The Returns to Education
A Review of Evidence, Issues and Deficiencies in the Literature

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Executive Summary

The estimation of the return to a year of schooling, both for the individual and society more generally, has been the focus of considerable debate in the economics literature. Simple analysis of average earnings for different levels of education can mask a number of issues, which stresses the role for multivariate regression analysis. Work based on this approach for the UK typically suggests a return to a year of schooling of between 7% and 9% using a relatively parsimonious specification controlling for schooling and 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. The returns to education may also differ across the wage distribution – for example the returns are higher for those in the top decile of the income distribution compared to those in the bottom decile. 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. In addition, given the increase in the supply of educated workers in most OECD countries there is also a concern that the skills workers bring to their job will exceed the skills required for the job leading to a lower return to 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. It is also 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 lowers the return to schooling by less than one percentage point.

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. 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 - 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. 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. 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.

Given this well-defined and positive return to schooling, unless there are benefits to society (social returns) over and above the private returns there is little argument for the taxpayer to subsidise individual study. The limited evidence for the UK that suggests that the social returns to education may be positive but vary by degree subject with the highest social return captured by medicine, non-biological sciences, social sciences and computing. Direct macroeconomic evidence that links growth to education is 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 that many of the important findings linking education to growth are based on restrictive functional form assumptions. To solve the issue of quantifying the wider impact of education on society, a parallel to the experimental approach adopted in the estimation of private returns is required, possibly suggesting that within-country rather than between-country analysis may be the route to quantifying the externality from education.

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1. Introduction

This paper is concerned with the returns to education. 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 than 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 do 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.

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 the 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: $\sum_{t=1}^{T-s} \frac{w_s - w_{s-1}}{(1+r_s)^t} = w_{s-1} + c_s$ 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 $r_s \approx \frac{w_s - w_{s-1}}{w_{s-1}} \approx \log w_s - \log w_{s-1}$ (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].

The empirical approximation of the human capital theoretical framework is the familiar functional form of the earnings equation $\log w_i = \mathbf{X_i} b + rS_i + d x_i + g x_i^2 + u_i$, where w_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 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.

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, 1998).

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

[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 $Y_s = Y_0 e^{rs} = \mathbf{X}be^{rs}$. 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: $\log w_x = \mathbf{X}b + rs + r h_0 x - (rh_0/2T)\, x^2 + \log(1-h_x)$.

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 paper.

2.2 Optimal schooling choices

It is useful at this point to consider the implications of endogenous schooling. As suggested above, within 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 (assuming an individual can borrow against this rate) 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. Thirdly, 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. Finally, Heckman *et al.* (1999) points to the difference between partial and general equilibrium analysis where in the latter case the gross wage distribution changes in a way which offsets the effect of any policy change through an incidence on the demand side of the market.

[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.

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 than the less able, and this raises the IRR for the more able[3]. 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[4]. On the other hand, the more able may have higher opportunity costs since they may have been able to earn more 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 (1999), 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 (1999) allows the optimal schooling to vary across individuals for a further reason: not only can 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 (Lang, 1993).

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

[3] 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.

[4] Whether schooling and ability are complementary factors in the production of human capital depends on the schooling system. From a policy perspective this is a choice variable. A schooling system in which a considerable amount of resources are spent on remedial teaching will show a different degree of complementarity than a schooling system in which there is more attention given to high ability students.

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 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.

Table 2.1 presents estimates of the rate of return to education based on multivariate (OLS) 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. For example 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 Great Britain, Northern Ireland and the Republic of Ireland have large returns relative to international standards.

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.

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. Furthermore the

ISSP data is designed for qualitative analysis and it seems likely therefore that there may be measurement error in earnings or schooling. As measurement error in general will bias the estimated return to education downward we should be cautious in the interpretation of these results.

