 9690 |
| MEN: | | | | |
| <i>OLS</i> | 8.65 <i>55.39</i> | 7.97 <i>77.02</i> | 7.34 <i>72.81</i> | 4.34 <i>26.80</i> |
| QUANTILE REGRESSIONS | | | | |
| 25th Percentile | 8.10 <i>39.62</i> | 7.73 <i>60.44</i> | 6.95 <i>54.48</i> | 3.90 <i>19.49</i> |
| 50th Percentile | 9.27 <i>52.58</i> | 8.09 <i>65.51</i> | 7.48 <i>62.83</i> | 4.49 <i>25.12</i> |
| 75th Percentile | 9.79 <i>48.60</i> | 8.43 <i>58.35</i> | 7.81 <i>58.14</i> | 4.72 <i>22.64</i> |
| No. of Observations | 16097 | 28288 | 33456 | 9938 |

Notes: Figures are coefficients on years of education variable in samples of all workers. *t* values in italic.

In addition to estimating the mean returns we can also investigate how this varies across individuals according to observable and unobservable characteristics. Table 3 reports estimates of a model that allows for the returns to education to differ across individuals both according to their observable characteristics (such as union status) and for unobservable reasons (see Harmon, Hogan and Walker (2003) for a more detailed discussion of the econometric model). The results suggests some variance in the returns across individuals for unobservable reasons – but this variance, due perhaps to unobserved ability differences, does not appear to be any larger, and is arguably smaller, for more recent cohorts.

Table 2
OLS and Quantile Regressions: Return to Degree vs. 2+ A Levels (LFS Men & Women 1993-2001 by birth cohort)

| | All | Born 1933-46 | Born 1947-57 | Born 1958-68 | Born 1969-77 |
|-----------------------------------|--------------|-----------------|-----------------|-----------------|-----------------|
| WOMEN: | | | | | |
| OLS | 21.51 | 22.38 | 22.98 | 18.95 | 23.77 |
| | <i>21.97</i> | <i>7.00</i> | <i>12.66</i> | <i>12.82</i> | <i>10.09</i> |
| QUANTILE REGRESSIONS | | | | | |
| 25th Percentile | 25.05 | 19.82 | 26.17 | 23.52 | 22.44 |
| | <i>19.29</i> | <i>4.86</i> | <i>12.95</i> | <i>13.31</i> | <i>7.17</i> |
| 50th Percentile | 22.41 | 27.26 | 27.27 | 19.76 | 22.90 |
| | <i>20.82</i> | <i>9.07</i> | <i>16.66</i> | <i>13.41</i> | <i>8.53</i> |
| 75th Percentile | 17.92 | 25.80 | 17.75 | 15.00 | 24.35 |
| | <i>13.61</i> | <i>5.78</i> | <i>6.98</i> | <i>7.97</i> | <i>7.85</i> |
| No. of Observations | 16454 | 1590 | 5011 | 6928 | 2925 |
| MEN: | | | | | |
| OLS | 10.61 | 10.87 | 11.63 | 9.61 | 15.58 |
| | <i>12.04</i> | <i>3.86</i> | <i>7.31</i> | <i>7.34</i> | <i>6.37</i> |
| QUANTILE REGRESSIONS | | | | | |
| 25th Percentile | 14.07 | 14.95 | 17.60 | 13.46 | 16.47 |
| | <i>3.64</i> | <i>3.97</i> | <i>9.09</i> | <i>9.33</i> | <i>5.45</i> |
| 50th Percentile | 11.33 | 9.21 | 12.92 | 10.21 | 20.34 |
| | <i>12.58</i> | <i>3.15</i> | <i>7.63</i> | <i>7.20</i> | <i>6.03</i> |
| 75th Percentile | 7.23 | 4.50 | 4.89 | 7.23 | 17.92 |
| | <i>6.28</i> | <i>1.39</i> | <i>2.32</i> | <i>4.48</i> | <i>5.30</i> |
| No. of Observations | 21901 | 3020 | 7333 | 8621 | 2927 |

Notes: Figures are coefficients on degree dummy variable in samples with 2+ A levels. *t* values in italic.

One characteristic that we would like to allow for is the type of institution attended. Unfortunately the LFS does not report type of degree-granting institutions. However, recent research by McKnight, Naylor and Smith (2000) uses the 6-month follow up of the First Destination Surveys of the Higher Education Statistics Agency (for 52 “old” universities) and finds some variance in returns, despite controlling in fine detail for subject studied, parental background, schooling experience, and exact A-level grades. However, this variance is not

very large – 90% of institutions lie within 5% of the mean effect⁶. The issue of varying returns by institution type is the focus of Chevalier and Conlon (2002) using surveys of UK graduates from 1996 and 1998⁷. They estimate the returns to undergraduates for four types of higher education institution: so-called Russell Group (named after the organisational body representing the major research universities in the UK); ‘Old’ universities, which are the remaining universities established prior to 1991; polytechnics which, after 1991, were granted university status; and Other Institutions which include other degree awarding institutions in the higher education sector mostly representing teaching qualifications and colleges of art and music.

Table 3

Random Coefficient Estimates of Mean and Std Dev of Education Years Coefficient: Men and Women in LFS 1993-2001

| | All cohorts | Born 1933-46 | Born 1947-57 | Born 1958-68 | Born 1969-77 |
|-------------------|-------------|-----------------|-----------------|-----------------|-----------------|
| <i>Men</i> | 7.72 | 9.29 | 8.26 | 7.64 | 4.48 |
| Mean Return | (115) | (48.6) | (72.2) | (68.9) | (26.2) |
| <i>Men</i> | 2.84 | 4.06 | 2.83 | 2.91 | 2.36 |
| Std Dev of Return | (17.2) | (8.98) | (9.42) | (11.4) | (5.52) |
| <i>Women</i> | 8.53 | 9.26 | 8.89 | 9.06 | 5.72 |
| Mean Return | (115) | (46.2) | (72.0) | (71.2) | (32.6) |
| <i>Women</i> | 3.11 | 3.74 | 3.17 | 2.97 | 1.70 |
| Std Dev of Return | (18.4) | (7.98) | (10.5) | (10.3) | (2.94) |

Notes: *t* values in parentheses. Models also contain union status, marital status, and health status and interactions of these with education years. A quadratic in age and year and region controls are also included. Source: Walker and Zhu (2002)

Table 4 presents estimates taken from their study for the pooled sample from the 1985 and 1990 graduate cohorts with earnings measured in 1996 (top panel) and the 1995 cohort with earnings measured in 1998 (bottom panel). The two sets of estimates suggest substantial returns to graduates of the elite established universities over the polytechnic graduates. However for the 1985-1990 cohorts the premium drops sharply when measures relating to degree class and subject, student ability (measured by A-level scores) and parental background are included. Interestingly this does not occur for the 1996 cohort and there

⁶ Moreover, in McKnight *et al.* (2000) earnings are not directly observed in the survey and are imputed from occupation averages taken from the New Earning Survey which will likely dampen the variance of earnings across individuals.

