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Schooling and Earnings in the UK — Evidence from the *ROSLA* Experiment*

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Abstract: Recent work by Angrist and Krueger (1991,1992) utilised an "experiment" to distinguish an individual's return to schooling where additional schooling is by choice compared to the return when extra schooling is compulsory. Here we use a similar "experiment" generated by the raising of the school leaving age (ROSLA) from 15 to 16 in the mid-'70s in England and Wales. Preliminary estimates from the Family Expenditure Survey over the period 1978-1986 suggest that for boys affected by ROSLA the effect of compulsory schooling is significantly less than that for elective schooling. In addition our estimates suggest that the loss of experience associated with this imposed extra year of schooling could well imply a negative net return for boys. However the results for girls suggest that additional compulsory schooling has the same effect as elective schooling.

I INTRODUCTION

Recent discussions on the level of educational attainment and participation in the UK have focused on the comparatively low level of participation in the post-compulsory sector. A report recently published by the NIESR examines UK participation in education or vocational training

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against the experience of Japan, Germany and France (NIESR, 1993). Analysis of the demand for education is guided by issues central to the classic human capital approach, in particular the notion of education as an investment of current time and effort in return for future returns, or the schooling-earnings relationship (see Freeman, 1986 and Polachek and Siebert, 1993 for excellent surveys).

Whilst the participation decision has been explored for the UK, for example Micklewright *et al.* (1989) and Pissarides (1982), little work has been completed on the precise nature of the returns to education unlike the US where the literature has developed and continues to be a strong research theme. The key relationship involves estimating an earnings equation of the form

$$Y_i = \alpha + \beta S_i + \delta X_i + u_i \tag{1.1}$$

where Y is an earnings measure, S represents a measure of schooling, X is a set of other variables assumed to affect earnings, and u is a disturbance term representing other forces which may not be explicitly measured, usually assumed independent of the X's and S (for review and discussion see Griliches, 1977, 1979).

More specifically assume the true equation to be estimated (excluding the set of other variables X_i for simplicity) is

$$Y_i = \alpha + \beta S_i + \gamma A_i + u \tag{1.2}$$

where A represents the unobserved ability. The concern about the formulation of an estimate of the return to schooling β is that ability may be associated with both wages and schooling. If, following Griliches (1977, 1979), we assume that ability has an independent positive effect on earnings which is somewhat different from the direct effect ability has on schooling, and also that there exists a relationship between schooling and ability, the following can be developed from (1.2):

$$Eb_{ys} = \beta + \gamma\, b_{AS} = \beta + \gamma\, \text{cov(AS)}/\text{ var S} \tag{1.3}$$

where b_{ys} and b_{AS} represent the relationship between income/schooling and ability/schooling respectively, and β represents the true parameter of the return to schooling. This clearly suggests that the simple least squares estimator b_{ys} is biased if the estimation procedure excludes (the unobservable) ability. If we take the assumption that ability has a positive effect on earnings (i.e., $\gamma > 0$) then clearly the bias in the least squares estimate is upwards.

The approaches adopted to deal with this issue have focused around three key directions (Blackburn and Neumark, 1993).

(i) Include explicit measures for ability to proxy for unobserved ability. This approach explores unobserved ability using test scores as an indicator of ability. IQ and other such tests are an example of such proxies (Griliches, 1977; Griliches and Mason, 1972). This approach has been widely criticised given that the implicit assumption is that ability must be significantly related to IQ. The arguments against this approach argue that ability should be treated as an unobservable which acts as a driving force or motivation behind an individual's schooling or work practices. To proxy ability with IQ may be ignoring the issue that an individual's IQ may have a lot to do with their ability but ability itself may have little to do with IQ.

(ii) The "siblings" or "twins" approach exploits a belief that siblings are more alike than a randomly selected pair of individuals, given that they share common heredity, financial support, peer influences, geographic and sociological influences etc. The approach, as surveyed by Griliches (1979) attempts to eliminate omitted ability bias by estimating the return to schooling from differences between siblings or twins in levels of schooling and earnings, based on a belief that these differences represent differences in innate ability or motivation, a truer picture of ability bias than simple test scores. This approach received much attention in the schooling-earnings literature in the late '70s and early '80s possibly as a result of the availability of suitable panel data or specialist studies like the Kalamazoo project. If the omitted variable, say ability (A), is such that siblings have the same level of A, then any estimate of β from within family data, i.e., differences in salary between brothers, will eliminate this bias. However, if ability has an individual component as well as a family component, which is not independent of the schooling variable, the within-family approach may not yield estimates which are any less biased.

