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THE RETURNS TO EDUCATION: MICROECONOMICS

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Abstract. In this paper we focus on education as a private decision to invest in 'human capital' and the estimation of the rate of return to that private investment. While the literature is replete with studies that estimate the 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. We outline the theoretical arguments underpinning the empirical developments and show that the evidence on private returns to the individual is compelling. Despite some of these issues surrounding the estimation of the return to schooling, our evidence, based on estimates from a variety of datasets and specifications, is that there is 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.

Keywords. Returns to education; Endogeneity; Signalling; Human capital

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. 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. 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

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a role for intervention. Another argument for intervention would be the existence of externalities associated with the educational attainment of individuals and this is the focus of the contribution in this issue by Sianesi and Van Reenen (2002).

In the next section we outline the theoretical arguments underpinning the empirical developments then pay particular attention to a number of the most important empirical difficulties. In section three we review some existing work and explore estimates from a variety of UK datasets and specifications. Section 4 considers the signalling role of education and the effect of credentials on wages. A related issue, discussed in section 5, 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. Finally, in section 6 we conclude.

2. The Human Capital Framework and the Returns to Schooling

2.1. *The Mincer Specification*

The analysis of the demand for education has been driven by the concept of human capital, 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 w, up to retirement at date T, net of the costs of education, c_s. So, at the optimum s, the PV of the sth year of schooling equals the costs of the sth year of education, and 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 sth year of schooling if $r_s > i$, the market rate of interest. If T is large then the left hand side of the equilibrium relationship 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

$$r_s \approx \frac{w_s - w_{s-1}}{w_{s-1}} \approx \log w_s - \log w_{s-1}$$

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(where $\approx$ means approximately equal to). This says that the return to the sth 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.

The empirical approximation of the human capital theoretical framework is the familiar functional form of the earnings equation

$$\log w_i = \mathbf{X}_i\beta + rs_i + \delta x_i + \gamma x_i^2 + u_i,$$

where 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, $\mathbf{X_i}$ 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 experience 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 estimate this 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%. Psacharopolous and Layard (1979) used the GB GHS 1972 data and found returns to schooling of a similar level, around 10%. See Willis (1986) and Psacharopolous (1994) for many more examples of this simple specification[1].

2.2. *Optimal Schooling Choices*

In the empirical work discussed above the schooling measure is treated as exogenous, although education is clearly an endogenous choice variable in the underlying human capital theory. It is useful therefore 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

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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 opportunity cost of schooling should fall (say, through a tuition subsidy or maintenance grant) the net effect on the incentive to invest in schooling should be positive. Fourthly, more schooling may imply a greater likelihood of receiving work related training while in employment (Blundell *et al.*, 1996), if formal education and on the job training are complements. Fifthly, there may be non-pecuniary benefits associated with education including those associated with having a more highly skilled job, such as status, not reflected in wages (Chevalier and Lydon, 2001). Finally, Heckman *et al.* (1999) point to the difference between partial and general equilibrium analysis where in the latter case the gross wage distribution changes in a way which partially offsets the effect of any policy change through an incidence on the demand side of the market. Thus, unless labour demand is perfectly elastic for all types of labour, then increases in individual incentives to invest in schooling, given the existing wage distribution, would be offset by changes in that distribution when the supply of educated labour increase and that of less educated labour falls.

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 from government or 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

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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, or lower marginal costs of, education choose higher levels of schooling. If there is no discount rate variance then the endogeneity will arise solely from the correlation between ability and education and since this is likely to be positive the bias in OLS estimates will be upwards (if ability increases wages later in life more than it increases wages early in life). If there is no ability variance, then the endogeneity arises solely from the (negative) correlation between discount rates and the amount of education 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. *Ability Bias*

In the Mincer specification the disturbance term captures unobservable individual effects and these individual factors may also influence the schooling decision, and hence 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 empirical 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[5]. 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

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are additive and common within twins so that they can be differenced out by regressing the wage difference within twins against their education differences. A final approach deals directly with the simultaneous relationship between schooling and earnings by specifying a two-equation system which is identified by exploiting instrumental variables that affect s but not w. We return to these in detail later in this paper.

3. Regression Analysis

3.1. *Comparative Analysis*

Because wages are determined by a variety of variables, some of which will be correlated with each other as well as with wages, 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 1 presents estimates of the rate of return to education based on multivariate (OLS) analysis from the International

Table 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.
Source: Trostel, Walker and Wooley (2002). Regression specification includes controls for age and age squared, and union status.

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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 1 we apply exactly the same estimation methods to data that has been constructed to be closely comparable across countries. The results show wide cross country variation.

These estimates have the advantage that they are all derived from common data that makes them broadly 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 will, in general, bias the estimated return to education downward we should be cautious in the interpretation of these results[6]. Therefore it might be interesting to consider cross-country rates of return derived from national surveys rather than a single consistent source such as like ISSP. Recent results from a pan-EU network of researchers (entitled Public 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 countries data. The main objective was to evaluate the private returns to education by estimating the relationship between wages and education across Europe. In a cross-country project it is preferable that data is reasonably comparable across countries, i.e. wage, years of 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[7].

Figure 1 is a summary of the returns broken down by gender. These are obtained from a parsimonious specification containing years of schooling and a quadratic in age alone. 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. Ireland and the UK are close to the top of the estimated returns in this cross-country review.