⁷ The 1996 survey was conducted among graduates from the 1985 and 1990 cohort whilst the 1998 survey focused on the 1995 cohort. Both postal surveys are based on alumni data from a selected but representative group of UK tertiary institutions.

seems to be a substantial premium to attending one of the established universities, from either the elite group or the remaining institutions, over the polytechnic sector which by this point would now be ‘new’ universities. One possible explanation for this would be an increase in the variance of ability in entrants to these institutions since their change in status. However an important caveat to note is that the wage measure is taken very shortly after graduation. This might point to an institution effect on starting wages or on the job quality as an alternative explanation for the difference between the two cohorts.

Table 4
Financial Returns to Undergraduates by Institution Type

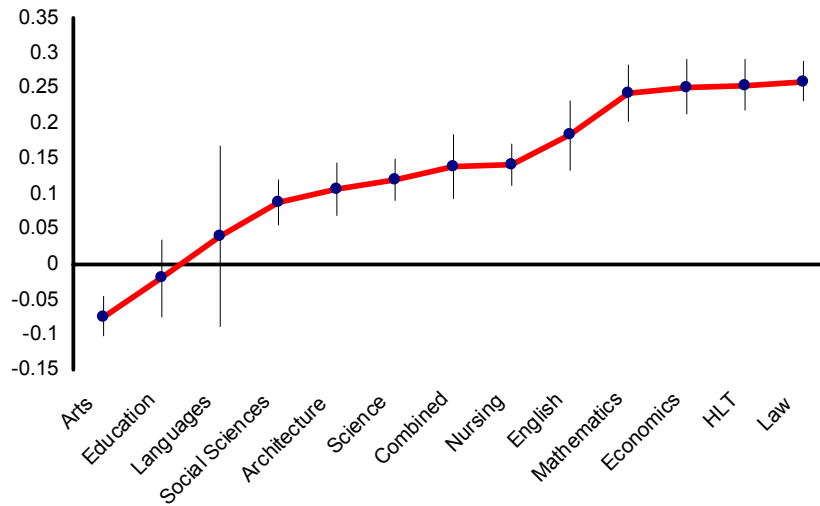
| | <i>1985-1990 (Cohorts pooled)</i> | | | |
|---------------------|-----------------------------------|-------------------|-------------------|-------------------|
| | Hourly wage in 1996 | | | |
| Russell Group | 0.091 (0.016) | 0.084 (0.016) | 0.049 (0.019) | 0.043 (0.018) |
| Old University | 0.037 (0.047) | 0.030 (0.034) | 0.008 (0.032) | 0.006 (0.031) |
| Other Institution | -0.061 (0.043) | -0.037 (0.023) | -0.041 (0.018) | -0.045 (0.016) |
| Prior A Level Score | | | ✓ | ✓ |
| Class of Degree | | ✓ | ✓ | ✓ |
| Subject of Degree | | ✓ | ✓ | ✓ |
| Parental Occupation | | | | ✓ |
| School Attended | | | | ✓ |
| No. of Observations | 5490 | 5490 | 5490 | 5490 |
| | <i>1995 Cohorts</i> | | | |
| | Annual wage in 1998 | | | |
| Russell Group | 0.129 (0.024) | 0.119 (0.025) | 0.109 (0.023) | 0.104 (0.021) |
| Old University | 0.066 (0.033) | 0.067 (0.037) | 0.056 (0.036) | 0.051 (0.034) |
| Other Institution | -0.056 (0.026) | -0.011 (0.025) | -0.013 (0.025) | -0.014 (0.024) |
| Prior A Level Score | | | ✓ | ✓ |
| Class of Degree | | ✓ | ✓ | ✓ |
| Subject of Degree | | ✓ | ✓ | ✓ |
| Parental Occupation | | | | ✓ |
| No. of Observations | 5847 | 5847 | 5847 | 5847 |

Note: robust standard errors corrected for clustering at the institution level. The hourly wage regression with controls for gender, cohort of graduation, observed labour market experience since graduation, employer’s size, permanent contract and region of residence. Omitted category - Polytechnic institution (top panel) and New University or former Polytechnic institution (bottom panel).

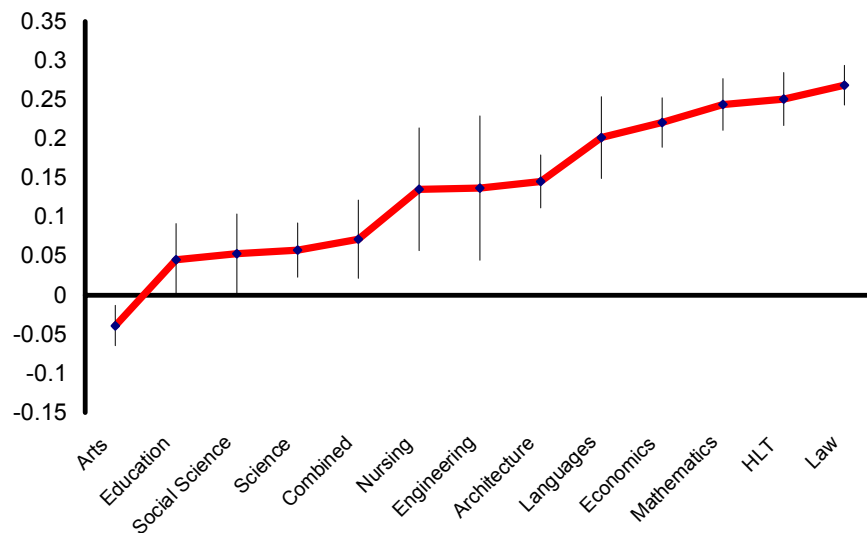
We can also see how the returns to a degree differs by subject studied since this is recorded in LFS data. Figures 4 shows the coefficients of degree subjects relative to 2 A-levels, from related work by Walker and Zhu (2002), for the subset of observations with at least 2 A-levels (higher degree coefficients are not reported) controlling for age, year and

region. There are large differences in coefficients (the figures also show the 95% confidence interval) with Law, Health, Economics and Business, and Mathematics considerably higher than Arts, Education, and other Social Sciences. Of course, these estimates fail to control for A-level score and this may explain some of the cross-subject differences – although traditionally Arts courses have demanded quite high scores to gain admission while maths and science courses have required lower standards on average. Moreover, the estimates clearly reflect important selection effects that would be difficult to resolve.

