Research Report

No 254

The Returns to Education

A Review of Evidence, Issues and Deficiencies in the Literature*

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* We are grateful to DfEE for funding this research and to the EU TSER programme for supporting our collaboration.

ISBN 1 84185 445 X
February 2001

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Executive Summary

Despite a well developed theoretical foundation, empirical work on the return to human capital, for example the effect of a year of schooling on the wage of an individual, has been the focus of considerable debate in the economics literature. A dominant feature of the literature that estimates human capital earnings relationships, is the implicit assumption that human capital is exogenous, and this has been the focus of recent research efforts.

Simple multivariate analyses of large UK datasets that contain information on earnings, education and characteristics, suggests a return to a year of schooling of between 7% and 9%. The basic specification assumes that (log) earnings are linear in education, so that each year of education adds the *same* percentage amount to earnings irrespective of the particular year of education. This may seem implausible but it has been difficult to find examples in the literature that conclusively prove that linearity is not a reasonable empirical approximation. There is limited evidence that some years of schooling carry 'sheepskin' effects – leaving school the year immediately following a credential awarding year for example may generate a lower return for that year generating a dip in the education/earning profile.

The returns to education seem to differ across the wage distribution. Our evidence points to returns being higher for those in the top deciles of the income distribution compared to those in the bottom deciles. Moreover this inequality may have increased in recent years. This finding has important implications for both education and tax and social security policy: the low return to investing in low ability individuals and the high return to investing in high ability individuals implies that educational investment should be skewed towards the high ability individuals. The resulting inequality may then be dealt with through redistributive tax and social security policy. Against this should be set the inefficiency associated with work disincentives induced by having a strongly redistributive tax and social security

Given the increase in the supply of educated workers in most OECD countries there is a concern that the skills workers bring to their job will exceed the skills required for the job: that is, the market for skilled workers does not clear. This will manifest itself in a lower return to schooling for the years of schooling in excess of those required for the employer. One of the main problems with this literature is the often poor definition of overeducation in available datasets, typically based on subjective measures given by the individual respondent. Where a more comprehensive definition is used based on job satisfaction the apparent negative effect of overeducation is eliminated when ability controls are included, but when overeducation appears to be genuine the penalty may be much larger than was first thought.

This has important implications for the variance in the quality of graduates produced by the higher education system. Firstly, a degree is not sufficient to ensure a graduate job – other complementary skills are expected by graduate employers. Secondly, since genuine overeducation can emerge it is clear that the labour market does not adjust fast enough. So a degree of manpower planning is required to ensure that particular types of graduate are not produced excessively.

It is possible that the return to education actually reflects the underlying ability that education *signals* – in other words education is a signal of inherent productivity of the individual rather than a means to enhance the productivity. Estimates presented here of the signalling component of the returns suggest that the effect is quite small.

Ideally the way we would wish to measure the return to schooling would be to compare the earnings of an individual with two different levels of schooling, but in practice only one level of education is observed for a particular individual. The literature has recently attempted to deal with this problem by finding ‘experiments’ in the economy that randomly assign groups of individuals to different levels of schooling. We can, for example, examine the wages of people who left school at 16 when the minimum school leaving age was raised to 16 compared to those that left school at 15 before the change in the minimum age legislation. This gives us a measure of the return to schooling for those that would not have chosen an extra year of schooling. The return to schooling from studies that use this methodology seem to be larger than those obtained using simple regression methods.

This simple idea can be embedded in a more sophisticated modelling procedure that can be used to deal with the endogeneity problem. The effect of this change in estimation procedure can be considerable. Average returns to schooling from simple regression methods are around 6% internationally but over 9% from these alternative methods. The UK appears to be at the higher end of the international range so, for the UK, the comparison is between 7 and 9% from simple methods to a range of 11% to 15% from the more sophisticated methods that attempt to control for the endogeneity of human capital. A concern about this methodology is that the higher returns found may reflect the return for the particular subgroup affected by the policy intervention. Thus, for example, changes in compulsory schooling laws may affect those individuals who place the least *value* on education – and as such estimates of the return to schooling based on these changes may be estimating the returns for that group.

Evidence on the net benefits to the economy, taking account of the increased earnings and the cost of providing education is limited in the UK, mainly to HE. The available evidence suggests that those net benefits are positive, but vary by degree subject with the highest return captured by medicine, non-biological sciences, social sciences and computing. Given the high return to education to the individual, unless there are benefits to society (social returns) over and above the private returns the argument for the taxpayer to provide extensive subsidies for education seem weak. Such benefits might include those with more education raising the productivity of those working along side them, and social cohesion benefits.

Direct cross-country macroeconomic evidence that links growth to education in confounded by the unclear nature of the causal relationship between average schooling levels and measures such as GNP growth. The microeconomic studies that are available confirm this and show how many of the important findings linking education to growth are based on restrictive functional form assumptions. What is needed to solve the issue of this wider impact of education on society is a parallel to the experimental approach adopted in the estimation of private returns. This suggests that within-country rather than between-country analysis may be the route to quantifying the externality from education.

The returns to education may be non-pecuniary. The link between job satisfaction and education is not heavily researched. Evidence presented here based on the BHPS data suggest that contrary to prior assumptions education may be negatively associated with job satisfaction due to the high aspirations that well-educated individuals may have for their careers. However this issue requires more attention than it has been given in the past.

Finally we present evidence on the effect of family background on education decisions. By exploiting the correlation between education and schooling-contingent parental income (child support from absent parents and Child Benefit) we find a large and statistically significant effect of income transfers to parents that increases the probability of staying in education past the age of 16 when a relatively parsimonious model is used. While the result is suggestive, we find that it becomes weaker when additional control variables are included in the model and a full evaluation of policies that provide schooling-contingent income would be required to provide the evidence on which to base policy.

Part A: Estimating the Private Return to Education

1. Introduction

This report is concerned with the returns to human capital. In particular we focus on education as a private decision to invest in "human capital" and we explore the "internal" rate of return to that private investment. Evidence that the private returns are disproportionately high relative to other investments with similar degrees of risk would suggest that there is some "market failure" that prevents individuals implementing their privately optimal plans. This may then provide a role for intervention. While the literature is replete with studies that estimate this rate of return using regression methods, where the estimated return is obtained as the coefficient on a years of education variable in a log wage equation that contains controls for work experience and other individual characteristics, the issue is surrounded with difficulties.

Here we explore conventional estimates from a variety of datasets and pay particular attention to a number of the most important difficulties. For example, it is unclear that one can give a productivity interpretation to the coefficient if education is a signal of pre-existing ability. Indeed, the coefficient on years of education may not reflect the effect of education on productivity if it is correlated with unobserved characteristics that are also correlated with wages. In this case, the education coefficient would reflect both the effect of education on productivity and the effect of the unobserved variable that is correlated with education. For example, "ability" (to progress in education) may be unobservable and may be correlated with the ability to make money in the labour market. Similarly, a high private "discount rate" would imply that the individual's privately optimal level of education would be low and, yet, such an unobservable characteristic conceivably may itself be positively correlated with high wages. Measurement error in the education variable can also lead to bias to the estimated coefficient – in this case, conventional estimation methods can suggest that the return to education is lower that is actually the case.

The signalling role of education may manifest itself in an effect of credentials on wages: there may be a pay premium associated with years of education that result in credentials being earned. This ought to manifest itself in a nonlinear relationship between (log) wages and years of education, and in there being a distribution of leaving education that is skewed away from years without credentials towards those years with credentials.

There may be other factors that affect the policy and economic interpretation of the statistical estimates: there may be "over"education where, because of labour market rigidities of some form, relative wages for different types of workers does not clear the markets for those types. For example, if the wage for highly educated workers is too high to clear the market, then this type of worker may take a job that requires only a lower level of skill and commands a lower wage. This overeducation would manifest itself as a lower estimate of the average return to education and ought to result, in the long run, in a decline in education levels. That is, if there is some factor that prevents relative wages to adjust then quantities will adjust instead. A related issue is the extent to which there is heterogeneity in the returns to education: returns may differ across individuals because they differ in the efficiency with which they can exploit education to raise their productivity. There may be individual-specific skills, for example social or analytical skills, which are complementary to formal education so that individuals with a large endowment of such skills reap a higher return to their investment in education than those with a low endowment. Thus, for example, some college graduates may not be well endowed with these complementary skills and may appear to be overeducated: in fact, they are simply less productive than other graduates in graduate jobs.

A topic that is much neglected in the existing literature is the non-pecuniary returns to education: education may yield both higher wages and change the non-pecuniary aspects of jobs. It is unclear, a priori, in which direction this would work: education may change preferences between the pecuniary and non-pecuniary elements of remuneration in either direction. However, education affects job satisfaction both directly and indirectly through its affect on wages, and here we investigate the extent to which education affects job satisfaction directly by controlling for wages.

Finally, we consider the “social” return to education, by which we mean the return to society over and above the private returns to individuals. Part of the private gross returns is given over to the government through taxation (and through reduced welfare entitlements). In addition to this tax wedge, the private return is indicative of whether the appropriate level of education is being provided, while the social return is suggestive of how that level should be funded. If there are significant social returns over and above the private returns there is then a case for providing a public subsidy to align private incentives with social optimality. This literature is less well developed than the research on private returns but features some of the same difficulties – in particular, measurement error in the education variable and simultaneity between (aggregate) education and GNP (aggregate income) – that cloud the interpretation of the estimated education coefficient.

2. The Human Capital Framework and the Returns to Schooling

2.1 A Brief Consideration of the Theory

The analysis of the demand for education has been driven by the concept of human capital approach and has been pioneered by Gary Becker, Jacob Mincer and Theodore Schultz. In human capital theory education is an investment of current resources (the opportunity cost of the time involved as well as any direct costs) in exchange for future returns. The benchmark model for the development of empirical estimation of the returns to education is the key relationship derived by Mincer (1974). The typical human capital theory (Becker (1964)) assumes that education, s, is chosen to maximise the expected present value of the stream of future incomes, up to retirement at date T, net of the costs of education, c_s. So, at the optimum s, the PV of the s^{th} year of schooling just equals the costs of the s^{th} year of education, so equilibrium is characterised by: $\sum_{t=1}^{T-s} \frac{w_s - w_{s-1}}{\left(1+r_s\right)^t} = w_{s-1} + c_s$ where r_s is called the internal rate of return (we are assuming that s is infinitely divisible, for simplicity, so "year" should not be interpreted literally). Optimal investment decision making would imply that one would invest in the s^{th} year of schooling if $r_s > i$, the market rate of interest. If T is large then the right hand side of the equilibrium expression can be approximated so that the equilibrium condition becomes $\frac{w_s - w_{s-1}}{r_s} = w_{s-1} + c_s$. Then, if c_s is sufficiently small, we can rearrange this expression to give $r_s \approx \frac{w_s - w_{s-1}}{w_s} \approx \log w_s - \log w_{s-1}$ (where $\approx$ means approximately equal to). This says that the return to the s^{th} year of schooling is approximately the difference in log wages between leaving at s and at s-1. Thus, one could estimate the returns to s by seeing how *log* wages varies with s[1].

Thus, the empirical approximation of the human capital theoretical framework is the familiar functional form of the earnings equation $\log w_i = \mathbf{X_i}\beta + rS_i + \delta x_i + \gamma x_i^2 + u_i$, where y_i is an earnings measure for an individual i such as earnings per hour or week, S_i represents a measure of their schooling, x_i is an experience measure (typically age-age left schooling), $\mathbf{X_i}$ is a set of other variables assumed to affect earnings, and u_i is a disturbance term representing other forces which may not be explicitly measured, assumed independent of $\mathbf{X_i}$ and s_i. Note that experience is included as a quadratic term to capture the concavity of the earnings profile. Mincer's derivation of the empirical model implies that, under the assumptions made (particularly no tuition costs), r can be considered the private financial return to schooling as well as being the proportionate effect on wages of an increment to S.

The availability of microdata and the ease of estimation has resulted in many studies, which essentially estimate the simple Mincer specification. In the original study Mincer (1974) used 1960 US Census data and used an experience measure known as potential

[1] In practice a number of further assumptions are typically made to give a specification that can be estimated simply. Mincer (1974) assumed that r_s is a constant - so $r = \Delta Y_t / h_t Y_t$, where Y_t is potential earnings and h_t is the proportion of period t spent acquiring human capital. During full-time education $h_t=1$ so $Y_s = Y_0 e^{rs}$. For post-school years, Mincer assumes that h_t declines linearly with experience, i.e $h_t = h_0 - (h_0/T)\ t$. So for x years of post-school work experience can be written as $Y_x = Y_s \exp\left(r\int_0^x h_t dt\right)$. Note that the rules of integration imply that $\int_0^x h_t dt = h_0 x - \frac{1}{2}\frac{h_0}{T}x^2$, and assuming that the Y_0 can be captured as a linear function of characteristics $\mathbf{X}$ we also have $Y_s = Y_0 e^{rs} = \mathbf{X}\beta e^{rs}$. Thus, we can write the expression for income after x years of experience and s years of schooling as $Y_x = Y_0 e^{rs} \exp r\left(h_0 x - \frac{h_0}{2T}x^2\right)$. Thus, taking logs, $\log Y_x = \log Y_0 + rs + rh_0 x - \left(\frac{rh_0}{2T}\right)x^2$ and, since actual earnings is $w_x = (1-h_x)Y_x$, we finally arrive at the conventional Mincer specification: $\log w_x = \mathbf{X}\beta + rs + rh_0 x - (rh_0/2T)x^2 + \log(1-h_x)$.

experience (i.e. current age *minus* age left full time schooling) and found that the returns to schooling were 10% with returns to experience of around 8%. Layard and Psacharopolous (1979) used the GB GHS 1972 data and found returns to schooling of a similar level, around 10% and see Willis (1986) and Psacharopolous (1994) for many more examples of this simple specification. In a few studies it has been applied to panel data. For example Lillard and Rosen (1978) attempt to estimate the extent to which the differentials in wages across individuals observed in a cross-section of data persist over time using the US Panel Study of Income Dynamics (PSID) for 1967-73. They estimate a standard earnings functions and show that the schooling and experience terms explain about 35% of the variance in log earnings across individuals, and about 44% of the average cross time of the log earnings of individuals. This suggests that most of the cross-section variance in earnings across individuals persists over time.

The Mincerian specification has been extended to address questions such as discrimination, effectiveness of training programmes, school quality, return to language skills, and even the return to "beauty" (see Hammermesh and Biddle (1994, 1998)).

Clearly in this empirical implementation the schooling measure is treated as exogenous, although education is clearly an endogenous choice variable in the underlying human capital theory. Moreover, in the Mincer specification the disturbance term captures unobservable individual effects and these individual factors may also influence the schooling decision, and induce a correlation between schooling and the error term in the earnings function. A common example is unobserved ability. This problem has been the preoccupation of the literature since the earliest contributions - if schooling is endogenous then estimation by least squares methods will yield biased estimates of the return to schooling.

There have been a number of approaches to deal with this problem. Firstly, measures of ability have been incorporated to proxy for unobserved effects. The inclusion of direct measures of ability should reduce the estimated education coefficient if it acts as a proxy for ability, so that the coefficient on education then captures the effect of education alone since ability is controlled for. Secondly one might exploit within-twins or within-siblings differences in wages and education if one were prepared to accept the assumption that unobserved effects are additive and common within twins so that they can be differenced out by regressing the wage difference within twins against the education difference. This approach is a modification of a more general fixed effect framework using individual panel data, where the unobserved individual effect is considered time-invariant. A final approach deals directly with the schooling/earnings relationship in a two-equation system by exploiting instrumental variables that affect S but not w. We return to these in detail later in this report.

2.2 Optimal Schooling Choices

It is useful at this point to consider the implications of endogenous schooling. As suggested above, the human capital framework, on which the original Mincer work was based, schooling is an optimizing investment decision based on future earnings and current costs: that is, on the (discounted) difference in earnings from undertaking and not undertaking education and the total cost of education including foregone earnings. Investment in education continues until the difference between the marginal cost and marginal return to education is zero.

A number of implications stem from considering schooling as an investment decision. Firstly, the internal rate of return (IRR, or r in this review) is the discount rate that equates the present value of benefits to the present value of costs. More specifically if the IRR is greater than market rate of interest more education is a worthwhile investment for the individual. In making an investment decision an individual who places more (less) value on current income than future income streams will have a higher (lower) value for the discount rates so individuals with high discount rates (high r_i) are therefore *less* likely to undertake education[2]. Secondly, direct education costs (c_s) lower the net benefits of schooling. Finally, if the probability of being in employment is higher if more schooling is undertaken then an increase in unemployment benefit would erode the reward from undertaking education. However, should the earnings gap between educated and non-educated individuals widen or if the income received while in schooling should rise (say, through a tuition subsidy or maintenance grant) the net effect on the incentive to invest in schooling should be positive.

A useful extension to the theory is to consider the role of the individual's ability on the schooling decision, whilst preserving the basic idea of schooling being an investment. Griliches (1977) introduces ability (*A*) explicitly into the derivation of the log-linear earnings function. In the basic model the IRR of schooling is partly determined by foregone income (less any subsidy such as parental contributions) and any educational costs. Introducing ability differences has two effects on this basic calculus. The more able individuals may be able to 'convert' schooling into human capital more efficiently[3] [4] than the less able, and this raises the IRR for the more able. One might think of this as inherent ability and education being complementary factors in producing human capital so that, for a given increment to schooling, a larger endowment of ability generates more human capital. On the other hand, the more able may have higher opportunity costs since they may have been able to earn more

[2] Thus the model implies that early schooling has a greater return than schooling later in life since there are fewer periods left to recoup the costs.

[3] Greater levels of ability may also increase the marginal cost of schooling by the positive effect of ability on earnings (and therefore foregone earnings while in education). The net effect can therefore be ambiguous.

[4] In the Griliches model there is a subtle extension often overlooked but highlighted by Card (1994). There can exist a negative relationship between optimal schooling and the disturbance term in the earnings function by assuming the presence of a second unmeasured factor (call this energy or motivation) that increases income and by association foregone earnings while at school, but is otherwise unrelated to schooling costs.

in the labour market, if ability to progress in school is positively correlated with the ability to earn, and this reduces the IRR

The empirical implications of this extension to the basic theory are most clearly outlined in Card (2000), which again embodies the usual idea that the optimal schooling level equates the marginal rate of return to additional schooling with the marginal cost of this additional schooling. However, Card (2000) allows the optimal schooling to vary across individuals for a further reason: not only can have different returns to schooling arise from variation in ability, so that those of higher ability 'gain' more from additional schooling, but individuals may also have different marginal rates of substitution between current and future earnings. That is, there may be some variation in the discount rate across individuals. This variation in discount rates may come for example from variation in access to funds or taste for schooling.