Also, although more desirable than the approach of ability "proxies" outlined above the problem of poorly specified data may be particularly damaging to this sophisticated approach, particularly if the measurement of schooling is prone to error both in the choice of measure and the reporting of the data, even in cross-sectional studies. The bias from measurement error in schooling is likely to increase by forming differences between twins. In the words of Griliches (1977) "Even if the errors (in measurement of a 'years of schooling' variable) are small their effect will be magnified as more variables are added to the equation in an attempt to control for 'other possible sources of bias'. We may kill the patient in our attempts to cure what may have been a rather minor disease originally."

(iii) A more recent approach to the problem of ability bias is exemplified by Angrist and Krueger, 1991, 1992 (hereafter AK91, AK92). This approach exploits natural variation in data caused by different influences on the schooling decision. AK91 explores how an individual's season of birth may imply that some students reach school leaving age after fewer years of compulsory education than others, allowing the distinction between compulsory and elective schooling to be fully explored. AK92 exploits the Vietnam-era draft lottery in the United States to examine a clear change in the motivation behind educational participation. The essence of this "natural experiment" approach is to provide a suitable instrument for schooling which is not correlated with ability.

In this paper we investigate formally the schooling-earnings link using pooled cross-section surveys from 1978-1986 Family Expenditure Surveys for the United Kingdom. More precisely we explore the impact of a change in educational policy in the early 1970s which saw the school leaving age rise from 15 to 16. The raising of the school-leaving age (ROSLA) experiment allows us to distinguish the return to schooling where additional schooling is elective compared to that when the additional schooling is compulsory. Section II describes the data used and the transformations employed in developing the ROSLA experiment, and presents estimates of the schooling/earnings relationship for the United Kingdom.

II ROSLA AND THE COMPULSORY / ELECTIVE DISTINCTION:

Angrist and Krueger's 1991 paper (AK91) estimates the schooling/earning relationship based on micro-level data from the US census, developed around a premise that season of birth is related to educational attainment given the dual policy background of a school start-age policy and a compulsory school attendance law.

As somebody born in the beginning of the year starts school at an older age these individuals can drop out after fewer years of compulsory education than others. AK91 establishes that the season of birth certainly influences the amount of education obtained by individuals. Those born in the beginning of the year appear to obtain less education than those born in the latter part of the year, the average stay in education being about one-tenth of a year lower for those born in the first quarter. It also appears that men born in the fourth quarter of the year receive more education than those born in the first quarter of the *following* year. Angrist and Krueger then examine the effects of season of birth where the compulsory school law is no longer a constraint, i.e., for those who have graduated from high school. The seasonal pattern here is much less pronounced, leading to the conclusion that season of birth is unrelated to post-high school educational outcomes.

AK91 addresses the question as to whether or not differences in education due to season of birth affect the return to education. Simple evidence suggests that those born in the first quarter (who appear to have lower levels of educational attainment) do receive lower earnings than those born in the latter period of the year. Estimates[1] suggest that men born in the first quarter earn 0.7 per cent lower wage and completed 0.126 fewer years of education than men born in the last three quarters of the year, and subsequent estimation by 2SLS uses quarter of birth dummies interacted with year of birth dummies as instruments in a simple human capital framework. The difference between OLS and 2SLS estimation is generally not statistically significant, which points to a general conclusion that omitted variable bias may not be as important an issue particularly in the immediate years of schooling which follow the compulsory schooling level, suggesting that ability becomes an issue only in attainment of education beyond the minimum school leaving age. This also suggests that the students who are compelled to attend school earn higher wages as a result.

Estimation

We present estimates of the returns to schooling using the raising of the school-leaving age as a means of exploring the "compulsory/elective" distinction. The data used is from the Family Expenditure Survey pooled over nine years, 1978-1986. The FES is a continuous budget survey of some 7,000 households per annum.[2] Our estimates come from a sample of 35,894 men and 28,205 women aged 18-64 in year of interview, from which we can estimate for both manual and non-manual workers. Full data descriptions and complete results are available from the authors on request.