Men



Women



Source: Walker and Zhu (2002)

Figure 4 *Returns to Degree by Degree Subject: LFS 1993-2001*

Of course, degree subject is itself the object of choices that students make. It has been suggested that children from poorer backgrounds choose subjects that have higher returns. On the other hand children from poorer backgrounds on average perform less well at school and this may affect their ability to pursue some subjects at University that demand high grades for admission. Table 5 examines the subject choice of degree holders in the GHS data, and exploits information on family background and O-level performance, using multinomial logit estimation. The omitted category is the choice of social science degree courses and the reference group for family background is having a father in a managerial position. Individuals with more O-levels are significantly more likely to choose a subject other than social science (the excluded group) or engineering, while the familial background of individuals has limited effect once we control for number of O-levels..

Table 5
Determinant of subject choice for degree holder (GHS 82-92, odd years)

| | Arts | Engineering | Science | Other | Combined |
|---------------------|-------------------|-------------------|-------------------|------------------|-------------------|
| Professional father | 0.072 (0.204) | 0.427 (0.267) | 0.086 (0.251) | 0.660 (0.238) | 0.743 (0.477) |
| Intermediate father | 0.081 (0.255) | 0.375 (0.371) | 0.251 (0.136) | 0.201 (0.312) | 0.924 (0.501) |
| Skilled father | 0.089 (0.204) | 0.522 (0.265) | 0.805 (0.167) | 0.784 (0.261) | 0.906 (0.323) |
| Semi-skilled father | -0.036 (0.377) | 0.157 (0.378) | 0.648 (0.375) | 0.056 (0.467) | 0.750 (0.470) |
| Unskilled father | -0.672 (0.545) | -0.731 (1.085) | -0.094 (0.482) | 0.095 (0.646) | 0.755 (1.216) |
| Other father | 0.294 (0.336) | 0.528 (0.473) | 0.373 (0.253) | 0.392 (0.315) | 0.949 (1.015) |
| Number of O-levels | 0.046 (0.021) | -0.007 (0.031) | 0.133 (0.048) | 0.069 (0.024) | 0.175 (0.078) |
| Female | 0.607 (0.262) | -2.391 (0.280) | -0.505 (0.212) | 0.595 (0.153) | -0.131 (0.578) |

Note: Model estimated by multinomial logit, robust standard errors are reported. The model includes dummies for survey year and a quadratic in age and a dummy variable for whether O-level information was missing. The omitted father's occupation is manager, the omitted subject is social science. Observation 1421, Pseudo R²: 0.06

A related issue is overeducation, typically where a lower return is observed to years of education that are surplus to those needed for the job. In order to analyse this issue total years of schooling for individuals must be split into required years and surplus years of education. There are a number of ways of measuring overeducation: subjective definitions based on self-

reported responses to a direct question to workers on whether they are overeducated; or the difference between actual schooling of the worker and the schooling needed for their job as reported by the worker (Dolton and Vignoles, 2000). Alternatively a more objective measure can be derived from comparing years of education of the worker with the average for the occupation category as a whole or the job level requirement for the position held. This is often criticized as the classification for the occupation may mix workers in jobs requiring different levels of education. Moreover required levels of education are typically the *minimum* required and not necessarily indicative of the level of education of the successful candidate. Chevalier (2003) deals directly with the definition of overeducation by noting that graduates with similar qualifications display variations in ability, which may over-estimate the extent and effect of overeducation on earnings. A sample of two cohorts of UK graduates is used collected by a postal survey organised by the University of Birmingham in 1996 among graduates from 1985 and 1990 covering the range of UK institutions. Based on measures of job satisfaction this study is able to sub-divide those considered overeducated into ‘apparently’ and ‘genuinely’ overeducated. The apparently over-qualified group is paid nearly 6% less than well-matched graduates but this pay penalty disappears when a measure of ability is introduced. Genuinely over-qualified graduates have a reduced probability of getting training and suffer from a pay penalty reaching as high as 33%. Thus genuine over-education appears to be associated with a lack of skills that can explain 30% to 40% of the pay differential so that much of what is normally defined as over-education is more apparent than real.

3. Distinctions in the existing literature

One approach to distinguishing between the two theories is to posit employer learning. Suppose employers do not observe productivity when workers are hired but workers will, with subsequent work experience, reveal their true productivity. Thus the correlation between wages and education should weaken with work experience. In jobs where screening is important we would expect to find that education explains much of the variation in wages early in working life. Riley (1979) divided his US Current Population Survey data into a group where screening is important (high education and low wage) and one where it is assumed not to be (low education and high wage). He showed that the ratio of unexplained residuals in the screened group relative to that for the unscreened group tended to rise with work experience.

Table 6 shows results for LFS data using tenure in the current job and looks for interactions between education and age, tenure or work experience on the grounds of employer learning. There is a large tenure effect and a significant interaction – but it is positive and not negative as signalling might lead up to expect. Similar results hold for age and accumulated work experience.

Whether such a distinction is effective at discriminating between the two theories is, however, debatable. If education is effective as a signal then the better educated really are more productive and so will earn more even after the employer has learned. Altonji and Pierret (1996) explore this issue ingeniously by looking at how the correlation between wages and productivity-related variables that are not observed by the employer at the time of hiring (but are observed by the researcher) changes with work experience. They find that coefficients on these proxy variables rise quickly with work experience, suggesting rapid learning by employers. This suggests that the signalling value of education is small.

Table 6
Interactions between Education and Tenure - LFS Men and Women 1993-2001

| | Men | Women |
|---------------------|-------------------|--------------------|
| Education years | 0.075 (70.41) | 0.092 (70.1) |
| Tenure | 0.0064 (6.92) | -0.0074 (10.81) |
| Tenure * Educ Years | 0.00036 (4.78) | 0.0016 (25.33) |
| N | 104170 | 103325 |

Note: *t* values in parentheses. Specifications also include controls for age quadratic, year of survey and region of residence.

A second approach uses direct information on ability from tests. This was followed first by Taubman and Wales (1973) and Pscharapolous and Layard (1974) and subsequently by Wolpin (1981). Wolpin, for example, divided his data into “dropouts” and non-dropouts and found that, controlling for ability and education, earnings differentials for those that completed a course were small relative to drop-outs.

A variation on the theme of dividing the data into screening-prevalent and non-prevalent groups divides the data according to competitive sector (the private sector or the self-employed) and the uncompetitive sector (the public sector or employees). Brown and Sessions (1999), for example, exploit the self-employed distinction for Italy. They argue that individuals who plan to become self-employed do not have as large an incentive to invest in

education. Thus the return to education for this group only reflects productivity while the returns for the employees reflects both human capital and a value as a signal. Psacharopoulos (1979) exploits the distinction between private and public sectors where he argued that wages could depart from productivity in the public sector but not in the competitive private sector. Thus, returns should be higher in the public sector. Such tests ignore the fact that the signalling model also suggests that productivity will increase with schooling levels which reflect ability. Moreover, this literature has failed to come to grips with the selection bias associated with being self-employed or being a public sector worker or that the underlying assumption is that individuals make the choice to be self-employed at the same time as their education decision⁸.

