<table>
<thead>
<tr>
<th><strong>Title</strong></th>
<th>Introduction [to Education and earnings in Europe : a cross country analysis of the returns to education]</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>Authors(s)</strong></td>
<td>Harmon, Colm; Walker, Ian; Westergaard-Nielsen, Niels</td>
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</tbody>
</table>
1. Introduction

Colm Harmon, Ian Walker and Niels Westergaard-Nielsen

INTRODUCTION

This volume is concerned with the returns to education, in particular on education as a private investment decision in "human capital", and explores 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 year of education in a log wage equation containing controls for work experience and other individual characteristics, the issue is surrounded with difficulties.

In the following chapters we present the findings of a major EU-funded initiative under the 5th Framework Programme. Between 1998 and 2000 a 15 country network of research teams took part in a research project entitled Public Funding and Private Returns to Education (known as PURE). The main objective was to estimate private returns to education evaluating the relationship between wages and education across Europe giving us the opportunity to calculate and compare returns across the PURE member countries.

The research agenda encompassed a number of areas. A prime focus was the analysis and comparison of wage and human capital structures and private returns to education between countries and within countries over time in order to uncover distinct trends as well as similarities and dissimilarities across countries. This included analysis of the impact of country-specific trends in educational returns and of changes over time in underlying market forces (supply-side and demand-side factors), and analysis of carefully differentiated measures of private returns by type and level of education in order to highlight and compare national systems of education. The project also examined the structure and evolution of the national systems of education, admission rules and systems of financial support for school attendance. Finally the effect of differing systems of public funding and admission rules on private returns to education and on earnings inequality was considered.
In the chapters that follow our partners from the PURE project present estimates of the private return to schooling for their respective country. The chapters will explain in detail the educational system that generates the schooling, the datasets used in the analysis and straightforward estimates of the returns. Some countries, due to data availability for example, are able to extend the analysis to consider some of the possible difficulties in the literature.

In this opening chapter we consider the issues involved in estimating returns to schooling to establish the context for the subsequent country-specific chapters. We explore conventional estimates from a variety of datasets and pay particular attention to a number of the most important difficulties. For example, it is unclear that one can give a productivity interpretation to the coefficient if education is a signal of pre-existing ability. Indeed, the coefficient on years of education may not reflect the effect of education on productivity if it is correlated with unobserved characteristics that are also correlated with wages. In this case the education coefficient would reflect both the effect of education on productivity and the effect of the unobserved variable that is correlated with education. For example, "ability" to progress in education may be unobservable and may be correlated with the ability to make money in the labour market. Similarly, a high private "discount rate" would imply that the individual's privately optimal level of education would be low and, yet, such an unobservable characteristic conceivably may itself be positively correlated with high wages.

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

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

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

THEORETICAL FRAMEWORK

The analysis of the demand for education has been driven by the concept of human capital approach and has been pioneered by Gary Becker, Jacob Mincer and Theodore Schultz. In human capital theory education is an investment of current resources (the opportunity cost of the time involved as well as any direct costs) in exchange for future returns.

The benchmark model for the development of empirical estimation of the returns to education is the key relationship derived by Mincer (1974). The typical human capital theory (Becker, 1964) assumes that education, s, is chosen to maximise the expected present value of the stream of future incomes, up to retirement at date T, net of the costs of education, c_s. So, at the optimum s, the PV of the s^{th} year of schooling just equals the costs of the s^{th} year of education, so equilibrium is characterised by

$$\sum_{i=1}^{r_s} \frac{w_i - w_{i-1}}{(1 + r_s)^i} = w_{t-1} + c_s$$

where r_s is called the internal rate of return (we are assuming that s is infinitely divisible, for simplicity, so "year" should not be interpreted literally). Optimal investment decision making would imply that one would invest in the s^{th} year of schooling if r_s>i, the market rate of interest. If T is
large then the right-hand side of the equilibrium expression can be approximated so that the equilibrium condition becomes

\[ \frac{W_s - W_{s-1}}{r_s} = w_{s-1} + c_s. \]

Then, if \( c_s \) is sufficiently small, we can rearrange this expression to give

\[ r_s \approx \frac{W_s - W_{s-1}}{W_s} \approx \log w_s - \log w_{s-1} \]

(where \( \approx \) means approximately equal to). This says that the return to the \( s^{th} \) year of schooling is approximately the difference in log wages between leaving at \( s \) and at \( s-1 \). Thus, one could estimate the returns to \( s \) by seeing how log wages varies with \( s \).

