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DOES EDUCATION RAISE PRODUCTIVITY, OR JUST REFLECT IT?*

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Education has an important effect on wages but it not clear whether this is because education raises productivity or because education is simply a signal of ability. We implement a number of existing tests for discriminating between these two explanations and find that they do not support the signalling hypothesis. However, we have severe reservations about these results and we propose an alternative test based on changes in education incentives caused by changes in the minimum school leaving age in the 1970s. Using this idea we find that UK data appear to strongly support the human capital explanation.

An important issue in the economics of education is that an individual's education has an effect on wages paid in the labour market. For example, Blundell *et al.* (2003) uses detailed education and subsequent earnings information for a cohort of male individuals born in 1958 to estimate that the returns to a UK degree (typically of 3-year duration) relative to graduating from high school at 18 (with 2 'A level' qualifications – a necessary but not sufficient condition for admission to university) is a 24% wage premium.

Human capital explanations, pioneered by Becker (1962) and Schultz (1963), suggest that the correlation between education and wages is due to education enhancing productivity. However earnings may rise in response to education not because of any effect on productivity but simply because education may act as a *signal* of productivity. Employers, believing that education is correlated with productivity, will screen workers for their education and pay higher wages to the more educated. 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 higher productivity individuals choose higher levels of education than lower productivity workers and earn correspondingly higher wages. The theory is largely due to Spence (1973, 1979) and the subsequent empirical literature was reviewed by Groot and Oosterbeek (1994). Riley (2001) provides an excellent modern survey.

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Distinguishing between the human capital and signalling/screening approaches has potentially important policy implications. The official UK inquiry into higher education, the 'Dearing Report' (National Committee of Inquiry into Higher Education, 1997) focused on the correlation between wages and education but made a distinction between this correlation and the causal effect that education has on productivity. The difference between the two is due to the signalling component of the returns to education. However the report relied on assumptions about the magnitude of this difference rather than having estimates available. The fundamental difficulty in unravelling the extent to which education is a signal of existing productivity, as opposed to enhancing productivity, is that the human capital and signalling theories both 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 using UK data. We find the results of these support the human capital explanation but we argue that these tests are weak since the differences that they rely on could be rationalised by either the signalling/screening or human capital theories. However, one test, originally suggested and implemented on cross state US data by Lang and Kropp (1986), 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 in recent times 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.

1. Stylised Facts on the Returns to Education

Much of the existing literature on returns to education has been concerned with the 1980s and early 1990s. Here we update that literature to cover the period 1993 onwards.[1] While the 1980s is widely regarded as a period of rapidly rising overall income inequality, we find that the 1990s is a period of relatively stable inequality; see also Machin (2004). We begin with conventional estimates for prime age (25–59) individuals in England and Wales using the large Labour Force Survey (LFS) data pooled from 1993 to 2001.[2] The LFS is the largest British survey and contains extensive labour market information. In particular, it has contained

[1] We do not consider wider endogeneity issues that have been the concern of Blundell *et al.* (2003) and Harmon and Walker (1995) for the UK.

[2] We exclude Scotland and Northern Ireland to reduce as far as possible the distortions caused by differences in the education systems in these regions. It is not possible to identify whether individuals currently resident in these regions were educated there. Moreover the reforms used later in this paper occurred at a different time to England and Wales. We also exclude those with zero or missing hours of work or earnings.

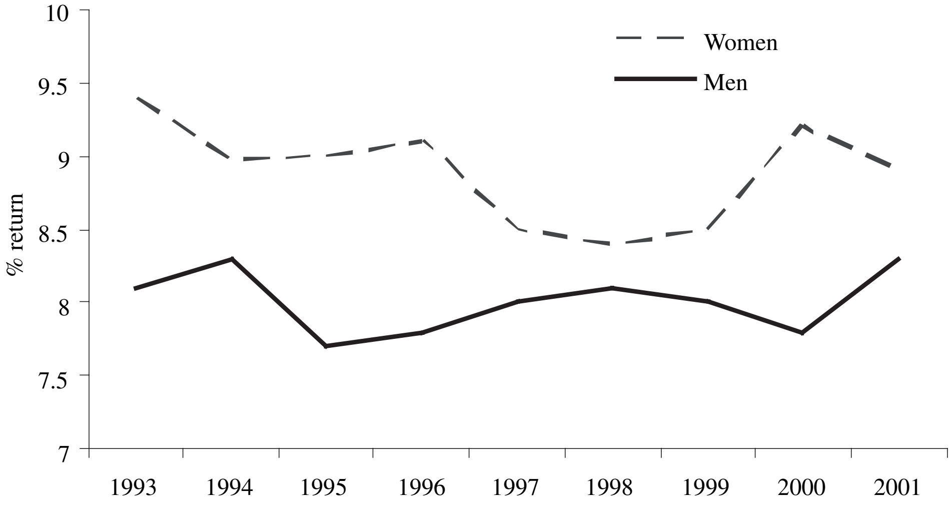

Fig. 1. *Percentage Effect of Additional Year of Education on Wages*

detailed earning and hours of work data since 1993. We compute an hourly wage rate from the ratio of usual earnings including overtime pay to usual hours (from the respondent's main job including overtime) and deflate all wages to 1993 prices using the Retail Price Index.

Figure 1 shows the coefficient on years of education[3] in each year of the LFS data, for men and women separately, controlling for a quadratic in age, region, decade of birth, having a work-limiting health problem, non-white, union and marital status. The samples are 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.