There are very few datasets that contain good income data for the self-employed to allow us to pursue the distinction between the returns to employees vs. the self-employed. However, the BHPS data in the UK does contain this information and we report estimates of the coefficient on education years in Table 7 where we find that the coefficients are not significantly different across the two groups, even when we control for selectivity. Table 8 shows a breakdown by public vs. private sector. There is a large effect of being a public sector worker and a significant interaction, but again it is in the wrong direction.

Table 7
Returns for Employed vs. Self-Employed – BHPS Waves 1-8

| | Employees | | Self-employed | | Signalling value |
|------------------|-------------------|-------|-----------------|------|------------------|
| | Return (s.e) | N | Return (s.e) | N | |
| <i>OLS</i> | | | | | |
| Men | 0.0641 (0.002) | 10001 | 0.0514 (0.008) | 1717 | 0.0131 (0.012) |
| Women | 0.1027 (0.002) | 9550 | 0.0763 (0.015) | 563 | 0.0264 (0.019) |
| <i>Selection</i> | | | | | |
| Men | 0.0691 (0.003) | 10001 | 0.0552 (0.022) | 1717 | 0.0139 (0.025) |
| Women | 0.1032 (0.002) | 9550 | 0.0784 (0.066) | 563 | 0.0248 (0.070) |

Note: Figures in parentheses are robust standard errors. The models include year dummies, marital status, and the number of children in three age ranges, region dummies, and regional unemployment rates. The Heckman selectivity estimates use father self-employed, mother self-employed, and housing equity as instruments.

⁸ Both Brown and Sessions (1999), and Arabshehani and Rees (1998) attempt to do so but fail to use convincing identification restrictions.

Table 8
Returns for Public vs. Private Sector: LFS Men and Women 1993-2001

| | Men | Women |
|---------------------------|-------------------|-------------------|
| Public sector | 0.441 (12.62) | 0.244 (4.85) |
| Education | 0.084 (41.4) | 0.099 (33.6) |
| Public Sector * Education | -0.022 (11.07) | -0.0045 (1.44) |
| N | 104002 | 103125 |

Note: *t* values in parentheses. Specifications also include controls for age quadratic, year of survey and region of residence.

Layard and Psacharopoulos (1974) suggested that, under signalling, individuals who complete qualifications slowly send a poor ability signal to employers and therefore face lower wages. Groot and Oosterbeek (1994), using Dutch longitudinal data, propose that accelerated qualifications provide a signal of high ability and therefore ought to be associated with higher wages. Both of these papers find that they reject the signalling explanation. This idea is related to the so-called “sheepskin” effect whereby qualifications have a return that exceeds the return to the number of years spent acquiring them so that there are discontinuities in the returns to schooling at points associated with acquiring qualifications. Hungerford and Solon (1987) did, in fact, find significant evidence in US CPS data of large returns in certificated years of education. However, subsequent work by Heywood (1994) suggested that these sheepskin effects were not, in fact, widespread but confined to certain sectors. In Tables 9a and 9b we pursue the idea of sheepskin effects in two ways: allowing each year of education to have an independent coefficient and then testing for linearity; and controlling for education years but allowing each qualification to have an independent effect. In both cases we can reject the null of no sheepskin effects.

Notwithstanding the empirical evidence, it is unclear why demand should be more concentrated in credential years in the signalling theory. Indeed, the costs of education will typically change in credential years - for example, in moving from (largely free) schooling to (relatively expensive) higher education. Moreover, knowledge may itself come in indivisible lumps and it makes sense for these lumps to be associated with credentials. Finally, the authors are not able to control for unobserved ability differences between those that complete a course and those that do not.

Table 9a
Sheepskin Effects: LFS Men and Women 1993-2001

| | Women | Men |
|---------------------|------------------|-------------------|
| Years of Education | 0.035 (23.1) | 0.025 (45.2) |
| Higher education | -0.106 (8.13) | -0.132 (26.5) |
| A-level | -0.391 (55.3) | -0.338 (89.4) |
| O-Level | -0.435 (47.8) | -0.371 (32.8) |
| Other qualification | -0.574 (42.1) | -0.545 (101.3) |
| No qualification | -0.668 (54.8) | -0.625 (110.6) |
| N | 103344 | 104118 |

Note: Default qualification is degree. *t* values in parentheses. Specifications also include controls for age quadratic, year of survey and region of residence.

Table 9b
Sheepskin Effects: LFS Men and Women 1993-2001

| | Men | Women |
|-------------|-----------------|-----------------|
| Left at 16 | 0.116 (12.2) | 0.148 (15.0) |
| Left at 17 | 0.277 (24.4) | 0.296 (23.6) |
| Left at 18 | 0.371 (31.4) | 0.404 (31.9) |
| Left at 19 | 0.375 (19.0) | 0.383 (18.7) |
| Left at 20 | 0.529 (22.1) | 0.458 (20.0) |
| Left at 21 | 0.613 (43.5) | 0.617 (42.9) |
| Left at 22 | 0.666 (42.1) | 0.589 (38.7) |
| Left at 23 | 0.671 (27.4) | 0.598 (29.3) |
| Left at 24+ | 0.762 (22.4) | 0.661 (24.8) |
| N | 103344 | 104118 |

Note: Default qualification is left at 15. *t* values in parentheses. Specifications also include controls for age quadratic, year of survey and region of residence.

It has been suggested that under human capital theory the content of the curriculum matters to wages. That the curriculum makes a difference to wages is consistent with human capital theory and is not an implication of signalling theory. In particular, Miller and Volker (1984) suggest that graduates employed in jobs that are directly related to their earlier degree studies are exploiting their human capital while those that are employed in some different field are only exploiting the value that their degree has as a signal. The difficulty with the approach is that neither occupation nor subject of study are exogenous and that the combination of the two is quite likely to be strongly associated with differences in motivation. The authors found some graduates did indeed earn more by working in their field of study, but many did not and it is this point that is regarded as evidence that signalling is important. This may suggest curriculum effects on wages that may point towards a human capital explanation rather than signalling.

It is difficult to think of occupations that employ both graduates working with their degree discipline and graduates from a non-specialist background but law is an obvious candidate. To investigate this idea further we separated out individuals who worked in the legal profession as solicitors, barristers and judges and estimated the impact of having studied law as an undergraduate on observed wages. The estimated effect, reported in Table 10 of having a law degree was close to 15% and statistically significant. This supports the idea that the curriculum matters and hence human capital theory. However, again there is an important selection problem this is ignored here. Moreover undertaking a law degree may be a signal of commitment that yields its own wage differential.

Table 10
Curriculum Effects - LFS Men and Women 1993-2001 working in Legal Profession

| | Men and Women |
|-----------------|------------------|
| Education years | 0.0306 (3.50) |
| Law graduate | 0.150 (3.26) |
| N | 427 |

Note: *t* values in parentheses. Specifications also include controls for age quadratic, year of survey, region and sex.