In practice a number of further assumptions are typically made to give a specification that can be estimated simply. Mincer (1974) assumed that \( r_s \) is a constant - so

\[ r = \Delta Y_s / h_s Y_s, \]

where \( Y_s \) is potential earnings and \( h_s \) is the proportion of period \( t \) spent acquiring human capital. During full-time education \( h_t = 1 \) so

\[ Y_s = Y_0 e^{r_s}. \]

For post-school years, Mincer assumes that \( h_t \) declines linearly with experience,

\[ h_t = h_0 - \left( h_0 / T \right) t. \]

So, for \( x \) years of post-school work experience, earnings can be written as

\[ Y_s = Y_s \exp \left( r \int_0^x h_t dt \right). \]

Note that the rules of integration imply that

\[ \int_0^x h_t dt = h_0 x - \frac{1}{2} \frac{h_0}{T} x^2, \]

and assuming that the \( Y_0 \) can be captured as a linear function of characteristics \( X \) we also have
\[ Y_s = Y_0 e^{ra} X \beta e^{ra}. \]

Thus, we can write the expression for income after \( x \) years of experience and \( s \) years of schooling as

\[ Y_s = Y_0 e^{ra} \exp\left( h_s x - \frac{h_s}{2T} x^2 \right). \]

Thus, taking logs,

\[ \log Y_s = \log Y_0 + rs + rh_s x - \left( \frac{rh_s}{2T} \right) x^2 \]

and, since actual earnings is

\[ w_e = (1 - h_s) Y_s, \]

we finally arrive at the conventional Mincer specification:

\[ \log w_e = X_\beta + rs + rh_s x - \left( \frac{rh_s}{2T} \right) x^2 + \log(1 - h_s). \]

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

\[ \log w_i = X_i \beta + rs_i + \delta x_i + \gamma x_i^2 + u_i, \]

where \( y_i \) is an earnings measure for an individual \( i \) such as earnings per hour/week, \( s_i \) represents a measure of their schooling, \( x_i \) is an experience measure (typically age minus age left schooling), \( X_i \) 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 \( X_i \) and \( s_i \). Note that experience is included as a quadratic term to capture the concavity of the earnings profile. In this context \( r \) can be considered the private return to schooling.

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

Firstly, measures of ability have been incorporated to proxy for unobserved effects. The inclusion of direct measures of ability should reduce the estimated education coefficient if it acts as a proxy for ability, so that the coefficient on education then captures the effect of education alone since ability is controlled for. Secondly the exploitation of within-twins or within-siblings differences is based on the assumption that unobserved effects are additive and common across twins so that they can be differenced out by regressing the wage difference between twins against the education difference. This approach is a modification of a more general fixed effect framework using individual panel data, where the unobserved individual effect is considered time-invariant. Finally the instrumental variable approach deals directly with the schooling/earnings relationship in a two-equation system. We return to these in detail later in this chapter.

Optimal Schooling Choice

It is useful at this point to consider theoretically the implications of endogenous schooling. One approach would be to consider a model similar to that presented by Willis (1986) which illustrates the concept of schooling as an optimizing investment decision based on future earnings, a decision based 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 ensures that the present value of benefits equals the present value of costs. More specifically if the IRR is greater than market rate of interest more education is a worthwhile investment for the individual. In making an investment decision an individual who places more (less) value on current income than future income streams will have a higher (lower) value for the discount rates so individuals with high discount rates (high \( r_i \)) are therefore less likely to undertake education. Secondly, direct education costs \( (c_s) \) lower the net benefits of schooling. Finally, if the probability of being in employment is higher if schooling is undertaken a rising level of unemployment benefit will erode the reward from undertaking education. However, should the earnings gap between educated and non-educated individuals widen or if the income received while in schooling should rise (say, through a tuition subsidy or maintenance grant) the net effect on schooling should be positive.
A useful extension to the theory is to consider the role of the individual’s ability on the schooling decision, whilst preserving the basic findings of the model of schooling as an investment. Griliches (1977) introduces ability (A) explicitly into the derivation of the log-linear earnings function. In this basic model the IRR of schooling is partly determined by foregone income (less any subsidy such as parental contributions) and any educational costs. Introducing ability differences has two effects on this basic calculus. The more able individuals may be able to ‘convert’ schooling into human capital more efficiently than the less able and this raises the IRR for the more able. One might think of this as inherent ability and education being complementary factors in producing human capital so that, for a given increment to schooling, a larger endowment of ability generates more human capital. On the other hand, the more able may have higher opportunity costs since they may have been able to earn more and this reduces the IRR.