The specification behind Figure 1 is extremely simple and assumes that the log wage is linear in years of education. One way of introducing greater flexibility is to control for qualifications rather than years of education. Figures 2 (*a*) and 2 (*b*) show the coefficients on selected qualification levels over time. By interacting qualifications with a time trend we test for whether there were significant long-run differences over time. There were none. Nor were there significant gender differences in the effects of 5+ O-Level qualifications (at grade 1–6 and the equivalent in the newer GCSE [grade A–C] and older CSE [grade 1] qualifications) relative to no qualifications, or in (undergraduate) degree relative to no qualifications. However the returns to 2+ A-Levels relative to no qualifications were significantly higher for men than women.[4]

[3] Measured as years of continuous full-time education.
[4] The effect of one A-level, not reported here, is somewhat higher for women than men while the effect of two A-Levels relative to having just O-Levels are higher for men than women. Other qualifications not reported are Masters degrees, Doctorates and other higher educational qualifications which are largely post-degree teaching qualifications.

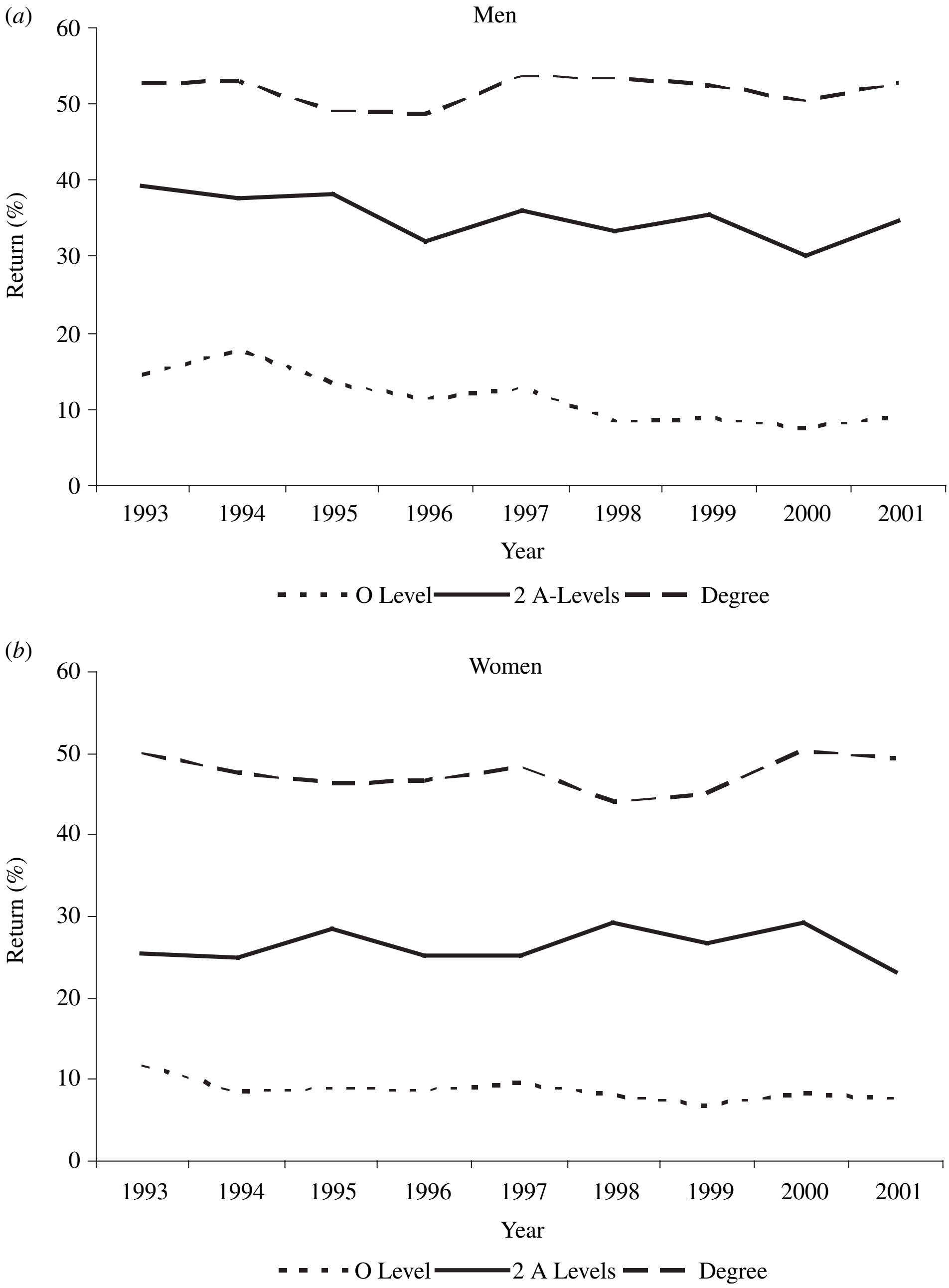

Fig. 2. *Percentage Effect of Educational Qualifications on Wages (a) Men (b) Women*

While there are no significant time trends in the wage effects of education years or qualifications in Figures 1 and 2, 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 education, if it is the case

Table 1

Age Participation Index by Social Class

	1940	1950	1960	1970	1980	1990	2000
Overall API	1.8	3.4	5.4	8.4	12.4	19.3	33.4
Top 3 social classes	8.4	18.5	26.7	32.4	33.1	36.7	47.8
Bottom 3 social classes	1.5	2.7	3.6	5.1	6.5	10.3	18.2
Social Class Gap	6.9	15.8	23.1	27.3	26.6	26.4	29.6

Note: GB undergraduate entrants aged <21 as a % of population aged 18 and 19.
Source: HMO, Department for Education and Skills, 2004.

that young workers are not a good substitute for older ones. Table 1 shows the participation rates in higher education of GB domiciled full time undergraduate entrants by social class, expressed as a percentage of the population of 18 and 19 year olds. The participation rate in higher education increased dramatically in the mid to late 1960s (due to the so-called 'Robbins' expansion) and again in the late 1980s (when institutions were given strong financial incentives to admit more students). Expansion of higher education has been a feature throughout the last six decades but the speed of expansion accelerated dramatically in the 1990s, with the overall participation rate increasing by 14 percentage points. While the 1960s expansion of higher education increased the social class gap in participation, since the 1970s the gap has broadly ceased deteriorating further.