3.2. *Specification and Functional Form*

Mincer's specification can be thought of as an approximation to a more general function of schooling (s) and experience (x) of the form: $\log w = F(s, x) + e$ where e is a random term that captures other (unobservable) determinants of wages. Many variants of the form of $F(.)$ have been tried. Murphy and Welch (1990), for example, concluded that $\log w = \mathbf{X}\beta + rs + g(x) + e$ where $\mathbf{X}$ 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.

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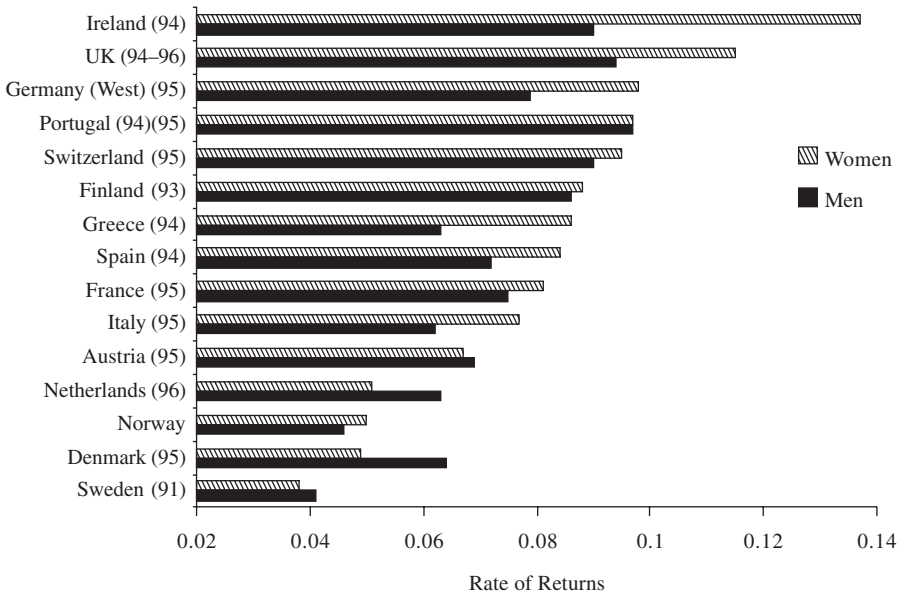

Figure 1. Returns to schooling in Europe, men and women (year closest to 1995).

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

However, there are few examples in the empirical literature that consider whether the way in which x enters the model has any substantial impact on the estimated schooling coefficient. Kjellstrom and Björklund (2001) show no impact using Swedish data while Heckman, Lochner and Todd (2001) using US census data suggest that the failure to allow for interactions between experience and schooling has important implications for the estimated rate of return to schooling, at least for recent census data.

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 experience constant is simply r, while the age-control specification implies that the estimate of the impact 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[8].

Table 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

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Table 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).

full-time work since leaving full-time education) indicates a slightly higher return to education. For example, the estimates for the UK using FRS data 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[9].

Other changes in specification generally do not lead to major changes in the estimated return to schooling. For example in Table 3 and Table 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[10]. As can be seen the results here are very robust to these changes in specification.

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. However the argument is becoming less true in recent cohorts. A simple way might be to use standard 'two-step' estimation methods as proposed by Heckman and Polachek (1974), which attempt to control for the selection by modelling what determines it. Table 5 shows the parameter estimates for women using BHPS and FRS. The results suggest a small effect due to the selection into employment. While selection is

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Table 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.

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Table 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.

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Table 5. UK BHPS and FRS: OLS, Heckman Selection.

	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)

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.

statistically significant the differences in the education returns are small in absolute value and insignificant.

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 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

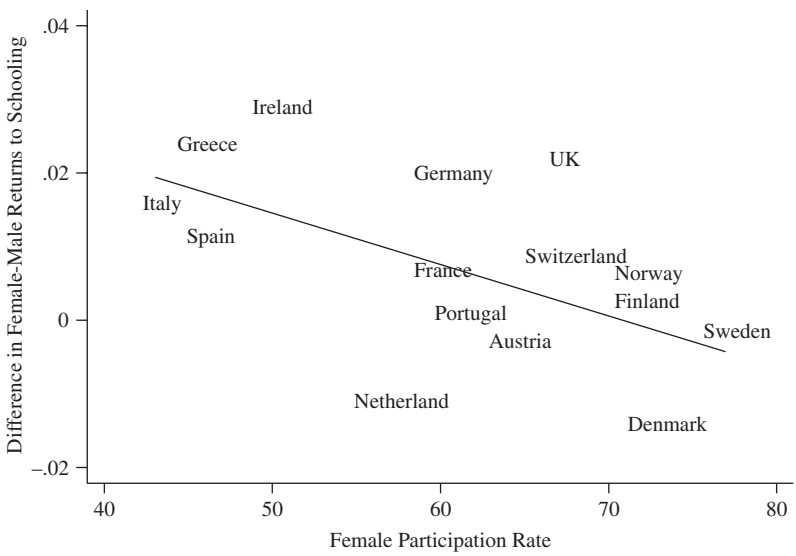

Figure 2. Female/Male Differentials in Returns and Female Participation Rate.

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

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the lowest differences in schooling returns while the countries with the lowest participation (typically the Mediterranean economies) have the amongst the largest. Ireland and the United Kingdom (and to a lesser extent Germany) are outliers in this regard in having relatively large gaps in returns across genders while being middle-ranked in terms of participation. From the perspective of the researcher however this may suggest some potential for bias from using samples of participants alone but it appears not to be a large problem. However, except for countries with high female participation rates, the issue merits more attention than it has received in the literature to date.