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

If ability levels are similar between individuals 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 high-education parents will have stronger tastes for schooling (or lower discount rates) and their children may “inherit” some of this. Moreover if ability itself is partly inherited then children with higher ability may be more likely than average to have lower discount rates. The reverse is true for children of lower-ability parents. Empirically this modification allows for an expression for the potential bias in the least squares estimate of the return to schooling to be derived. This bias will be determined by the variance in ability relative to the variance in discount rates as well as the covariance between them. This “endogeneity” bias arises because people with higher marginal returns to education choose higher levels of schooling. If there is no discount rate variance then the endogeneity will arise solely from the correlation between ability and education and since this is likely to be positive the bias in ordinary least squares (OLS) estimates will be upwards (if ability increases wages later in life more than it increases wages.
early in life). If there is no ability variance, then the endogeneity arises solely from the (negative) correlation between discount rates and OLS will be biased downwards if discount rates and wages are positively correlated (for example, if ambitious people earn higher wages and are more impatient).

Thus, the direction of bias in OLS estimates of the returns to education is unclear and is, ultimately, an empirical question.

STYLISTED FACTS

The availability of microdata and ease of estimation resulted in many studies which essentially estimate the simple Mincer specification. In the original study, Mincer (1974) used 1960 US census data and an experience measure known as potential experience (i.e. current age minus age you left full time schooling). The returns to schooling were found to be 10% with returns to experience of around 8%. Layard and Psacharopoulos (1979) used the British General Household Survey (GHS) 1972 data and found returns to schooling of a similar level, around 10%. The Mincerian specification has also been used to address questions such as discrimination, effectiveness of training programmes, school quality, return to language skills, and even the return to "beauty" (see Hammermesh and Biddle (1994, 1998). See Willis (1986) and Psacharopolous (1994) for many examples of this simple specification. In a few studies it has been applied to panel data but this strand of research still suggests that most of the cross-section variance in earnings across individuals is persistent.

Within the PURE project it was possible to evaluate the relationship between wages and education across Europe. In a cross-country study like this, it is preferable that data be more or less comparable across countries, that is wages, experience and years of schooling should be calculated in a similar fashion. Since each country use their own national surveys, this condition is hard to maintain to some extent. However for the purpose of this review we formulated a common specification across our research partners and collected estimates of the return to schooling from each. All PURE partners have estimated this return to education using the log of the hourly gross wage where available (with the exception of Austria, Greece, Italy, Netherlands and Spain who use net wages). Figure 1.1 is a summary of the returns broken down by gender. We find that for some countries like the UK, Ireland, Germany, Greece and Italy there is a substantial variation in returns between gender. Returns to women are significantly higher than returns to men. Scandinavia (Norway, Sweden and Denmark) is characterized by relatively low returns. We see that the lowest returns in Europe are in fact found in Sweden, Norway and Denmark.

Most partners had access to longitudinal data (or at least a combination of cross-sections) for human capital variables and earnings which gives us the
opportunity to identify trends in returns in the European countries. Table 1.1 contains such information. There does not seem to be a clear pattern in the trends. In total there seems to be 15 cases of no trend, 10 cases of increasing returns, and 5 cases of decreasing returns. Countries characterized by decreasing returns for both males and females are Austria and Sweden. Countries characterized by increasing returns are Denmark, Portugal and Italy. The remaining PURE countries are either characterized by no trend or by different male-female trends.

Figure 1.1 Returns to schooling: men and women (year closest to 1995)

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
\[ \log w = X \beta + rS + g(x) + e \]

where \( X \) are individual observable characteristics that affects wages and \( g(\cdot) \) was a third or fourth order polynomial of the experience measure, provided the best approximation for the model. However, there are no examples in the empirical literature that suggest that the way in which \( x \) enters the model has any substantial impact on the estimated schooling coefficient.