If ability levels are similar across individuals then the effects are relatively unambiguous - lower discount rate individuals choose more schooling. However, one might expect a negative correlation between these two elements: high-ability parents, who would typically be wealthier, will tend to be able to offer more to their children in terms of resources for education. Moreover highly educated parents will have stronger tastes for schooling (or lower discount rates) and their children may "inherit" some of this. Indeed, if ability is partly inherited then children with higher ability may be more likely than the average child to have lower discount rates. The reverse is true for children of lower ability parents. Empirically this modification allows for an expression for the potential bias in the least squares estimate of the return to schooling to be derived. This bias will be determined by the variance in ability relative to the variance in discount rates as well as the covariance between them. This "endogeneity" bias arises because people with higher marginal returns to education choose higher levels of schooling. If there is no discount rate variance then the endogeneity will arise solely from the correlation between ability and education and since this is likely to be positive the bias in OLS estimates will be upwards (if ability increases wages later in life more than it increases wages early in life). If there is no ability variance, then the endogeneity arises solely from the (negative) correlation between discount rates and OLS will be biased downwards if discount rates and wages are positively correlated (for example, if ambitious people earn higher wages and are more impatient). Thus, the direction of bias in OLS estimates of the returns to education is unclear and is, ultimately, an empirical question.

2.3 Returns to Schooling – Stylised Facts and Potential Issues

The Family Expenditure Survey (FES) is a random sample of approximately 7000 households each year and years of education is available for every year from 1978. Figure 2.1 shows the relationship between full-time school leaving age and the log real hourly wage (there are few observations above age 24 and below 15 or in the 19/20 "dip") for men and women aged 21-59 in Great Britain. Note that the relationship for men is distinctly flatter than for women. The Family Resources Survey (FRS) data is a random sample of approximately 25,000 households conducted every year from 1993/4 and Figure 2.2 shows the same relationship between wages and education in that data. Of course this work, by simply plotting the average wage for each schooling group, neglects the effect of any other control variables. In particular, one could only deduce the returns to education from this figure if it were true that other variables that affect wages were uncorrelated with schooling. This is unlikely to be true – in particular, older people in the sample are likely to have lower levels of education but more work experience. Figure 2.3 shows the average relationship between log wages and age for individuals with different levels of schooling (for GB men aged 21-59) in FES while Figure 2.4 shows the average relationships for men and women in FRS pooled for the three years 93/4-96/7. Note the characteristically flatter shape for those with lower levels of education and for women.

Figure 2.1 *Education and Wages*
GB Employed Men and Women aged 15-59 in FES 1978-1999.

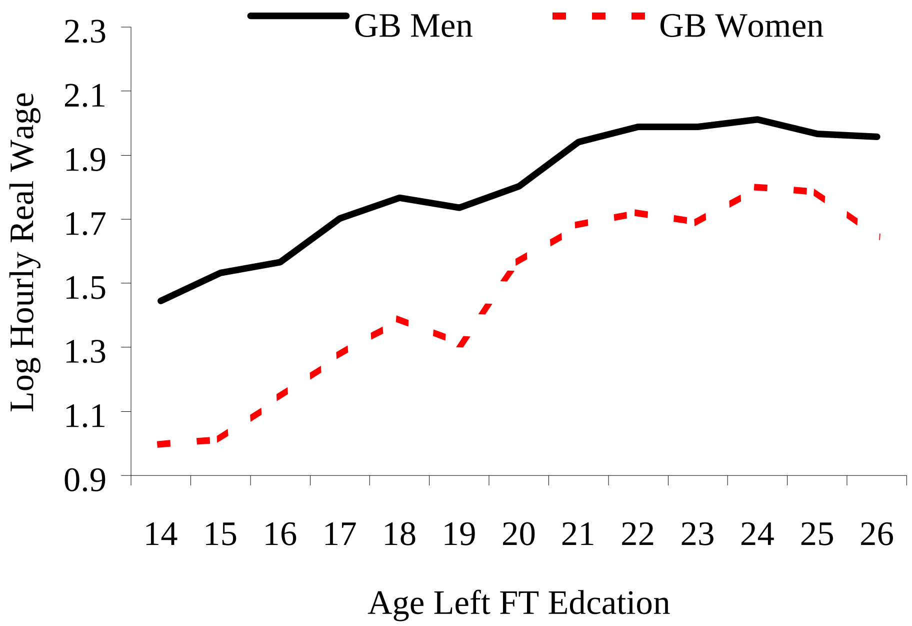


Figure 2.2 *Education and Wages*
GB Employed Men and Women aged 15-59 in FRS 1993/4-1996/7.

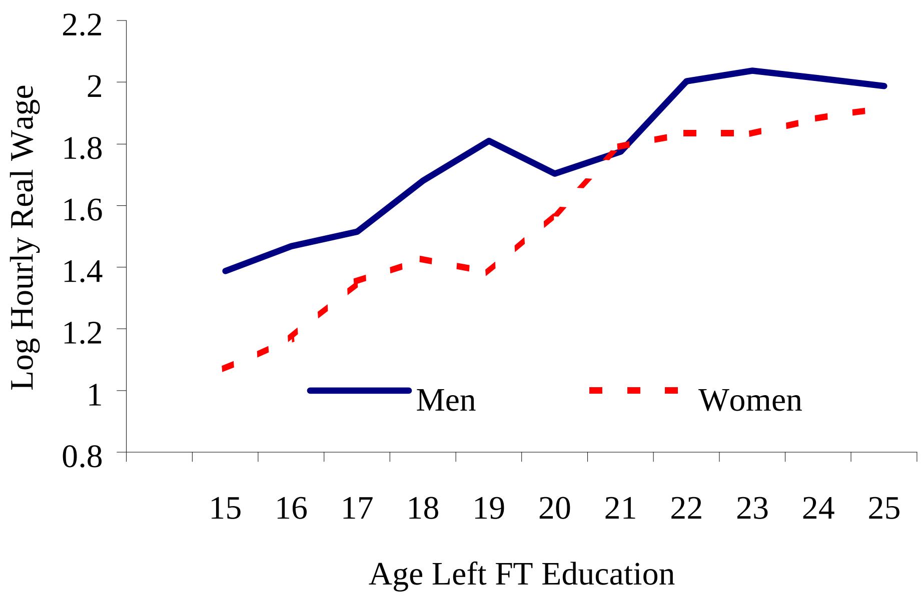


Figure 2.3 *Age and Wages by Age Left School*
GB Employed Men and Women aged 15-59 in FES 1978-1999.

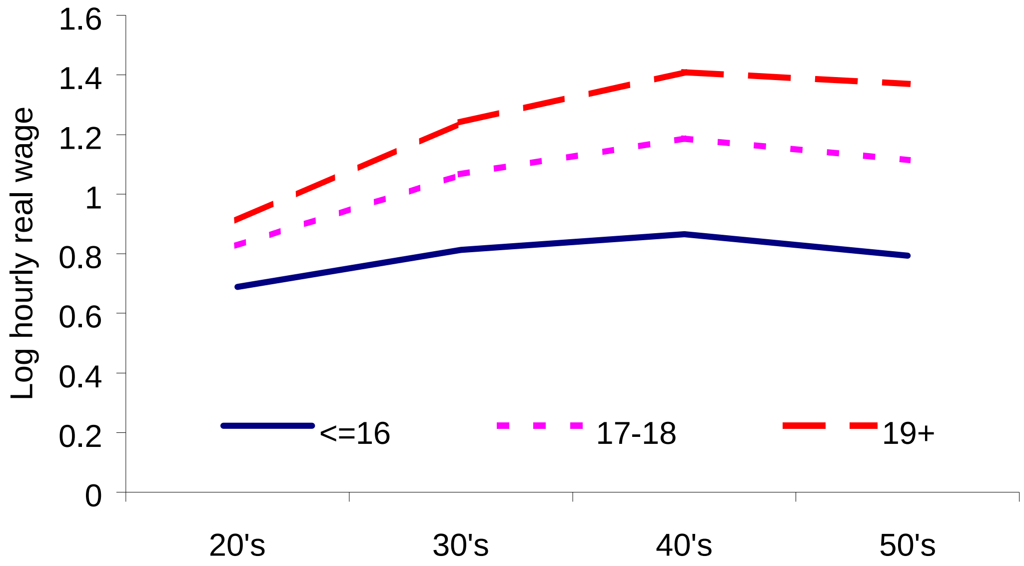
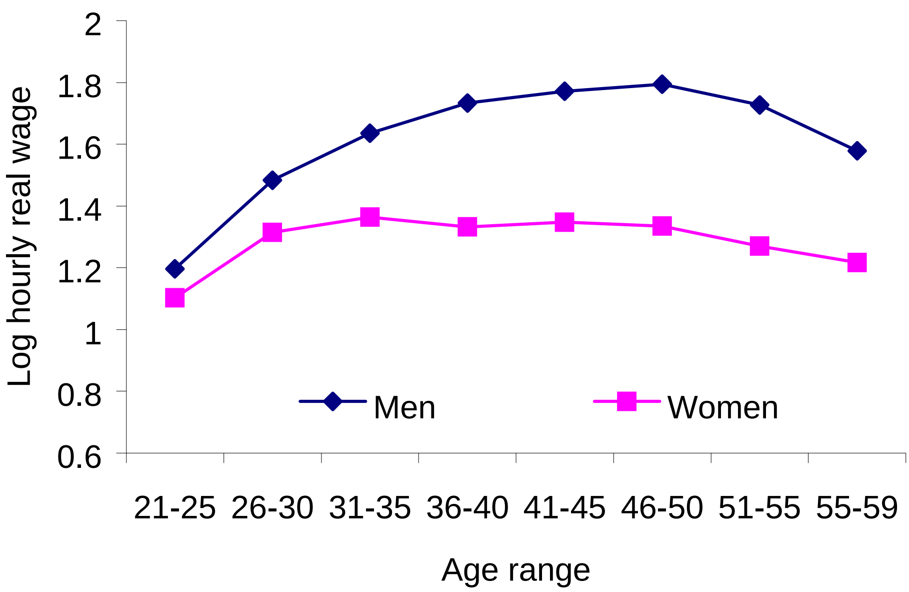


Figure 2.4 *Age and Wages*
GB Employed Men and Women aged 15-59 in FRS 1993/4-1996/7.



Again, one needs to be cautious about using these figures to deduce the returns to work experience because the older more experienced workers will typically have lower education levels.

2.4 Regression Analysis

Because wages are determined by a variety of variables, some of which will be correlated with each other, we need to use multivariate regression methods to derive meaningful estimates of the effect on wages of any one variable – in particular, of education.

We present results from a wide variety of datasets – both for within the UK and across countries. Thus, Table 2.1 presents estimates of the rate of return to education based on multivariate (OLS, or Ordinary Least Squares) analysis from the International Social Survey Programme (ISSP) data that are drawn together from national surveys that are designed to be consistent with each other. The British data in ISSP is taken from the British Social Attitudes Surveys. In Table 2.1 we apply exactly the same estimation methods to data that has been constructed to be closely comparable across countries. The results (standard errors are in italics) show that GB (and indeed Northern Ireland) has large returns relative to international standards.

These estimates have the advantage that they are all derived from common data that makes them exactly comparable. But they do so at the cost of simplicity. In particular, the estimated models contain controls only for age and union status – including further control variables would be likely to reduce the estimated schooling coefficient. Thus, it might be also interesting to consider cross-country rates of return derived from national surveys rather than a single consistent source such as like ISSP. Recent results from a pan-EU network of researchers (entitled Public Fund and Private Returns to Education (known as PURE)) do precisely this – derive estimates from national datasets in a way that exploits the strengths of each countries data. The main objective was to evaluate the private returns to education by estimating the relationship between wages and education across Europe. In a cross-country project it is preferable that data is reasonably comparable across countries, i.e. wage, years of

Table 2.1 Cross Country Evidence on the Returns to Schooling – ISSP 1995

	Male		Female	
Australia	0.0509	*0.0042*	0.0568	*0.0071*
West Germany	0.0353	*0.0020*	0.0441	*0.0036*
Great Britain	0.1299	*0.0057*	0.1466	*0.0069*
USA	0.0783	*0.0045*	0.0979	*0.0058*
Austria	0.0364	*0.0033*	0.0621	*0.0049*
Italy	0.0398	*0.0025*	0.0568	*0.0036*
Hungary	0.0699	*0.0053*	0.0716	*0.0051*
Switzerland	0.0427	*0.0065*	0.0523	*0.0143*
Poland	0.0737	*0.0044*	0.1025	*0.0046*
Netherlands	0.0331	*0.0025*	0.0181	*0.0050*
Rep of Ireland	0.1023	*0.0051*	0.1164	*0.0081*
Israel	0.0603	*0.0069*	0.0694	*0.0077*
Norway	0.0229	*0.0025*	0.0265	*0.0032*
N Ireland	0.1766	*0.0111*	0.1681	*0.0127*
East Germany	0.0265	*0.0032*	0.0450	*0.0041*
New Zealand	0.0424	*0.0050*	0.0375	*0.0058*
Russia	0.0421	*0.0042*	0.0555	*0.0043*
Slovenia	0.0892	*0.0104*	0.1121	*0.0091*
Sweden	0.0367	*0.0047*	0.0416	*0.0047*
Bulgaria	0.0495	*0.0100*	0.0624	*0.0091*
Canada	0.0367	*0.0072*	0.0498	*0.0083*
Czech Rep	0.0291	*0.0069*	0.0454	*0.0077*
Japan	0.0746	*0.0066*	0.0917	*0.0151*
Spain	0.0518	*0.0071*	0.0468	*0.0099*
Slovakia	0.0496	*0.0070*	0.0635	*0.0078*

Note: Standard Errors in italics.

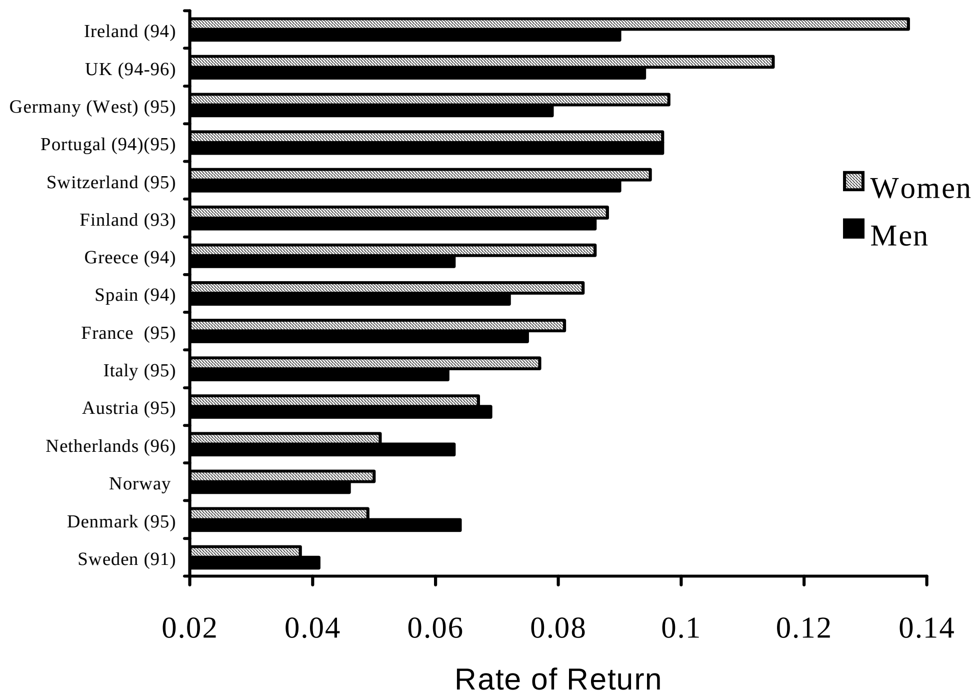
schooling and experience should be calculated in a similar fashion. However, since each country uses their own national surveys, this condition is hard to meet exactly. All PURE partners adopted a common specification and estimated the return to education using log of the hourly gross wage where available[5] [6]. Figure 2.5 is a summary of the returns broken down by gender. We find that for some countries like the UK, Ireland, Germany, Greece and Italy there is a substantial variation in returns between genders, - the returns to women are

[5] Austria, Netherlands, Greece, Spain, and Italy use net wages.

[6] Further details will be available in Harmon, Walker and Westergard-Nielsen (2001). An alternative to using ISSP or the 15 different datasets that lie behind Figure 2.5 is to use Eurostat's ECHP (European Community Household Panel). The advantage of ECHP is obviously that each variable has been specified the same way, regardless of the country. The disadvantage, however, is that ECHP is inferior to most of the register based datasets used in this study in terms of reliability (quality) and number of observations (quantity).

significantly higher than the returns to men. Scandinavia (Norway, Sweden, and Denmark) is characterized by relatively low returns. Again the UK is close to the top of the estimated returns in this cross-country review.

Figure 2.5 Returns to schooling in Europe, men and women (year closest to 1995)



2.5 Specification and Functional Form

Mincer's specification can be thought of as an approximation to a more general function of schooling (*S*) and experience (*x*) of the form: $\log w = F(S, x) + e$ where *e* is a random term that captures other (unobservable) determinants of wages. Many variants of the form of *F(.)* have been tried. Murphy and Welch (1990), for example, concluded that $\log w = \mathbf{X}\beta + rS + g(x) + e$ where **X** are individual observable characteristics that affects wages and g*(.)* was a 3rd or 4th order polynomial of the experience measure, provided the best approximation for the model. However, there are no examples in the empirical literature that suggest that the way in which *x* enters the model has any substantial impact on the estimated schooling coefficient,

However, experience is seldom well measured in typical datasets and is often proxied by age minus the age left education, or even just by age alone. Note that to compare the specification that uses age with one that uses recorded or potential experience one needs to adjust for the difference in what is being held constant: the effect of *S* on log wages - holding age constant is simply *r*, while the experience-control specification implies that the estimate of education on wages that hold age constant needs to be reduced by the effects of *S* on experience – that is, one needs to subtract the effect of a year of experience[7]. Table 2.2 illustrates the effect of including different experience measures in schooling returns estimation. Here we use the pooled FRS data and data from the British Household Panel Survey (BHPS) to show that the estimated return to education varies little with the experience measure in the context of a multivariate regression. In this table we report estimates based on ordinary least squares (OLS) techniques controlling for different definitions of experience where experience is introduced as a quadratic term as suggested by the Mincer specification.

Using a quadratic in age tends to produce the lowest returns of approximately 8% (6.5%) for men and 11% (10.3%) for women in FRS (BHPS). Using potential experience (age minus education leaving age) or actual experience (recorded in the data as the weighted sum of the number of years of part-time and full-time work since leaving full-time education) indicates a slightly higher return to education of, for example, 10% for men and 12% for

[7] If the wage equation is $\log w_i = \mathbf{X_i}\beta + rS_i + \delta x_i + \gamma x_i^2 + u_i$ then the adjustment is to subtract $\delta - 2\gamma\left(A-S\right)$. Since the average value of *A*-*S* is around 25, and (for men) δ is about 0.05 and γ is about –0.0005 the adjustment is small .

women in FRS. However, the sample sizes are large and the estimates are very precise so even these small differences are generally statistically significant[8].