To explore whether returns have fallen across cohorts, we present, in Table 2, estimates of mean and quantile regressions of the coefficient on years of education for a number of specific birth cohorts. The oldest cohort is pre-Robbins, the next is the cohort that experienced the Robbins expansion, the third cohort is the immediate post-Robbins cohort, while the last cohort is the one that experienced the 1990s expansion.[5] The idea behind looking at quantile regressions is that it gives us a feel for how returns vary across the ability distribution. On average, we would expect less able individuals to be concentrated in the bottom of the earnings distribution and the more able towards the top (Buchinsky, 1994). If it is the case that the expansion of education has drawn from the bottom tail of the ability distribution we might expect there to be a fall in returns at the bottom of the earnings distribution as an increasing number of less-able individuals move into higher levels of education. There is a significantly lower coefficient on years of education for the most recent cohort although the fall has not been disproportionately large for the bottom quantile relative to others. So it appears that returns have fallen for the most recent cohort but this fall has not been associated with any particular part of the distribution – in particular, the bottom quartile has not fallen significantly more than higher deciles.

In Tables 3 (*a*) and 3 (*b*) we explore whether the drop in the returns to education for the youngest cohort is observed at all level of education or if the returns to only specific qualifications have dropped. They present mean (i.e. OLS) and

[5] Additionally, the minimum school leaving age was 15 for the first two cohorts and 16 for the last two.

Table 2

Returns to Year of Education – Quantile Regressions: Men and Women

	Born 1933–46	Born 1947–57	Born 1958–68	Born 1969–77
Women				
OLS	0.103 (54.19)	0.099 (83.61)	0.094 (36.50)	0.055 (16.44)
25th percentile	0.101 (60.28)	0.101 (84.43)	0.099 (68.76)	0.062 (29.25)
50th percentile	0.116 (67.95)	0.110 (97.09)	0.100 (74.47)	0.059 (26.70)
75th percentile	0.111 (57.07)	0.104 (79.32)	0.092 (69.47)	0.051 (25.49)
No. of observations	19,065	34,739	36,234	10,451
Men				
OLS	0.086 (40.38)	0.077 (53.65)	0.070 (26.85)	0.040 (12.84)
25th percentile	0.084 (44.09)	0.081 (62.02)	0.072 (53.37)	0.040 (17.11)
50th percentile	0.092 (51.22)	0.077 (72.71)	0.071 (58.86)	0.043 (20.31)
75th percentile	0.097 (55.69)	0.077 (66.00)	0.072 (61.75)	0.043 (19.12)
No. of observations	21,885	31,998	35,601	9,009

Note: LFS 1993–2001 by birth cohort. Robust t-values in parentheses.

quantile regression estimates of the coefficient on O-levels relative to no qualification, A-levels relative to O-levels and Degree relative to A-levels. Over time returns to O-levels have remained stable. Returns to A-levels relative to O-levels increased substantially after the increase in minimum school leaving age. This could be due to the introduction of comprehensive, as opposed to selective, schools that was taking place around the same time. The first two cohorts faced the 11-plus exam that determined whether they followed an academic track or a vocational track. Since both O-levels and A-levels are academic qualifications, the ability of these two groups of individuals may have been fairly similar, making them close substitutes to employers. However with the growth in comprehensive schooling, an ability differential may well have opened between O-levels and A-level students.

Returns to a degree have decreased substantially for the latest cohort across all quantiles.[6] These results are not apparent in the previous literature. For example, Gosling *et al.* (2000) reported an increase in the returns to degree over the period 1978–95 in the smaller Family Expenditure Surveys and General Household Surveys. However, their analysis was not conducted by cohort. It would therefore appear that, for graduates, the youngest generation is not a perfect substitute for older generations, confirming the findings of Card and Lemieux (2001) also using GHS data. These results also provide little support for the idea that the returns to education are lower at the bottom of the distribution. In fact, for women, the results seem to indicate the opposite. If this comes about because lower social class children, who may suffer from more credit rationing, are overrepresented at the

[6] In addition to estimating the mean returns we can also investigate how this varies across individuals according to observable and unobservable characteristics. For example Harmon *et al.* (2003) estimate a model, using LFS data, 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. However the variance associated with unobserved differences does not appear to be any larger, and is arguably smaller, for more recent cohorts.