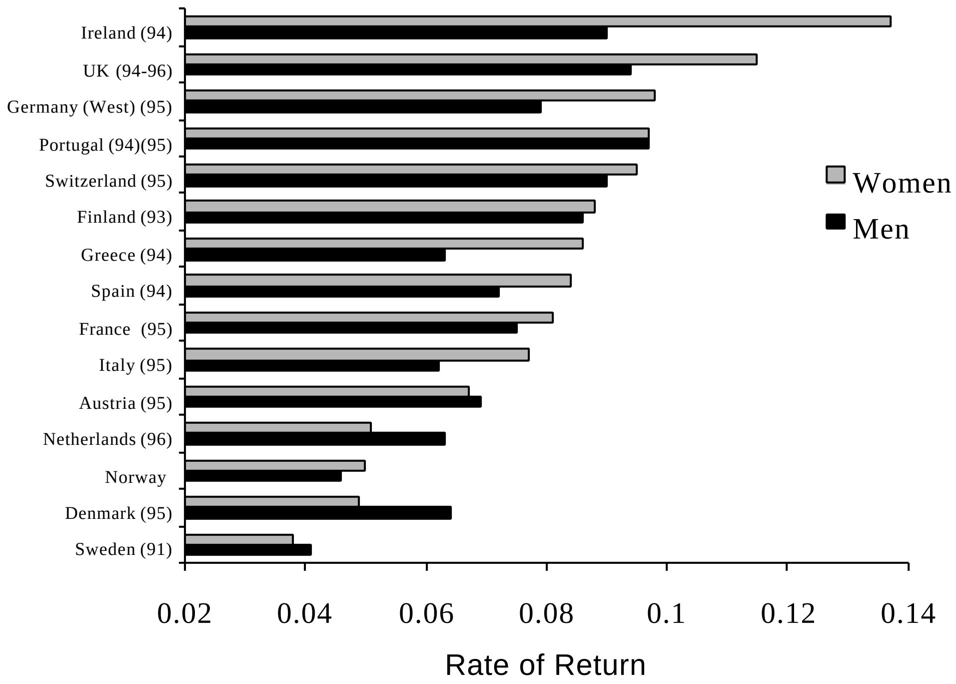
Therefore it might be interesting to consider cross-country rates of return derived from national surveys rather than a single consistent source such as ISSP. Recent results from a pan-EU network of researchers (entitled Public Funding and Private Returns to Education (known as PURE) do precisely this – derive estimates from national datasets in a way that exploits the strengths of each country's 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 schooling and experience should be calculated in a similar fashion. However, since each country uses its 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 2.1 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 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.

[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 Table 2.1 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).

Figure 2.1: Returns to Schooling in Europe, Men And Women (Year Closest to 1995)



2.4 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 $\log w = \mathbf{X}b + rS + g(x) + e$ where $\mathbf{X}$ 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 (see Kjellstrom and Bjorklund, 2001).

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].

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

Table 2.2 illustrates the effect of including different experience measures in schooling returns estimation. In this table we report OLS estimates controlling for different definitions of experience using our European estimates of the returns to schooling. Using a quadratic in age tends to produce the lowest returns. Using potential experience (age minus education leaving age) or actual experience (typically recorded 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. For example, the estimates for the UK are 10% for men and 12% for women compared to 8% and 11% respectively when age is used as the proxy for experience. 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: 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 2.3 and 2.4 we estimate for men and women the return to schooling using the British Household Panel Survey (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 changes in specification.

[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

[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.

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 non-participation is thought to be much less common for men than women.

Table 2.3: 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 2.4: 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.