Table 2.2 FRS and BHPS: Sensitivity of OLS Results to the Experience Measure

Definition of Experience:	Men **Education**	 **Experience**	 **Experience^2**	Women **Education**	 **Experience**	 **Experience^2**
FRS						
Age	**0.079**	0.089	-0.0010	**0.108**	0.023	-0.0003
	(0.001)	(0.003)	(0.00004)	**(0.002)**	(0.003)	(0.00004)
Potential	**0.094**	0.051	-0.0009	**0.115**	0.021	-0.0004
experience	**(0.001)**	(0.001)	(0.00003)	**(0.002)**	(0.001)	(0.00003)
Actual	**0.096**	0.051	-0.0009	**0.122**	0.042	-0.0007
experience	**(0.001)**	(0.001)	(0.00003)	**(0.001)**	(0.001)	(0.00004)
BHPS						
Age	**0.064**	0.076	-0.0008	**0.103**	0.040	-0.0005
	(0.002)	(0.005)	(0.00006)	**(0.002)**	(0.005)	(0.00006)
Potential	**0.076**	0.043	-0.0008	**0.106**	0.017	-0.0003
experience	**(0.002)**	(0.002)	(0.00005)	**(0.003)**	(0.002)	(0.00004)
Actual	**0.078**	0.043	-0.0008	**0.116**	0.031	-0.0006
experience	**(0.002)**	(0.002)	(0.00004)	**(0.002)**	(0.002)	(0.00005)

Note: Figures in parentheses are robust standard errors. The models include year dummies.

The UK is by no means an outlier in this respect. In Table 2.3 we illustrate this point using our European estimates of the returns to schooling. We estimate the simple model separately for men and women using potential experience, actual experience, and using age. Again using potential experience is generally associated with slightly higher returns than when using age. However returns when estimated in a model using potential experience are not significantly different from returns estimated using actual experience.

[8] The adjustment suggested in the previous footnote suggests that the age-constant estimates of the effect of a year of education are smaller than even these small raw differences suggest

Table 2.3 Returns to Education in Europe (year closest to 1995).

	MEN			WOMEN		
Definition of control for experience:	*Potential experience*	*Actual experience*	*Age*	*Potential experience*	*Actual experience*	*Age*
Austria (95)	0.069		0.059	0.067		0.058
Denmark (95)	0.064	0.061	0.056	0.049	0.043	0.044
Germany (West) (95)	0.079	0.077	0.067	0.098	0.095	0.087
Netherlands (96)	0.063	0.057	0.045	0.051	0.042	0.037
Portugal (94)(95)	0.097	0.100	0.079	0.097	0.104	0.077
Sweden (91)	0.041	0.041	0.033	0.038	0.037	0.033
France (95)	0.075		0.057	0.081		0.065
UK (94-96)	**0.094**	**0.096**	**0.079**	**0.115**	**0.122**	**0.108**
Ireland (94)	0.090	0.088	0.065	0.137	0.129	0.113
Italy (95)	0.062	0.058	0.046	0.077	0.070	0.061
Norway	0.046	0.045	0.037	0.050	0.047	0.044
Finland (93)	0.086	0.085	0.072	0.088	0.087	0.082
Spain (94)	0.072	0.069	0.055	0.084	0.079	0.063
Switzerland (95)	0.090	0.089	0.076	0.095	0.089	0.086
Greece (94)	0.063		0.040	0.086		0.064
Mean	**0.073**	**0.072**	**0.058**	**0.081**	**0.079**	**0.068**

Source: Information collected in the PuRE group by Rita Asplund (ETLA, Helsinki).

Other changes in specification generally do not lead to major changes in the estimated return to schooling. For example in Table 2.4 and 2.5 we estimate for men and women the return to schooling using BHPS including a range of different controls including union membership and plant size, part-time status, marital status and family size[9]. As can be seen the results here are very robust to these different range of controls.

[9] Controls for occupation were not included. Typically occupation controls result in the estimated return to education being reduced because the estimate is then conditional on occupation. Part, perhaps much, of the returns to education is due to being able to achieve higher occupational levels rather than affecting wages within an occupation.

Table 2.4 Men in BHPS: Sensitivity to Changes in Control Variables

	None	Plant size and union	Children and marriage	Part-time	Children marriage and PT	Plant size union, and PT	All controls
Education	**0.064**	**0.062**	**0.065**	**0.064**	**0.065**	**0.062**	**0.063**
	(0.002)	**(0.002)**	**(0.002)**	**(0.002)**	**(0.002)**	**(0.002)**	**(0.002)**
Medium Plant	-	0.157	-	-	-	0.157	0.153
		(0.012)				(0.012)	(0.012)
Large Plant	-	0.241	-	-	-	0.242	0.243
		(0.013)				(0.012)	(0.013)
Union member	-	0.079	-	-	-	0.079	0.080
		(0.011)				(0.011)	(0.011)
No. of children	-	-	0.017	-	0.017	-	0.019
			(0.006)		(0.006)		(0.005)
Married	-	-	0.144	-	0.145	-	0.144
			(0.016)		(0.016)		(0.016)
Co-habit	-	-	0.095	-	0.095	-	0.107
			(0.020)		(0.020)		(0.020)
Divorced	-	-	0.050	-	0.050	-	0.058
			(0.025)		(0.025)		(0.024)
Part-time	-	-	-	-0.020	-0.007	0.024	0.036
				(0.041)	(0.041)	(0.039)	(0.040)

Note: Figures in parentheses are robust standard errors. The models include age and age squared, year dummies, region dummies, and regional unemployment rates.

Table 2.5 Women in BHPS: Sensitivity to Changes in Control Variables

	None	Plant size and union	Children and marriage	Part-time	Children marriage and PT	Plant size union, and PT	All controls
Education	**0.103**	**0.095**	**0.101**	**0.097**	**0.097**	**0.092**	**0.092**
	(0.002)	**(0.002)**	**(0.002)**	**(0.002)**	**(0.002)**	**(0.002)**	**(0.002)**
Medium Plant	-	0.158	-	-	-	0.130	0.130
		(0.010)				(0.010)	(0.010)
Large Plant	-	0.258	-	-	-	0.217	0.216
		(0.012)				(0.012)	(0.012)
Union member	-	0.214	-	-	-	0.197	0.195
		(0.012)				(0.012)	(0.012)
No. of children	-	-	-0.077	-	-0.037	-	-0.032
			(0.006)		(0.006)		(0.006)
Married	-	-	0.001	-	0.029	-	0.025
			(0.018)		(0.018)		(0.018)
Co-habit	-	-	0.021	-	0.024	-	0.025
			(0.022)		(0.022)		(0.021)
Divorced	-	-	-0.009	-	-0.002	-	0.003
			(0.023)		(0.022)		(0.021)
Part-time	-	-	-	-0.220	-0.197	-0.165	-0.156
				(0.009)	(0.011)	(0.009)	(0.010)

Note: Figures in parentheses are robust standard errors. The models include age and age squared, year dummies, region dummies, and regional unemployment rates.

A further point relates to the issue of using samples of working employees for the purposes of estimating these returns. To what extent is the return to schooling biased by estimation being based only on these workers? This has typically thought not to be such an issue for men as for women since voluntary non-participation is thought to be much less common for men than women. There are two ways of illuminating the extent to which the estimated education return may be affected by this sample selection. One might compare OLS estimates with estimates of "median" regressions. Bias in OLS arises because individuals with low productivity tend to predominate amongst non-participants. Thus, using a selected sample of workers is to truncate the bottom of the wage distribution and hence raise the mean of the distribution over what it would otherwise be if no selection took place. Since OLS passes through the mean of the estimating sample it will be affected by the truncation in the data. However, the median of the data is unaffected by the truncation so there should be no bias in median regressions. Secondly, one could also use standard "two-step" estimation methods as proposed by Heckman et al. (1974), which attempt to control for the selection by modelling what determines it.

Table 2.6 BHPS and FRS: OLS, Heckman Selection, and Median Regression

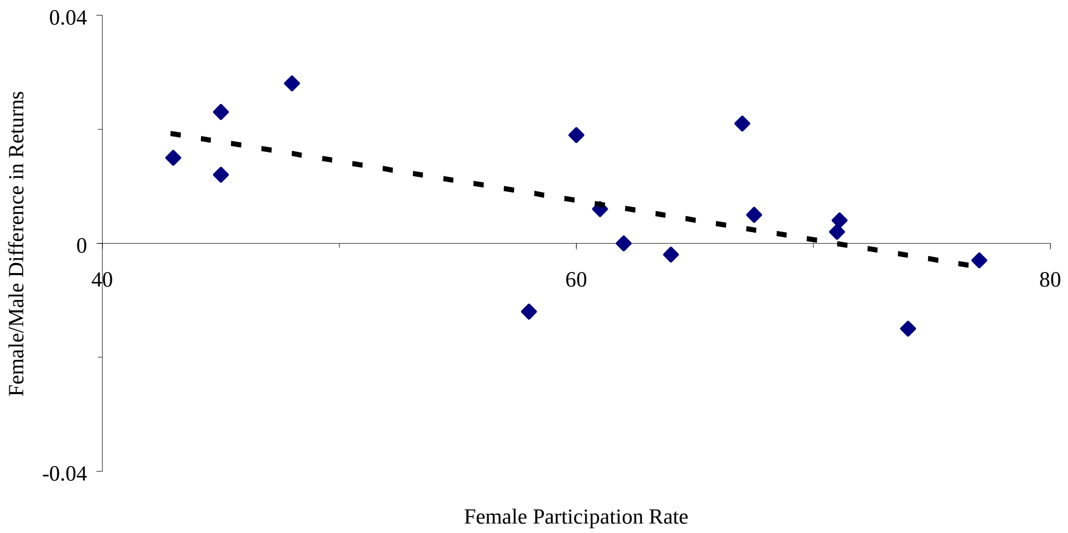
	FRS Women			BHPS Women		
	Education	Age	Age^2	Education	Age	Age^2
OLS	**0.109**	0.026	-0.0003	**0.103**	0.040	-0.0005
	(0.002)	(0.003)	(0.00004)	**(0.002)**	(0.005)	(0.0001)
Heckman	**0.109**	0.016	-0.0001	**0.102**	0.060	-0.0007
two-step	**(0.002)**	(0.004)	(0.0001)	**(0.003)**	(0.006)	(0.0001)
Median	**0.122**	0.024	-0.0003	**0.118**	0.034	-0.0003
regression	**(0.002)**	(0.004)	(0.00004)	**(0.002)**	(0.005)	(0.0001)

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 rate. In the Heckman two-step case we use household unearned income as well as the variables from the wage equation in the participation equation.

Table 2.6 shows the parameter estimates for women using BHPS and FRS. The results show slightly higher returns under the median regression method suggesting a small effect due to the selection into employment. While statistically significant the differences are small in absolute value.

Since non-participation is more common amongst women than men we might imagine that the returns to women would be biased downwards relative to men and the size of this bias may depend on the relative participation rates. Figure 2.6 examines the relationship between the average participation rate for women in employment and the percentage difference between male and female returns to schooling for the countries in the PURE network. The figure shows that countries with the highest rates of female participation (typically the Nordic grouping) have the lowest differences in schooling returns[10] while the countries with the lowest participation (including Ireland and the UK) have the largest. This suggests that there is some bias from using samples of participants alone but it appears not to be large. However, the issue merits more attention than it has received in the literature to date.

Figure 2.6 Female/Male Differentials in Returns and Female Participation Rate



[10] We are grateful to Jens Jakob Christensen for assistance in compiling the data for this figure.

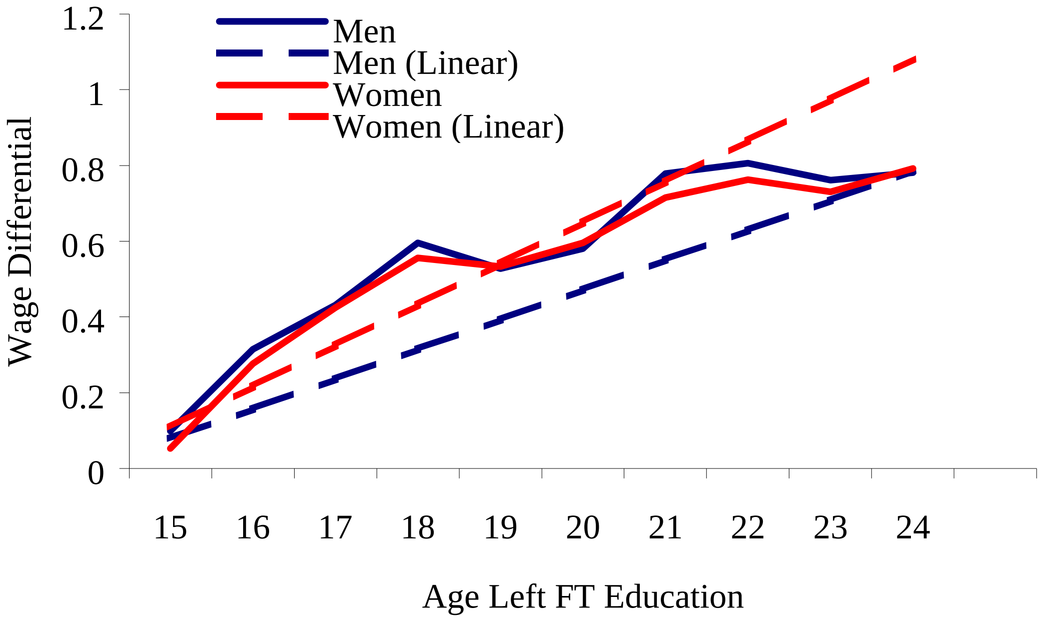
2.6 *Alternative Measures of Schooling Attainment*

Measuring schooling in terms of years of education has a long history in the US. There are practical reasons for this as years of schooling is the measure recorded in the major datasets such as the Census and, pre 1990, the Current Population Survey (CPS). Moreover schooling in the US does not follow a nationally (or state) based credential system but is one where grades generally follow years, so education is a fairly continuous variable at least up to high school graduation. However in Europe there are alternative streams that may lead to the quite different credentials as outcomes. Estimation based on credentials rather than years of schooling is therefore an alternative structure for recovering the returns to schooling. However this is only necessary if the wage return from increments of education deviates from linearity in years of education. Consider a comparison of two measures of the returns to schooling; one based on years of schooling and another based on dummy variables for the highest level of schooling completed. If the extra (or marginal) return to a three year degree programme compared to leaving school with A-levels is approximately three times the estimated return to a year of A-level schooling then the linear specification in years of schooling is equivalent to the alternative based on the credential.

Some argue that credentials matter more than years of schooling – the so-called “sheepskin” effect. For example there may be a wage premium over the average return to schooling for fulfilling a particular year of education (such as the final year of college, or high school). Hungerford and Solon (1987) demonstrate the existence of these nonlinearities. Park (1999) also notes a deviation from linearity in the returns to years of schooling between the completion of high school and the completion of college/university. His estimates suggest that the marginal return to schooling is not constant but rather ‘dips’ between these two important transition points.

Figure 2.7 illustrates this point using the FRS data for 1994 to 1997. The dashed lines represent the estimated return to years of schooling based on an OLS specification. The solid lines plot the return to schooling based on the inclusion of dummy variables for each corresponding leaving age. There appear to be clear effects at age 18 and age 21 most likely corresponding to the completion of A-levels and the completion of university.

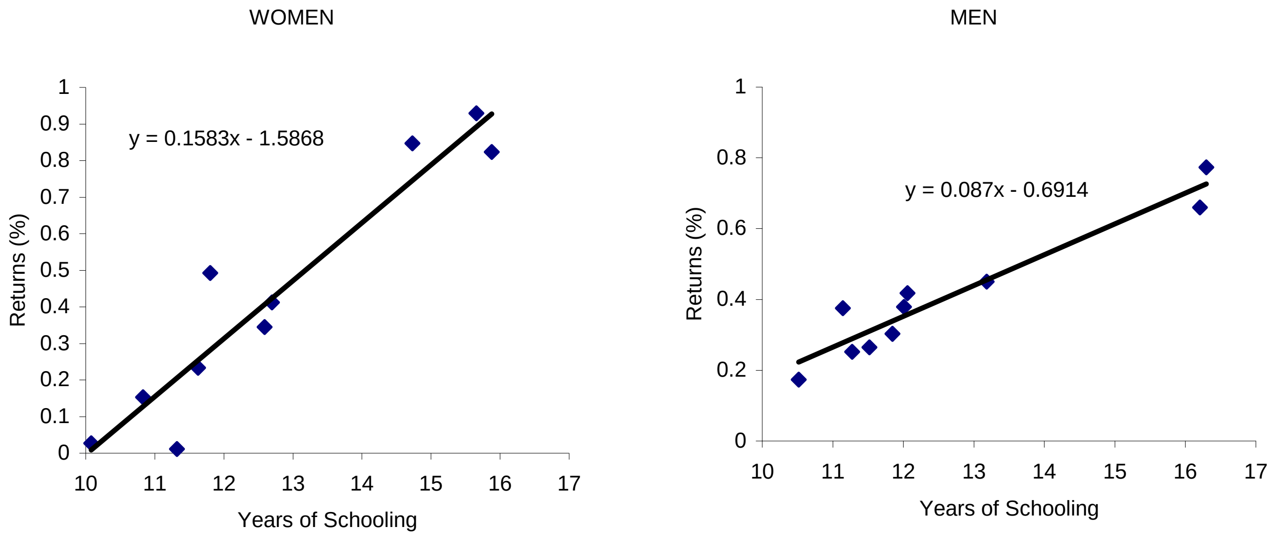
Figure 2.7 Sheepskin Effects in FRS 94-97



However, Figure 2.8 illustrates how the underlying assumption of linearity, while a strong assumption, is nonetheless remarkably hard to reject. In this figure we plot the average return for a number of popular credentials in the UK data (including apprenticeships, national vocational qualifications and other forms of education) against the average number of years of schooling for holders of these credentials. From fitting a simple regression through these points we see that a linear form seems to be a reasonable approximation so that the average returns to a year of schooling is about 16% for women and 9% for men.

An important question is how to allow for the affect of vocational qualifications obtained outside of formal schooling – for example, qualifications obtained while in full-time work. Nursing qualifications are an example. The age left full-time schooling is not a good guide to the human capital of such individuals and, to the extent that such vocational qualifications add to productivity and wages, this will induce our estimated return to education to be biased downwards.