A related but logically distinct approach, due to Kroch and Sjoblom (1994) argues that individual education relative to one's cohort allows employers to infer ability (assuming that

education capacity is fixed and that cohorts do not differ in terms of their ability). They find that relative education has only a weak effect on earnings while the absolute level of education had a large coefficient and conclude, therefore, that signalling is weak relative to human capital. Table 11 investigates the effects of relative education. We define this as education minus mean education for the same birth year cohort. Signalling suggests that relative education matters rather than education per se and, yet, here we find no significant effects. However relative education may reflect cohort size if there is fixed capacity in education institutions.

Table 11
Relative Education Effects: LFS Men and Women 1993-2001

| | Men | Women |
|--------------------|------------------|-------------------|
| Education years | 0.0726 (20.7) | 0.0950 (33.9) |
| Relative education | 0.0018 (0.54) | -0.0025 (1.02) |

Note: *t* values in parentheses. Specifications also include controls for age quadratic, year of survey, region and union status.

4. School Leaving and the Minimum School Leaving Age

In contrast to the tests above, which depend on differences in returns to education across different types of workers, the idea that able individuals attempt to signal their ability by acquiring more education than less able individuals lies behind the earlier work of Lang and Kropp (1984) and, more recently, by Bedard (2002).

The property that Lang and Kropp (1986) exploit is that, under full information, a change in the minimum level of education possibly only affects the decision to exit for those individuals who wanted to leave at the previous minimum but does not affect those with education levels above the new minimum point. In contrast, under a signalling equilibrium a mandatory increase in the education level of those at the minimum will also increase education levels for those with higher than the minimum level of education. The effect of the increase in the minimum affects the whole of the distribution of education, not just the bottom of the distribution. The argument in Bedard (2002) is essentially symmetric: the relaxation of some constraint that previously prevented some individuals from achieving a high level of education allows those with lower levels of education to reduce their education levels. Thus, she looks for an effect of having a local university on high school drop-out rates and finds the drop-out rate is higher when a college is present.

In England and Wales there was an increase in the minimum school leaving age in 1973⁹ - this was referred to as RoSLA (Raising of the school leaving age). Prior to RoSLA close to 25% of each cohort left at the minimum of 15, while after the reform compliance was high and less than 5% were recorded as leaving at 15. The institutional organisation of education in England and Wales at the time of the reform meant that children were divided at age 11 according to a test score. Academic children then attended “Grammar” schools (or at private schools), took “O-level” qualifications at 16, many would attain the required 5 O-level passes and many of these would proceed and take “A-levels” at 18; around one-third of those with the minimum 2 A-level passes required to apply for university entrance would gain admission to university and the subsequent drop-out rate was negligible. In contrast non-academic children attended “Secondary Modern” schools from 11 and either left at 15 unqualified or took “CSE” qualifications at 16 and just a few of these would continue their schooling. From the late 1960’s a programme of comprehensive schooling was introduced gradually across the country and this was (largely) completed by the late 1970’s.

To illustrate the effect of the RoSLA, in Figure 5 we plot the residuals for secondary school outcomes for individuals born 5 years before and after the reform, taken from a regression of years of education against quarter of birth and LFS survey dummies for the entire LFS sample from the pooled 1993-2001 data. We observe a significant movement in the residual for no qualifications below trend for the birth cohort first affected by RoSLA (1957Q3) and a somewhat smaller movement above trend for CSE and GCSE attainment from the same point. A-level attainment on the other hand does not appear to differ across the time series. To focus more closely on this issue Figure 6 plots the average attainment of secondary school outcomes by month of birth for the 1956-1958 birth cohort. Individuals born in September 1957 are the first ones affected by the RoSLA reform and this is clearly illustrated in the plot, again in particular for outcomes where no qualification is received, which drops sharply¹⁰. We again also observe an marginal increase in numbers taking CSE and to a lesser extent GCSE but no obvious change in the numbers taking A-levels before and after the reform.

⁹ Scotland changed two years later. But Scotland has quite a different education system and the typical school leaving age prior to the reform was 16 in any case.

¹⁰ A simple Chow test on the ‘no qualification’ series suggest a significant structural break in the series for individuals born in the 4th quarter of 1957 with p-values close to zero for both men and women..