Any estimation of a human-capital framework such as the models discussed in the preceding sections require a number of key variables to be defined. Given the nature of the FES, experience can only be defined by a simple variable which measures the number of years since the individual has left the education system. The variables EDUC15 — EDUC21P define the school-leaving age of the individual, from 15 to greater than 21 years of age respectively. The creation of the dummy variable for ROSLA was based around the change in the school-leaving age in the early '70s. Students who would normally have reached the minimum school-leaving age of 15 in 1973

1. Using Wald methodology which computes the return to education as the ratio of the difference in earnings by quarter of birth to the difference in years of education by quarter of birth.

2. However we are not dealing with a traditional "panel" or longitudinal study. The data does not refer to the same individuals over time. For an excellent review of the issues involved in panel data estimation see Hsiao (1986).

had to stay on for an additional year of compulsory education as ROSLA took effect.[3] The cohort of 15 year olds who left school in 1972 were the last such grouping in the UK educational system. Hence our study establishes a simple dummy variable which takes the value of one if the respondent was born in 1958 or later, and zero otherwise.[4] In this way the ROSLA dummy defines a sample for whom the minimum school leaving age was 16 as opposed to 15. By interacting the ROSLA variable with EDUC16 allows us to examine where the individual left school at 16 in the period when this would represent a year of elective schooling as opposed to when it was enforced by ROSLA.[5] A further variable POSTED is defined as the number of years of post-compulsory education both for when compulsory education ended at 15 (pre-ROSLA) and 16 (post-ROSLA). POSTEDR defines the interaction of POSTED and the ROSLA variable. Although not reported here we also include full occupational, industry and regional dummies, plus regressors to control for movements in earnings over the course of the sample period.[6]

The base for our initial estimates is education to 15 years, with Wales, 1978 and Domestic Services representing the base region, year and industry respectively. For estimates involving all males/females and manual males/females the omitted occupational category is unskilled. For non-manual males/females the omitted category is shop worker. Our estimates (presented in Tables 1(a) and (b)) follow an OLS estimation of the returns to schooling based on a simple human capital formulation, with LNWAGE, the log of the hourly wage rate as determined from the FES, as the dependent variable. OLS was chosen as an estimation method for more than just simplicity. Both AK91 and AK92 and Blackburn and Neumark (1993) have explored the growing belief that differences between estimates produced via OLS and 2SLS, if any, are generally small and statistically insignificant. Also the lack of any strong instruments in the FES data mitigated against modelling schooling participation as part of a two-stage approach.[7]

3. Although there are, in effect, some flexibilities in the regulations based around birth dates. However, for the purposes of the estimation procedures employed here the effect of these are not included.

4. The year 1957 represented the last birth year for whom the 15 year minimum SLA applied. The 1958 cohort represented the first group for which the 16 year minimum SLA applied.

5. Thus, EDUC16R=(EDUC16*ROSLA). EDUC16 can only be one if the individual left school at 16, EDUC16R can only be one if the individual left school at 16 after the ROSLA legislation took effect, and zero otherwise.

6. Full results including these variables are available on request from the authors.

7. Given the lack of variables such as ethnic and religious backgrounds, residency issues and characteristics such as parental occupation the estimation of a 2SLS procedure intuitively seems less feasible.

Table 1(a): *OLS Estimates (Males)*

	Dependent Variable LNWAGE All Male	Manual Males	Non-Manual Males
CONSTANT	0.8970	0.9728	0.8008
EXP	0.0444	0.0349	0.0527
EXPSQ	–0.0008	–0.0006	–0.0008
EDUC16	0.1147	0.0899	0.1317
EDUC16R	–0.0402	–0.0903	–0.0647
EDUC17	0.1234	0.0693	0.1560
EDUC18	0.1679	0.0563	0.2266
EDUC19	0.2015	0.1114	0.2613
EDUC20	0.2267	0.1174	0.2871
EDUC21P	0.3669	0.2000	0.4363
ADJ.R^2	0.3934	0.2709	0.3831
NUMBER OF OBSERVATIONS	35,894	21,502	14,392

Note: All statistically significant at 95 per cent level unless otherwise indicated by *. Omitted category is education to 15, Wales, 1978, Domestic Services. For All/Manual samples the base is unskilled. Shop workers are the base elsewhere. Regressions also include regional, occupational and industry groupings dummies as regressors, as well as yearly trend variables to account for natural wage movement and productivity changes.