Figure 2.8 Estimated Returns to Qualifications – BHPS

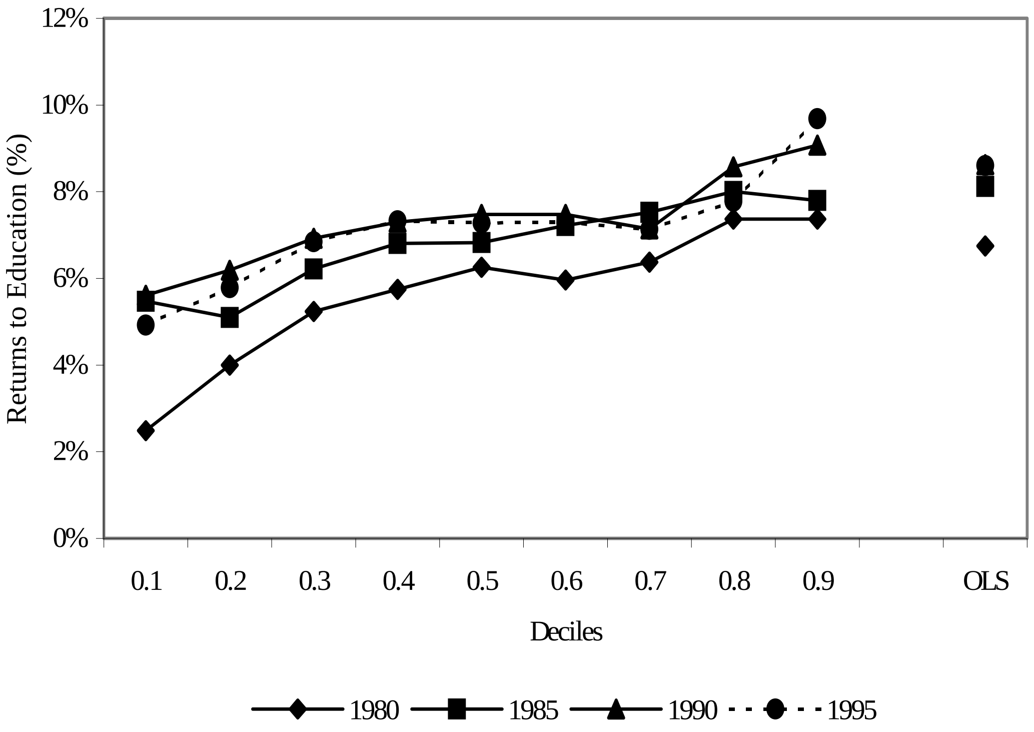


2.7 Variation in the Returns to Education across the Wage Distribution

It is possible that the returns to schooling may be different for individuals in the upper part of the wage distribution as compared to individuals in the lower portion of the wage distribution. One of the properties of OLS estimation is that the regression line contains or passes through the mean of the sample. An alternative methodology to OLS is available known as quantile regression (QR) which, based on the entire sample available, allows us to estimate the return to education within different quantiles of the wage distribution. While OLS captures the effect of education on someone on the mean wage, the idea behind QR is to look at the returns at some other part of the wage distribution, say the bottom quartile. Then comparing the estimated returns across the whole of the wage distribution we can infer the extent to which education exacerbates or reduces underlying inequality. Of course, the method requires that there is a sufficiently wide spread of education that we can identify the returns for each decile – we require that some in the top deciles have low education and some in the bottom deciles have high education. The UK data appears to be satisfactory in this respect and we find that the return is statistically significant for each decile, and we also find that the top decile is significantly higher than the bottom decile. The method is fully flexible and allows the returns in each decile to be independent of any other decile. Our simple specification does restrict the returns to be the same for everyone within the decile group – just as our OLS linear specification restricts the returns to be the same for the whole sample.

Figure 2.9 presents the average OLS return to schooling (from FES data for 1980, 1985, 1990 and 1995) together with the returns to schooling in different deciles of the wage distribution. The OLS figures show that over the four half-decades the returns to schooling, on average, have broadly increased, especially between 1980 and 1985. There is a clear implication in this figure that the returns to schooling are higher for those at the very top of the wage distribution compared to those at the very bottom (although the profiles are flat across the middle range of the wage distribution). The returns at the bottom of the distribution seem to have risen across this period which is shown by the graph getting flatter, and there is some suggestion, comparing the 1980's with the 1990's, that the returns have risen at the top of the distribution. One factor behind the distribution of wages is the distribution of inherent ability so that lower ability individuals predominate in the bottom half of the distribution. Thus education appears to have a bigger impact on the more able than the less able and this complimentarity between ability and education seems to have become larger over time.

Figure 2.9 Quantile Regressions for GB: FES Men



Source: Harmon, Walker and Westergard-Nielsen (2001)

Table 2.7 Quantile Regressions

	Year	1st dec.	9th dec.	OLS	Year	1st dec.	9th dec.	OLS
Austria	1981	9.2	12.6	10.5	1993	7.2	12.8	9.7
Denmark	1980	4.7	5.3	4.6	1995	6.3	7.1	6.6
Finland	1987	7.3	10.3	9.5	1993	6.8	10.1	8.9
France	1977	5.6	9.8	7.5	1993	5.9	9.3	7.6
Germany	1984	9.4	8.4		1995	8.5	7.5	
Greece	1974	6.5	5.4	5.8	1994	7.5	5.6	6.5
Italy	1980	3.9	4.6	4.3	1995	6.7	7.1	6.4
Ireland	1987	10.1	10.4	10.2	1994	7.8	10.4	8.9
Netherlands	1979	6.5	9.2	8.6	1996	5.3	8.3	7.0
Norway	1983	5.3	6.3	5.7	1995	5.5	7.5	6.0
Portugal	1982	8.7	12.4	11.0	1995	6.7	15.6	12.6
Spain	1990	6.4	8.3	7.2	1995	6.7	9.1	8.6
Sweden	1981	3.2	6.6	4.7	1991	2.4	6.2	4.1
Switzerland	1992	8.2	10.7	9.6	1998	6.3	10.2	9.0
UK	**1980**	**2.5**	**7.4**	**6.7**	**1995**	**4.9**	**9.7**	**8.6**

Table 2.7 is also based on the work of the PURE[11] research group. In most countries and for most years it would seem that there is complementarity between education and ability and that this is either getting stronger or, at least, no weaker over time.

2.8 Summary of the Results

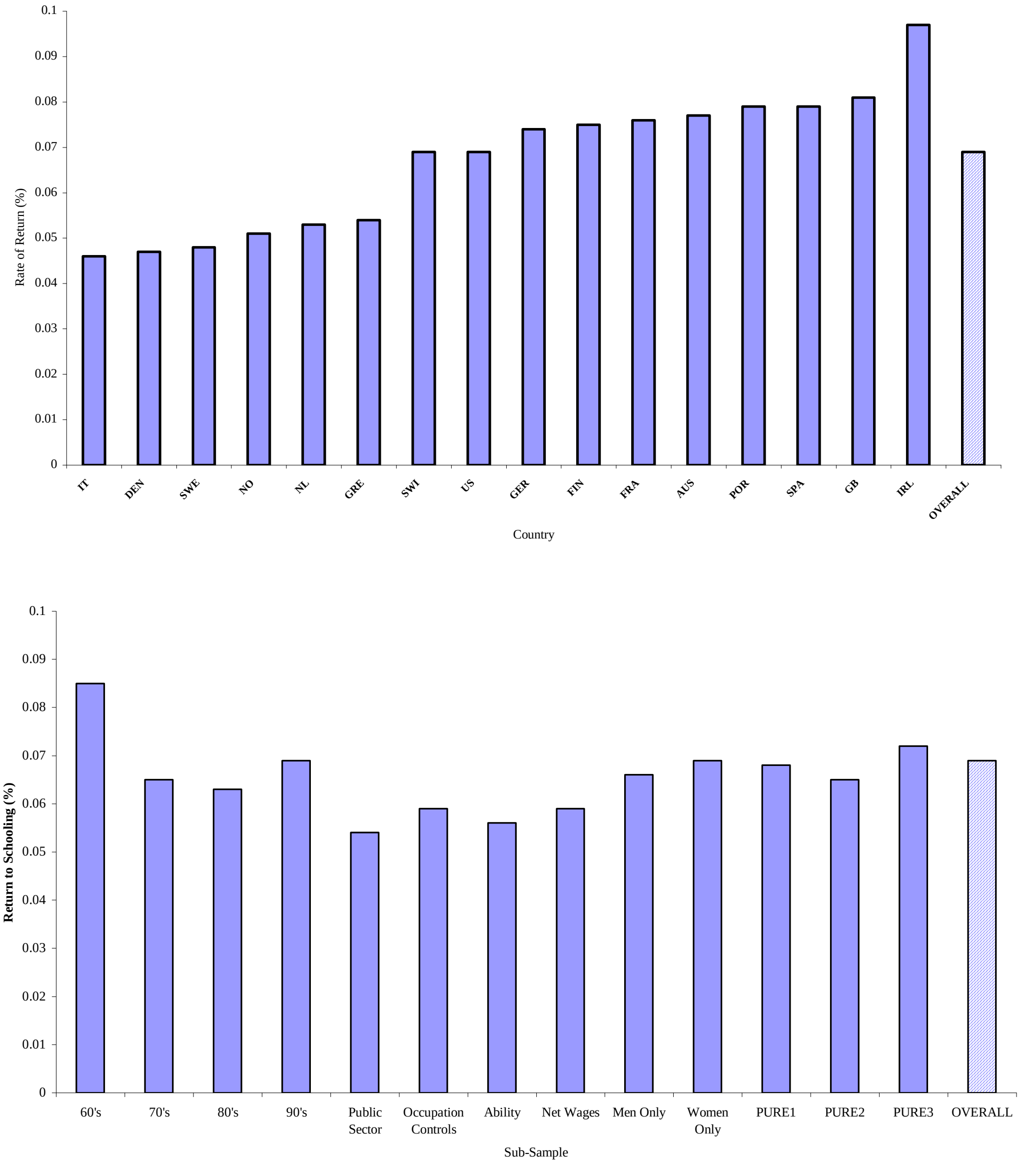
To summarize the various issues discussed above we use the methods common in meta-analysis to provide some structure to our survey of returns to schooling and to provide a framework to determine whether our inferences are sensitive to specification choices. A meta-analysis combines and integrates the results of several studies that share a common aspect so as to be 'combinable' in a statistical manner. The methodology is typical in the clinical trials in the medical literature. In its simplest form the computation of the average return across a number of studies is now achieved by weighting the contribution of an individual study to the average on the basis of the standard error of the estimate (see Ashenfelter, Harmon and Oosterbeek (1999) for further details).

In Figure 2.10 we present the findings of a simple meta-analysis based on the collected OLS estimated rates of return to schooling from the PURE project supplemented by a number of findings for the US. Well over 1000 estimates were generated across the PURE project on three main types of estimated return to schooling - existing published work (labelled PURE1 in the figure), existing unpublished work (labelled PURE2), and new estimates produced for the PURE project (labelled PURE3). Each block refers to a different sample of studies that share some characteristic (for example, “US” indicates only studies

[11] We are grateful to Pedro Pereira and Pedro Silva Martins for providing this information.

based on US originated studies, “Net wages” indicates that the dependent variable was net rather than gross ages, and “Ability” indicates that ability controls were included).

Figure 2.10 Returns to Schooling – A Meta Analysis



A number of points emerge from the figure. Despite the points raised earlier in this chapter there is a remarkable similarity in the estimated return to schooling for a number of possible cuts of the data with an average return of around 6.5% across the majority of countries and model specifications. There are number of notable exceptions. That Nordic countries generally have lower returns to schooling is confirmed while at the other extreme the returns for the UK and Ireland are indeed higher than average. In addition estimated returns from studies of public sector workers, and from studies where net (of tax) wages are only available average about 5%[12]. Estimates produced using samples from the 1960's also seem to have produced higher than average returns.

2.9 Other Sources of Variation in Returns: Quality vs. Quantity

Numerous studies have considered the link between school-quality and earnings. Our concern here is, not so much with whether school quality "matters", but that high levels of schooling may be associated with a high quality school experience. Thus, the *S* coefficient in traditional wage regressions may pick up both the effect of quantity and of quality, as this would imply an upward bias to the estimated education **quantity** effect.

Studies such as Johnson and Stafford (1973) defined the research agenda in this area suggesting strong quality/earnings links where controls for quality using state-level data matched to microdata were included. On the other hand, the effects of school quality on outcomes such as educational attainment and earnings have been studied extensively by Hanushek (see his meta-study in 1992) that has been interpreted as implying that "quality doesn't matter". However more recently a new consensus has emerged, as expressed in Betts (1996), that the early studies may have overstated the quality effect, and note that the quality effect may not be stable over time. The more recent contributions in the literature, as reviewed in Card and Krueger (1996), suggest small effects of school inputs on wages. In particular, there is some evidence from "natural experiments" as well as real experiments now available that suggest there is some effect of quality. Card and Krueger (1997) exploit

[12] Note that we would expect the net returns to be lower than gross by an amount approximately equal to the average tax rate.

the differential changes in class sizes that occurred over time between North and South Carolina. Angrist and Lavy (1997) exploit the rule in Israeli schools that an extra teacher must be added every time the class size exceeds 40. Harmon and Walker (2000) exploit the changes in school selection rules that were changed at different times in different areas in the UK in the 1960/70's on wages and find significant effects (but not of class size at secondary level). Finally, the STAR experiment in Tennessee randomly allocated children to either smaller classes or to classes with a teacher assistant and compared the outcomes with a control group. In each of these cases there were statistically significant quality effects, although usually the size of the effects were small casting doubt on the economic effectiveness of quality investments.

Thus, it seems likely that our estimates of the returns to the quantity of schooling are not greatly affected by the omission of quality. Indeed, in Harmon and Walker (2000) the effects of quantity seemed quite robust to the inclusion of quality controls.

2.10 Other Sources of Variation in Returns: Over-Education

Given the increase in the supply of educated workers in most OECD countries in the last two decades a concern has arisen in the schooling returns literature that if growth in the supply of educated workers outpaces the demand for these workers, overeducation in the workforce is the likely result. In other words the skills workers will bring to their work will exceed the skills required for the job. Mason (1996) suggests that 45% of UK graduates are in 'non mainstream' graduate jobs. The manifestation of this for the worker is a lower return to years of education that are somehow 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. The difference in the returns to these measures is a measure of overeducation.

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. Clearly these may be open to measurement error. Moreover the educational requirement for new workers may exceed those of older workers in a given firm. 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 for the choice of classification for the occupation, which, depending on the industry SIC digit level chosen 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.

Groot and Maassen van den Brink (2000) show the often conflicting results from this literature based on a meta-analysis of the returns to education and overeducation literature (some 50 studies in total). A total of 26% of studies show evidence that a statistically significant difference in the returns to required years and surplus years exists. The meta regression analysis found that when over-education is defined by comparison with the average years of schooling within occupation categories the incidence of overeducation falls. The average return to required years of education is 7.9% but this rises when more recent data is used or when required education is defined by self-reported methods. The average return to over-education or surplus years in excess of the requirement for the job is 2.6%.

Dolton and Vignoles (2000) test three hypotheses regarding overeducation for the UK graduate labour market based on the National Survey of 1980 Graduates and Diplomates which asks the respondents what the minimum requirement for the position currently held was. The first test, that the return to surplus years of education is the same as the return to required years of education, is conclusively rejected by the data. New graduates that were overeducated earned considerably less than those in graduate jobs with the penalty greatest in jobs with the lowest required qualifications. The penalty was also higher for women. The second test is that the return to surplus education differs by degree class. This is rejected – those who are overeducated with first or upper second-class degrees earn the same as those overeducated with a lower class of degree. Their final test is that the returns to surplus education differ between sectors, specifically between the public and private sectors, and again this is rejected. Dolton and Vignoles (2000) conclude therefore that the return to surplus education based on their measure is lower than for required education and that this cannot be explained by difference in degree class or differences in employment sector.

Chevalier (2000) deals directly with the definition of overeducation by noting that graduates with similar qualifications are not homogeneous in their endowment of skills leading to a variation in ability, which may lead to an over-estimation of the extent and effect of over-education 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 30 higher education institutions covering the range of UK institutions. Graduates from the 1985 and 1990 cohorts were selected, leading to a sample of 18,000 individuals. By using measures of job satisfaction this study is able to sub-divide those considered 'over-educated' into 'apparently' and 'genuinely' over-educated. 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 but much of what is normally defined as over-education is more apparent than real.

3. Signalling

An important issue to address is the extent to which the estimates of returns to education reflect not just the productivity enhancing effect of education but an effect on earnings of the underlying ability that education *signals*. This idea stems from work by Spence (1970). There is a fundamental difficulty in unravelling the extent to which education is a signal of existing productivity as opposed to enhancing productivity: both theories are observationally equivalent – they both suggest that there is a positive correlation between earnings and education, but for very different reasons.

There are three approaches to finessing this problem. One would attempt to control for ability and see if education still has as strong an effect on earnings – any difference could be attributed to the signalling value of education. A variation on this approach would be to estimate the education/earnings relationship for the self-employed, where education has no value as a signal since individuals know their own productivity and have no need to signal it to themselves by acquiring more education, or for public sector employees which is less competitive and hence can afford to have pay differ from productivity. Thus the difference between the returns to education for employees *vs.* the self-employed or between public vs private sector employees is the value of education as a signal. A second approach would be to compare estimated returns which control for ability with those that do not. Since education is correlated with wages fro both human capital and because it is a signal of ability then including ability controls should account for the latter effect and then the education variable just picks up the effect via human capital. DfEE's evidence in the Dearing Report referred to the discount for the signalling value of education as α and thought that the observed returns to education ought to be discounted by an α-factor of between 0 and 0.4. However there is precious little evidence available in the literature and the paucity of the literature is testament to the difficulty of the problem.

In Table 3.1 British Household Panel Survey data, which contains information on whether, one's parents were self-employed and on housing equity both of which are likely to be associated with self-employment (but are not likely to be very well correlated with current wages). The results here suggest quite comparable rates of return and imply that the signalling component is quite small. The main problem with the self-employed/employee distinction is that self-employment is not random - individuals with specific (and typically unobservable) characteristics choose to be self-employed). Thus, the bottom half of the table show the effects of education on wages when we use the Heckman two-step method to control for unobservable differences between employees and the self-employed. The results are essentially unchanged.

Table 3.1 Signalling – Returns for Employed vs. Self-Employed - BHPS

	Employees		**Self-employed**		
	Return	**N**	**Return**	**N**	**Signalling value**
BHPS – OLS					
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)
BHPS - Heckman					
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.

The second approach to distinguishing between ability and productivity is to directly include ability measures. 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. Moreover, the ability measures need to indicate ability to make money rather than ability in an IQ sense. It seems unlikely that any ability measure would be able to satisfy both of these requirements exactly and we pursue the issue here with two specialised datasets. The GB National Child Development Survey (NCDS) is a cohort study of all individuals born in GB in a particular week in 1958 whose early development was followed closely and whose subsequent careers have been recorded including earnings. Various ability tests were conducted at the ages of 7, 11 and 16. The International Adult Literacy Survey (IALS) datasets record earnings and ability at the time of interview. In the IALS data the literacy level is measured on three scales: prose, document and quantitative, taken at the age the respondent is when surveyed. Prose literacy is the knowledge required to understand and use information from texts, such as newspapers, pamphlets and magazines. Document literacy is the knowledge and skill needed to use information from specific formats, for example from maps, timetables and payroll forms. Quantitative literacy is defined as the ability to use mathematical operations, such as in calculating a tip or compound interest.

In order to provide an actual measure of literacy each individual was given a score for each task, which varied depending on the difficulty of the assignment. Scores for each scale ranges from 0-500, which is subsequently subdivided into five levels. Level 1 has a score range from 0-225 and would indicate very low levels where, for example, instructions for a medicine prescription would not be understood. The interval 226-275 defines Level 2 where individuals are limited to handling material that is not too complex and clearly defined. Level 3 ranges from 276-325 and is considered the minimum desirable threshold for most countries while Level 4 (326-375) and Level 5 (376-500) show increasingly higher skills which integrate several sources of information or solve complex problems[13].

In Table 3.2 we provide estimates from NCDS and IALS data that control for a variety of ability variables. In NCDS, 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. We compare these results with using controls at age 11 and at age 16, and current age using

[13] See Dearden *et al* (2000) for further detailed analysis of these datasets.

IALS. As we expect, using ability controls at later ages confounds the effects of education on ability scores and the apparent bias appears to be larger. 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 is a rather small difference and suggests little signalling value to education. In the IALS work, following Cawley *et al.* (1996) we compute a combination of tests as a measure of functional literacy/cognitive ability[14]. We measure ability by taking the first principal component from the three test score vectors (for document literacy, prose literacy and quantitative literacy). The principal component is a linear combination of the underlying variables which captures as much of the variation in the variables as possible.