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 2.5: UK 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)

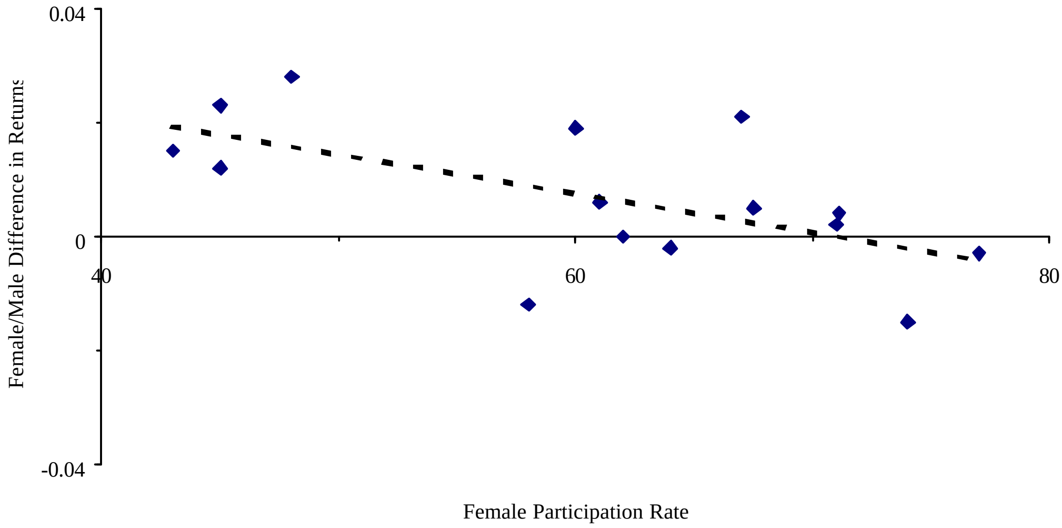
Note: Figures in parentheses are robust standard errors. The models include year dummies, marital status, and the number of children in three age ranges, region dummies, and regional unemployment rate. In the Heckman two-step case we use household unearned income as well as the variables from the wage equation in the participation equation.

Table 2.5 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 2.2 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.

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

Figure 2.2: Female/Male Differentials in Returns and Female Participation Rate



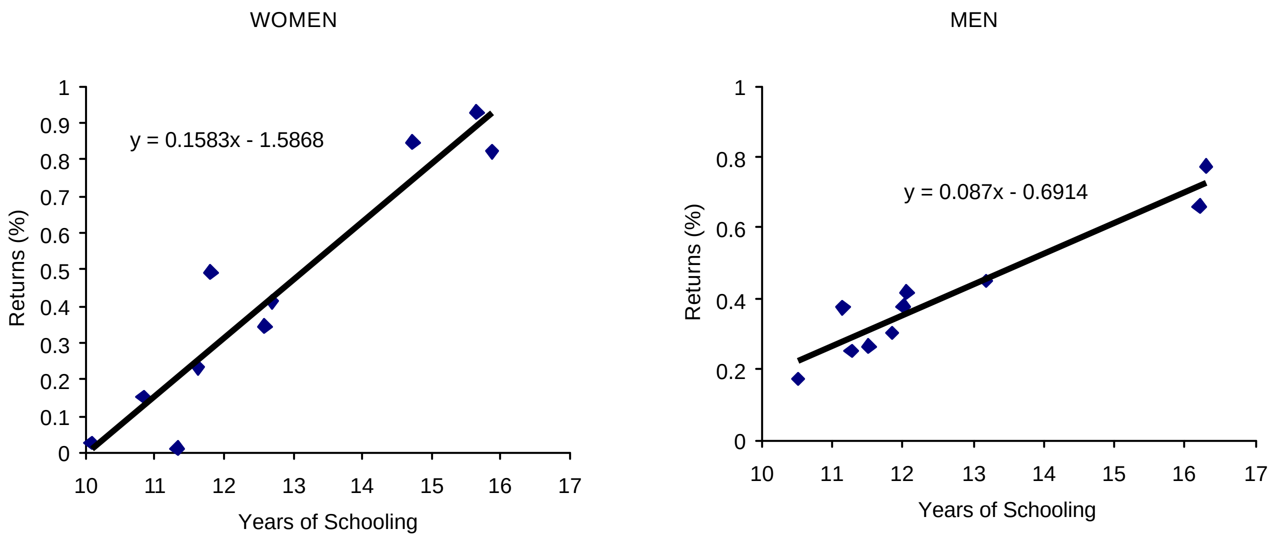
2.5 Alternative measures of schooling attainment

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 2.3 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[11][12].