**Table 1.1** **Trends in returns: men and women**

<table>
<thead>
<tr>
<th>Country</th>
<th>Relative size of returns in 1980s</th>
<th>Trend Men</th>
<th>Trend Women</th>
</tr>
</thead>
<tbody>
<tr>
<td>Austria</td>
<td>+</td>
<td>down</td>
<td>Down</td>
</tr>
<tr>
<td>Denmark</td>
<td>+</td>
<td>up</td>
<td>Up</td>
</tr>
<tr>
<td>Germany</td>
<td>+</td>
<td>-</td>
<td>-</td>
</tr>
<tr>
<td>Netherlands</td>
<td>+</td>
<td>up</td>
<td>down</td>
</tr>
<tr>
<td>Portugal</td>
<td>+</td>
<td>up</td>
<td>up</td>
</tr>
<tr>
<td>Sweden</td>
<td>+</td>
<td>down</td>
<td>down</td>
</tr>
<tr>
<td>France</td>
<td>0</td>
<td>-</td>
<td>-</td>
</tr>
<tr>
<td>UK</td>
<td>+</td>
<td>up</td>
<td>-</td>
</tr>
<tr>
<td>Ireland</td>
<td>+</td>
<td>- (up)</td>
<td>-</td>
</tr>
<tr>
<td>Italy</td>
<td>+</td>
<td>up</td>
<td>up</td>
</tr>
<tr>
<td>Norway</td>
<td>+</td>
<td>-</td>
<td>-</td>
</tr>
<tr>
<td>Finland</td>
<td>+</td>
<td>-</td>
<td>up</td>
</tr>
<tr>
<td>Spain</td>
<td>+</td>
<td>-</td>
<td>up</td>
</tr>
<tr>
<td>Switzerland</td>
<td>+</td>
<td>-</td>
<td>-</td>
</tr>
<tr>
<td>Greece</td>
<td>+</td>
<td>0</td>
<td>-</td>
</tr>
</tbody>
</table>

*Notes*

+ indicates relatively high returns, + indicates relatively low returns, 0 indicates neither high nor low returns (returns close to average) and – indicates no obvious trend.

However, experience is seldom well measured in typical datasets and is often proxied by age minus the age left education, or even just by age alone. Note that to compare the specification that uses age with one that uses recorded or potential experience one needs to adjust for the difference in what is being held constant: the effect of \( S \) on \( \log \) wages, holding age constant, is simply \( r \) while the experience-control specification implies that the estimate of education on wages that hold age constant needs to be reduced by the effects of \( S \) on experience – that is, one needs to subtract the effect of a year of experience. For example if the wage equation is

\[ \log w_i = X_i \beta + rS_i + \delta x_i + \gamma x_i^2 + u_i \]

then the adjustment is to subtract
\[ \delta - 2\gamma(A-S) \].

Since the average value of \( A-S \) is around 25, and (for men) \( \delta \) is about 0.05 and \( \gamma \) is about -0.001 the adjustment is of the order of -0.25 or about 2.5%.