3.5. *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 education routes 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 (1996) 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 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 and that the average returns to a year of schooling is about 16% for women and 9% for men[11,12].

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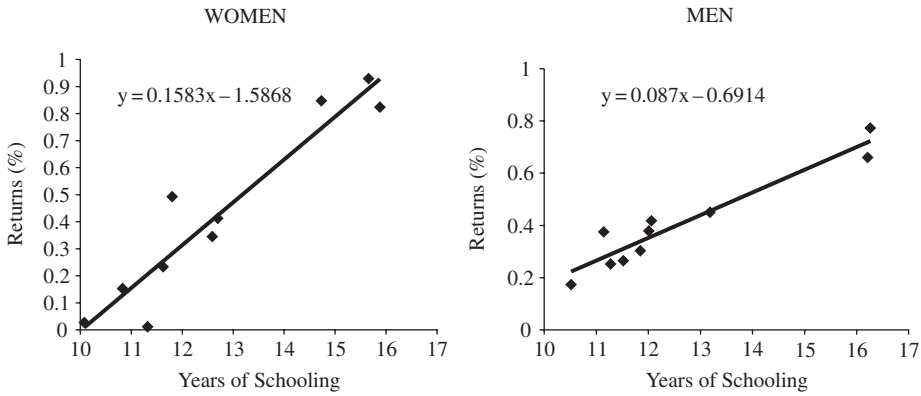

Figure 3. Estimated Returns to Qualifications — BHPS.

3.6. *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 passes through the mean of the sample. An alternative methodology to OLS is available known as quantile regression (QR) which, while based on the entire sample available, allows us to estimate the return to education within different quantiles of the wage distribution (Buchinsky, 1994) by weighting observations in an appropriate way. While OLS captures the effect of education on someone on the mean wage, the idea behind QR is to look at the returns at some other part of the wage distribution, say the bottom quartile. Then comparing the estimated returns across the whole of the wage distribution we can infer the extent to which education exacerbates or reduces underlying inequality.

Of course, the method requires that there is a sufficiently wide spread of education that we can identify the returns for each decile — we require that some in the top deciles have low education and some in the bottom deciles have high education. The UK data appears to be satisfactory in this respect and we find that the return is statistically significant for each decile, and we also find that the top decile is significantly higher than the bottom decile. The method is fully flexible and allows the returns in each decile to be independent of any other decile. Our simple specification does restrict the returns to be the same for everyone within the decile group — just as our OLS linear specification restricted the returns to be the same for the whole sample.

Figure 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

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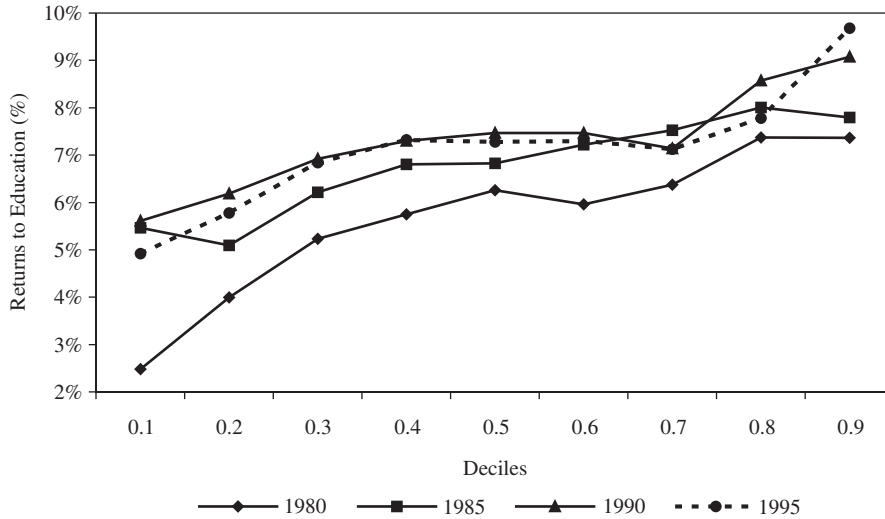

Figure 4. Quantile Regressions for GB: FES Men.

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

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). Although the differences are not large the returns at the bottom of the distribution do appear to have risen across this period which is shown by the graph getting flatter[13]. There is also 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 should predominate in the bottom half of the distribution. Thus one explanation for this figure is that education has a bigger impact on the more able than the less able and this 'complementarity' between ability and education seems to have become larger over time[14].

Table 6, based on the work of the PURE research group subsequently published by Pereira and Silva-Martins (2002), suggest that in most countries and for most years there is a complementarity between education and ability and that this is either getting stronger or, at least, no weaker over time.

3.7. *Meta Analysis*

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

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Table 6. Quantile Regressions.

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

Source: Pereira and Silva-Martins (2002).

several studies that share a common aspect so as to be 'combinable' in a statistical manner. The methodology is typical in the clinical trials in the medical literature. In its simplest form the computation of the average return across a number of studies is now achieved by weighting the contribution of an individual study to the average on the basis of the standard error of the estimate (see Ashenfelter, Harmon and Oosterbeek (1999) for further details).

In Figure 5a and Figure 5b 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. Some 1010 estimates were generated across the PURE project[15] on three main types of estimated return to schooling — existing published work, existing unpublished work, and new estimates produced for the PURE project. Each block refers to a different sample of studies that share some characteristic (for example, 'US' indicates only studies based on US data, 'Net wages' indicates that the dependent variable was net rather than gross wages, 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 both average about 5%. Estimates produced using samples from the 1960's also seem to have produced higher than average returns.