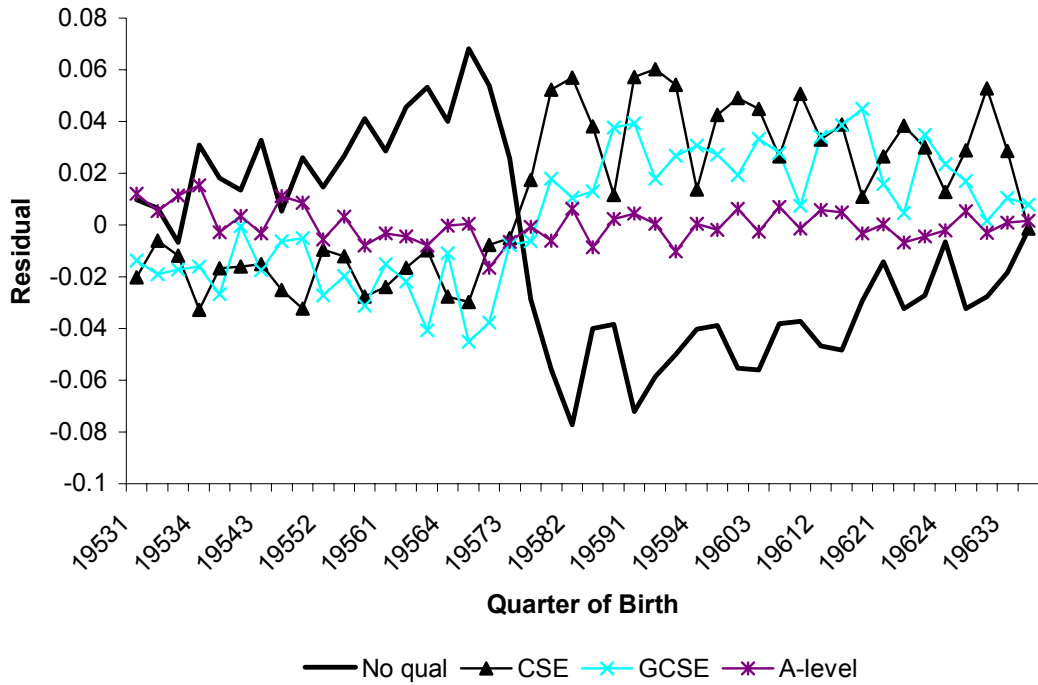


Figure 5 *Highest Secondary School Outcome: detrended series (LFS 1993-2001)*

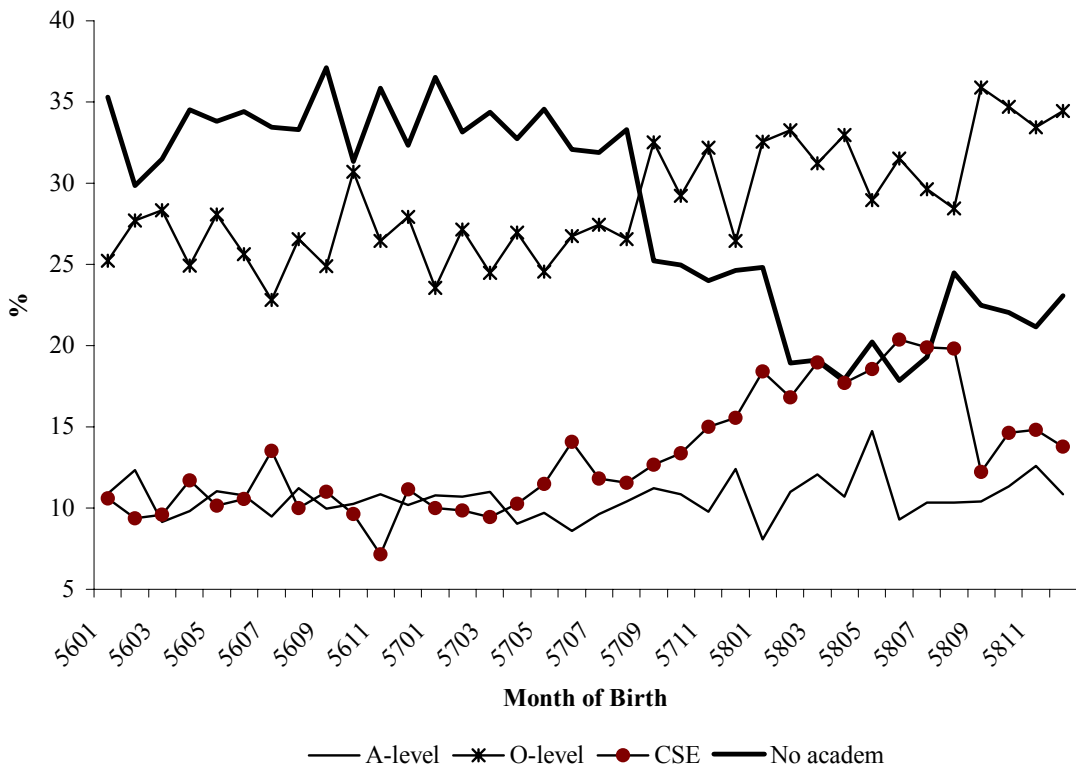


Figure 6 *Highest Secondary School Outcome: Individuals born between January 1956 – December 1958 (LFS 1993-2001)*

To focus on the effects of the RoSLA reform we select only the cohorts that were born between 1958 +/- 5 years from our datasets. Figures 7a and 7b shows the distribution of school leaving age for these cohorts broken down by pre and post reform. It is clear from these that almost all those that left at 15 prior to 1973 now left at 16 post 1973. There would appear to be essentially no change in the post 16 distribution. This can be examined using the Kolmogorov-Smirnov test of equality of distribution which is based on the maximum difference in the cumulative distribution between two populations (in our case the pre-and-post RoSLA schooling distribution between 17 and 25 years). Based on this test we can reject the null of equality of distribution for women only. We cannot reject this null for the sample of males. A simpler test based on a Duncan (Duncan and Duncan, 1955) displacement index, which can be interpreted as the proportion of one group that will have to change its education choice in order to make the distributions equal between the two groups, confirms this outcome. For men only 2.83% of those in the 17-25 years portion of the post-RoSLA distribution would need to change education level in order to completely equalize the pre-and-post RoSLA distribution. For women this is slightly higher at 8%.

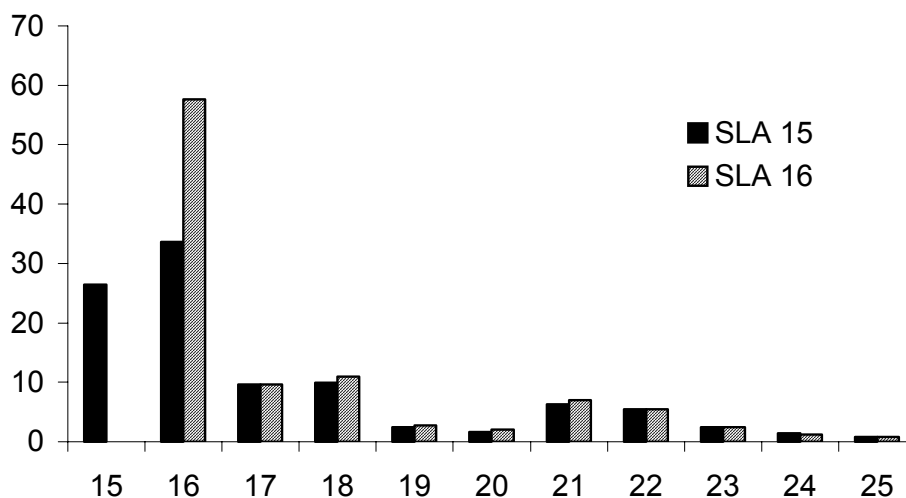


Figure 7a *Pre and Post RoSLA School Leaving Age Distribution – Men born 1953-1963 in LFS 1993-2001*

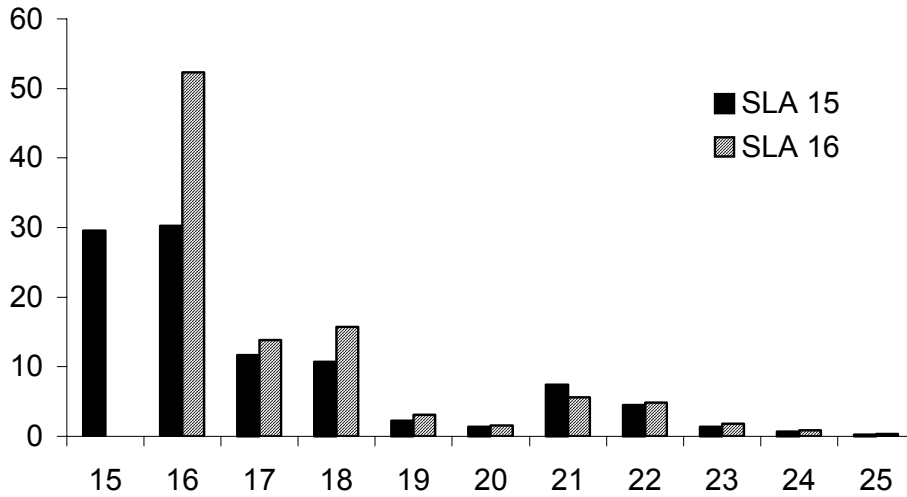


Figure 7b *Pre and Post RoSLA School Leaving Age Distribution – Women born 1953-1963 in LFS 1993-2001*

Figures 8a and 8b show that there are marked differences in the school leaving age distribution – across the whole distribution. However RoSLA is still seen to have no effect on the distribution above 16 – it simply shifts people from 15 to 16. Breakdowns for parental social class and for parental self-employment also show no statistically significant effects above 16 on the distribution.