**Table 1.2 Returns to education in Europe (year closest to 1995)**

<table>
<thead>
<tr>
<th></th>
<th></th>
<th></th>
<th></th>
<th></th>
<th></th>
<th></th>
<th></th>
</tr>
</thead>
<tbody>
<tr>
<td>Austria</td>
<td>0.069</td>
<td>-</td>
<td>0.059</td>
<td>0.067</td>
<td>-</td>
<td>0.058</td>
<td></td>
</tr>
<tr>
<td>Denmark</td>
<td>0.064</td>
<td>0.061</td>
<td>0.056</td>
<td>0.049</td>
<td>0.043</td>
<td>0.044</td>
<td></td>
</tr>
<tr>
<td>W. Germany</td>
<td>0.079</td>
<td>0.077</td>
<td>0.067</td>
<td>0.098</td>
<td>0.095</td>
<td>0.087</td>
<td></td>
</tr>
<tr>
<td>Netherlands</td>
<td>0.063</td>
<td>0.057</td>
<td>0.045</td>
<td>0.051</td>
<td>0.042</td>
<td>0.037</td>
<td></td>
</tr>
<tr>
<td>Portugal</td>
<td>0.097</td>
<td>0.100</td>
<td>0.079</td>
<td>0.097</td>
<td>0.104</td>
<td>0.077</td>
<td></td>
</tr>
<tr>
<td>Sweden</td>
<td>0.041</td>
<td>0.041</td>
<td>0.033</td>
<td>0.038</td>
<td>0.037</td>
<td>0.033</td>
<td></td>
</tr>
<tr>
<td>France</td>
<td>0.075</td>
<td>-</td>
<td>0.057</td>
<td>0.081</td>
<td>-</td>
<td>0.065</td>
<td></td>
</tr>
<tr>
<td>UK</td>
<td>0.094</td>
<td>0.096</td>
<td>0.079</td>
<td>0.115</td>
<td>0.122</td>
<td>0.108</td>
<td></td>
</tr>
<tr>
<td>Ireland</td>
<td>0.090</td>
<td>0.088</td>
<td>0.065</td>
<td>0.137</td>
<td>0.129</td>
<td>0.113</td>
<td></td>
</tr>
<tr>
<td>Italy</td>
<td>0.062</td>
<td>0.058</td>
<td>0.046</td>
<td>0.077</td>
<td>0.070</td>
<td>0.061</td>
<td></td>
</tr>
<tr>
<td>Norway</td>
<td>0.046</td>
<td>0.045</td>
<td>0.037</td>
<td>0.050</td>
<td>0.047</td>
<td>0.044</td>
<td></td>
</tr>
<tr>
<td>Finland</td>
<td>0.086</td>
<td>0.085</td>
<td>0.072</td>
<td>0.088</td>
<td>0.087</td>
<td>0.082</td>
<td></td>
</tr>
<tr>
<td>Spain</td>
<td>0.072</td>
<td>0.069</td>
<td>0.055</td>
<td>0.084</td>
<td>0.079</td>
<td>0.063</td>
<td></td>
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<tr>
<td>Switzerland</td>
<td>0.090</td>
<td>0.089</td>
<td>0.076</td>
<td>0.095</td>
<td>0.089</td>
<td>0.086</td>
<td></td>
</tr>
<tr>
<td>Greece</td>
<td>0.063</td>
<td>-</td>
<td>0.040</td>
<td>0.086</td>
<td>-</td>
<td>0.064</td>
<td></td>
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<tr>
<td><strong>Mean</strong></td>
<td>0.073</td>
<td>0.072</td>
<td>0.058</td>
<td>0.081</td>
<td>0.079</td>
<td>0.068</td>
<td></td>
</tr>
</tbody>
</table>

Table 1.2 illustrates the effect of including different experience measures in schooling returns estimation based on data supplied by the PURE partners. In this table we report estimates based on OLS techniques controlling for different definitions of experience where experience is introduced as a quadratic term as suggested by the Mincer specification. Using a quadratic in age tends to produce the lowest returns, while using potential experience (age minus education leaving age) or actual experience (typically recorded in some datasets as the weighted sum of the number of years of part-time and full-time work since leaving full-time education) indicates a slightly higher return to education.

In Table 1.3 we estimate for men the return to schooling using the UK 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. As can be seen the result here is very robust to these different range of controls.