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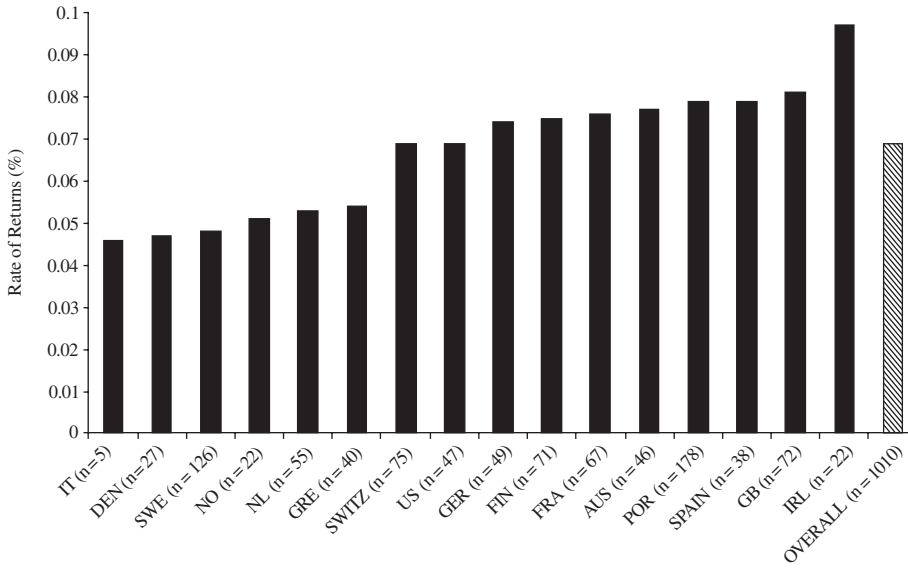

Figure 5a. Meta Analysis — Cross Country.

Source: Harmon, Walker and Westergaard-Nielson (2001).

3.8. *Other Sources of Variation in Returns: Over-Education*

Given the increase in the supply of educated workers in most OECD countries in the last two decades a concern has arisen in the schooling returns literature that if growth in the supply of educated workers outpaces the demand for these workers, overeducation in the workforce is the likely result. In other words the skills workers 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 surplus to those needed for the job. In order to analyse this issue total years of schooling for individuals must be split into required years and surplus years of education. The difference in the returns to these measures is a measure of overeducation.

There are a number of ways of measuring overeducation: subjective definitions based on self-reported responses to a direct question to workers on whether they are overeducated; or the difference between actual schooling of the worker and the schooling needed for their job as reported by the worker. Clearly these are subjective and may be subject to measurement error. Moreover the educational requirement for new workers may exceed those of older workers in a given firm since inexperience needs to be compensated for by higher education. 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

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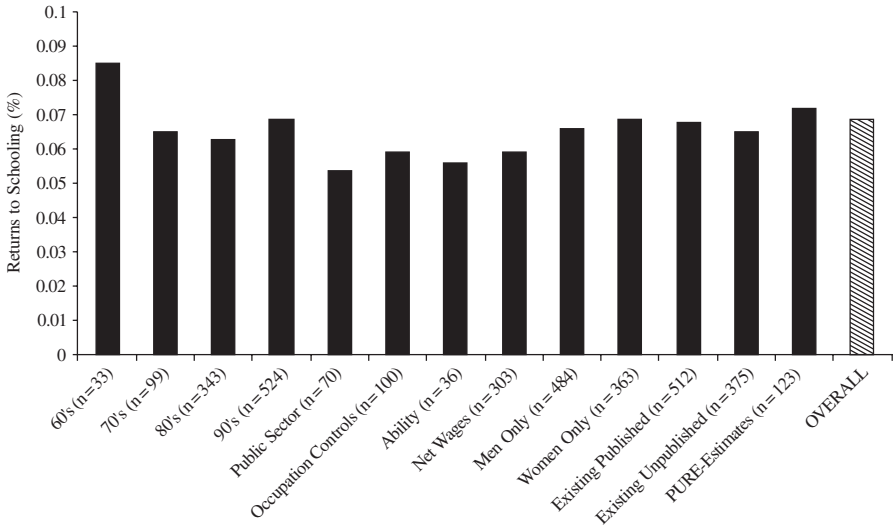

Figure 5b. Meta Analysis — Varying the Samples of Estimated Returns.

Source: Harmon, Walker and Westergaard-Nielson (2001). The time periods referred to in classifications 60's/70's/80's and 90's refer to the data years used to estimate the models. PUBLIC SECTOR refers to estimates solely for public sector workers, OCCUPATION CONTROLS refers to estimates where such controls are included in the specification, ABILITY is where explicit ability controls are included in the specification, NET WAGES is where the dependent variable refers to net rather than gross wages, MEN/WOMEN only refer to specifications based exclusively on male and female samples, EXISTING PUBLISHED/UNPUBLISHED and PURE-ESTIMATES refer to estimated returns to schooling from existing published and unpublished work across different countries and estimates generated exclusively for PURE based on a simple schooling/age/age squared specification.

classification for the occupation, which may mix workers in jobs requiring different levels of education depending on how tightly defined the industry classification is. Moreover required levels of education are typically the *minimum* required and not necessarily indicative of the level of education of the successful candidate.