Table 3

Relative Returns to Qualifications – Quantile Regressions:

(*a*) Men

	Born 1933–46	Born 1947–57	Born 1958–68	Born 1969–77
O-levels vs No qualification				
OLS	0.284 (19.59)	0.311 (40.02)	0.255 (18.68)	0.252 (14.23)
25th percentile	0.180 (12.40)	0.276 (24.28)	0.222 (17.95)	0.219 (9.04)
50th percentile	0.287 (22.45)	0.330 (25.15)	0.259 (22.50)	0.224 (10.28)
75th percentile	0.369 (20.96)	0.358 (27.18)	0.262 (20.50)	0.236 (7.43)
No. of observations	6,204	7,607	9,538	2,512
A-levels vs O-levels				
OLS	−0.046 (3.69)	−0.007 (0.57)	0.084 (8.75)	0.080 (7.29)
25th percentile	0.010 (0.75)	0.017 (1.52)	0.103 (12.04)	0.079 (5.96)
50th percentile	−0.043 (3.52)	−0.010 (1.22)	0.088 (13.83)	0.079 (5.81)
75th percentile	−0.100 (6.42)	−0.041 (4.55)	0.082 (10.24)	0.081 (4.91)
No. of observations	9,599	14,614	18,386	4,721
Degree vs A-levels				
OLS	0.480 (41.01)	0.398 (55.02)	0.331 (28.93)	0.220 (9.66)
25th percentile	0.505 (37.13)	0.450 (52.88)	0.356 (44.36)	0.235 (17.04)
50th percentile	0.521 (47.68)	0.407 (57.30)	0.337 (45.82)	0.250 (16.90)
75th percentile	0.509 (39.32)	0.379 (44.15)	0.328 (41.66)	0.245 (16.17)
No. of observations	10,580	17,125	17,978	4,727

(*b*) Women

	Born 1933–46	Born 1947–57	Born 1958–68	Born 1969–77
O-levels vs No qualification				
OLS	0.245 (28.01)	0.232 (24.48)	0.260 (26.80)	0.231 (10.18)
25th percentile	0.217 (30.30)	0.196 (30.53)	0.193 (18.95)	0.196 (6.97)
50th percentile	0.285 (37.91)	0.262 (41.56)	0.288 (23.25)	0.266 (9.43)
75th percentile	0.280 (26.71)	0.264 (32.97)	0.323 (29.97)	0.274 (8.91)
No. of observations	9,926	15,549	15,667	3,613
A-levels vs O-levels				
OLS	−0.021 (1.88)	0.018 (1.86)	0.110 (11.21)	0.078 (6.06)
25th percentile	−0.067 (4.74)	−0.023 (2.48)	0.077 (9.09)	0.074 (5.25)
50th percentile	−0.026 (2.14)	0.006 (0.62)	0.114 (12.95)	0.102 (7.21)
75th percentile	0.017 (1.02)	0.048 (4.72)	0.135 (18.63)	0.078 (5.36)
No. of observations	5,803	13,162	19,100	5,094
Degree vs A-levels				
OLS	0.528 (30.55)	0.490 (49.80)	0.400 (27.15)	0.299 (14.85)
25th percentile	0.587 (24.44)	0.568 (46.62)	0.467 (42.82)	0.359 (20.03)
50th percentile	0.579 (35.46)	0.547 (55.45)	0.412 (42.96)	0.278 (28.24)
75th percentile	0.509 (28.85)	0.485 (43.52)	0.358 (38.24)	0.259 (16.68)
No. of observations	3,295	8,796	11,754	3,869

Note: LFS 1993–2001 by birth cohort. Robust t-values in parentheses.
Specification also includes a quadratic function in age, year and regional dummies.

bottom of the wage distribution then our results would require that this was accompanied by gender bias by parents.

2. Distinctions in the Existing Literature on Signalling vs Human Capital

The conclusion from Section 1 is that there is no significant time trend in the returns to education, particularly to higher education. Although we do see a drop

Table 4

Interactions between Education and Tenure: Men and Women

	Men	Women
Education years	0.075 (70.41)	0.092 (70.10)
Tenure	0.0064 (6.92)	−0.0074 (10.81)
Tenure × Education Years	0.00036 (4.78)	0.0016 (25.33)
N	104,170	103,325

Note: LFS 1993–2001. Robust t values in parentheses.
Specifications also include controls for age quadratic, survey year and region.

in the returns for the youngest cohort – the cohort who had experienced a large increase in higher education participation – this fall in returns is not concentrated at the bottom of the wage distribution. However, none of this allows us to draw inferences about whether more education makes people more productive, or more productive people choose to get more education to distinguish themselves (to employers) from the less productive.

One approach to distinguishing between the two theories, of signalling and human capital, is to allow for the possibility of employer learning. Suppose employers do not observe productivity when workers are hired but workers will eventually, with subsequent work experience, reveal their true productivity. Thus, if the correlation between wages and education is due to signalling then it should weaken with work experience. Riley (1979) divided the US Current Population Survey data into a group where screening was thought to be important (high education and low wage occupations) and one where it is thought not to be important (low education and high wage occupations). 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. Whether such a distinction, based on essentially arbitrary splits of the data by occupation, is effective at discriminating between the two theories is, however, debatable. Altonji and Pierret (2001) and Galindo-Rueda (2003) explore this issue 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. These papers report that coefficients on productivity rise quickly with work experience, suggesting rapid learning by employers. This implies that the signalling value of education is small. While our data are not rich enough to replicate this kind of test, Table 4 shows results for LFS data using tenure in the current job and looks for interactions between education and tenure to test for employer learning.[7] In our LFS data there is a large tenure effect and a significant interaction – but it is positive and not negative as signalling might lead us to expect.[8] Moreover, an alternative explanation for the interaction would be that learning begets more learning (Heckman, 2000). Thus, it is unlikely that such a test can really discriminate between the two theories.

[7] Johnes (1998) also estimates a model with education and tenure interactions and finds similar results to ours.

[8] Similar results, available on request, hold for age and accumulated work experience.

An alternative way of dividing the data would be into the competitive sector (the private sector and/or the self-employed) and the uncompetitive sector (the public sector and/or employees). Brown and Sessions (1999), for example, exploit the self-employed distinction using Italian data. They argue that individuals who plan to become self-employed do not have as large an incentive to invest in education.[9] Moreover the return to education for this group only reflects productivity while the returns for the employees reflect both human capital and a value as a signal. They report higher level of education and higher returns for employees than for the self-employed, as predicted by the signalling model. Psacharopolous (1979) exploits a similar distinction between private and public sectors and argues that wages could exceed productivity in the public sector but not in the competitive private sector. The lack of competition in the public sector allows higher returns to education in that sector. The author then reports higher returns in the public sector supporting some signalling value of education.