Table 1(b): *OLS Estimates (Females)*

	Dependent Variable LNWAGE All Female	Manual Females	Non-Manual Females
CONSTANT	0.7781	0.8853	0.9103
EXP	0.0176	0.0090	0.0207
EXPSQ	–0.0003	–0.0002	–0.0004
EDUC16	0.0561	0.0018*	0.0841
EDUC16R	0.0112*	0.0010*	0.0069*
EDUC17	0.0885	0.0080*	0.1163
EDUC18	0.1695	0.0892	0.2947
EDUC19	0.1639	0.0372*	0.2007
EDUC20	0.2348	0.0023*	0.2721
EDUC21P	0.2873	0.0121*	0.3234
ADJ.R^2	0.4031	0.1183	0.3867
NUMBER OF OBSERVATIONS	28,205	10,825	17,380

Note: All statistically significant at 95 per cent level unless otherwise indicated by *. Omitted category is education to 15, Wales, 1978, Domestic Services. For All/Manual samples the base is unskilled. Shop workers are the base elsewhere. Regressions also include regional, occupational and industry groupings dummies as regressors, as well as yearly trend variables to account for natural wage movement and productivity changes.

Of particular interest from these estimates is the EDUC16 and EDUC16R variables, whose interpretation of these variables is of central importance. Until the policy change of ROSLA the minimum school leaving age was 15 which corresponds to the educational attainment of our base individual. EDUC16 thus represents the return to the individual who stays on until 16. If, for example, we look at the complete male sample the additional year of education yields a return of almost 11.5 per cent. Hence we obtain the familiar result that suggests the returns to education are positive, consistent with the findings of a number of studies. However, this interpretation does not hold for those who were affected by ROSLA. The inclusion of EDUC16R enables us to examine the return to education at 16 when this represented an extra year of compulsory rather than elective schooling. Any differential between the returns for 16 year old school-leavers pre- and post-ROSLA could be interpreted as a difference in the schooling/earnings relationship when schooling is elective rather than when it is compulsory.

The results presented in Tables 1(a) and (b) clearly imply that the returns to schooling are positive significant. For males the coefficient on EDUC16 suggests return in the region of 11 per cent but when examined employing a manual/non-manual distinction the return is 9 per cent and 13 per cent respectively. Clearly the choice of remaining in education past the compulsory school-leaving stage (our default) is beneficial in terms of earnings profiles. As one would expect the marginal return to education as determined by EDUC17-EDUC21P is significantly different between manual and non-manual workers clearly suggesting that staying on in education yielded less to the manual worker than the equivalent schooling did for his non-manual counterpart. In general, however, these estimates suggest a qualification-obtaining effect, given the return on EDUC18 (broadly equivalent to A-level) and EDUC21P (where the individual is likely to have obtained a third-level degree/diploma). The effect of experience (EXP) again is clearly positive. For the 16 year old school-leaver the loss of a year of experience is more than offset by the return to that extra year of schooling.

As explained earlier EDUC16R picks up on the interaction between school-leaving age and the change in legislation brought about by ROSLA. Across our sample the clear indication is that the 16 year old school-leaver faces a different return to education pre- and post-ROSLA. For the complete male sample the effect of EDUC16R is a lowering of the school-leaving age/earnings relationship for those who leave school in their 16th year in the post-legislation period by about 4 per cent. If one examines the male sample by occupational structure the result is even more pronounced. For manual males the return to education is close to 9 per cent for the first additional year but this is completely offset by the effect of ROSLA given the role of

EDUC16R in the estimation. Furthermore, with the non-manual sample the returns to remaining in education until 16 yields a 13 per cent return pre-ROSLA but EDUC16R of –0.0647 would indicate a post-ROSLA return of about 6.7 per cent. These estimates suggest that for manual males the returns to additional schooling past the age of 15 are lower than the non-manual equivalent whether or not this schooling is elective or compulsory. More importantly given that ROSLA all but eliminates the gain from schooling for the manual males the direct implication from our estimation is that in terms of their schooling/earnings profile the wages paid to these individuals was not affected by an additional year of education when it was compulsory. Effectively the 16 year old school-leaver was not perceived any differently in the job market from his 15 year old equivalent in the pre-ROSLA period.