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

Dolton and Vignoles (2000) test three hypotheses regarding overeducation for the UK graduate labour market based on the National Survey of 1980 Graduates

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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 overestimation of the extent and effect of over-education on earnings. A sample of two cohorts of UK graduates is used collected by a postal survey organised by the University of Birmingham in 1996 among graduates from 30 higher education institutions covering the range of UK institutions. Graduates from the 1985 and 1990 cohorts were selected, leading to a sample of 18,000 individuals. By using measures of job satisfaction this study is able to sub-divide those considered 'over-educated' into 'apparently' and 'genuinely' over-educated. The apparently over-qualified group is paid nearly 6% less than well-matched graduates but this pay penalty disappears when a measure of ability is introduced. Genuinely over-qualified graduates have a reduced probability of getting training and suffer from a pay penalty reaching as high as 33%. Thus genuine over-education appears to be associated with a lack of skills that can explain 30% to 40% of the pay differential so that much of what is normally defined as over-education is more apparent than real.

4. Signalling

The literature has been dominated by human capital theory and the econometric analysis has been interpreted within this framework. An alternative literature asserts that it calculates 'social' returns by calculating the present value of costs and benefits of education net of taxes and subsidies from assumptions about what the private gross returns might be (see, for example, OECD (2001) for these computations across countries). In fact, this is nothing more that the private return adjusted for tuition costs and tax liabilities.

However, an important concern is that education may have a value in the labour market not because of any effect on productivity but for 'spurious' reasons. In particular, education may act as a signal of ability (or other characteristics

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that employers value because it contributes to productivity but which they cannot easily observe). Suppose employers believe that education is correlated with productivity, then this will be confirmed by their experience if it is the case that high productivity individuals choose high levels of education. This will be true if the costs of acquiring education is sufficiently lower for high productivity individuals than it is for low productivity individuals. Thus, the market will be characterised by a separating equilibrium where high productivity individuals choose high levels of education in these specific conditions. The theory is largely due to Spence (1973) and the subsequent literature has recently been reviewed by Riley (2001)[16].

There is a fundamental difficulty in unravelling the extent to which education is a signal of existing productivity as opposed to enhancing productivity: both suggest that there is a positive correlation between earnings and education, but for very different reasons. There are several approaches to finessing this problem. One 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 (see Brown and Sessions, 1998). Less convincingly it has been suggested that employees in the public sector can be paid a wage that differs from productivity because the absence of free entry does not impose the constraints of competition. Of course in the signalling model the difference between wages for educated versus less educated individuals does not have to exactly reflect differences in productivity but has to be sufficiently positive to generate self selection (see Psacharopoulos, 1983). 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.

In Table 7 we report results based on British Household Panel Survey data. The OLS results here suggest quite comparable rates of return and imply that the signalling component is quite small. A potential 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. BHPS 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 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.

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Table 7. Signalling — Returns for Employed vs. Self-Employed — BHPS.

	Employees		Self-employed		
	Return	N	Return	N	Signalling value
BHPS–OLS					
Men	0.0641 (0.002)	10001	0.0514 (0.008)	1717	0.0131 (0.012)
Women	0.1027 (0.002)	9550	0.0763 (0.015)	563	0.0264 (0.019)
BHPS–Heckman					
Men	0.0691 (0.003)	10001	0.0552 (0.022)	1717	0.0139 (0.025)
Women	0.1032 (0.002)	9550	0.0784 (0.066)	563	0.0248 (0.070)

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

The National Child Development Survey (NCDS) is a cohort study of all individuals born in England and Wales in a particular week in 1958 whose early development was followed closely and whose subsequent labour market careers have been recorded including earnings. Various ability tests were conducted at 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.

In Table 8 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

Table 8. 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.

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and women separately. We compare these results using controls at age 11 and at age 16 in NCDS. Finally, we use the ability information taken at the current age in 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 exhibits a rather small difference and suggests little signalling value to education.

5. Endogenous Schooling

5.1. *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 (Rubin, 1974).

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 but there may be real-world events, or 'natural experiments', which can be arguably considered as assigning individuals randomly to different outcomes. The essence of this 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 age 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 9 shows the relevant wages.

Table 9. 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.

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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 effect is the impact 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 in a natural experiment is chosen, not randomly, but independently of any characteristics that affect wages. 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 10 shows that by examining these differences between groups the estimated return to schooling is around 16% for men and 18% for women.

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

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Table 10. 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	

(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 $\log w_i = \mathbf{X}_i\beta + rs_i + u_i$ where $s_i = \mathbf{Z_i}'\alpha + v_i$. Estimation of the log wage equation by OLS will yield an unbiased estimate of β only if 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 ability measures, such as IQ are possible examples. It is important to note that even a very extensive list of variables included in the vector **X** will never be exhaustive. So estimates of the return to schooling based on OLS will not give the causal effect of schooling on earnings[17] as the schooling coefficient β captures some of the effects that would otherwise be attributed to the omitted variables. For 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 value of 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 to suppose 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 $\mathbf{X}_i$ (see the discussion in Heckman (1990) for further details). 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

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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.

5.2. *Results from IV Studies — International Evidence*

In Figure 6 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 (n = 863) of 6.5% we see much larger returns to schooling in IV studies (n = 79) generally (of about 9%) and from IV studies based on education reforms in particular (n = 17, around 13 to 14%) — close to the difference in differences estimates presented earlier. 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).