Table 3.2 Returns to Schooling by Gender in NCDS and IALS: Ability Controls

		Without ability controls	With ability controls
NCDS - GB	Women	0.107 (0.007)	0.100 (0.008)
Controls at age 7	Men	0.061 (0.006)	0.051 (0.006)
NCDS - GB	Women	0.107 (0.007)	0.081 (0.009)
Controls at age 11	Men	0.061 (0.006)	0.036 (0.007)
NCDS - GB	Women	0.107 (0.007)	0.071 (0.009)
Controls at age 16	Men	0.061 (0.006)	0.026 (0.007)
IALS – GB	Women	0.106 (0.014)	0.077 (0.013)
Current age controls	Men	0.089 (0.009)	0.057 (0.009)

Note: Standard errors 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; while in IALS they are the residual formed by regressing current age ability measures against schooling and age to purge these effects.

In Table 3.3 and 3.4 we look in more detail for (age 7) ability effects in NCDS by including interactions between ability measures and education[15]. Each specification includes years of education, and the first specification (column 1) excludes parental controls for education. Specification 3 adds test score results to measure ability effects, while

[14] The resurgence of interest in this issue seems to stem from the Bell curve controversy of Herrnstein and Murray (1994) which examines a large number of correlations between social outcomes and scores from test batteries.

[15] See Harmon and Walker (2000) fro more details.

specification 4 adds these and interactions between ability and years of education (to allow ability to have a larger effect the longer one stays at school). While we find some significant effects of ability on wages the effect of education itself is reasonably robust to the inclusion of these variables again suggesting that education plays a largely productivity enhancing role.

Table 3.3 NCDS Women: Ability, Parental Background and the Returns to Education

	1	2	3	4
Child's education	0.120 (0.006)	0.107 (0.007)	0.100 (0.008)	0.125 (0.016)
Parental background	No	Yes	Yes	Yes
Child ability measures				
Maths 25-50%	-	-	0.064 (0.033)	0.050 (0.038)
Maths 50-75%	-	-	0.046 (0.032)	0.052 (0.039)
Maths 75-100%	-	-	0.045 (0.037)	0.069 (0.046)
English 25-50%	-	-	0.045 (0.039)	0.022 (0.042)
English 50-75%	-	-	0.063 (0.032)	0.073 (0.038)
English 75-100%	-	-	0.108 (0.037)	0.169 (0.046)
Ability / child education interactions				
Maths 25-50%	-	-	-	0.018 (0.024)
Maths 50-75%	-	-	-	-0.003 (0.021)
Maths 75-100%	-	-	-	-0.010 (0.022)
English 25-50%	-	-	-	0.022 (0.036)
English 50-75%	-	-	-	-0.029 (0.022)
English 75-100%	-	-	-	-0.049 (0.022)
Sample sizes	2739	1981	1981	1981

Table 3.4 NCDS Men: Ability, Parental Background and the Returns to Education

	1	2	3	4
Education	0.075 (0.005)	0.061 (0.006)	0.051 (0.006)	0.087 (0.014)
Parental background	No	Yes	Yes	Yes
Child Ability measures				
Maths 25-50%	-	-	0.023 (0.031)	0.023 (0.032)
Maths 50-75%	-	-	0.067 (0.033)	0.064 (0.038)
Maths 75-100%	-	-	0.108 (0.037)	0.090 (0.044)
English 25-50%	-	-	0.006 (0.029)	0.011 (0.031)
English 50-75%	-	-	0.068 (0.034)	0.082 (0.036)
English 75-100%	-	-	0.107 (0.037)	0.193 (0.044)
Education/ability interactions				
Maths 25-50%	-	-	-	0.012 (0.018)
Maths 50-75%	-	-	-	0.013 (0.018)
Maths 75-100%	-	-	-	0.022 (0.018)
English 25-50%	-	-	-	-0.026 (0.020)
English 50-75%	-	-	-	-0.041 (0.022)
English 75-100%	-	-	-	-0.079 (0.021)
Sample sizes	3169	2319	2319	2319

Table 3.5 shows how the various scores in the GB and Northern Ireland IALS change against a number of individual and family controls based on a regression of the score against

the individual characteristics. As we would expect education has the largest effect. However even the effect of a degree (compared to no qualification) on ability is just a little over one standard deviation but only about one-fifth of a standard deviation when compared to someone with A-levels. Thus, a degree seems to have just a modest effect on ability in this sense. Family effects on ability are jointly significant but small in magnitude, with the effect of coming from a parental background with upper secondary or university education rarely raising any of the scores by more than 18 points (or about one-third of a standard deviation). Having controlled for education we find no differences in the ability levels of those in employment and those not working. Similarly training has little direct effect (Denny and Harmon, 2000a).

Table 3.5 What Determines Basic Skills? Marginal Effects – GB and NI 1995.

Explanatory	Quantitative		Document		Prose	
variables	GB	NI	GB	NI	GB	NI
Lower Secondary	**27.7**	**20.6**	**26.5**	**15.0**	**29.5**	**24.1**
Higher Secondary	**54.8**	**45.6**	**50.0**	**40.7**	**49.4**	**42.8**
Diploma/Certificate	**64.2**	**57.0**	**61.4**	**51.9**	**64.6**	**57.9**
Degree	**87.9**	**68.0**	**77.2**	**58.4**	**76.2**	**69.7**
Postgraduate	**80.5**	**68.9**	**74.3**	**61.3**	**72.9**	**70.5**
Father-Lower Secondary	**10.2**	1.9	**12.3**	-1.4	**15.7**	-0.5
Father-Higher Secondary	**21.1**	**9.8**	**25.2**	7.0	**18.8**	9.6
Father-Diploma/Certificate	**19.1**	**19.8**	**21.6**	**15.1**	**25.0**	**13.0**
Father-Degree	**16.4**	**15.0**	**20.9**	**15.8**	**19.9**	**15.5**
Father-Postgraduate	**10.3**	9.4	**17.3**	**13.3**	**21.9**	0.8
Mother-Lower Secondary	-2.1	5.1	1.7	5.6	-2.7	6.0
Mother-Higher Secondary	2.2	**17.1**	10.3	**16.3**	6.8	**23.0**
Mother-Diploma/Certificate	2.1	**15.3**	11.0	**26.4**	9.5	**16.2**
Mother-Degree	**13.4**	**15.7**	11.4	**15.1**	**17.4**	**12.6**
Mother-Postgraduate	**18.9**	**19.2**	23.9	-9.1	**26.6**	-47.3
Trained	**15.7**	**17.4**	21.0	**19.6**	**21.5**	**20.0**
Employed	**13.5**	**24.4**	10.5	**20.9**	5.5	**15.2**

Note: Significant determinants are in **bold**

Table 3.6 presents some earnings equation estimates for Great Britain[16]. In these regressions, measures of ability are included in the specification. However the included

[16] Earnings data in IALS are reported in intervals and hence we use the estimator proposed by Stewart (1983).

ability measure is the residual from a regression of the scores against the age, education and other characteristics of the individual (similar to the work that formed the basis of the discussion of Table 3.5). By using this residual we are, as far as possible, purging the ability measure of the effect of being measured at the time of survey and not pre-schooling as in the NCDS regressions.

Specification (1) presents the simple wage equation without ability. In specification (2) we introduce the ability measures for the three types of skill. The measures of ability in themselves have small direct effects with quantitative literacy variables having the largest impact. Returns to years of schooling are lower. In specification (3) the single composite measure of literacy based on principal components is used (labelled *functional* literacy) and again we see a positive effect on earnings with the return to schooling reduced.

In order to quantify the effect of literacy using this composite measure it is useful to consider the distribution of the score and from this compute the wage penalty or premium. For the UK, the penalty from being in the first quartile of the ability distribution rather than at the median is 15% while the premium from being at the third quartile compared to the median is 13% suggesting a quite uniform distribution of the scores[17]. The evidence therefore suggests that the direct return to ability is quantitaively small - individuals need to jump across quartiles of the score distribution (and to make these jumps individuals would require long periods of education as seen in Table 3.5)[18].

[17] In comparison for Northern Ireland the penalty for being at the first quartile rather than the median is about 10% and being at the 3rd quartile is worth 7.7% over the median.

[18] Dearden, McIntosk, Myck and Vignoles (2000), in an analysis of the IALS and NCDS find support for the general finding here that literacy and numeracy have a positive direct effect on earnings. They suggest that it is the possession of a basic core level of skills that is crucial which supports our finding that while the returns are positive and statistically significant a large movement in the score is needed to generate economically

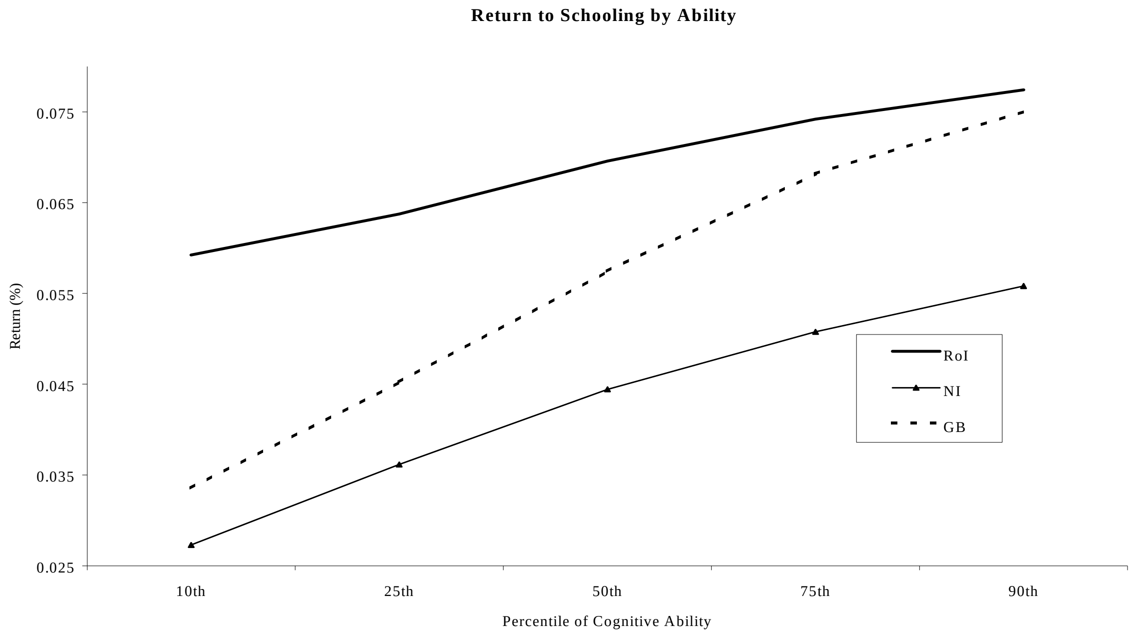
Table 3.6 Earnings and Ability in IALS, GB Men Aged 16-64

	(1) GB Schooling Only		(2) GB Schooling & Ability		(3) GB Schooling & Ability	
	Coeff	*S.E.*	*Coeff*	*S.E.*	*Coeff*	*S.E.*
Years of Schooling	0.089	*0.009*	0.057	*0.009*	0.059	*0.009*
Document Literacy	--	--	-0.02	*0.002*	--	--
Prose Literacy	--	--	0.002	*0.001*	--	--
Quant Literacy	--	--	0.004	*0.001*	--	--
Functional Literacy (F.L)	--	--	--	--	0.058	*0.007*
Sample size	988		988		988	

It is possible, however, that the relationship between earnings and ability comes via the interaction between schooling and ability. In Figure 3.1 we illustrate the results from allowing for such an interaction effect which is based on regressions of earnings on schooling, ability and an interaction between the schooling and ability for GB, Northern Ireland and the Republic of Ireland. The graph shows the effects of education on earnings at different parts of the ability distribution. It appears that the return to schooling is different across ability levels and that GB seems particularly sensitive to this. The returns range from 2.5% for the bottom decile of ability to 7% for the top decile. This is evidence perhaps of what Cawley *et al* (1998) refer to as 'dynamic complementarity' and seems stronger in GB than in either NI or the Republic of Ireland. This evidence seems to substantiate the earlier quantile regression results.

significant returns. They also confirm that numeracy is more valued but that verbal ability is more important in raising the probability of employment.

Figure 3.1 Cognitive ability and the return to schooling.



4. Treatment Effects

4.1 Isolating the Effect of Exogenous Variation in Schooling

If you want to know how an individual's earnings are affected by an extra year of schooling you would ideally compare an individual's earnings with *N* years of schooling with the same individual's earnings after *N+1* years of schooling. The problem for researchers is only one of the two earnings levels of interest are observed and the other is unobserved (Harmon and Oosterbeek, 2000).

The problem is analogous to those encountered in other fields, such as medical science: either a patient receives a certain treatment or not so observing the effectiveness of a treatment is difficult as all we actually observe is only the effect of treatment on those who are treated. In medical studies the usual solution to this problem is by providing treatment to patients on the basis of random assignment. In the context of education this is rarely feasible. However, there are still possibilities to tackle the problem, that the treated are not the same as the untreated in unobservable ways, and labour economists have made significant progress in this area in the past 10 years. The key idea is to look for real-world events (as opposed to real experiments), which can be arguably considered as events that assign individuals randomly to different treatments. Randomly here has as its more precise definition that there is no relation between the event and the outcome of interest. Such events have been dubbed "natural experiments" in the literature. The essence of this natural experiment approach is to provide a suitable instrument for schooling which is not correlated with earnings and in doing so provide a close approximation to a randomized trial such as might be done in an experiment for a clinical study.

A very direct way of addressing the issue of the effect of an additional year of education on wages is to examine the wages of people who left school at 16 when the minimum school leaving was raised to 16 compared to the wages of those that left school at

15 just before the minimum was raised to 16. The FRS data is large enough for us to select the relevant cohort groups to allow us do this and Table 4.1 shows the relevant wages.

Table 4.1 Wages and Minimum School Leaving Ages (£/hour)

	Left at 15 pre RoSLA (1)	Left at 16 pre RoSLA (2)	Left at 16 post RoSLA (3)	% difference between (3) and (1) (4)	% difference between (2) and (1) (5)
Men	7.66	9.56	8.90	**14.9**	24.8
Women	5.25	6.25	5.81	**10.7**	19.0

Note: RoSLA refers to the "raising of the school leaving age" from 15 to 16, which occurred in 1974.

The effect of the treatment of *having to stay on* at school gives the magnitude of interest for policy work – the effect of additional schooling for those that would not have normally chosen an extra year. If we suppose that all those that left at 16 post RoSLA would have left at 15 had they been pre-RoSLA then we get a lower bound to the effect of the treatment: this is 14.9% for men and 10.7% for women. The former figure is very close to that obtained in Harmon and Walker (1995) using more complex multivariate methods. In contrast the upper bound of the treatment is the effect of an additional year of schooling that had been chosen: this earned a larger premium of 24.8% for men and 19.0% for women which reflects the fact that these people who chose to leave at 16 are different people from those that left at 15 in terms of their other characteristics.

More formally the treatment group is chosen, not randomly, but independently of any characteristics that affect education. Thus, one could not, of course, group the data according to ability but grouping by cohort to capture a before and after affect may be legitimate. The variable that defines the natural experiment can be thought of as a way of "cutting the data" so that the wages and education of one group can be compared with those of the other: that is, one can divide the between-group difference in wages by the difference in education to form an estimate of the returns to education. The important constraint is that the variable that defines the sample separation is not, itself, correlated with education. There may be differences in observable variables between the groups - so the treatment group may, for example, be taller than the control group – and since these differences may contribute to the differences in wages and/or education one might eliminate these by taking the differences over time within the groups and subtract the differences between the groups. Hence, the methodology is frequently termed the difference-in-differences method.

If the data can be grouped so that the differences between the levels of education in the two groups is random, then an estimate, known as a Wald estimate, of the returns to education can be found from dividing the differences in wages across the groups by the difference in the group average level of education.

A potential example is to group observations according to their childhood smoking behaviour. The argument for doing this is that smoking when young is a sign of having a high discount rate – since young smokers reveal that they are willing to incur the risk of long term damage for short term enjoyment. Information on smoking when young is contained in the General Household Survey for GB, for even years from 1978-96, and Table 4.2 shows that by examining these differences between groups the estimated return to schooling is around 16% for men and 18% for women.

Table 4.2 Wald Estimates of the Return to Schooling – Grouped by Smoking

Even GHS 78-96		Smoker (at 16)	Non-smoker (at 16)	Difference	Wald Estimate
Men	Log Wage	2.36	2.51	0.16	0.16/0.97 = **0.164**
	Educ Yrs	12.11	13.08	0.97	
Women	Log Wage	2.01	2.18	0.17	0.17/0.90 = **0.188**
	Educ Yrs	12.52	13.42	0.90	

A closely related way of controlling for the differences in observable characteristics is to control for them using multivariate methods. This is the essence of the instrumental variables approach. That is the variable that is used for grouping could be used as an explanatory variable in determining the level of education. This is useful since it allows the use of multivariate methods to control for other observable differences between individuals with different levels of education. It is also useful in cases where the variable is continuous – the research can exploit the whole range of variation in the instrument rather than simply using it to categorise individuals into two (or more) groups. By exploiting instruments for schooling that are uncorrelated with earnings the IV approach will generate unbiased estimates of the return to schooling.

Consider the model $\log w_i = X_i \beta + rS_i + u_i$ where $S_i = \mathbf{Z_i}'\alpha + v_i$. Estimation of the log wage equation by OLS will yield an unbiased estimate of β only if the S_i is exogenous, so that is there is no correlation between the two error terms. If this condition is not satisfied alternative estimation methods must be employed since OLS will be biased. The correlation might be nonzero because some important variables related to both schooling and earnings are omitted from the vector **X**. Motivation, or other ability measures, besides IQ are examples. It is important to note that even a very extensive list of variables included in the vector **X** will never be exhaustive. An estimate of the return to schooling based on OLS will not give the causal effect of schooling on earnings[19] as the schooling coefficient β captures some of the effects that would otherwise be attributed to the omitted ability variable. For instance, if the omitted variable is motivation, and if both schooling and earnings are

[19] In this example the source of correlation between s and ε is that a relevant explanatory variable is omitted. Other sources for such correlation might be measurement error in s and self-selection bias.

positively correlated with motivation, OLS estimation ignores that more motivated persons are likely to earn more than less motivated persons even when they have similar amounts of schooling.

In order therefore to model the relationship between schooling and earnings we must use the schooling equation to compute the predicted or fitted value for schooling. We then replace schooling in the earnings function with this predicted level. As predicted schooling is correlated with actual schooling this replacement variable will still capture the effect of education on wages. However there is no reason that predicted schooling will be correlated with the error term in the earnings function so the estimated return based on predicted schooling is unbiased. This is the two-stage-least-squares method which is a special case of the instrumental variables (or IV) method and which captures its essence.

The difficulty for this procedure is one of "identification". In order to identify or isolate the effect of schooling on earnings we must focus our attention on providing variables in the vector $\mathbf{Z_i}$ that are not contained in $\mathbf{X_i}$ [20] That is, there must exist a variable which is a determinant of schooling that can legitimately be omitted from the earnings equation. In essence this amounts to examining how wages differ between groups whose education is different for exogenous reasons. For example, some individuals may have faced a minimum school leaving age that differed from that faced by others, or may have started school at an earlier age for other reasons (that are uncorrelated with the wages eventually earned) such as having a high rate of time preference and this will be reflected in other youth behaviour such as smoking when young.