Figure 2.3: Estimated Returns to Qualifications – BHPS



2.6 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 (Buchinsky, 1994). 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

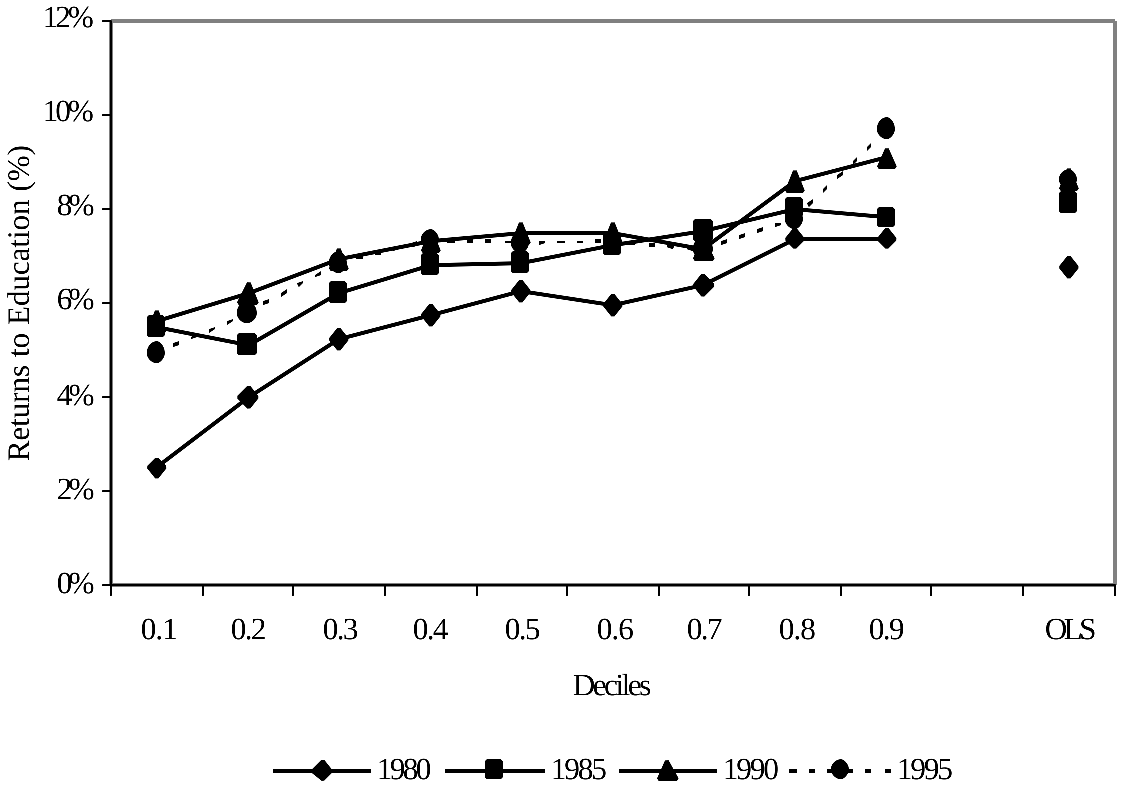
[11] Note that Figure 2.3 simply groups the wage and schooling data by highest qualification and therefore does not control for other differences across groups, such as age. Since age is positively correlated with wage and negatively with education this omission is likely to cause the least squares estimates of the returns from the grouped data to be biased upwards.

[12] Krueger and Lindahl (1999) present comparable figures for US, Sweden and Germany.

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 2.4 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 2.4 Quantile Regressions for GB: FES Men



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

Table 2.6 is also based on the work of the PURE[13] 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.