Table 11 outlines some of the results of the key papers 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

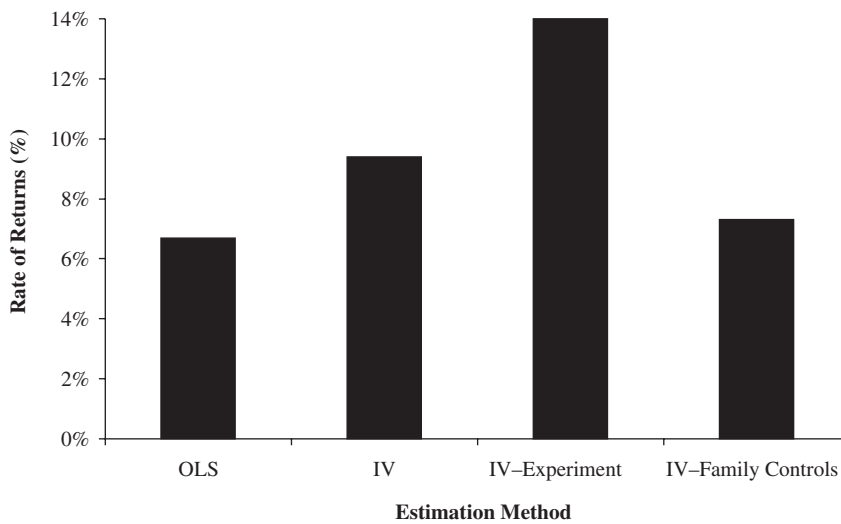

Figure 6. Meta-Analysis of Models with Endogenous Schooling.

Source: Harmon, Walker and Westergaard-Nielson (2001). Total number of estimates = 942.

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Table 11. IV Studies.

Study	Sample	OLS	IV	Instruments
Angrist and Krueger (1991)	US 1970/1980 Census: Men born 1920–29, 1930–39, 1940–49	0.063 (0.000)	0.081 (0.033)	Year * Quarter of Birth; State * Quarter of Birth
Angrist and Krueger (1992)	US 1979–85 CPS: Men born 1944–53 (potential Vietnam War draftees).	0.059 (0.001)	0.066 (0.015)	Draft Lottery Number * Year of Birth
Card (1995)	US NLS: Men aged 14–24 in 1966 sampled as employed in 1976.	0.073 (0.004)	0.132 (0.049)	Nearby college in county of residence in 1966.
Butcher and Case (1994)	US PSID 1985: White women aged 24 +	0.091 (0.007)	0.185 (0.113)	Presence of siblings (sisters)
Uusitalo (1999)	Finnish Defence Forces Basic Ability Test Data matched to Finnish income tax registers.	0.089 (0.006)	0.129 (0.018)	Parental income and education, location of residence.
Meghir and Palme (1999)	Sweden — Males	0.028 (0.007)	0.036 (0.021)	Swedish curriculum reforms.
Duflo (1999)	Indonesian — Males	0.077 (0.001)	0.091 (0.023)	Indonesian school building project.

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Denny and Harmon (2000)	Ireland — ESRI 1987 Data — Males	0.080 (0.006)	0.136 (0.025)	Irish school reforms — abolition of fees for secondary schooling.
Dearden (1998)	UK NCDS: *Men*	0.048 (0.004)	0.055 (0.005)	Family composition, parental education
Harmon and Walker (1995)	UK FES 78–86. *Males 16–64*.	0.061 (0.001)	0.152 (0.015)	School leaving age changes.
Harmon and Walker (1999)	UK GHS 92. *Males 16–64*.	0.049 (0.000)	0.140 (0.005)	School leaving age changes and educational reforms.
Harmon and Walker (2000)	UK NCDS: *Men*	0.050 (0.005)	0.099 (0.019)	Family background.
Pons and Gonzalo (2001)	Spain: *Males 16–64*	0.064 (0.004)	0.107 (0.010)	Education policy interventions, family background, season of birth.

Note: Standard Errors in parentheses. See Card (2001) for additional comment.

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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[18].

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 instrument was the random number given to individuals in the lottery draft used to conscript young men to fight in Vietnam. 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 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. He finds returns of 13.2% compared to OLS estimates of closer to 7%. However again the estimates were rather imprecise. Butcher and Case (1994), in one of the few examples based on a sample of women, find that women who grew up in households with a sister obtained less education than women raised only with brothers. Using this as an instrument they again find IV exceeding OLS and in fact the estimated return more than doubles in this study. Uusitalo (1999) uses the fact that all eligible Finnish males must complete military service where aptitude tests are undertaken. By matching this data to income tax registers his study estimates earnings equations for males based on parental background instruments. 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 birth cohort and the intensity of the treatment depended on the district they lived in (as investment was targeted at regions were enrollment was historically low). 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. Similarly Denny and Harmon (2000) look at a fundamental change in the Irish educational system in the 1960's, which affected the entire population of school-age individuals in a way which differed across socio-economic backgrounds. Finally Pons and Gonzalo (2001) estimate the return to schooling for Spain using instruments based on education policy interventions, family background variables and season of birth instruments and find that the return to schooling rises from an OLS estimate of 6.4% to an IV return of 10.7%[19].

There are a small number of examples in the UK literature using this approach which are also summarised in Table 11. 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 and found

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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 introduction of student maintenance awards). Across a number of UK 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.