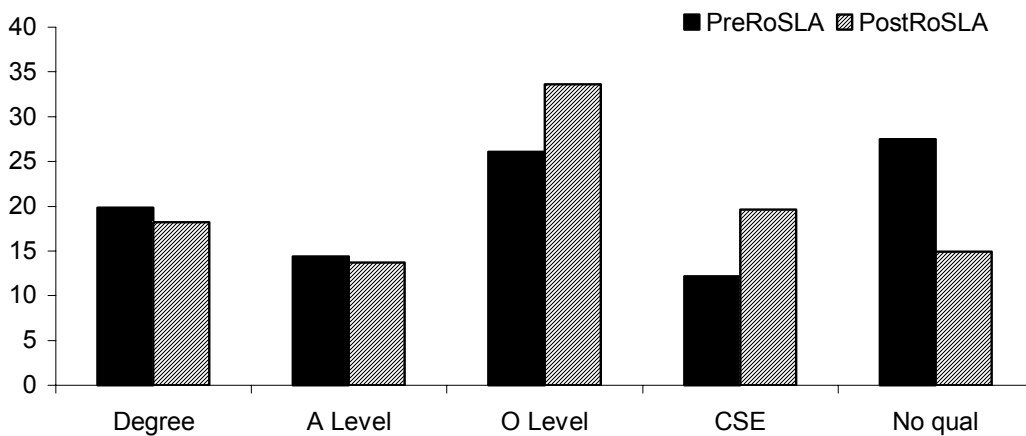


Figure 8a *Breakdown of Highest Qualification Pre and Post RoSLA - LFS 1993-2001 Men born 1953-1963*

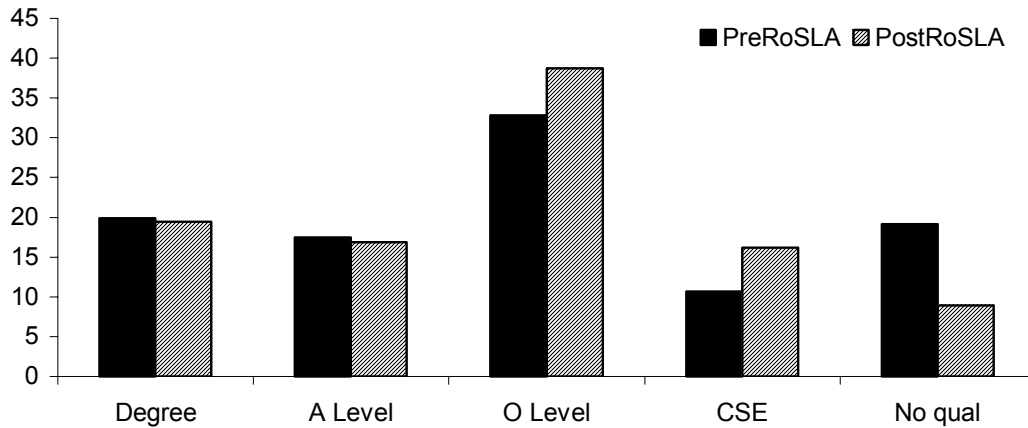


Figure 8b *Breakdown of Highest Qualification Pre and Post RoSLA - LFS 1993-2001 Women born 1953-1963*

It could be that the aggregate data is hiding important changes between types of people. For example, the policy might be expected to have a bigger effect on the post 16 distribution for those who would otherwise be most likely to want to have higher education – but who were prevented from doing so pre-reform. Unfortunately the LFS data contains little background information that might be useful for looking in more depth. However, the General Household Surveys, which although smaller, are available back to the 1960’s and do contain two useful pieces of background information that may well be associated with education: parental social class and early smoking experience. Indeed, the latter variable has been used by Evans and Montgomery (1994) as an instrument for education. Figure 8 shows the distribution (for men only). Those that did not smoke early in life have, on average, 1.1 additional years of schooling and even when one control for social background there is still a statistically significant effect that exceeds 0.7 for both men and women. We find that RoSLA has no effect above 16 for either early smokers or the non-smokers.

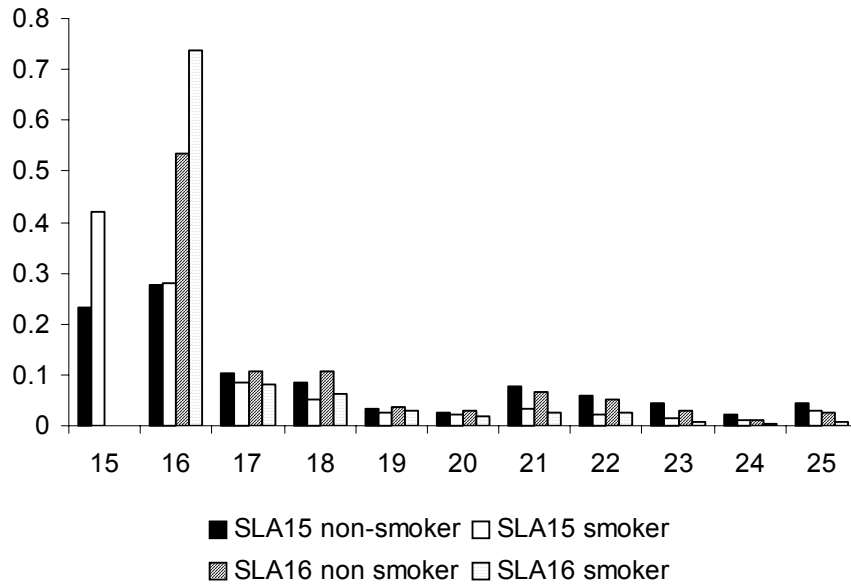


Figure 9 *Pre and Post RoSLA School Leaving Age Distribution by smoking status at age 16- MEN born 1953-63, GHS 82-92 (odd years)*

We also tested for the effects of RoSLA using simple models of the probability of attaining a particular qualification, conditional on having the preceding qualification level. Table 12 reports the marginal effect from the RoSLA dummy in a number of specifications. For men the impact of RoSLA is solely focused on the movement from no qualifications to CSE (specification 1). This finding is robust to the inclusion of paternal socioeconomic status (specification 2) and these controls are jointly significant. It is also robust to the inclusion of paternal socioeconomic status interacted with the RoSLA dummy (specification 3) although these additional interactions are not jointly significant in the regressions. This finding is repeated for women for specifications 1 and 2. Note also that, consistent with earlier findings, women also seem more likely to choose O-level over no qualification but that this is not robust to the inclusion of paternal socioeconomic background controls.