Table 2.6: Quantile Regressions

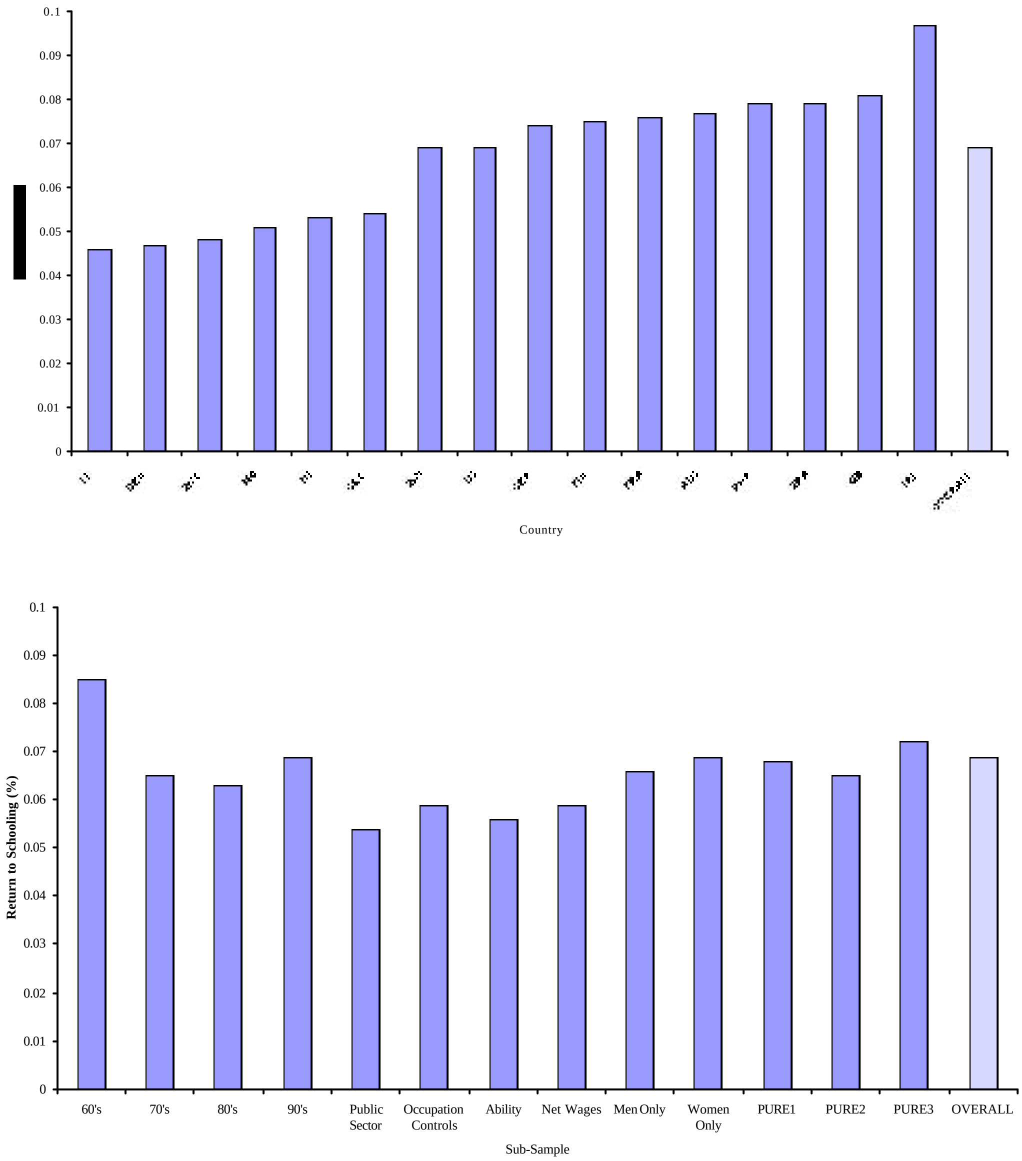
	Year	1st dec.	9th dec.	OLS	Year	1st dec.	9th dec.	OLS
Austria	1981	9.2	12.6	10.5	1993	7.2	12.8	9.7
Denmark	1980	4.7	5.3	4.6	1995	6.3	7.1	6.6
Finland	1987	7.3	10.3	9.5	1993	6.8	10.1	8.9
France	1977	5.6	9.8	7.5	1993	5.9	9.3	7.6
Germany	1984	9.4	8.4		1995	8.5	7.5	
Greece	1974	6.5	5.4	5.8	1994	7.5	5.6	6.5
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

2.7 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).