This literature has largely failed to come to grips with the selection bias associated with being self-employed or being a public sector worker – the underlying assumption is that individuals make the choice to be self-employed at the same time as their education decision. Moreover 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. The BHPS data, however, does contain good income data and we can estimate a simple selectivity model using housing equity and dummy variables for whether the father or the mother is self-employed as instruments for self-employment. We report estimates of the coefficient on education years in Table 5. We find that the coefficients are not significantly different across the two groups, even when we control for selectivity.

The LFS data also allow us to identify public sectors workers. Table 6 shows a breakdown by public vs. private sector. There is a large positive direct effect of being a public sector worker (not reported). The estimated return to education is significantly larger in the private sector for men and for women although only the male results seem economically important. Unfortunately, in LFS, there are no obvious exclusion restrictions available to us to examine this issues correcting for potential selection bias associated with the decision to be a public sector worker.

A second approach to distinguishing between ability and productivity is to include ability measures directly. The main problem with the ability controls method is that the ability measures need to be uncontaminated by the effects of education or they will pick up the productivity enhancing effects of education. The National Child Development Survey (NCDS) is a cohort study of all individuals born in England and Wales in a particular week in 1958 whose early development was followed closely and whose subsequent labour market careers have been recorded including earnings. Various ability tests were conducted at

[9] Blanchflower and Oswald (1990) find that the self-employed at age 23 were twice as likely as employees to have predicted at age 11 their worker type 12 years later.

Table 5

Returns for Employed vs Self-Employed

	Employees		Self-employed		
	Return	*N*	Return	*N*	Signalling value
OLS					
Men	0.0641 (32.05)	10,001	0.0514 (6.43)	1,717	0.0131 (1.09)
Women	0.1027 (51.35)	9,550	0.0763 (5.09)	563	0.0264 (1.39)
Selection					
Men	0.0691 (23.03)	10,001	0.0552 (2.37)	1,717	0.0139 (0.56)
Women	0.1032 (51.6)	9,550	0.0784 (1.19)	563	0.0248 (0.35)

Note: BHPS Waves 1–8. Robust t-statistics in parentheses.
The models include year dummies, marital status, and the number of children in three age ranges, region dummies, and regional unemployment rates.

Table 6

Returns for Public vs Private Sector: Men and Women

	Private		Public		
OLS	Return	*N*	Return	*N*	Signalling value
Men	0.080 (102.47)	73,439	0.051 (52.82)	21,252	0.029 (35.37)
Women	0.087 (97.53)	58,628	0.079 (75.58)	36,911	0.008 (8.38)

Note: LFS 1993–2001. Robust t-statistics in parentheses.
Specifications also include controls for age quadratic, year of survey and region.

the ages of 7, 11 and 16. We use the results of Maths and English ability tests at age 7 as controls and show the estimated rates of returns for men and women separately in Table 7. We also show results based on the International Adult Literacy Survey (IALS) dataset which records earnings and ability measured at the time of interview on three scales: prose, document and quantitative. As we might expect, using ability controls taken at later ages confounds the effects of education on ability scores and the apparent bias appears to be large. Thus, the results at age 7 are probably our most accurate estimates of the extent to which education is picking up innate ability and this exhibits a rather small difference and suggests little signalling value to education.

Layard and Psacharopolous (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 reflected in 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

Table 7

Returns to Schooling by Gender in NCDS and IALS: Ability Controls

	Without ability controls	With ability controls
NCDS - GB Controls at age 7		
Women	0.107 (15.29)	0.100 (12.5)
Men	0.061 (10.17)	0.051 (8.5)
IALS – GB Current age controls		
Women	0.106 (7.57)	0.077 (5.92)
Men	0.089 (9.88)	0.057 (6.33)

Note: Robust t values in parentheses. Estimating equations include a quadratic in age, and a monthly time trend.
Ability controls in the NCDS equations are English and Maths test scores in quartiles.
IALS control are the residual formed by regressing current age ability measures against schooling and age to purge these effects.

acquiring qualifications. Hungerford and Solon (1987) did, in fact, find significant evidence in the US CPS data of large returns in certificated years of education.[10] In Tables 8 (*a*) and 8 (*b*) we pursue the idea of sheepskin effects in two ways: controlling for education years but allowing each qualification to have an independent effect; and allowing each year of education to have an independent coefficient and then testing for linearity. A test of joint significance of qualifications suggests that a sheepskin effect may exist, as even after controlling for years of education, each qualification has a positive and significant return. Similarly, in Table 8 (*b*), we test for nonlinearity by including years of education and dummy variables for each year of education (the omitted categories are left at 15 and left at 25). If the returns to education were linear, these dummies would not be jointly significant. However, we cannot reject the null hypothesis, which suggests that returns to education are non-linear.

Notwithstanding this empirical evidence, it is unclear why demand should be more concentrated in years where credentials are awarded 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 to be associated with credentials.

Kroch and Sjoblom (1994) and Johnes (1998) argue that an individual's education relative to one's cohort allows employers to infer 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 9 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, this test is also suspect because

[10] However, subsequent work by Heywood (1994) suggested that these sheepskin effects were not, in fact, widespread but confined to certain sectors.