Table 13 presents earnings regressions which exploit this experiment. Individuals with no qualifications earn more in the post-RoSLA cohort, while there are no significant differences in the returns to other qualification. This premium for individuals with no qualification is consistent with the human capital model since individuals with no qualification but a school leaving age of 16 have an extra year of schooling compared to other individuals with no qualification.

Table 12
*Effect of Change in Minimum School Leaving Age on the Probability of Achieving
 Qualification Levels GHS 1982-92 (odd years) cohort born 1953-1963*

| Men | | | | |
|--------------------------|-----------------|------------------|---------------------------------------|--------------------------------------|
| | CSE | O-level | A-level/ conditional on O-level | Degree/ conditional on A-level |
| (1) | 0.101 (4.04) | 0.022 (0.78) | 0.056 (1.44) | -0.098 (1.80) |
| (2) = (1) + paternal SES | 0.105 (4.24) | 0.024 (0.83) | 0.057 (1.44) | -0.097 (1.78) |
| $\chi^2 (6)^A$ | 356.5 | 530.4 | 93.5 | 30.5 |
| (3) = (2) + SES*SLA | 0.070 (1.74) | -0.022 (0.52) | 0.068 (1.33) | -0.115 (1.67) |
| $\chi^2 (6)^A$ | 233.1 | 275.2 | 25.1 | 20.1 |
| $\chi^2 (6)^B$ | 4.8 | 3.5 | 7.9 | 6.8 |
| Observations | 5166 | 5166 | 2650 | 1268 |
| Women | | | | |
| (1) | 0.075 (2.59) | 0.078 (2.10) | 0.018 (0.38) | -0.040 (0.54) |
| (1) + paternal SES | 0.070 (1.74) | -0.022 (0.52) | 0.068 (1.33) | -0.115 (1.67) |
| $\chi^2 (6)^A$ | 181.0 | 330.4 | 93.5 | 30.5 |
| (3) = (2) + SES*SLA | 0.034 (0.77) | -0.017 (0.31) | 0.028 (0.44) | -0.020 (0.22) |
| $\chi^2 (6)^A$ | 106.7 | 154.1 | 25.1 | 20.10 |
| $\chi^2 (6)^B$ | 3.5 | 7.03 | 7.9 | 6.8 |
| Observations | 2812 | 2812 | 1836 | 768 |

Note: (t-stat) adjusted for heteroskedasticity. Model (1) also includes dummies for birth year, parental origin, survey years and smoking behaviour at 16.

A: F-test for joint significance of the paternal Socio Economic Status dummies

B: F-test for joint significance of the interactions between paternal Socio Economic Status dummies and minimum school leaving age

Table 13

Determinants of log wage, Individuals born between 1953-1963 (GHS 82-92 Odd years)

| | Men | Women |
|--------------------------|-------------------|-------------------|
| A-levels | -0.180 (4.64) | -0.160 (3.74) |
| O-levels | -0.203 (7.03) | -0.329 (8.79) |
| CSE | -0.340 (11.05) | -0.457 (10.96) |
| No qualification | -0.428 (16.21) | -0.698 (16.89) |
| Rosla16 | -0.039 (1.11) | -0.134 (2.93) |
| A-levels * ros16 | 0.027 (0.55) | 0.040 (0.75) |
| O-levels * ros16 | 0.022 (0.58) | 0.096 (2.02) |
| CSE * ros16 | 0.058 (1.49) | 0.038 (0.72) |
| No qualification * ros16 | 0.086 (2.32) | 0.181 (3.26) |
| Observations | 5166 | 2812 |
| R-squared | 0.30 | 0.36 |

Note OLS with robust standard errors (t-stat). The specification also includes a quadratic in age and dummies for survey years and region of residency.

5. Conclusion

Our review of the evidence on the effect of education on wages suggested that the effect, on average, was large – perhaps approaching 10% per additional year of education. However, we also have substantial evidence that the variance across individuals is large. Surprisingly, there is only limited support for variance associated with the type of higher education institution attended. This supports recent US research¹¹ that finds no role for attending an “elite” institution once controls for ability and background are included. However, we do find substantial cross subjective differences suggesting that curriculum matters.

Our attempts to test whether the effects of education were due to enhanced productivity we found little, if any, evidence to support the alternative explanation – that education differences simply reflect pre-existing ability differences. However, we are

¹¹ See Dale and Krueger, 2002.

doubtful of the value of these tests which attempt to discriminate between the theories by looking at how the correlation between education and wages differs across groups. Thus, we revisit an old idea suggested originally by Land and Kropp (1986) that under the signalling story any reform that affects the education decisions of a specific group will have a spillover effect on other groups not directly affected. In the UK the raising of the minimum school leaving age is one such reform. Our evidence on the schooling years distribution suggests that, contrary to Land and Kropp (1986), there are no “ripples” from RoSLA – RoSLA just affected people at the minimum. We view this as support for the human capital interpretation of the correlation between education and wages.

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Appendix:

Table A1

Returns to years of education by cohort : Men (LFS 93-2001)

| | Cohort Born | Cohort Born | Cohort Born | Cohort Born |
|--------------|--------------------|--------------------|--------------------|--------------------|
| | 33-46 | 47-57 | 58-68 | 69-77 |
| age | 0.041 (1.83) | 0.057 (8.58) | 0.095 (12.03) | 0.162 (5.91) |
| age2 | -0.001 (2.43) | -0.001 (7.27) | -0.001 (9.80) | -0.002 (4.31) |
| tea==15 | -0.468 (26.66) | -0.409 (31.02) | 0.000 (.) | 0.000 (.) |
| tea==16 | -0.246 (14.53) | -0.229 (15.16) | -0.245 (22.71) | -0.108 (6.24) |
| tea==17 | -0.131 (4.73) | -0.099 (8.50) | -0.112 (12.44) | -0.043 (2.10) |
| tea==19 | -0.001 (0.04) | 0.005 (0.21) | -0.020 (1.50) | -0.021 (0.49) |
| tea==20 | 0.010 (0.29) | 0.083 (2.33) | 0.054 (3.12) | 0.081 (2.78) |
| tea==21 | 0.126 (5.56) | 0.203 (12.46) | 0.234 (25.16) | 0.187 (6.12) |
| tea==22 | 0.162 (4.24) | 0.168 (10.09) | 0.220 (19.57) | 0.205 (5.81) |
| tea==23 | 0.131 (3.26) | 0.146 (10.49) | 0.177 (13.35) | 0.134 (5.19) |
| tea==24 | 0.190 (3.33) | 0.235 (7.01) | 0.135 (4.93) | 0.086 (2.51) |
| tea==25 | 0.223 (9.54) | 0.187 (7.16) | 0.151 (6.32) | 0.082 (2.18) |
| Observations | 21910 | 32052 | 35682 | 10218 |
| R-squared | 0.21 | 0.22 | 0.22 | 0.18 |

Notes: Model includes dummies for year of survey and region of residence. Robust standard errors – t statistics in parentheses.