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

Figure 2.5: Returns to Schooling – A Meta Analysis



In Figure 2.5 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[14] 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

[14] However it should be noted that these are not independent estimates. For example multiple estimates of the return to education may be retrieved from a single study within a country. See Krueger (2000) for a discussion of the implication of this in meta-analysis using class size effects.

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 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).

A number of points emerge from the figure. Despite the issues raised earlier in this paper 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 a 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%. Estimates produced using samples from the 1960's also seem to have produced higher than average returns.

2.8 Other sources of variation in returns: overeducation

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. The literature started in the US in the late 1970's with Freeman's *The Overeducated American*. Duncan and Hoffman (1981) is the first paper that proposed the now popular specification of the wage equation that disaggregates total years of schooling into 'required' years and the surplus, which defines 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 papered 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 overeducation 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-papered methods. The average return to overeducation 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 hypothesis, 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 hypothesis 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 hypothesis 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 overeducation on earnings. A sample of two cohorts of UK graduates is used collected by a postal survey organised by the University of Birmingham in 1996 among graduates from 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 overeducation 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 overeducation 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 for both human capital reasons 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. There is little evidence available in the literature and the paucity of the literature is testament to the difficulty of the problem.

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.

In Table 3.1 British Household Panel Survey data 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.

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[15].

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 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.

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[16]. 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 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.

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

[16] See Harmon and Walker (2000) for more details.

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

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 that only one of the two earnings levels of interest are observed and the other is unobserved.

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 the outcome. 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 wages. 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 $\log w_i = X_i b + rS_i + u_i$ where $S_i = \mathbf{Z_i}'\alpha + v_i$. Estimation of the log wage equation by OLS will yield an unbiased estimate of b 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[17] as the schooling coefficient b captures some of the effects that would otherwise be attributed to the omitted ability variable. For

[17] 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.

instance, if the omitted variable is motivation, and if both schooling and earnings are 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 $\mathbf{X_i}$ [18] 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 random reasons (*i.e.* reasons that are uncorrelated with the wages eventually earned) such as smoking when young which, as we suggest above, is associated with one's rate of time preference.

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).

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

Figure 4.1: Meta-Analysis of Models with Endogenous Schooling

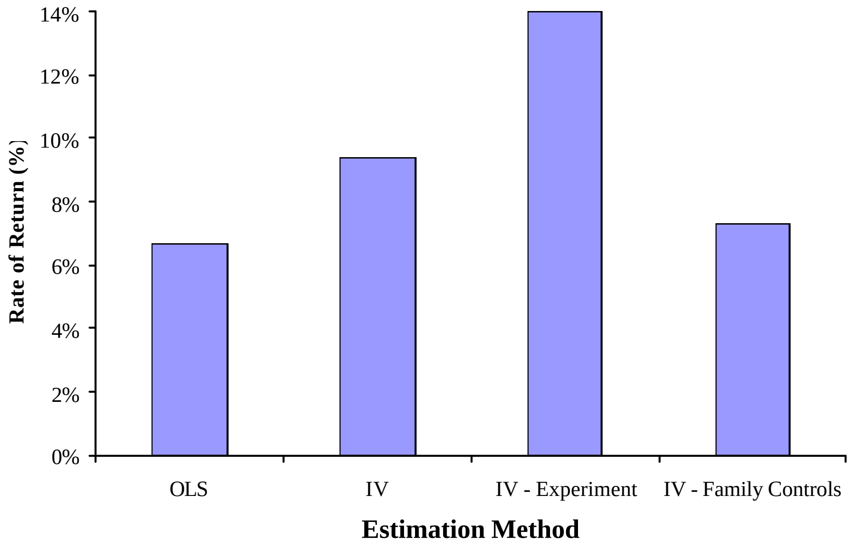


Table 4.3 outlines some of the key results in this literature. Angrist and Krueger (1991) use the presence of compulsory schooling law variation across US states and the quarter of the year in which a person was born as the basis of their instruments. The underlying idea here is that a person who has 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[19].