[20] See the discussion in Heckman (1990) for further details.

4.2 Results from IV Studies – International Evidence

In Figure 4.1 we present the results of a meta analysis of studies which treat schooling as endogenous, based on the PURE dataset of results used earlier. Compared to an average from OLS of 6.5% we see much larger returns to schooling in IV studies generally (of about 9%) and from IV studies based on education reforms in particular (of around 13 to 14%). In contrast, IV studies that use family background as instruments have returns on average close to the OLS estimate. In the few examples where the legitimacy of family background variables as instruments has been tested, they have been shown to be weak (Rischall, 1999).

Figure 4.1 Meta-Analysis of Models with Endogenous Schooling

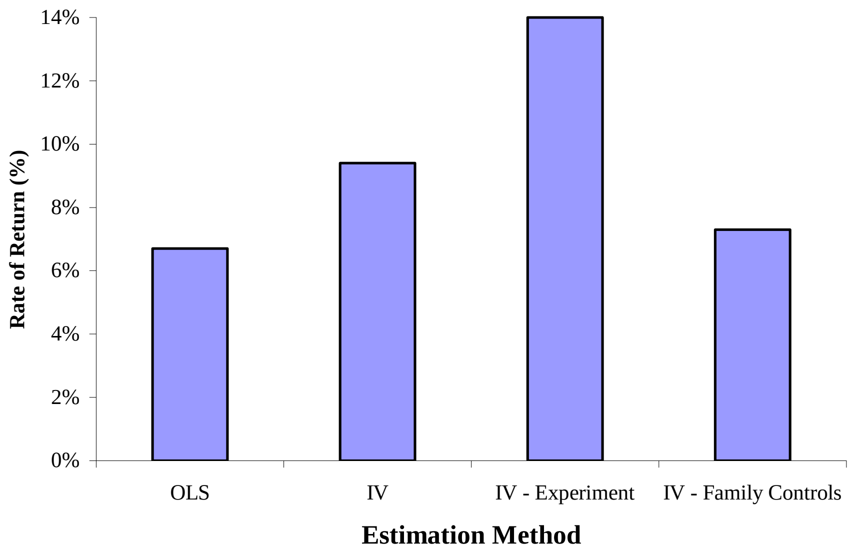


Table 4.3 outlines the key results in this literature for the non-UK studies. Angrist and Krueger (1991) use the presence of compulsory schooling law variation across US states and the quarter of the year in which a person was born as the basis of their instruments. The underlying idea here is that a person who has been born early in the year (the first quarter) reaches the minimum school leaving age after a smaller amount of schooling than persons born later in the year. The actual amount of schooling attained is directly related to the quarter in which they were born while at the same time there seems no reason to believe that

quarter of birth has an own independent effect on earnings. Direct estimation by OLS gives an estimate of the return to schooling of 0.063 whereas the IV method gives an estimate of 0.081[21].

Table 4.3 IV Studies – International

Study	Sample	*OLS* %	*IV* %	*Instruments*
Angrist and Krueger (1991)	US 1970/1980 Census: Men born 1920-29, 1930-39, 1940-49	7.0 (0.000)	10.1 (0.033)	Year * Quarter of Birth; State * Quarter of Birth
Angrist and Krueger (1992)	US 1979-85 CPS: Men born 1944-53 (hence potential Vietnam War draftees).	5.9 (0.001)	6.6 (0.015)	Draft Lottery Number * Year of Birth
Card (1995)	US NLS: Men aged 14-24 in 1966 sampled as employed in 1976.	7.3 (0.004)	13.2 (0.049)	Nearby college in county of residence in 1966.
Butcher and Case (1994)	US PSID 1985: White women aged 24+	9.1 (0.007)	18.5 (0.113)	Presence of siblings (sisters)
Uusitalo (1996)	Finnish Defence Forces Basic Ability Test Data matched to Finnish income tax registers.	8.9 (0.006)	12.9 (0.018)	Parental income and education, location of residence.
Meghir and Palme (1999)	Sweden – Males	2.8 (0.007)	3.6 (0.021)	Swedish curriculum reforms.
Duflo (1999)	Indonesian – Males	7.7 (.001)	9.1 (0.023)	Indonesian school reforms – school building project.
Denny and Harmon (2000)	ESRI 1987 Data – Males	8.0 (0.006)	13.6 (0.025)	Irish school reforms – abolition of fees for secondary schooling.

Note : Standard Errors in parentheses

21 The study of Angrist and Krueger has been criticized by Bound, Jaeger and Baker (1995). They argue that quarter of birth may have an impact on earnings other than only through the effect on schooling. Studies from other social sciences indicate that the timing of births over a year is related to social background. Parents with lower social backgrounds tend to get children spread evenly over the year, while parents from higher social classes get children during more concentrated in particular seasons.

In another study, Angrist and Krueger (1992) exploit the idea that because college enrolment led to draft exemptions potential draftees for the Vietnam campaign had this exogenous influence on their schooling decision. The instruments are based around numbers assigned on the basis of month and day of birth from which a 'draft lottery' was conducted. Again the IV results are higher than OLS but the difference is insignificant, perhaps reflecting later work that suggested the instrument was only marginally significant to the education decision (see Bound *et al.* (1995)). Card (1995) uses an indicator for the distance to college as an instrument for schooling based on the observed higher education levels of men who were raised near a four-year college and finds returns of 13.2% compared to OLS estimates of closer to 7%. However again the estimates were rather imprecise. Butcher and Case (1994), in one of the few examples based on a sample of women, again find IV exceeding OLS and in fact the estimated return more than doubles in this study.

Uusitalo (1996) uses the fact that all eligible Finnish males must complete military service, where aptitude tests are undertaken. By matching this data to tax and census registers this study estimates earnings equations for males based on instruments constructed to indicate parental background variables and the location of residence. The findings again suggest an increase in IV over OLS of some 100%, again statistically significant. A somewhat different approach is used in the paper by Esther Duflo (1999) where estimation is based on the exposure of individuals to a massive investment program in education in Indonesia in the early 1970's. Individuals were assigned to the treatment on the basis of their date of birth (pre and post reform) and the district they lived in (as investment was a function of local level needs assessment). Meghir and Palme (1999) pursue a similar strategy in their analysis of reforms in Sweden in the 1950's that were intended to extend the schooling level nationally. This was piloted in a number of school districts prior to its adoption nationally and it is from this pre-trial experiment that the variation in attainment comes. Both these papers rely on large-scale reforms, which can be thought of as "natural experiments" since their effect differed across individuals. A similar approach is used in Denny and Harmon (2000b) in looking at a fundamental change in the educational system in 1960's Ireland which affected the entire population of school-age individuals in a way which differed across socio-economic backgrounds.

There are a small number of examples in the UK literature using this approach summarised in Table 4.4. Dearden (1995, 1998) repeats the idea in Butcher and Case (1994) by using sibling presence as an instrument for schooling. This study employed National Child Development Study (NCDS) data from the United Kingdom and found increased estimates of the return to schooling compared to the OLS equivalents. In a series of papers Harmon and Walker (1995, 1999, 2000) use changes in the compulsory school leaving age laws in the 1950's and 1970's as instruments, as well as other educational reforms (such as the Robbin's Act) and peer effects. Across a number of datasets a robust finding emerges that compared to OLS estimates of the order of 5-7% per year of schooling, the IV estimated returns were significantly higher.

Table 4.4 IV Studies - UK

	Data	*OLS*	IV	*Instruments*
Dearden (1998)	UK NCDS: *Men*	4.8% (0.004)	5.5% (0.005)	Family composition, parental education, social class.
Harmon and Walker (1995)	UK FES 78-86. *Males 16-64.*	6.1% (0.001)	15.2% (0.015)	School leaving age changes.
Harmon and Walker (1999)	UK GHS 92. *Males 16-64.*	4.9% (0.000)	14.0% (0.005)	School leaving age changes and educational reforms.
Harmon and Walker (2000)	UK NCDS: *Men*	5.0% (0.005)	9.9% (0.019)	Measures of peer effects and education system level effect.

The differences between IV and OLS here are clearly large, and support the international evidence that we have. While these results concur with the simple Wald estimates earlier it is, nevertheless, important that this difference is subjected to more detailed examination. In Table 4.5 we report the results from a number of datasets and specifications that use smoking status as an instrument. The rationale for using smoking as an instrument is given in Evans and Montgomery (1997) where it is argued that smoking is indicative of strong particular time preference: that is, high discount rates so that individuals who smoke show that they place considerable weight on satisfying current wants at the expense of the future. Smoking at age 16 is not correlated with current earnings[22] but is correlated with educational choices. In the table we see larger estimated returns from the IV estimations than the OLS results.

Table 4.5 Further IV Results – Smoking as an Instrument

	Men		**Women**	
Data and instruments	Estimated returns	N	Estimated returns	N
GHS: OLS	0.064 (0.002)	14424	0.092 (0.002)	11759
GHS: Current Smoking	0.205 (0.012)	14424	0.163 (0.011)	11759
GHS: Smoking at 14/16/18	0.095 (0.007)	17907	0.126 (0.008)	17047
BHPS: OLS	0.064 (0.002)	8284	0.103 (0.002)	8987
BHPS: Current smoking	0.209 (0.014)	8284	0.168 (0.011)	8987
NCDS: OLS (with family controls)	0.061 (0.006)	3169	0.107 (0.007)	1981
NCDS: Current smoking (with family controls)	0.191 (0.031)	2311	0.215 (0.043)	1978
NCDS: Smoked at 16 (with family controls)	0.080 (0.033)	1972	0.207 (0.032)	1692
NCDS: OLS (no family controls)	0.075 (0.005)	3169	0.120 (0.006)	2319
NCDS: Current smoking (no family controls)	0.203 (0.029)	3161	0.241 (0.030)	2736
NCDS: Smoked at 16 (no family controls)	0.084 (0.030)	2486	0.219 (0.025)	2150

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. Numbers of observations differ because of missing values for some variables.

The element of this work that seems most noticeable is the often very large returns obtained when current smoking is used (estimates of around 20%) compared with the more modest increases when smoking at 16 is used (estimates of around 8% for men, although

[22] Because of income effects current smoking and current income are likely to be correlated invalidating this as a choice of variable.

larger for women). For the reasons already mentioned there may be some violation of the strict rules for the validity of the instrument when using current smoking in that some correlation with current earnings is quite likely.

4.3 Why are the IV Estimates Higher than OLS?

As discussed above Card (1995) presents a model of endogenous schooling, which shows that individuals invest in schooling until the marginal return to schooling is equal to their marginal discount rate. Therefore less educated workers have either lower returns to schooling (i.e. they are less able) or higher discount rates (i.e. they have less taste for education or poorer backgrounds). If an intervention used as an instrument in an IV estimation induces those from the low-education group to participate further then the associated return will reflect the marginal returns for the low-education group, which may well exceed the return for the population as a whole. Similar research by Lang (1993) also considers this issue of heterogeneous returns, sometimes labeled 'discount rate bias'.

In the Card (2000) model the return to education is allowed to vary across the population, and the marginal return to schooling is a decreasing function of schooling. When the instrument is formed on the basis of membership of a treatment group the IV estimate of the return to schooling is the difference in expected log earnings between the control group and the treatment group, divided by the difference in expected schooling for the two groups. This implies that if all individuals in the population have the same marginal return the IV estimate is a consistent estimate of the average marginal rate of return. However, if the return to schooling is allowed to vary across individuals the IV estimate is the rate of return for the subgroup most affected by the treatment/instrument. If only one subgroup is affected by the intervention the IV estimator will yield the marginal rate of return *for that subgroup*.

In this respect the IV estimator can exceed the conventional OLS estimator if the intervention affects a subgroup with relatively high marginal return to schooling. In the context of Card's model this is possible as low amounts of schooling can imply higher marginal returns to schooling if the relative variation in ability is small. If the intervention affects those with below-average schooling levels the IV estimate will be larger than the 'average' OLS result. This is suggested as a rationale for the results in, for example, Angrist and Krueger (1991, 1992) concerning changes in compulsory schooling laws, and is a specific example of the more general issue of estimating returns for the marginal groups hit by the treatment known as Local Average Treatment Effects (or LATE[23]).

Moreover, as noted by Dearden (1995) if our instrument(s) is correlated with the true measure of education but uncorrelated with the measurement error in schooling the IV approach can be used, and the presence of measurement error should not affect the estimated IV return to education which will be consistent. What will differ is the interpretation placed on the difference between OLS and IV results. As such the difference can now be attributed to a combined effect of measurement error and the endogeneity of schooling. The research by Ashenfelter and Krueger (1995) calculates the reliability ratio (the ratio of variance of the measurement error to total variance in S) in years of schooling measures in survey data at 90%, suggesting that approximately 10% of the total variance in schooling is due to measurement error. Moreover Uusitalo (1999) uses information on schooling from register data that is updated directly from school, so the degree of measurement error is almost certainly much smaller. Despite this both studies find in favour of large and significant downward bias in least squares estimates. On this evidence measurement error appears an unlikely candidate for explaining the IV/OLS difference.

[23] There are two arguments in the LATE literature – either unobserved heterogeneity in returns, or higher returns for specific groups such as the disadvantaged for example.

Finally, the negative correlation may be a result of optimizing behaviour of individuals. Assuming another unmeasured factor that affects income but is unrelated to ability is the approach of Griliches (1977) and Blackburn and Neumark (1995). For example if there is a component that affects the marginal costs of education but not the marginal benefits, such as foregone earnings, the optimizing framework will lead to a negative correlation between schooling and the earnings function residual.

4.4 Instrument Relevance and Instrument Validity

Bound *et al.* (1995) urges caution in the use of IV. IV can be thought of as a way of splitting the variance in schooling into an endogenous component and an exogenous component. This is done by including a variable (or variables) into an equation to explain schooling decisions which is (are) not in the wage equation. The essence of their argument is that the consistency of IV assumes such instrumental variables are correlated with the schooling decisions of individuals but not with the earnings outcomes for individuals. So if this is not the case, and if there is only a weak relationship between the instrument and schooling, then estimation by IV will lead to large inconsistencies. Thus, a weak relationship between schooling and the instruments will raise the problem of inconsistency in the use of IV. In addition, a strong relationship between the instruments and the error in the wage equation will also raise the inconsistency problem and this problem will be magnified if the instruments are not strongly correlated with S. As an example Bound *et al* re-estimate the results from Angrist and Krueger (1991) and find that the hundreds of instruments used in that study are mostly uncorrelated with S which can result in IV being more biased that OLS.

A similar argument has been put forward for the case of invalid instruments. Again Bound and Jaeger (1995), based on a replication of the original paper finds that quarter of birth does seem to have an effect on wages invalidating the case made in Angrist and Krueger (1991). Family background variables are also likely to come into this category.

Non-random assignment to treatment and control groups can potentially arise in natural experiments. As suggested in Card (1999), in the study by Harmon and Walker (1995) people born before 1958 were considered as the control group and those post 1958 were the treatment on the basis of the implementation of the change in school leaving age. However older cohorts may be different in other ways – their education may have been affected by World War II for example (see Ichino and Winter-Ebmer (2000)).

Finally publication bias is suggested by Ashenfelter, Oosterbeek and Harmon (1999). The average return to schooling in a meta analysis of schooling returns estimated by OLS is 6% compared to an average of over 9% from IV estimates. Ashenfelter *et al*. estimate the probability of being observed in a sample of estimated returns as a declining function of the *p*-value on your result. In other words more significant results have a higher chance of being observed. When this is corrected for, about two-thirds of the gap between the average OLS estimated return and the average IV estimated return can be accounted for.

4.5 Further Evidence – Fixed Effect Estimators

Table 4.6 illustrates some recent findings from the literature based on samples of siblings or twins. This approach exploits a belief that siblings are more alike then a randomly selected pair of individuals, given that they share common heredity, financial support, peer influences, geographic and sociological influences etc. This literature 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 then simple test scores. This approach received much attention in the schooling-earnings literature in the late seventies and early eighties, 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.

Table 4.6 Twins/Siblings Research on Schooling Returns

Author	Data	OLS	IV
Ashenfelter and Rouse (1999)	Princeton Twins Survey	7.8%	10%
Rouse (1997)	Princeton Twins Survey	7.5%	11%
Miller et al (1995)	Australian Twins Register	4.5%	7.4%
Isaccson (1999)	Swedish same sex twins	4%	5.4%
Ashenfelter and Zimmermann (1997)	NLS Young Men	4.9%	10%
Bonjour, Haskel and Hawkes (2000)	St Thomas' Twins Research Unit girls	6.2%	7.2%

The survey by Griliches (1979) concludes that the estimated return to schooling, where ability bias is purged via differencing within twin pairs, is lower that the estimated return from the whole sample. The research of Blanchflower and Elias (1999) argues that twins may represent a quite distinct population grouping, making generalizations to the population as a whole difficult. Moreover Jaeger and Solon (1999) point out that the US twins data seems to have larger differences in *S* that randomly matched unrelated individuals would have, casting doubt on the data. However more fundamental criticisms of this approach have focused on the underlying assumptions. 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 that 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 more 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. If schooling is measured with error this will account for a larger fraction of the differences between the twins than across the population as a whole. This would imply that the bias from measurement error in schooling is likely to increase by forming differences between twins.

Recent contributions to the twins literature have attempted to deal with the measurement error problem by instrumenting the education of twin A using the measure of the education of Twin A *as reported by Twin B.* Ashenfelter and Krueger (1994) collected data at an annual twins festival in 1991, and find against the conventional result of upward bias in OLS estimates. Moreover, correcting for measurement error in the self-reported schooling level generates a much larger estimate of the schooling return, in the order of 12-16%. The possible non-randomness of this dataset and the relatively small samples used led to criticisms. However the findings of Ashenfelter and Zimmerman (1992) support this result. The work of Martin *et al.* (1994), which uses a much larger sample of twins, from an Australian representative survey, and employs the same technique as Ashenfelter and Krueger (1994), also finds strong evidence of downward bias in the least squares estimates. The only UK study is by Bonjour *et al* (2000) and is for a sample of women participating in a health panel.

The major weakness of all of these studies is that little or no attention has been given to why twins have different levels of education. The literature assumes that within-twins differences in education is randomly distributed and it is not obvious that this is the case. If it is not the case then the twins literature faces precisely the same endogeneity problem that has plagued the rest of the literature.

Other panel data techniques have been employed to address this problem. By treating the unobserved heterogeneity as a fixed effect, individual panel data can be used to eliminate it. It is assumed that the unobservables are time invariant, and hence observations on the same individual at different time periods yield the information necessary to isolate the effect of the unobservable. The applicability of panel data to estimates of schooling returns is limited. This is due to the nature of the panel that we only observe earnings information following completion of schooling. Taking first differences in earnings will eliminate not only the unobservable fixed effect but the schooling information also. Information is therefore required on individuals' earnings before and after schooling, and as such is only available for those who return to education later in their lives. While this appears unlikely Angrist and Newey (1991) find some 19% of working male respondents in the National Longitudinal Survey of Youths (NLSY, a cohort study conducted in the US which followed young people through time) reporting a higher level of schooling in later waves of the data, undermining the assumption that schooling can be thought of as a fixed effect[24].