The UK differences between IV and OLS here are clearly large, and support the international evidence that we have. While these IV results concur with the simple Wald estimates earlier it is, nevertheless, important that this difference is subjected to more detailed examination. In Table 12 we show results from a number of datasets and specifications that use smoking status as an instrument as in Evans and Montgomery (1994), 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. It is assumed that smoking at age 16 is not correlated with current earnings but is correlated with educational choices. In the table we see larger estimated returns from the IV estimations than the OLS results for GHS. Very large returns are obtained when current smoking is used compared with the more modest increases when smoking at 16 is used. This is likely to be because current smoking and current income are correlated which invalidate current smoking as in instrumental variable. One objection to using smoking at 16 as an IV is that it may be correlated with current wages via its association with parental background. Thus, in the final block of Table 12 we also control for family background although we find that the estimated return is similar to the previous block with no family controls.

5.3. *Why are the IV Estimates Higher than OLS?*

In the Card (1999) 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 a weighted return, where the weights reflect the extent to which the subgroup is affected by the treatment or 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 in the context of heterogeneity arising from differences in discount rates.

Given this interpretation it is then clear that in this respect the IV estimator can exceed the conventional OLS estimator if the intervention affects a subgroup with

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Table 12. Further IV Results — Smoking as an Instrument.

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

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.

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relatively high marginal return to schooling. In Card's (1999) model this is possible as low amounts of schooling can imply higher marginal returns to schooling if the variation in ability is small relative to the variation in the discount rate. If the intervention affects those with below-average schooling levels the IV estimate will be larger than the 'average' OLS result reflecting the higher discount rate for those with low schooling. This is suggested as a rationale for the results in, for example, Angrist and Krueger (1991) concerning compulsory schooling laws, and is a specific example of the more general issue of estimating effects for the marginal groups hit by the treatment known as Local Average Treatment Effects (or LATE — see Imbens and Angrist, 1994).

Moreover if the 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. 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 (1994) calculates the reliability ratio (the ratio of variance of the measurement error to total variance in schooling) of years of schooling 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 measurement error being therefore relatively minor problems in these studies, both find large and significant downward bias in least squares estimates. On this evidence measurement error appears an unlikely candidate for explaining the IV/OLS difference.

5.4. *Instrument Relevance and Instrument Validity*

Bound *et al.* (1995) urge 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 consistency of IV depends on the assumption that the instrumental variables are correlated with the schooling decisions of individuals but not with the earnings outcomes for individuals. If there is a relationship between the instrument and wages, estimation by IV can lead to large inconsistencies. Moreover a weak relationship between schooling and the instruments will add to this problem. As an example Bound *et al.* (1995) 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 and this can result in IV being more biased that OLS.

In natural experiments the non-random assignment to treatment and control groups gives rise to similar problems to the inconsistency of IV in natural experiments. Card (1999) points out that the study by Harmon and Walker (1995) divides the sample according to one of three possible levels of compulsory schooling

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age: people born after during 1933 — 1958 were considered as the control group and those pre-1933 and post-1958 were the treatment group on the basis of the implementation of the two changes in school leaving age. However older cohorts may be different in other ways — in particular the education level of the pre-1933 cohort may have been affected by World War II (see also, for example, 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.* model the probability of being observed in a sample of published returns as an increasing function of the significance level on the difference between OLS and IV. In other words more significant results have a higher chance of being observed in the published sample. 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.

5.5. *Further Evidence — Fixed Effect Estimators and Matching Methods*

5.5.1. *Panel Estimators*

Panel data techniques can be used to address heterogeneity — 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 eliminate the effect of the unobservable. However the applicability of panel data to estimating schooling returns is limited because it is in the nature of panel data 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. Angrist and Newey (1991) find some 19% of working male respondents in the National Longitudinal Survey of Youths (NLSY, a cohort study conducted in the US which followed young people through time) reporting a higher level of schooling in later waves of the data, undermining the assumption that schooling can be thought of as a fixed effect[20]. However there are no further examples in this literature.

5.5.2 *Twins or Siblings Data*

Table 13 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

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Table 13. Twins/Siblings Research on Schooling Returns.

Author	Data	OLS	IV
Ashenfelter and Rouse (1998)	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%
Isacsson (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%

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. The method is based on a belief that these differences eliminate differences in innate ability or motivation. 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 wages between brothers, will eliminate the bias associated with this unobservable.

The survey of twins work by Griliches (1979) concludes that the estimated return to schooling, where ability bias is purged via differencing within twin pairs, is lower that the estimated return from the whole sample (i.e. without differencing). 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 some 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. The problem of measurement error may be particularly damaging in methods based on differencing such as the twins literature in that the bias from measurement error in differenced schooling is likely to be larger.

Recent contributions to the twins literature have attempted to deal with the measurement error problem by instrumenting the education of twin A using the measure of the education of twin A *as reported by twin B*. Ashenfelter and Krueger (1994) collected data at an annual twins festival in 1991, and find against the conventional assumption that OLS are biased upwards but rather find in favour of the results found in the IV literature. Moreover, correcting for measurement error in the self-reported schooling level generates a much larger estimate of the schooling return, in the order of 12–16%. The possible non-randomness of this dataset and the relatively small samples used have led to criticisms but the findings of Ashenfelter and Zimmerman (1997) based on siblings data support this result. Moreover the work of Miller *et al.* (1994) uses a much larger sample of twins from an Australian survey and, employing 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

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while this is for a sample of women participating in a health panel the authors do show that the sample nevertheless matches LFS quite well. Moreover their attempts to instrument within-twins differences do not seem to suggest endogeneity.

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, with the exception of the Bonjour *et al*. (2000) study. The literature assumes that within-twins differences in education is randomly assigned 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.