If one considers this year spent in education as a year of experience foregone then the opportunity cost of the ROSLA year becomes more pronounced. The year of experience yields a return of between 3.5-5 per cent in earnings terms. The direct implication of including this estimate in our analysis is that for the individual who would have left school at 15 but was now affected by ROSLA this return to experience represents the opportunity cost of ROSLA. In the case of the manual male sample this would imply the net return to education[8] from elective schooling would be 5.5 per cent, but the post-ROSLA net return would be negative suggesting that forcing the individual into additional schooling may be damaging in terms of earnings potential. For the non-manual sample the same analysis suggests net returns to education before the policy change of 7.9 per cent and 1.5 per cent afterwards. Again the clear implication is that remaining in education where elective has a different consequence in terms of earnings than remaining by compulsion, even when returns to experience foregone are taken into consideration.

The inclusion of occupational dummy variables in our estimation often raises the criticism that the choice of occupation may be subject to unobservable variable bias in the manner of the "ability bias" issue in returns to education. However, the classification of occupational structure as defined by the Family Expenditure Survey is so broad that it is doubtful that this would represent a significant problem. Estimates of the models presented in this paper excluding occupational status as regressors all return estimates of the key parameters which are only marginally different from the results displayed here, leading us to conclude that bias of this form is not present in the current model.

Table 1(b) presents equivalent results for women, which differ significantly

8. Defined as (EDUC16-EXP) for the pre-ROSLA and ((EDUC16-EDUC16R)-EXP) post-ROSLA.

from those for men. Interestingly the results for EDUC16R are both positive and insignificant across the three samples which suggests that the return to schooling is positive irrespective of whether that schooling is elective or compulsory. The specification of an earnings equation for women encompasses issues beyond the scope of this paper.[9] Indeed, the recent literature on returns to education such as those referred to in Section I have rarely attempted to model the returns for females. One criticism often levied is that of sample selection bias (Heckman, 1979). If participation is non-random then the estimates of population characteristics produced will not accurately describe the true population distribution of characteristics no matter how big the sample size is. Unless some correction can be made for the non-randomness of the observed sample a bias will result. The suggestion here is that unobservables which are possibly correlated with the wage may also be possibly correlated with the participation decision hence participation may have to be modelled separately as part of the two-step procedure developed by Heckman (1979).[10] While not attempted in this paper the procedures have been explored in Aljebory and Walker (1993) on earnings equations for women using the FES data and while the coefficient on the inverse Mills Ratio, included as part of the Heckman two-step procedure, is generally significant, the inclusion rarely affects the estimated return to education. We conclude therefore that in terms of the estimated return to education presented in this paper the sample selection issue is not a serious estimation problem.

It would appear that our estimates of returns to education in the initial post-compulsory year are significantly lower than the male equivalent, and for manual women the coefficient on EDUC16 is not significant. The estimates for this sample would suggest that attainment of post-compulsory education for this sector requires A-level equivalent qualifications before any significant wage effect takes place, despite the fact that we are dealing with skilled, semi-skilled and unskilled workers only in this sample. Effectively, the first two years of educational attainment have no influence on wages and the benefit of education past 18 is also insignificant. The explanatory power of our estimates for manual women is far below that for both the other female categories and the estimates for the male sample, which suggests some important omissions from our variable list. The other variables, such as experience, all have very marginal effects although significant. The clear implication is that the schooling/earning relationship is far less significant for women than men, with elective/compulsory distinctions not apparent. With

9. Such as discrimination effects, social class issues and the effect of equal pay legislation for example.

10. See Wright and Ermisch (1991) for one example of the literature in this area.

the exception of manual workers the message appears to be that educational attainment for women can only be beneficial, irrespective as to whether or not that education is compulsory. Female manual workers appear to find little reward from education. Also, in general the ability of education to lead to higher earnings is less pronounced for women than men, lending possible weight to some discriminatory issues. All of these comments must, however, be placed in the context of the rather more complex issues involved in estimating female earnings equations. It would seem appropriate therefore not to consider the female situation in depth for the remainder of the paper.