[24] Moreover the assumption implicit in this procedure is that the returns to years of continuous schooling is the same as the return to schooling when resumed after an interruption, which may not be realistic.

Part B: Social/Non-Pecuniary Returns to Education

5. The Social Returns to Education

5.1 Externalities from Education

A clear message of the previous section is that there is a significant private return (which just includes the costs and benefits that flow to the student) to education and the OLS estimates can be considered at least a lower bound to the true value of this return.

As noted in Sianesi and Van Reenen (1999) and Dutta *et al.* (1999) persistently high returns to individuals undertaking higher education suggests that individuals may be underinvesting in education for some reason: for example, there may be some failure in the credit market that demands that collateral backing is required to obtain a loan. This collateral requirement may well prevent individuals from borrowing against their expected future income. Thus, one intervention that may well be necessary is to provide or guarantee education loans.

However, in the absence of such market imperfections, a high private return it not itself a reason why taxpayers resources should be invested in encouraging educational participation unless there are benefits to society over and above the benefits to the individual. Greenaway and Haynes (2000) discuss the possibility that graduates raise the productivity of non-graduates such that aggregate productivity is higher. Moreover there may be social cohesion benefits from education participation rates being increased through government interventions, such as lower crime – at least some of which may be difficult to insure against (insurance against violent crime is not commonly available). It is clearly easier to imagine such effects being important at low levels of education but less easy to envisage for higher education.

Dutta, Sefton and Weale (1999) calculate social rates by comparing the earnings profiles for male university graduates and non-graduates who have A-levels and using a baseline assumption for the cost of producing a graduate of £4,790 per annum plus earnings foregone while studying[25]. Social rates of return for three groupings of degree subjects are then estimated. These rates of return for graduates range from zero (for broadly humanities and biological sciences) to over 11 per cent (for medicine, other science and computing, business studies and social studies). These are lower than their private rate of return estimates. Evidence in OECD (1998) cited in Greenaway and Haynes (2000) suggests that social rates of return in the OECD are around 10 per cent, and higher in countries where students make a contribution to costs (such as Australia, Canada and the US). However, this analysis makes no allowance for wider benefits to the economy.

5.2 Human Capital and Growth – Macroeconomic Evidence

Aggregating a Mincer human capital earnings function (HCEF) to the economy level we get $\ln \overline{w}_{jt} = r_{jt}\overline{S}_{jt} + e_{jt}$, where $\overline{w}_{jt}$ and $\overline{S}_{jt}$ are the mean wage (in practice, GDP per capita is used) and schooling respectively in country *j* at time *t*. Differencing removes technological differences that are part of the error term terms to give $\Delta \ln \overline{w}_{j} = \Delta r_{j}\overline{S}_{jt} + r_{jt}\Delta\overline{S}_{j} + \Delta e_{jt}$, so the *S* coefficient shows how returns have changed over time, while the ΔS coefficient gives the (social) rate of return in j at time *t*. Psacharopolous (1994) found that the Mincerian (private) return fell on average by 1.7% over 12 years from the mid 1970's across a wide range of OECD countries, while O'Neill (1995) found that the (social) return rose by 58% in developed countries and 64% in LDCs between 1967 and 1985. The implication is that the externality has been growing over time.

[25] Earnings foregone is calculated as the average earnings of A-level workers less approximate earnings while studying of £1,000 per annum.

The idea that growth rates should converge is in a feature of many macro-studies – those below their steady-state growth rate should catch up with those above. That is $\Delta W_j = \beta\left(W_{jt-1} - W_j^*\right) + u_j$ where W = log of w and W^* is the steady state level of GDP (per capita). Then the macro growth equation would become $\Delta W_j = \beta W_{jt-1} + rS_{j,t-1} + ... + e_j$ where variables such as “rule-of-law” index, inflation, and capital are sometimes included. In addition an interaction $W_{jt-1}S_{j,t-1}$ may be included to capture the idea that the speed of convergence may be faster the higher is the level of education.

Such growth equations are usually estimated from pooled cross-section data spanning 5 (or more) years. Classic examples are Barro and Sala-I-Martin (1995), Barro and Lee (1993) and Benhabib and Spiegel (1994). However there are some differences between what is usually estimated in the growth modelling literature and micro work in the Mincer tradition. Much of the macro-growth literature excludes ΔS, the change in schooling levels in the economy. The growth literature also typically includes controls to capture the steady state level of GDP – including the S_{jt-1} term.

There are a number of empirical difficulties with this literature mainly related to the nature of the causal relationship between schooling and growth. The interpretation of the S coefficient in $\Delta W_j = \beta W_{jt-1} + rS_{j,t-1} + ... + e_j$ could be interpreted as a return in terms of the ‘steady state’ growth of the economy - educated countries grow faster. However more indirect effects are possible. Schooling may better enable the workforce to develop and adapt to new technologies that will also allow educated countries to grow faster. But paradoxically countries with low levels of average schooling might have better opportunities to grow by adopting technology developed abroad. The return to S may have risen or fallen which can jeopardize the interpretation in these growth models. However anticipated growth in an economy could cause an increase in the demand for education. Indeed Topel (2000) has argued that “little can be learned” from macro growth equations because either a positive or a negative coefficient on human capital is “consistent with the idea that human capital is a boon to growth and development”.

5.3 Human Capital and Growth – Microeconomic Evidence

Krueger and Lindahl (1999) strongly criticise many of the macro contributions in this area and point to the micro foundations of the analysis and the strong assumptions underpinning the findings. For example many of the more general results linking education and growth might stem from imposing constant-coefficient and linearity restrictions on the data. This point is reaffirmed in Trostel (2000) who shows how the limited microeconomic evidence on human capital production is not helpful as it imposes important restrictions on the estimates of the returns to scale to the inputs. Although constant returns may be an appropriate assumption for some educational services (i.e., teaching) this does not imply constant returns to scale in producing human capital, which is embodied in individuals. In Trostel’s model the returns to scale is inferred from the rate of return to education. Data from the International Social Survey Programme is used to estimate (private) rates of return to education and rejects a constant marginal rate of return to education, which is shown to equate to a rejection of constant returns to scale in producing human capital. The marginal rate of return to schooling is shown to be significantly increasing at low levels of education indicating significant increasing returns, and the marginal rate of return decreases significantly at high levels education (thus indicating significant decreasing returns).

Krueger and Lindahl (1999) also stress how causality can be confused – it is not clear that cross-country differences in education are a cause of income, or a result of income or income growth. Therefore, while considerable effort has been placed in the exogeneity or endogeneity of schooling in private returns estimation based on microeconomic data, little or no effort has been made in the possible endogeneity of education in cross-country macro specifications. Similarly human capital enhancement projects can result in other investments to enhance growth introducing a second source of omitted variable bias in cross-country study. The call in the Krueger and Lindahl research is for an experimental approach to be adopted in the social returns literature to repeat, in essence, what we extensively discussed earlier in the report for the estimation of private returns. In view of the difficulty in finding a 'one size fits all' experiment the conjecture in this research is that establishing the social returns and quantifying the likely externalities from education is likely to be more successful from within region study rather than between country study.

A literature is beginning in this vein but unfortunately the evidence is already conflicting. Moretti (1999a) examines US census information for otherwise similar workers within cities with higher and lower education levels. He differences out the potential attraction of the city for particular workers as well as the endogeneity of the growth in education across cities. What is found is that a 1% increase in the share of college educated workers raises the earnings of school dropouts by 2.2%, of high school graduates by 1.3% and college graduates by 1.1%. All gains are net of costs. In this paper Moretti instruments for average schooling with changes in the city age structure, the costs of schooling and presence of low cost or free post-high school college. Individual schooling is however left as exogenous. In a later paper (Moretti, 1999b) the human capital externality is found to be greatest in human-capital-intensive production. Plants situated in cities with higher than average education levels have higher investment in computers and new machinery. Investment in computers in-plant is also found to be associated with usage outside the plant.

Acemoglu and Angrist (1999) consider implications of, like Moretti (1999a), treating average schooling as endogenous. However they also allow for the endogeneity of individual schooling. In their econometric specification they show that if the OLS and IV estimates of the private return to schooling differ only instrumenting average schooling can raise considerable specification problems. They use compulsory schooling laws in the US to instrument individual schooling and they instrument the average level of schooling in each state using the differences in child labour laws across states. Compared to least square estimates of the private return to education of around 6% estimates based on IV range from 7% to just over 9%. However the social returns estimated in this paper are smaller at around 2% per year of average schooling. Acemoglu and Angrist conclude that their study offers little evidence for sizeable social returns to education, at least over the range of variation in average statewide education induced by changing the compulsory schooling laws.

5.4 Other Externalities from Education

Blundell *et al* (1999) consider the evidence on the returns to the employer of education and training. The difficulty is well known here – data is hard to obtain which measures elements such as productivity, competitiveness and profitability and this in confounded by the need to consider the role the employer may take in funding the investment in human capital particularly in the case of training.

Other more indirect benefits from education may be possible. Freeman (2000) suggest that there is little *direct* evidence linking education to reductions in crime and the perceived linkage relates to the effect that education has on factors such as unemployment and inequality. For example upward trends in inequality are associated with higher levels of both property and violent crime (see Kelly, (2000)). Winter-Ebmer and Raphael (1999) find positive effects of unemployment on crime that are not just statistically significant but large in size. Leigh (1998), in a review of work published in this area, concludes that increased education is positively and strongly correlated with absence of violent crime, measures of health, family stability and environmental benefits.

Lochner (1999) develops and estimates a model of the decisions to work, to educate yourself, and to commit crime and allows for the possibility of all of these choices being endogenous. The model suggests that education is correlated with crimes *that require less skill*. Part of the model allows for simulation of the effects of education subsidies on external outcomes and predicts that education subsidies reduce crime. In so far as possible, empirical implications were explored using various large scale US micro datasets. Ability and high school graduation significantly reduce the participation of young men in crime and the probability of incarceration. Evidence from the census data supports a general finding that states with higher rates of high school participation and tougher penalties have the lowest index for property crime.

6. Non-Pecuniary Returns to Education

Despite the re-emergence, in recent years, of job satisfaction as a topic for analysis in economics, there has been little research that looks in detail at the relationship between job satisfaction and education. Most papers, when looking at British data, find that the more educated are less satisfied with their job. However, Blanchflower and Oswald (1999) find that job satisfaction is actually increasing with education, thought they do not control for the wage. The results are not so clear-cut for U.S. data.

This section[26] discusses some of these results, and presents new results that corroborate this evidence using the 1998 wave of the *British Household Panel Survey*. Here, we are interested in the relationship between education and job satisfaction, not wages and job satisfaction, as has been the case for the majority of job satisfaction studies. Hence we will not be discussing the importance of relative wages, exogeneity assumptions, etc.

There are only a few studies that explicitly look at job satisfaction and education. Tsang *et al.* (1991) derive a measure of the *required* education for each individual's job, and use this to obtain a measure of *surplus* schooling (actual schooling minus required schooling). Their results show that *required* education is positively related to schooling, and *surplus* schooling is negatively related to job satisfaction. Battu *et al*. (1999), in a similar study, look at how job mismatch is related to job satisfaction. Their measure of mismatch is based on the response to a question that asks UK university graduates whether their qualification a requirement for the job specification. Not surprisingly, they find a negative relationship between the extent of the mismatch and job satisfaction. Assuming that the individual's level

[26] This section draws on research by Arnaud Chevalier and Reamonn Lydon and we are grateful to them for their assistance.

and type of education affect the quality of the job match, one would expect to find that job satisfaction is related to education. Hersch (1991) has analysed the same relationship between education, mismatch and job satisfaction and finds similar results.

Table 6.1 below presents results that attempt to explain the variation in levels of job satisfaction across individuals. The dependent variable is a discrete increasing scale of satisfaction and included in the independent variables are controls for a postgraduate degree, an undergraduate degree, A-levels, O-levels, apprenticeships and no qualifications. These are the highest educational attainments as reported by the respondent. This is essentially the same estimation as in Clark and Oswald (1996), though we include controls for the number of periods unemployed and the number of weeks unemployed. As we expect, the effect on job satisfaction of being more highly educated is negative and (almost) monotonically increasing. The negative correlation between education and job satisfaction continues to hold, even when we control for wages. The fact that more highly educated people are less satisfied may seem counter intuitive, but is supported by the idea of job mismatch and may also arise if there is an 'aspirations' effect. That is, more educated people have higher expectations from their jobs and these aspirations fail to be realised. Note, that when the same estimation was done using satisfaction-with-pay as the dependent variable, the results were essentially the same, but when the wage was dropped from the estimation the negative correlation disappeared.

In order to get some idea of the size of the education effect on job satisfaction we create a 'baseline' person close to the means/medians in the sample: male, no periods of unemployment, 0.8 weeks unemployed, 3.42 log weekly hours, aged 37, lives in the South East, works in a clerical/secretarial occupation, has excellent health, does not get a bonus, and is not a member of a union. We obtain the predicted probabilities from the estimated model, comparing the results when we hold all variables constant at their baseline values, changing only the level of education. The results are shown in Table 6.2, and show that moving from the lowest to the highest level of education on the scale, reduced the probability of being satisfied with the job by 15%.

Table 6. 1 Job Satisfaction Equation

Dependent Variable = Job Satisfaction	Coefficient	Std. Error
Log Gross Monthly Wage	0.057	0.040
Postgraduate Degree	**-0.358**	0.106
Undergraduate Degree	**-0.256**	0.058
A-Level Only	**-0.334**	0.064
O-Level Only	**-0.161**	0.057
Apprentice/Other Qualification	-0.099	0.073
1 U/e Spell in Year to 01.09.98	**0.282**	0.109
2 U/e Spells in Year to 01.09.98	0.288	0.305
3 U/e Spells in Year to 01.09.98	-0.135	0.371
Number of Weeks U/e in year to 01.09.98	**-0.008**	0.005
Log Weekly Hours	**-0.264**	0.053
Male	**-0.163**	0.036
Log Likelihood	-7865.9	
Pseudo R-Squared	0.026	
Observations	5,385	

Note: Figures in bold are significant at the 5% level. Controls for industry, occupation, union membership, health, region and age are also included. The omitted category is no qualifications.

Table 6.2   Change in the Probability of Being 'Satisfied' according to education:

Education	Pr(Satisfied)
Postgraduate degree	0.450
Undergraduate degree	0.495
A-level	0.461
O-level	0.530
Apprentice	0.557
No Qualifications	0.595

The results suggest small and statistically insignificant effect of wages on job satisfaction that seems implausible and implies that the monetary increment to compensate for an increase in education is extremely high and positive such that the net returns to education is negative allowing for its effect on job satisfaction. Table 6.3 translates the estimated coefficients into probabilities of being job satisfied. Note that degree graduates have higher job satisfaction than individuals whose highest qualification is A-levels, although apart from this higher levels of education are associated with lower satisfaction. The methodology is typical of the literature and relies on education being randomly assigned and this seems implausible in this context. Further research is clearly required.

Part C: Schooling Decisions

7. Early School Leaving and Household Backgrounds

7.1 Does Early Leaving Matter?

Considerable evidence exists that suggests that the impact of early school leaving can be significant, particularly in the knock-on effects for the family of those individuals who leave education at an early age. Early school leaving seems likely to be an important part of the transmission mechanism that induces the strong observed intergenerational correlation of incomes and poverty between successive generations. For example, O'Neill and Sweetman (1998) provide direct evidence that parental background is an important determinant of a child's future welfare. A son whose father was unemployed twenty years earlier is almost twice as likely to be unemployed as a son whose father was not unemployed. This dependency remains significant after controlling for a range of son's characteristics including education, ability and family composition. Dearden, Machin and Reed (1997) report a clear intergenerational correlation between fathers and both sons and daughters in terms of labour market earnings and years of schooling based on analysis of the NCDS cohort.

There is also considerable evidence linking child poverty to parental education attainment. For example Kesselman (1994) examines changes in Canadian tax law for the treatment of children and illustrates how the alternatives to direct income transfers for tackling child poverty should include methods to enhance the employability of poor parents through education. The effectiveness of enhanced income transfers is questioned, as they seem to have less impact on the lifetime prospects of poor children.

Financial returns to education are clearly significant. Yet modelling the choice of individuals to pursue schooling explicitly taking account of these returns is not common. Recently Cameron and Heckman (1998) used the US NLSY panel and found weak current parental income effects while strong parental wealth effects in schooling choices. For the UK Micklewright (1989) showed that the proportion of sixteen-year olds in Britain who stay on at school is low by OECD standards and based on the NCDS cohort examined the probability of completing education at the minimum legal age. Family background, in the form of class and parental education, is shown to have a large effect even when ability and school type are controlled for. Chevalier and Lanot (2000) attempt to disentangle the effect of family income on the child's educational attainment from other characteristics, which might also affect schooling decision such as parental education. They find that the direct effect of family income on child's schooling attainment is overstated by the raw correlation in the data and that a policy that relies on increasing parental incomes to increase post-compulsory education is unlikely to be effective, and would be expensive.

One difficulty with this work is that parental income in NCDS data is available as categorical data and the variable contains many missing values. Moreover, the income data is simply income and not income contingent on attending school and hence the existing literature does not pick up the substitution effect associated with Educational Maintenance Allowances (EMAs) through the reduction in the opportunity cost of staying on at school. While there has been no evaluation of the EMA pilots to date, below we exploit the correlation between education and schooling-contingent parental income. There are two sources of such income: child support from absent parents and Child Benefit.

7.2 Evidence from the Family Resources Survey

Our work is based on Family Resources Survey (FRS) pooled for 1994, 95, 96 and 97. We drop over 18's because the data is censored by leaving home – only post 18 children in HE/FE are recorded as external children, those that leave home but do not go to HE/FE disappear from the data, so we confine our attention to staying on at 16. The pooled data contains 4416 households with 16-18 year old children. The raw data for sons' shows: strong social class effects – nonmanual sons have a staying on rate which is 30% higher than manual sons; strong regional effects – the North and Midlands staying on rates are about 10% lower than the South; strong effects of father's education at low levels – if the father left school at 18 rather than 15 this adds about 30% to staying on rate; strong effects of current income – staying on rises across deciles of the parental income distribution at about 3% per decile.

The data contains information on the Council Tax band of the house - for renters this picks up area effects that might reflect peer group influences; while for owner occupiers the CT band picks up both this effect and an effect of wealth so we can net out the area effect by looking at the effect of the CT band on the children of owner occupiers minus the effect of CT band on the children of renters. The idea here is that CT band for owner occupiers picks up both area and wealth effects since higher housing value is associated with high housing equity. We find weak wealth effects and strong area effects on staying on rates in the raw data for sons.

The data for daughters shows: weak social class effects – nonmanual daughters have a staying on rate which is about 15% points higher than that for manual daughters; strong regional effects – the North and Midlands are about 10% lower than the South; some effect of father's education at low levels – father leaving at 18 rather than 15 adds about 20% to the staying on rate; some effect of current parental income –staying on rises at about 2% per decile of the income distribution; weak area (peer group) effects; and weak wealth effects.