5.5.3. *Matching Methods*

In their survey Blundell *et al* (2001) consider the estimation of the return to schooling using instrumental variables, control function approaches and propensity score matching methods. Using the NCDS panel they found that the OLS estimate of the return was significantly lower than that obtained using IV or control function approaches. Based on matching methods the estimated returns fell somewhere between OLS and IV when considering the effect of the treatment on those treated, but below both OLS and IV when considering the effect of the treatment on the non-treated.

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 endogenous, and this has been the focus of recent research efforts. With respect to the returns to 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 unobserved variables that affect wages are uncorrelated with schooling — which seems implausible. Multivariate regression analysis based on UK microdata suggests a return to a year of schooling in the UK of between 7% and 9% for men and between 9% and 11% for women when a specification controlling for schooling and experience is used. 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 insensitive to changes in this simple OLS specification (such as including controls for marital status/family size/union membership) but some differences are worth noting. Using different measures of experience (based on actual reported experience and so-called 'potential' experience or the difference between current age and the age left school) will tend to raise the return to schooling by approximately 1%.

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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. There is limited evidence that some years of schooling carry a premium or penalty — 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.

Given the increase in the supply of educated workers in most OECD countries there is a concern in the literature that the skills that workers bring to their job 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. A degree of manpower planning may be required to ensure that particular types of graduate are not produced excessively.

The returns to education may 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: one (possibly extreme) example is that the low return to investing in low ability individuals and the high return to investing in high ability individuals implies that educational investment ought to be skewed towards the high ability individuals. The resulting inequality may then be dealt with through redistributive tax and social security policy.

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

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percentage point. This can be higher where the ability measure is taken at an older age — this is likely to be because, at older ages, the ability measure is almost certainly contaminated by the effect of schooling.

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, allowing estimating using IV. 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 using IV, with the UK at the higher end of the international range, between 7% and 9% from OLS to a range of 11% to 15% from the IV 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 detailed 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. One might be tempted to conclude that this high return implies there are benefits to society (social returns) over and above the private returns so there is little argument for the taxpayer to subsidise individual study. But this may be simply a marginal return in which case we have to ask why this marginal group has high returns. The different approaches taken in the literature to recover the returns to education accruing at the wider macro-economic level are the focus of the contribution in this issue by Sianesi and Van Reenen (2002).

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Notes

1. The Mincerian specification has been extended to address questions such as discrimination, effectiveness of training programmes (Blundell *et al*, 1996), school quality (Card and Krueger, 1996), return to language skills (Borjas, 1999), and even the return to 'beauty' (Hammermesh and Biddle (1998)).
2. Thus the model implies that early schooling has a greater return than schooling later in life since there are more periods left to recoup the costs.
3. In the Griliches model there is a subtle extension often overlooked but highlighted by Card (1995). 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 partly 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.
5. However Cawley, Heckman and Vytlacil (1998) demonstrate that measured cognitive ability and schooling are so highly correlated that one cannot separate their effects without imposing restrictions which are rejected by the data.
6. Indeed for the data for Great Britain and Northern Ireland the information on schooling is top-coded at 18 which is likely to bias the estimated return to schooling upwards.
7. Austria, Netherlands, Greece, Spain, and Italy use net wages. Further details are available in Harmon, Walker and Westergard-Nielsen (2001). An alternative 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).
8. If the wage equation is $\log w_i = \mathbf{X_i}\beta + rS_i + \delta x_i + \gamma x_i^2 + u_i$ then the adjustment is to subtract $\delta - 2\gamma(A - S)$. For an average value of $A - S$ is around 20, the adjustment involved would be modest and of the order of 2%.
9. 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.
10. Controls for occupation were not included. Typically occupation controls result in the estimated return to education being reduced because the estimate is then conditional

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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.

11. Note that Figure 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. Dearden (1998) however does show that credentials may matter in education systems that are heavily based on such qualifications such as the UK.
13. There may, of course, be some overhanging effects from the severe recessionary period in the late 1970's on this part of the wage distribution.
14. Deschenes (2001) using CPS data for the United States finds that the return to education rose by 50% between 1979 and 2000 but that the return after parsing out the effects of differential ability and heterogeneity bias between cohorts is approximately 30%. On balance however the dominant effect in his study is the heterogeneity problem (the correlation between the return to education and the educational attainment of the individual) with smaller effects coming from either a rise in return to unobserved ability or from increasing ability-education sorting between cohorts.
15. 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 a meta-analysis of class size effects.
16. The implication of the correlation between education and wages being due to signalling rather than human capital is that the 'social' returns to education will be small relative to the private returns — adding one more year of education to every individual adds nothing to the productivity of the labour force and does not improve the ability of employers to sort high productivity individuals from low. Thus, raising the education of the labour force would have no impact on GDP. Thus, it is important to try to distinguish between the signalling and human capital theories because the same private returns can have very different implications for social returns. See Sianesi and Van Reenen (2002).
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.
18. The study of Angrist and Krueger has been criticized by Bound and Jaeger (1996) who argue that quarter of birth may have an impact on earnings other than through its effect on schooling.
19. Generally speaking natural experiments ignore the possibility that the treatment may have an impact on individuals other than the treatment group. 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 downward bias in the estimated return to a year of schooling. However this may not be a major issue as treatments tend to affect the flow, for example via some reform, while the stock tends to be large. See Philipson (2000) for further discussion of this argument.
20. 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.

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