An Alternative Formulation

One of the problems of this estimation procedure is that we are focusing our attention on the returns to education for 16 year old school-leavers pre- and post-school-leaving age legislation. In some ways the implicit assumption is that those who left school at 16 post-ROSLA would have left at 15 had it been possible. Given a lack of any indication that this would be the case we may be restricting the model, irrespective of the fact that the assumption may actually be quite a strong one. With this in mind we estimated an alternative model which maintained the full occupational, regional, industrial and time dummies but instead of examining the effect of school-leaving legislation via the returns to an additional year of schooling pre- and post-ROSLA we now specify the ROSLA dummy as a regressor in a model formulated around the return to post-compulsory education.

The variable POSTED as defined earlier represents the number of years of post-compulsory education calculated from the reported school-leaving age and the minimum school-leaving age which applied to the individual (usually 15 for those born pre-1958 and 16 otherwise), with POSTEDSQ, POSTEDCU representing the squared and cubed values of POSTED in a cubic specification of post-compulsory education. In this more flexible approach reported in Tables 2(a) and (b) the impact of ROSLA legislation on the returns to post-compulsory education can be examined.

The results strengthen original arguments concerning the distinction between compulsory and elective schooling. The ALL MALES sample indicates a clear return to a year of post-compulsory education, in terms of the addition to LNWAGE, of 0.0578.[11] The ROSLA dummy yields a negative coefficient of –0.047. The relationship between the two results may be best illustrated by the following simplified diagram.

11. Computed from 0.0626-0.0048 (where 0.0048=2(0.0024)).

Table 2(a): *OLS Estimates (Males)*

Dependent Variable LNWAGE	All Male	Manual Males	Non-Manual Males
CONSTANT	0.9603	1.0348	0.8868
EXP	0.0365	0.0279	0.0440
EXPSQ	–0.0006	–0.0005	–0.0007
POSTED	0.0626	0.0465	0.0813
POSTEDSQ	–0.0024	–0.0041	–0.0039
POSTEDCU	0.0001*	0.0001*	0.0001*
ROSLA	–0.0468	–0.0851	–0.0819
ADJ.R^2	0.3945	0.2731	0.3848
NUMBER OF OBSERVATIONS	35,894	21,502	14,392

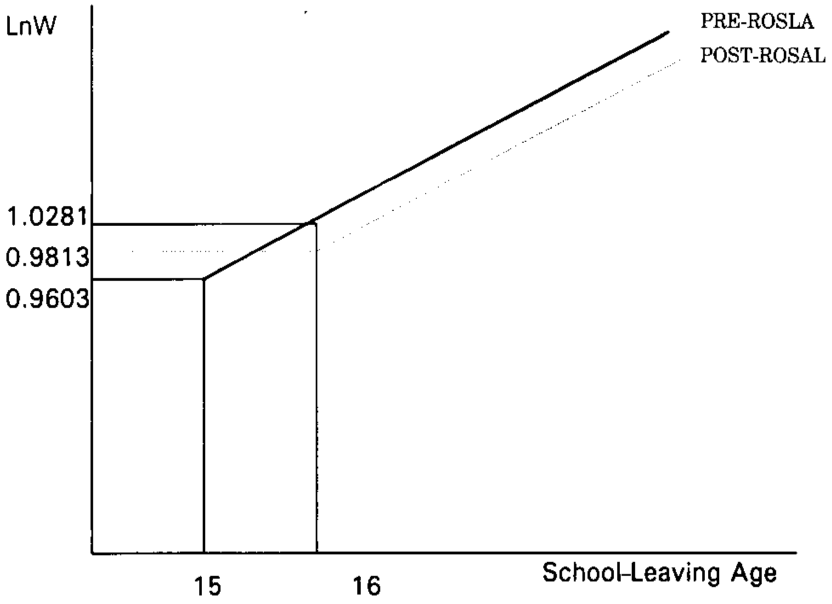
Note: All statistically significant at 95 per cent level unless otherwise indicated by *. Omitted category is education to 15, Wales, 1978, Domestic Services. For All/Manual samples the base is unskilled. Shop workers are the base elsewhere. Regressions also include regional, occupational and industry groupings dummies as regressors, as well as yearly trend variables to account for natural wage movement and productivity changes.