Table 1.3 Men in BHPS: sensitivity to changes in control variables

<table>
<thead>
<tr>
<th></th>
<th>None</th>
<th>Plant size and union</th>
<th>Part-time</th>
<th>Plant size union, and PT</th>
<th>All</th>
</tr>
</thead>
<tbody>
<tr>
<td>Education</td>
<td>0.064 (0.002)</td>
<td>0.062 (0.002)</td>
<td>0.064 (0.002)</td>
<td>0.062 (0.002)</td>
<td>0.063 (0.002)</td>
</tr>
<tr>
<td>Medium plant</td>
<td>-</td>
<td>0.157 (0.012)</td>
<td>-</td>
<td>0.157 (0.012)</td>
<td>0.153 (0.012)</td>
</tr>
<tr>
<td>Large plant</td>
<td>-</td>
<td>0.241 (0.013)</td>
<td>-</td>
<td>0.242 (0.012)</td>
<td>0.243 (0.013)</td>
</tr>
<tr>
<td>Union member</td>
<td>-</td>
<td>0.079 (0.011)</td>
<td>-</td>
<td>0.079 (0.011)</td>
<td>0.080 (0.011)</td>
</tr>
<tr>
<td>No. of children</td>
<td>-</td>
<td>-</td>
<td>-</td>
<td>-</td>
<td>0.019 (0.005)</td>
</tr>
<tr>
<td>Married</td>
<td>-</td>
<td>-</td>
<td>-</td>
<td>-</td>
<td>0.144 (0.016)</td>
</tr>
<tr>
<td>Cohabitating</td>
<td>-</td>
<td>-</td>
<td>-</td>
<td>-</td>
<td>0.107 (0.020)</td>
</tr>
<tr>
<td>Divorced</td>
<td>-</td>
<td>-</td>
<td>-</td>
<td>-</td>
<td>0.058 (0.024)</td>
</tr>
<tr>
<td>Part time</td>
<td>-</td>
<td>-</td>
<td>-0.020 (0.041)</td>
<td>0.024 (0.039)</td>
<td>0.036 (0.040)</td>
</tr>
</tbody>
</table>

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

Using Samples of Workers

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

Table 1.4  BHPS: OLS, Heckman selection, and median regression

<table>
<thead>
<tr>
<th></th>
<th>FRS</th>
<th>Women</th>
<th></th>
<th></th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Educ</td>
<td>Age</td>
<td>Age^2</td>
<td></td>
</tr>
<tr>
<td>OLS</td>
<td>0.109</td>
<td>0.026</td>
<td>-0.0003</td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.002)</td>
<td>(0.003)</td>
<td>(0.000)</td>
<td></td>
</tr>
<tr>
<td>Heckman two-step</td>
<td>0.109</td>
<td>0.016</td>
<td>-0.001</td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.002)</td>
<td>(0.004)</td>
<td>(0.000)</td>
<td></td>
</tr>
<tr>
<td>Median regression</td>
<td>0.122</td>
<td>0.024</td>
<td>-0.0003</td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.002)</td>
<td>(0.004)</td>
<td>(0.000)</td>
<td></td>
</tr>
</tbody>
</table>

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

Table 1.4 shows the parameter estimates for women using BHPS again. The results show slightly higher returns under the median regression method suggesting a small effect due to the selection into employment. While statistically significant the differences are small in absolute value. Since non-participation is more common among 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 1.2 examines the relationship between the average participation rate for women in employment and the percentage difference between male and female returns to schooling for the countries in the PURE network. The figure shows that countries with the highest rates of female participation (typically the Nordic grouping) have the lowest differences in schooling returns while the countries with the lowest participation (including Ireland and the UK) have the largest. This suggests that there is some bias from using samples of participants alone and further research needs to be done to establish the size of this bias.

Variation in the Returns to Education across the Wage Distribution

It is possible that the returns to schooling may be different for individuals in the upper part of the wage distribution as compared to individuals in the lower portion of the wage distribution. One of the properties of OLS estimation is that the regression line contains or passes through the mean of
the sample. An alternative methodology is available to OLS known as quantile regression (QR) which, based on the entire sample available, allows us to estimate the return to a particular level of education within different quantiles of the wage distribution. The idea with QR is to compare the returns at one part of the distribution, say the bottom quartile, with another part, say the top quartile. The comparison then allows us to infer the extent to which education exacerbates or reduces underlying inequality. Median regression, met earlier, is simply where the 50th percentile is the focus of attention.

Figure 1.2 Female/male differential in returns and female participation rates

![Graph showing female/male differential in returns to schooling and female participation rate.]

Table 1.5 is based on the work of the PURE project and was compiled by Pedro Pereira and Pedro Silva-Martins. Comparisons are possible between two points in time across the range of countries, typically 15 - 20 years apart. The OLS results show that over the period the returns to schooling, on average, have broadly increased. There is a clear implication from the comparisons between the 90th and 10th percentile that the returns to schooling are higher for those at the top of the wage distribution compared to those at the bottom (although for some countries the profiles are flat across a range of the wage distribution). There is some suggestion that the returns have risen at the top of the distribution. One factor behind the distribution of wages is the distribution of inherent ability so that lower ability individuals predominate in the bottom half of the distribution. Thus education may have
a bigger impact on the more able than the less able and this complementarity between ability and education is either getting stronger or not much weaker over time.