7.3 Modelling Difficulties

Parental income and parental education (and other variables) are correlated – so we cannot make inferences about the effect of either without controlling for both, Thus we need to model both (all) effects simultaneously. Moreover, unobservable effects are also likely to be correlated with income – for example, parental income may be directly correlated with both school leaving and with the provision of a good home working environment that may also be reflected in later school leaving. Thus, we cannot make inferences about the effects of income without controlling for unobservable factors that affect school leaving. Thus, we need to use multivariate methods and we need to instrument parental income to allow for the correlation with unobservables that affect child education[27].

The role of current income in this model is to pick up “credit market constraints”, while the role of wealth is to pick up long run effects of being brought up in a richer environment. There would be a case for policy to reduce child poverty if wealth were significant. Schooling contingent income is likely to have a more powerful effect than other forms of income since it affects the opportunity cost of staying on. There would be a case for EMAs if this were significant.

Thus, below we present estimates of the *probability* of early school leaving in the FRS microdata containing explanatory variables such as: parental incomes; parental education; family and individual characteristics such as siblings, gender, race, and region; wealth (proxied by an interaction variable between council tax band and owner occupier); area affects (proxied by council tax band for renters); other characteristics (employment status, working mother); and, finally, child support and child benefit.

In order to overcome the potential endogeneity of parental income we replace incomes by predicted incomes which depends on: education, work experience, etc. and on two possible *instrumental variables:* a variable that captures the effect of the 1974 increase in the minimum school leaving age, and union membership. The predicted income variable then picks up the effect of exogenous differences in income across individuals. The

[27] In general, there appears to be a strong correlation between low income and bad child outcomes and this may motivate child poverty policy. However, there is almost no evidence that giving poor parents more money makes for better children – poor parents may simply be more likely to be poor at parenting, or they may not be skilled at spending money well on behalf of their children (or may spend it on themselves). The paucity of evidence indicating that parental income has a *causal* effect on child outcomes motivates direct interventions like SureStart.

maintained hypothesis in the methodology is that school leaving decisions are uncorrelated with parental union status and with whether the parents were born before or after the raising of the school leaving age (-15): we assume that these variables only affect parental incomes but not the child's school leaving age.

We compare three sets of estimates: a model that contains just a minimal set of control variables (siblings, gender, race, region, year) as well as the principal economic variables of interest: parental incomes (predicted), child support and child benefit. We refer to this model as "No controls" below. Secondly we estimate a "Basic controls" model that additional controls for parental education and other characteristics (employment status, working mother). Finally, we estimate a "Full controls" that also includes: wealth (proxied by council tax band * owner occupier interaction), and Area affects (proxied by council tax band). The idea of estimating these three models is to see if any effect of parental income on school leaving is sufficiently strong to still be present when we include other observable variables that may also be correlated with school leaving.

Since the analysis is based on estimating a model of the *probability* of leaving school at 16 the transform the estimated coefficients into graphs that show the effects of each of the variables on an individual who has a (relatively high) probability of leaving school of 50%. That is we fix the characteristics of the individual and then choose a value for the error term so that the model predicts a probability of early school leaving of 50%. We then compute the effect on the probability of changing each variable in turn. For example, Figure 7.1 shows the effects of parental income on the school leaving for each of the specifications. The figure shows the predicted effect and the 95% confidence interval around that prediction. With no controls this (large) increase in income has a large and statistically significant effect – raising the probability to over 70%. However, this prediction is not robust to including other controls

– for example, with a full set of control variables we find only a small and insignificant effect (indicated by the confidence interval bar crossing the horizontal axis).

Figure 7.1 The Effects of Father's Income - Extra £100 pw (evaluated at 50% probability)

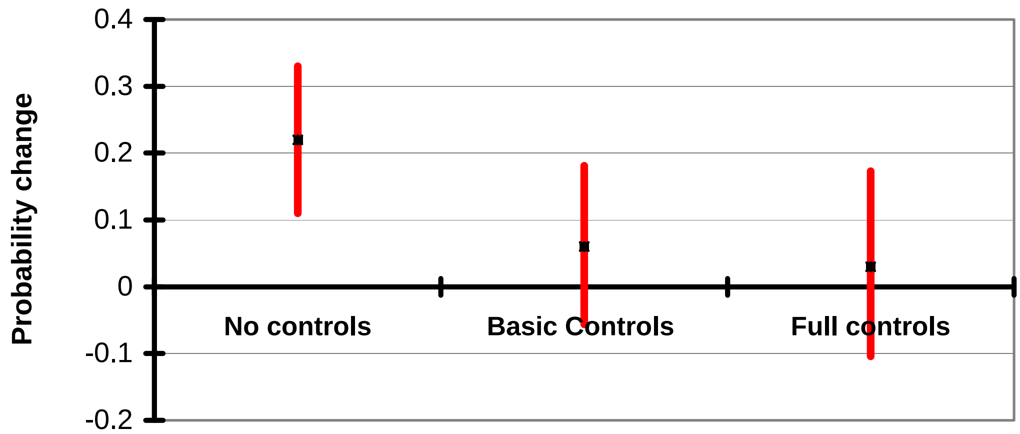
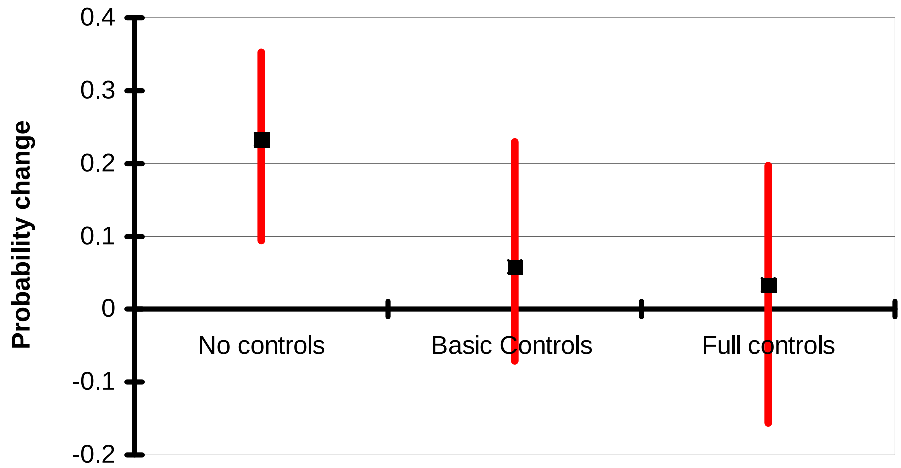


Figure 7.2 shows the effect of an increase in schooling-contingent income. Here we simulate the effect of a transfer of £25 per week. Again we find that although we can easily generate a large effect, the effect becomes smaller and less precisely estimated when additional control variables are included. Thus, with full controls the effect of such a change, akin to an EMA of realistic magnitude, is just a 3.4% increase in the probability, although this figure is estimated with a high standard error.

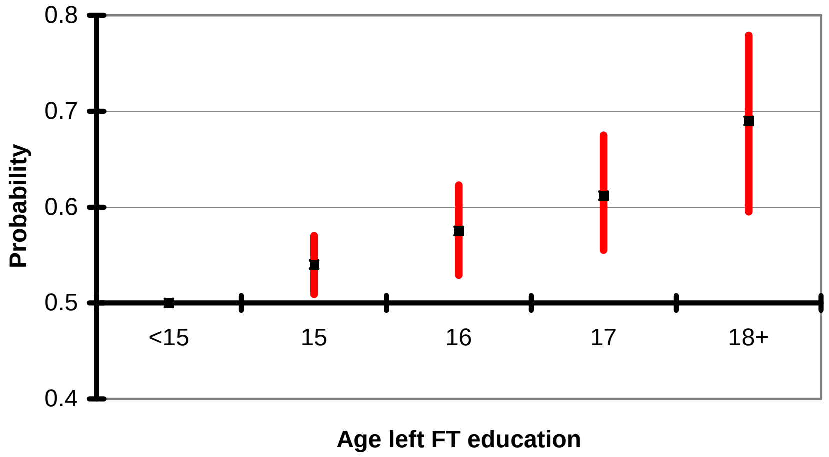
Figure 7.2 Schooling contingent income - Extra £25 pw - evaluated at 50% probability



Thus our results do not provide robust support for a policy of using EMAs to promote later school leaving. However, our estimates depend on variation in schooling contingent income in the FRS data and this is essentially a function of household characteristics. Thus, it is difficult to untangle the effects of the income from the effects of the characteristics that that income depends on. A more comprehensive evaluation of EMAs must await analysis of the data generated by pilot schemes around the country.

The effect of mother's education is explored in Figure 7.3, which comes from a specification that includes a full set of control variables. We find a strong effect - an additional year of education for a mother induces a predicted increase in participation of approximately 4% points.

Figure 7.3 Effect of Mother's Education - evaluated at 50% probability



The CT band information is used as a crude control for area. For renters this is the only effect that this variable would have, while for owner-occupiers it would pick up both area and wealth effects. Of course, for both renters and owners CT band could be associated with other characteristics such as income or class. However, we control for income and education in our analysis and we found no class effects (using socio-economic group) once on controls for income and parental education and employment status were included. Figure

7.4 shows that for renters children in E band houses have significantly later school leaving than band A children. Figure 7.5 shows the effect for owner-occupiers *minus* the effect for renters and so could, arguably, be thought to have factored out the area effect to leave only the effect of wealth.

Figure 7.4 Area Effects (renters) - Evaluated at 50% probability

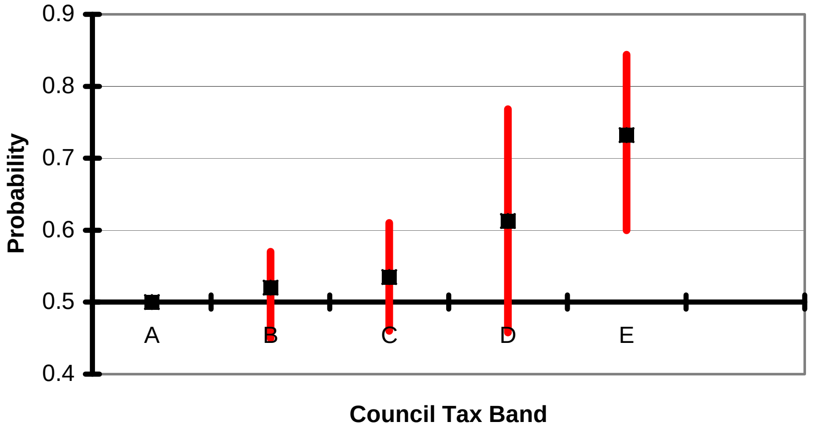
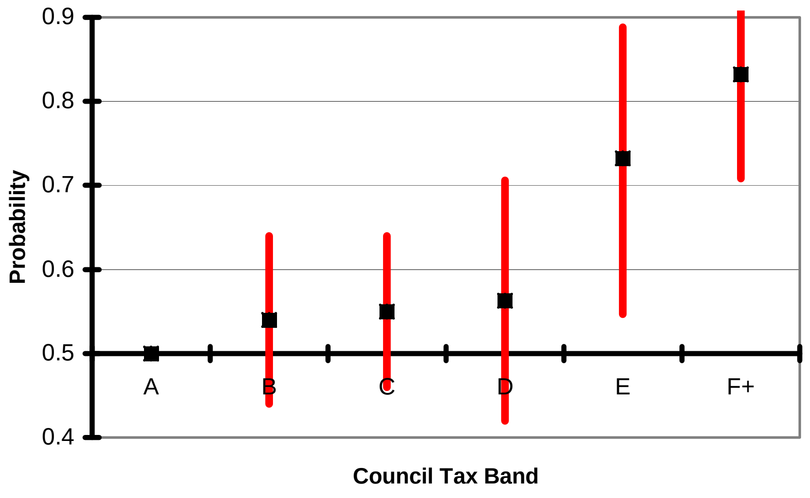


Figure 7.5 Area Effects (owners minus renters) - Evaluated at 50% probability



8. Conclusion

Despite a well developed theoretical foundation, the estimation of the return to a year of schooling has been the focus of considerable debate in the economics literature. A dominant feature of the literature that estimates human capital earnings function, is that schooling is exogenous, and this has been the focus of recent research efforts. With respect to the returns from schooling for an individual a number of conclusions can be drawn.

The simple analysis of average earnings for different levels of education can mask a number of issues. The omission of additional controls assumes that variables that affect wages are uncorrelated with schooling – which seems implausible. For example older people are likely to have lower levels of education but higher levels of work experience giving very different 'returns' for a given level of schooling. Multivariate regression analysis based on Ordinary Least Square (or OLS) suggests a return to a year of schooling of between 7% and 9% when a relatively parsimonious specification is used based on controlling for schooling and experience (measured with age and its square to capture the potential for diminishing returns to experience). This would appear to be at the upper end of returns to schooling in Europe, where Nordic countries in particular have low average returns to schooling. The returns to schooling are relatively stable to changes in this simple OLS specification (such as including controls for marital status/family size/union membership) but some differences are worth noting. Using different measures of experience (based on actual reported experience and so-called 'potential' experience or the difference between current age and the age left school) will tend to raise the return to schooling by approximately 1%. Including occupational controls will tend to have the opposite effect, lowering the return by around 1%. Basing the estimation on samples of employed persons may also bias the returns to schooling downwards, at least for samples of women, but our evidence suggested that this effect, although significant, was small.

The basic specification assumes that (log) earnings are linear in education, so that each year of education adds the *same* percentage amount to earnings irrespective of the particular year of education. This may seem implausible but it has been difficult to find examples in the literature that conclusively prove that linearity is not a valid assumption. There is limited evidence that some years of schooling carry ‘sheepskin’ effect – leaving school the year immediately following a credential awarding year for example may generate a lower return for that year generating a dip in the education/earning profile. However, the literature has not really addressed the endogeneity of schooling despite the strong disincentives to leave school in particular years implied by the results.

The returns to education may also differ across the wage distribution. Evidence based on quantile regression methods suggests that the returns are higher for those in the top decile of the income distribution compared to those in the bottom decile. Moreover this inequality may have increased in recent years. One explanation for this phenomenon is a complementarity between ability and education – if higher ability persons earn more this might explain the higher returns in the upper deciles of the wage distribution.

This finding has important implications for both education and tax and social security policy: the low return to investing in low ability individuals and the high return to investing in high ability individuals implies that educational investment should be skewed towards the high ability individuals. The resulting inequality may then be dealt with through redistributive tax and social security policy.

Given the increase in the supply of educated workers in most OECD countries there is a concern that the skills workers bring to their job will exceed the skills required for the job. This will manifest itself in a lower return to schooling for the years of schooling in excess of those required for the employer. One of the main problems with this literature is the often poor definition of overeducation in available datasets, typically based on subjective measures given by the individual respondent. Where a more comprehensive definition is used based on job satisfaction the apparent negative effect of overeducation is eliminated when ability controls are included, but when overeducation appears to be genuine the penalty may be much larger than was first thought.

This has important implications for the variance in the quality of graduates produced by the higher education system. Firstly, a degree is not sufficient to ensure a graduate job – other complementary skills are expected by graduate employers. Secondly, since genuine overeducation can emerge it is clear that the labour market does not adjust fast enough. So a degree of manpower planning is required to ensure that particular types of graduate are not produced excessively.

It is possible that the return to education actually reflects the underlying ability that education *signals* – in other words education is a signal of inherent productivity of the individual rather than a means to enhance the productivity. Estimates presented here of the signalling component of the returns suggest that the effect is quite small. Based on datasets where direct measures of ability are available the inclusion of ability measures lowers the return to schooling by less than one percentage point. This can be higher where the ability measure is taken at an older age – however caution must be exercised in interpreting these results as the ability measure is almost certainly contaminated by the effect of schooling. However, consistent with the earlier discussion of the complementarity between education and ability evidence from the International Adult Literacy Survey (IALS) suggests that the return to schooling is different across the distribution of ability – those in the bottom decile of the ability measure have returns to schooling of around 2.5%, substantially lower than the average returns of approximately 7%.

Ideally the way we would wish to measure the return to schooling would be to compare the earnings of an individual with two different levels of schooling, but in practice only one level of education is observed for a particular individual. The literature has recently attempted to deal with this problem by finding 'experiments' in the economy that randomly assign groups of individuals to different levels of schooling. We can, for example, examine the wages of people who left school at 16 when the minimum school leaving age was raised to 16 compared to those that left school at 15 before the change in the minimum age legislation. This gives us a measure of the return to schooling for those that would not have chosen an extra year of schooling. The return to schooling from studies that use this methodology seem to be larger than those obtained using OLS. Alternatively a more sophisticated modelling procedure based on Instrumental Variables can be used to deal with this problem.

The effect of this change in estimation procedure can be considerable. Average returns to schooling from OLS are around 6% internationally but over 9% from these alternative methods . The UK appears to be at the higher end of the international range so, for the UK, the comparison is between 7 and 9% from OLS to a range of 11% to 15% from the IV/experimental methods. A concern about this methodology is that the higher returns found may reflect the return for the particular subgroup affected by the policy intervention. Thus, for example, changes in compulsory schooling laws may affect those individuals who place the least *value* on education – and as such estimates of the return to schooling based on these changes may be estimating the returns for that group. In short, care should be taken in the interpretation of IV estimated returns to schooling as an indicator of the return to all individuals without careful knowledge of the effect of the interventions used in estimation of the return.

An additional concern is that the intervention actually has only a weak effect on schooling and that this lack of information in the instrument can introduce or exaggerate bias in the estimated returns. While, in the work presented here the instruments seem to be quite strong, there are many examples in the literature where weak or invalid instruments have been used, particularly instruments based on family background.

The evidence on private returns to the individual is therefore compelling. Despite some of the subtleties involved in estimation there is still an unambiguously positive effect on the earnings of an individual from participation in education. Moreover, the size of the effect seems large relative to the returns on other investments. Evidence on the net benefits to the economy, taking account of the increased earnings and the cost of providing education is limited in the UK, mainly to HE. The available evidence suggests that those net benefits are positive, but vary by degree subject with the highest return captured by medicine, non-biological sciences, social sciences and computing. Given the high return to education to the individual, unless there are benefits to society (social returns) over and above the private returns the argument for the taxpayer to provide extensive subsidies for education seem weak. Such benefits might include those with more education raising the productivity of those working along side them, and social cohesion benefits.

Direct macroeconomic evidence that links growth to education in confounded by the unclear nature of the causal relationship between average schooling levels and measures such as GNP growth. The microeconomic studies that are available confirm this and show how many of the important findings linking education to growth are based on restrictive functional form assumptions. What is needed to solve the issue of this wider impact of education on society is a parallel to the experimental approach adopted in the estimation of private returns. This suggests that within-country rather than between-country analysis may be the route to quantifying the externality from education.

The returns to education may be non-pecuniary. The link between job satisfaction and education is not heavily researched. Evidence presented here based on the BHPS data suggest that contrary to prior assumptions education may be negatively associated with job satisfaction due to the high aspirations that well-educated individuals may have for their careers. However this issue requires more attention than it has been given in the past.

Finally we present evidence on the effect of family background on education decisions. There is considerable evidence suggesting that early school leaving is an important part of the transmission mechanism for poverty in successive generations. By exploiting the correlation between education and schooling-contingent parental income (child support from absent parents and Child Benefit) we find a large and statistically significant effect of income transfers to parents that increases the probability of staying in education past the age of 16 when a relatively parsimonious model is used. While the result is suggestive, we find that it becomes weaker when additional control variables are included in the model and a full evaluation of EMA's would be required to provide the evidence on which to base policy.

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