Table 2(b): *OLS Estimates (Females)*

Dependent Variable LNWAGE	All Females	Manual Females	Non-Manual Females
CONSTANT	0.7892	0.8718	0.9463
EXP	0.0140	0.0091	0.0157
EXPSQ	–0.0002	–0.0002	–0.0003
POSTED	0.0324	0.0105*	0.0473
POSTEDSQ	–0.0016	–0.0008*	–0.0003*
POSTEDCU	0.0001	0.0000*	0.0000*
ROSLA	0.0122*	0.0125*	–0.0016*
ADJ.R^2	0.4024	0.1174	0.3856
NUMBER OF OBSERVATIONS	28,205	10,825	17,380

Note: All statistically significant at 95 per cent level unless otherwise indicated by *. Omitted category is education to 15, Wales, 1978, Domestic Services. For All/Manual samples the base is unskilled. Shop workers are the base elsewhere. Regressions also include regional, occupational and industry groupings dummies as regressors, as well as yearly trend variables to account for natural wage movement and productivity changes.

Here we present the returns to an individual pre- and post-ROSLA. Before the legislation the individual who left school at 15 received a mean hourly (log) wage of 0.9603. If he remained in education until 16 the increase was



0.0578, bringing the wage to 1.0281. The schooling-earnings profile is defined by the solid line (although here represented by a linear relationship). If the individual was caught by the ROSLA effect the increment was 0.0578 – 0.0468 = 0.0110, hence the individual who left school at 16 post-ROSLA saw his earnings profile defined by the dashed line beginning at 0.9813. If the schooling/earnings relationship was not affected by whether schooling was compulsory or not the return for the 16 year old post-ROSLA school-leaver should equal the return to the individual who left school at 16 after one year of elective schooling. If ROSLA is potentially damaging in terms of the schooling/earnings relationship then the 16 year old may be made worse off than his pre-ROSLA counterpart as is the case here.

If we return now to our estimates for the manual/non-manual males the suggestion again is that the return from post-compulsory education is significantly lower for the manual men, and where ROSLA has taken effect the gains may even be completely offset. In all cases the effect of ROSLA is to reduce the effect of post-compulsory schooling. Again with manual workers the return indicated by the POSTED variable is much lower than the non-manual equivalent, clearly suggesting that participation in post-compulsory education may have been less advantageous than for the non-manual sector.

The effect of ROSLA is quite pronounced. For the manual sample the ROSLA dummy suggests that the wage return for a 16 year old minimum age school-leaver may be below that of his 15 year old equivalent pre-ROSLA given that the increment from the additional schooling is less than the negative ROSLA coefficient. For the non-manual sample the effect of ROSLA is not as strong but it is still as dramatic. The return to a year of post-compulsory education is about 8 per cent but the ROSLA dummy all but eliminates that effect.

The estimates presented for women supports the view expressed earlier that the compulsory/elective schooling distinction does not hold for the female sample. The return to post-compulsory education is again below that of the male equivalent indicating less incentive effects for women to remain in education. The return to education does not, however, suffer from the same problem as the male sample given the result that ROSLA is in all cases positive and insignificant. The suggestion for manual women is that remaining in education yields no return in earnings terms as POSTED is insignificant for this group, which is consistent with our earlier findings.

In the results presented in Tables 2(a) and (b) we are holding the slope of the schooling/earnings profile constant pre- and post-ROSLA. If we remove this restriction by including POSTEDR, the interacted POSTED/ROSLA term, the estimates reported in Tables 3(a) and (b) are obtained. The estimated constant and the coefficients on experience and the other unreported variables remain largely unchanged by this formulation. However, the effect of the ROSLA legislation is different. By allowing the slope of the schooling/earnings profile to change after the legislation we are taking account of the fact that, firstly, the year spent in education may have contributed to the stock of human capital for the individual, and, secondly, the productivity of workers may have changed over the period. In this sense the specification using POSTEDR allows for these changes to be absorbed into the schooling/earnings profile. POSTEDR has a significant, negative coefficient, which is therefore in line with our earlier estimates. However, comparing Table 2 to Table 3 reveals a sizeable difference in the magnitude of the effect on earnings attributable to ROSLA. The POSTEDR coefficients are in the range (–)0.018-0.026 which are quite different from the estimates for ROSLA reported earlier of (–)0.047-0.082. It would appear therefore that allowing the schooling/earnings profile to shift for the post-ROSLA legislation grouping does alter the negative effect of the change in the school-leaving age, dampening it significantly although the effect does remain significant. Increases in worker productivity and schooling attainment appear to have sizeable affects for the post-ROSLA generation. However, we must resist attributing this solely to the change in minimum school-leaving age and recognise the very strong external factors faced by this grouping.