Table 1.5 Quantile regressions

<table>
<thead>
<tr>
<th>Year</th>
<th>1st Dec.</th>
<th>9th Dec.</th>
<th>OLS</th>
<th>Year</th>
<th>1st Dec.</th>
<th>9th Dec.</th>
<th>OLS</th>
</tr>
</thead>
<tbody>
<tr>
<td>Austria</td>
<td>1981</td>
<td>9.2</td>
<td>12.6</td>
<td>10.5</td>
<td>1993</td>
<td>7.2</td>
<td>12.8</td>
</tr>
<tr>
<td>Denmark</td>
<td>1980</td>
<td>4.7</td>
<td>5.3</td>
<td>4.6</td>
<td>1995</td>
<td>6.3</td>
<td>7.1</td>
</tr>
<tr>
<td>Finland</td>
<td>1987</td>
<td>7.3</td>
<td>10.3</td>
<td>9.5</td>
<td>1993</td>
<td>6.8</td>
<td>10.1</td>
</tr>
<tr>
<td>France</td>
<td>1977</td>
<td>5.6</td>
<td>9.8</td>
<td>7.5</td>
<td>1993</td>
<td>5.9</td>
<td>9.3</td>
</tr>
<tr>
<td>Germany</td>
<td>1984</td>
<td>9.4</td>
<td>8.4</td>
<td>1995</td>
<td>8.5</td>
<td>7.5</td>
<td>1.0</td>
</tr>
<tr>
<td>Greece</td>
<td>1974</td>
<td>6.5</td>
<td>5.4</td>
<td>5.8</td>
<td>1994</td>
<td>7.5</td>
<td>5.6</td>
</tr>
<tr>
<td>Italy</td>
<td>1980</td>
<td>3.9</td>
<td>4.6</td>
<td>4.3</td>
<td>1995</td>
<td>6.7</td>
<td>7.1</td>
</tr>
<tr>
<td>Ireland</td>
<td>1987</td>
<td>10.1</td>
<td>10.4</td>
<td>10.2</td>
<td>1994</td>
<td>7.8</td>
<td>10.4</td>
</tr>
<tr>
<td>Netherlands</td>
<td>1979</td>
<td>6.5</td>
<td>9.2</td>
<td>8.6</td>
<td>1996</td>
<td>5.3</td>
<td>8.3</td>
</tr>
<tr>
<td>Norway</td>
<td>1983</td>
<td>5.3</td>
<td>6.3</td>
<td>5.7</td>
<td>1995</td>
<td>5.5</td>
<td>7.5</td>
</tr>
<tr>
<td>Portugal</td>
<td>1982</td>
<td>8.7</td>
<td>12.4</td>
<td>11.0</td>
<td>1995</td>
<td>6.7</td>
<td>15.6</td>
</tr>
<tr>
<td>Spain</td>
<td>1990</td>
<td>6.4</td>
<td>8.3</td>
<td>7.2</td>
<td>1995</td>
<td>6.7</td>
<td>9.1</td>
</tr>
<tr>
<td>Sweden</td>
<td>1981</td>
<td>3.2</td>
<td>6.6</td>
<td>4.7</td>
<td>1991</td>
<td>2.4</td>
<td>6.2</td>
</tr>
<tr>
<td>Switzerland</td>
<td>1992</td>
<td>8.2</td>
<td>10.7</td>
<td>9.6</td>
<td>1998</td>
<td>6.3</td>
<td>10.2</td>
</tr>
<tr>
<td>UK</td>
<td>1980</td>
<td>2.5</td>
<td>7.4</td>
<td>6.7</td>
<td>1995</td>
<td>4.9</td>
<td>9.7</td>
</tr>
</tbody>
</table>

Summary: 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 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 et al., 1999, for further details).