Table 8

Sheepskin Effects: Men and Women

(*a*) Default qualification is degree

	Women	Men
Years of Education	0.035 (23.10)	0.025 (45.20)
Degree and above	0.583 (50.45)	0.533 (47.51)
Other tertiary education	0.508 (90.07)	0.442 (43.94)
Degree	0.262 (27.31)	0.274 (31.91)
A-level	0.220 (38.03)	0.248 (27.75)
O-Level	0.079 (27.14)	0.072 (8.21)
F (5,∞)	4556.2	1645.6
N	103,344	104,118

(*b*) Omitted years of exit are left at 15 and left after 24

	Men	Women
Years of education	0.058 (48.79)	0.066 (16.78)
Left at 16	0.136 (33.85)	0.095 (21.82)
Left at 17	0.200 (26.98)	0.177 (19.58)
Left at 18	0.241 (31.32)	0.203 (17.85)
Left at 19	0.172 (19.43)	0.105 (6.69)
Left at 20	0.186 (20.03)	0.186 (8.99)
Left at 21	0.272 (35.12)	0.263 (16.61)
Left at 22	0.202 (20.56)	0.226 (7.95)
Left at 23	0.099 (7.60)	0.142 (4.08)
Left at 24	0.058 (3.50)	0.108 (2.29)
F (9,∞)	937.3	373.6
N	99,742	100,506

Note: LFS 1993–2001. Robust t values in parentheses.
Specifications also include controls for age quadratic, year of survey and region of residence.

Table 9

Relative Education Effects: Men and Women

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

Note: LFS 1993–2001. Robust t values in parentheses.
Specifications also include controls for age quadratic, year of survey, region and union status.

relative education may simply reflect a cohort size effect if there is fixed capacity in education institutions.

3. School Leaving and the Minimum School Leaving Age

The tests outlined above argue that differences in returns to education across different types of workers identify signalling effects. However, if a low productivity group were to raise its education because of a policy intervention, the more productive would also want to invest in more education to continue to distinguish themselves from the less productive. If, on the other hand, education makes people more productive, educating one group to a higher level has no effect on the decisions of others. The idea that more able individuals attempt to signal their ability by acquiring more education than less able individuals lies behind the earlier work by Lang and Kropp (1986) and, more recently, by Bedard (2001).

The traditional signalling model of education presumes that a separating equilibrium exists with each worker type having a unique wage and education level associated with their education costs. Suppose there are low, medium and high productivity students with total costs of education given by C_H, C_M and C_L in Figure 3. Faced with their costs and a wage schedule given by the step function w (S), we would expect net income (the vertical gap between the wage schedule and the respective cost functions) maximising workers to choose education levels of S_H, S_M and S_L, and so workers self-select employment contracts with appropriate wages that reflect their productivities.[11] In equilibrium, employers find that their prior beliefs, that workers' education levels correctly reveal their productivities, are confirmed.

Suppose a minimum schooling level of S_M is imposed, the lowest productivity workers will then be constrained and will choose their next best alternative, S_M. The subsequent experience of firms will force them to cut w (S_M) to reflect the lower average productivity of workers with S_M. Medium productivity workers will be faced with this lower wage rate and they will be inclined to move to some

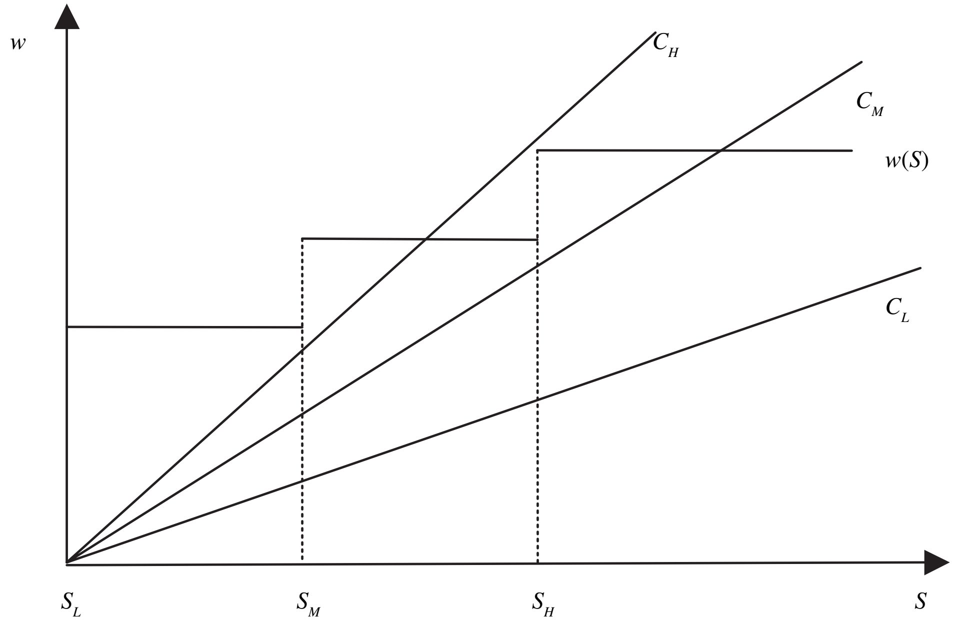

Fig. 3. *Separating Equilibrium and Minimum Schooling Effects*

[11] Strictly speaking the levels of S are not unique but all that is required is that they exist and are ordered appropriately.

higher level of S. Employer expectation fulfilment will not be restored until medium productivity workers all choose a level of S higher than S_M and again receive a wage $w(S_M)$ appropriate to their productivity. For example, if medium productivity workers find that S_H at a wage $w(S_M)$ provides higher net income than S_M now that a S_L is no longer available to type L workers, then type H workers will find employers' expectations of workers with S_H are no longer met and will choose some higher level of S at $w(S_H)$ that is unattractive to type M workers but better for them than S_H at $w(S_M)$. Type H workers would move to some even higher level to restore the separating equilibrium. Thus equilibrium is restored with all worker types paid their productivity and self-selecting new, higher, levels of S. Thus imposing a minimum above S_L will, in general, causes all education levels to rise.