Table 3(a): *OLS Estimates (Males)*

Dependent Variable LNWAGE	*All Male*	*Manual Males*	*Non-Manual Males*
CONSTANT	0.9429	0.9651	0.8330
EXP	0.0365	0.0316	0.0456
EXPSQ	–0.0006	–0.0005	–0.0007
POSTED	0.0719	0.0611	0.0887
POSTEDSQ	–0.0033	–0.0054	–0.0045
POSTEDCU	0.0001*	0.0002	0.0001*
POSTEDR	–0.0256	–0.0256	–0.0173
ADJ.R^2	0.3954	0.2717	0.3845
NUMBER OF OBSERVATIONS	35,894	21,502	14,392

Note: All statistically significant at 95 per cent level unless otherwise indicated by *. Omitted category is education to 15, Wales, 1978, Domestic Services. For All/Manual samples the base is unskilled. Shop workers are the base elsewhere. Regressions also include regional, occupational and industry groupings dummies as regressors, as well as yearly trend variables to account for natural wage movement and productivity changes.

Table 3(b): *OLS Estimates (Females)*

Dependent Variable LNWAGE	*All Females*	*Manual Females*	*Non-Manual Females*
CONSTANT	0.8194	0.8734	0.9624
EXP	0.0120	0.0094	0.0136
EXPSQ	–0.0002	–0.0002	–0.0003
POSTED	0.0342	0.0067*	0.0515
POSTEDSQ	–0.0012*	–0.0004*	–0.0009*
POSTEDCU	–0.0001	0.0000*	_0.0000*
POSTEDR	–0.0075	0.0118*	–0.0115
ADJ.R^2	0.4025	0.1176	0.3860
NUMBER OF OBSERVATIONS	28,205	10,825	17,380

Note: All statistically significant at 95 per cent level unless otherwise indicated by *. Omitted category is education to 15, Wales, 1978, Domestic Services. For All/Manual samples the base is unskilled. Shop workers are the base elsewhere. Regressions also include regional, occupational and industry groupings dummies as regressors, as well as yearly trend variables to account for natural wage movement and productivity changes.

III CONCLUSION

In this paper we have explored returns to education using UK micro data, based around the natural variation caused by the raising of the school-leaving age in the early '70s, following the "natural experiment" approach of Angrist and Krueger (1991, 1992).

The findings suggest that compulsory education has got a different impact on the earnings of males than elective schooling to the same level. The tentative suggestion is that for certain sub-samples the net effect of the ROSLA policy change may have been to shift their age-earnings profile such that the wage the 16 year old school-leaver makes pre- and post-ROSLA is different, even to the extent that for manual males the impact appears to make the 16 year old school leaver post-ROSLA actually worse off than his 15 year old equivalent prior to the policy change. For women the net effect seems to be in favour of enforced schooling, although statistically and intuitively the argument is much less convincing and for the manual female sample the return to education is insignificant. The clear implication is that in the eyes of the employer individuals who have obtained the minimum level of schooling, irrespective of whether that is until age 15 or 16, are broadly identical. The loss of experience in terms of "on the job" time may also play a rôle in consolidating the effect of compulsory schooling rules.

It would, of course, be misleading not to report other possible events. The detrimental effect of the "cohort size" argument (more entering the market in a particular period places downward pressure on wages) and the fact that the first batch from the ROSLA generation entered the labour market at a particularly depressed time (mid '70s) may have delivered a "double-whammy" effect which has persisted over a lengthy period. (Nickell, 1993). Ultimately, however, the question as to whether the compulsory schooling law is a beneficial ruling or not remains a broader question of social and private costs of school attendance.

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