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

[19] 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.

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.

Table 4.3: IV Studies

Study	Sample	*OLS* %	*IV* %	*Instruments*
Angrist and Krueger (1991)	US 1970/1980 Census: Men born 1920-29, 1930-39, 1940-49	6.3 (0.000)	8.1 (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 (1999)	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 (2000b)	Ireland - ESRI 1987 Data – Males	8.0 (0.006)	13.6 (0.025)	Irish school reforms – abolition of fees for secondary schooling.
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.

Note : Standard Errors in parentheses

Uusitalo (1999) 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 45%, again statistically significant. A somewhat different approach is used in the paper by 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 that affected the entire population of school-age individuals in a way which differed across socio-economic backgrounds[20].

There are a small number of examples in the UK literature using this approach that are also summarised in Table 4.3. 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: 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.

[20] The natural experiment usually ignores 'spillover' effects of the treatment. If, for example, the school leaving age is raised, those that leave school just before the increase belong to a group of low educated people who have no competition from younger cohorts with the same low level of schooling. This may increase the wages of this group and lead to a bias in the estimated return to a year of schooling. See Philipson (2000) for further discussion of this argument.

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 4.4 we paper 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[21] but is correlated with educational choices. In the table we see larger estimated returns from the IV estimations than the OLS results.

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 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?

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*. Similar research by Lang (1993) also considers this issue of heterogeneous returns, sometimes labeled 'discount rate bias'.

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 effects for the marginal groups hit

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

by the treatment known as Local Average Treatment Effects (or LATE – see Imbens and Angrist, 1994[22]).

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.

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.

[22] 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.

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 the 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 4.5 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 4.5: Twins/Siblings Research on Schooling Returns

Author	Data	OLS	IV
Ashenfelter and Rouse (1999)	Princeton Twins Survey	7.8%	10%
Rouse (1999)	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 than 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 Bound and Solon (1998) 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 papering 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 papered 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-papered 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 Miller *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) papering a higher level of schooling in later waves of the data, undermining the assumption that schooling can be thought of as a fixed effect[23].

[23] 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.

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 under-investing 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 is 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[24].

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% (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%, 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

[24] Other distortions may influence the decision to subsidise educational investments. For example without subsidy a progressive tax system will act as a disincentive to participation in education. Moreover

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

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 DS 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.

The idea that growth rates should converge is a feature of many macro-studies – those below their steady-state growth rate should catch up with those above. That is $\Delta W_j = b\left(W_{jt-1} - W_j^*\right) + u_j$ where W = log of w and W^* is the steady state level of GDP (per capita). Then the macro growth equation would become $\Delta W_j = bW_{jt-1} + rS_{j,t-1} + ... + e_j$ 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 DS, 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 $\Delta W_j = bW_{jt-1} + rS_{j,t-1} + ... + e_j$ 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 of 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 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[26].

A literature is beginning in this vein but unfortunately the evidence is already conflicting. Moretti (1998) 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, 1999) 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 (1998), 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

[26] Of course finding the average rate of return across a number of countries may not be informative from a policy perspective. Education policy, which is meant to be informed by this cross-country result, is formulated at the level of countries.

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 is 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). Raphael and Winter-Ebmer (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. 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 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 papered 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. Given this high return unless there are benefits to society (social returns) over and above the private returns there is little argument for the taxpayer to subsidise individual study. The limited evidence for the UK that suggests that the social returns to education may be positive but vary by degree subject with the highest social return captured by medicine, non-biological sciences, social sciences and computing.

Direct macroeconomic evidence that links growth to education is 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.

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