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 education 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 may 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 (2001) 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, the author looks for an effect of having a local university on high school drop-out rates and finds the high school drop-out rate is higher when a college is present, thus supporting some signalling value of education.

In England and Wales there was an increase in the minimum school leaving age in 1973, commonly 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.[12] To illustrate the effect of the RoSLA, Figure 4 plots the average secondary school educational attainment by month of birth for the 1956–8 birth cohort. Individuals born in September 1957 are the first ones affected by the RoSLA reform which is clearly illustrated in the plot which, in particular for outcomes where no qualification is received, drops sharply.

[12] 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 an academic test. Academic children attended 'Grammar' schools and took 'O-level' qualifications at 16. Many would attain the required 5 O-level passes and 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 gained 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 – just a few of these would continue their schooling. From the late 1960s a programme of comprehensive schooling was introduced gradually across the country which was largely completed by the late 1970s. In the 1970s the distinction between O-levels and CSEs disappeared when they were scrapped in favour of GCSEs taken by all students.

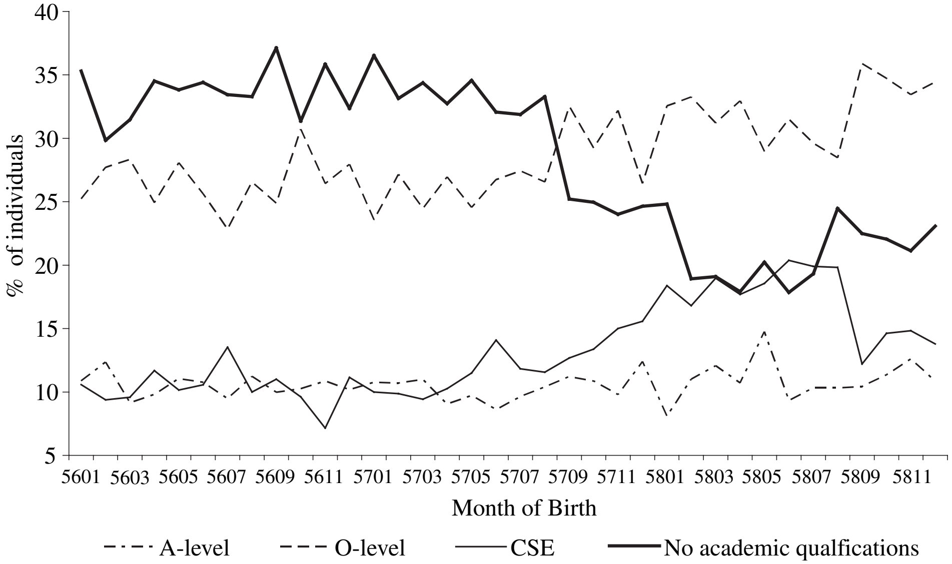

Fig. 4. *Highest Secondary School Outcome: Individuals Born Between January 1956–December 1958*

A simple Chow test on the 'no qualification' series suggests 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. We observe a marginal increase in numbers taking CSE and to a lesser extent O-Level but no obvious change in the numbers taking A-levels before and after the reform. To focus on the effects of the RoSLA reform we select only the cohorts that were born in a +/− 2-year window around 1958 from our datasets. Figures 5 (*a*) and 5 (*b*) show the distribution of school leaving age for these cohorts, broken down by pre and post reform. There are marked differences in the school leaving age across the whole distribution. However RoSLA has no obvious effect on the distribution above 16 – it simply shifts people from 15 to 16 with almost all those that left at 15 prior to 1973 now leaving at 16 post 1973.

We can examine whether there is any change in the post 16 distribution using the Kolmogorov-Smirnov test of equality of distributions, 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 the two distributions for women only (the p-values are 0.065 and 0 for men and women respectively). We cannot reject the null for the sample of males. A simpler indicator is a Duncan displacement index (Duncan and Duncan, 1955), 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. This indicator is similar to the Kolmogorov-Smirnov D-statistics. For men only 2.83% of those in the 17–25 years portion of the post-RoSLA

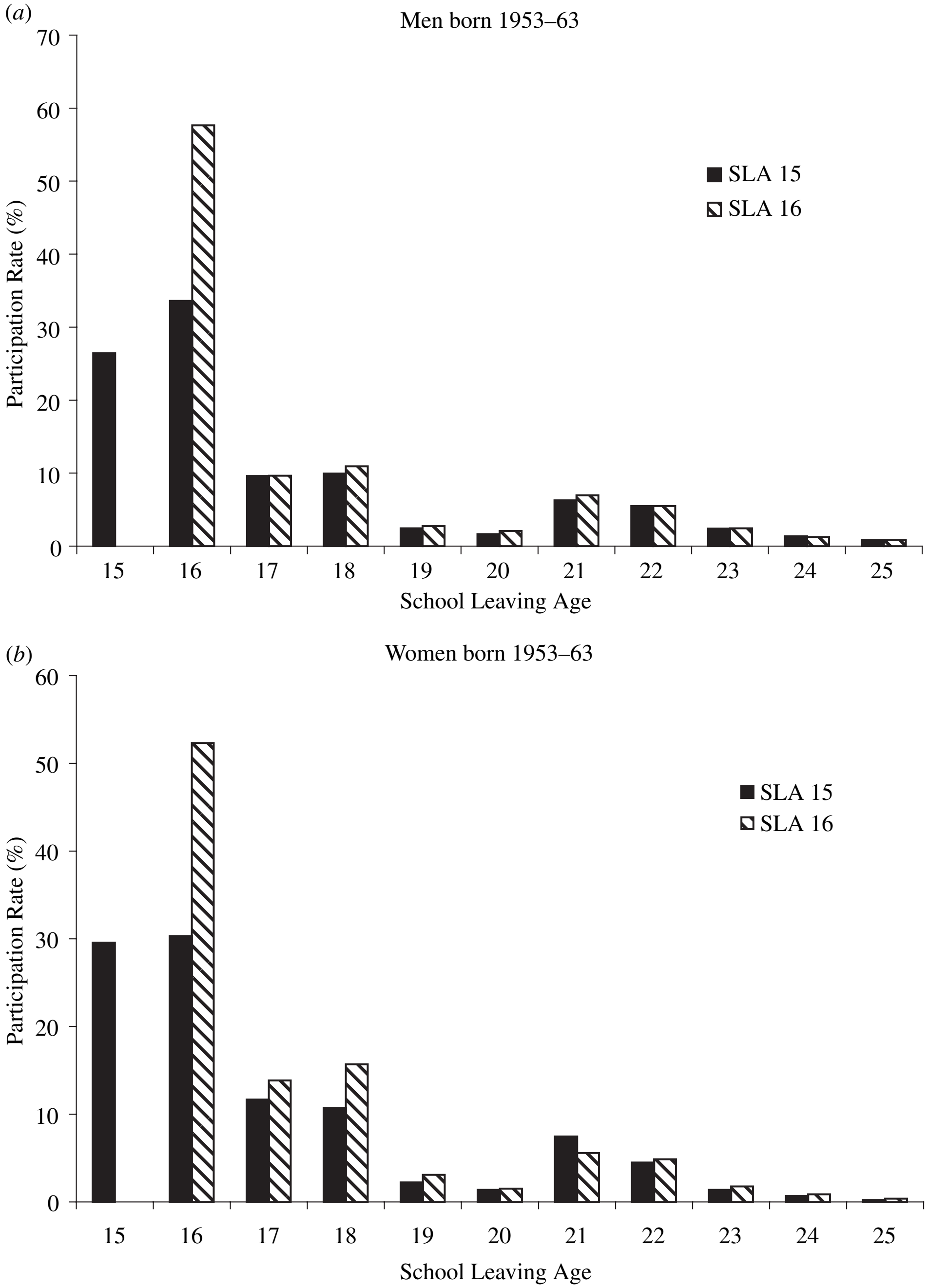

Fig. 5. *Pre- and Post-RoSLA School Leaving Age Distributions*

distribution would need to change education level in order to completely equalise the pre- and post-RoSLA distribution. For women this is slightly higher at 8%.

Table 10

Effect of Minimum School Leaving Age on the Probability of Achieving Qualification Levels

	CSE	O-level	A-level|O-level	Degree|A-level
Men				
(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)
$\chi^2(6)$*	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)
$\chi^2(6)$*	233.1	275.2	25.1	20.1
$\chi^2(6)$†	4.8	3.5	7.9	6.8
Observations	5,166	5,166	2,650	1,268
Women				
(1)	0.075 (2.59)	0.078 (2.10)	0.018 (0.38)	−0.040 (0.54)
(2) = (1) + paternal SES	0.070 (1.74)	−0.022 (0.52)	0.068 (1.33)	−0.115 (1.67)
$\chi^2(6)$*	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)
$\chi^2(6)$*	106.7	154.1	25.1	20.10
$\chi^2(6)$†	3.5	7.03	7.9	6.8
Observations	2,812	2,812	1,836	768

Note: General Household Surveys 1982–92 (odd years), cohort born 1953–63. Robust t-statistics in parentheses.
Model 1 also includes dummies for birth year, parental origin, survey years and smoking behaviour at 16.
*F-test for joint significance of the paternal Socio Economic Status dummies. The critical value for χ^2 (6) = 12.6 at the 5% level.
†F-test for joint significance of the interactions between paternal Socio Economic Status dummies and minimum school leaving age.
The critical value for $\chi^2(6)$ = 12.6 at the 5% level.

We also tested for the effects of RoSLA using simple models of the probability of attaining a particular qualification, conditional on having the preceeding qualification level. Table 10 reports the marginal effect from the RoSLA dummy in a number of specifications estimated using the General Household Surveys where alternate years of data contain information on parental socio-economic status (SES) – information that is not available in LFS data. 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 socio-economic status (specification 2) and these controls are jointly significant. It remains marginally significant 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. The interactions between socio-economic background and RoSLA accounts for possible financial constraints where individuals from lower social background may not be able to signal their ability due to the financial cost of education. These interactions are never significant suggesting that the effect of RoSLA on educational achievement is not dependent on social background. Thus RoSLA had no effect on reducing educational inequality. Note also that, consistent with earlier findings, women seem more likely to choose O-level over no qualification but that this is not robust to the inclusion of paternal socioeconomic background controls. The effect of RoSLA on educational achievements are limited, only men increased their probability of gaining CSE by 10 percentage points. There is no change in educational attainment at higher levels of qualification.

4. 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. In testing 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 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 idea suggested originally by Lang 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 Lang 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. The alternative signalling model would predict that some of those that would have left at 16 would, after RoSLA, now leave later to continue to distinguish themselves to employers from those now leaving at 16 due to the reform. Indeed this argument would suggest that those that would have left at 18 pre-RoSLA would now consider leaving later for the same reasons. If signalling had any importance we might expect changes in educational attainment right through the educational distribution and this does not appear to have occurred.

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