In Figure 1.3 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 USA. Well over 1000 estimates were generated across the PURE project on three main types of estimated return to schooling- existing published work (labelled PURE1), existing unpublished work (PURE2), and new estimates produced for the PURE project (PURE3). Each column refers to a different sample of studies (for example only studies based on US-originated studies).
A number of points emerge from the figure. Despite the points raised earlier in this chapter there is a remarkable similarity in the estimated return to schooling for a number of possible cuts of the data with an average return of around 6.5% capturing to a large extent the returns for different countries and different model specifications. There are a number of notable exceptions. The Scandinavian countries generally have lower returns to schooling while at the other extreme the returns for the UK and Ireland are indeed higher than average. In addition estimated returns from studies of public sector workers, and from studies where net (of tax) wages are only available average about 5% (although we would expect the net returns to be lower than those from gross earnings by an amount approximately equal to the average tax rate). Estimates produced using samples from the 1960s also seem to have produced higher than average returns.
ENDOGENEITY OF SCHOOLING

Isolating the Effect of Exogenous Variation in Schooling

If you want to know how an individual’s earnings are affected by an extra year of schooling you would ideally compare an individual’s earnings with \( N \) years of schooling with the same individual’s earnings after \( N + 1 \) years of schooling. The problem for researchers is only one of the two earnings levels of interest are observed and the other is unobserved (Harmon et al., 2000). The problem is analogous to those encountered in other fields, such as medical science: either a patient receives a certain treatment or not so observing the effectiveness of a treatment is difficult as all we actually observe is the effect of treatment on those who are treated. In medical studies the usual solution to this problem is by providing treatment to patients on the basis of a random assignment scheme. In the context of education studies this is sometimes possible but is usually not feasible. However, there are still possibilities to tackle the problem that the treated are not the same as the untreated in unobservable ways and labour economists have made significant progress in this area in the past 10 years. The key idea is to look for real-world events (as opposed to real experiments), which can be arguably considered as events that assign individuals randomly to different treatments. Randomly here has as its more precise definition that there is no relation between the event and the outcome of interest. Such events have been dubbed “natural experiments” in the literature. The essence of this natural experiment approach is to provide a suitable instrument for schooling which is not correlated with earnings and in doing so provide the closest equivalent to a randomized trial in an experiment in a clinical study.

A very direct way of addressing the issue of the effect of an additional year of education on wages can be seen where we 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 who left school at 15 just before the minimum was raised to 16. The Family Resources Survey for the UK is large enough for us to select the relevant cohort groups to allow us do this and Table 1.6 shows the relevant wages.

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 who would not have normally chosen an extra year. 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 effect of an additional year of schooling that had been chosen earned a larger premium of 24.8% for men and 19.0% for women which
reflects the fact that these are different people to those that left at 15 in terms of their other characteristics.

**Table 1.6 Wages and minimum school leaving ages (£/hour)**

<table>
<thead>
<tr>
<th></th>
<th>(1) Left at 15</th>
<th>(2) Left at 16</th>
<th>(3) difference between (3) and (1)</th>
<th>% difference between (2) and (1)</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>pre-reform</td>
<td>pre-reform</td>
<td>post-reform</td>
<td></td>
</tr>
<tr>
<td>Men</td>
<td>7.66</td>
<td>9.56</td>
<td>8.90</td>
<td>14.9</td>
</tr>
<tr>
<td>Women</td>
<td>5.25</td>
<td>6.25</td>
<td>5.81</td>
<td>10.7</td>
</tr>
</tbody>
</table>

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

**Table 1.7 Wald estimates of the return to schooling**

<table>
<thead>
<tr>
<th>Even GHS 78-96</th>
<th>Smoker (at 16)</th>
<th>Non-smoker (at 16)</th>
<th>Difference</th>
<th>Wald estimate</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>Men</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Log wage</td>
<td>2.36</td>
<td>2.51</td>
<td>0.16</td>
<td>0.16/0.97 =</td>
</tr>
<tr>
<td>Educ years</td>
<td>12.11</td>
<td>13.08</td>
<td>0.97</td>
<td>0.164</td>
</tr>
<tr>
<td><strong>Women</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Log wage</td>
<td>2.01</td>
<td>2.18</td>
<td>0.17</td>
<td>0.17/0.90 =</td>
</tr>
<tr>
<td>Educ years</td>
<td>12.52</td>
<td>13.42</td>
<td>0.90</td>
<td>0.188</td>
</tr>
</tbody>
</table>

If the data can be grouped so that the differences between the levels of education in the two groups is random, then an estimate of returns to education (known here as a Wald estimate) can be found from dividing the
Introduction

differences in wages across the groups by the difference in the group average education. A potential example is to group observations according to their childhood smoking behaviour. This information is contained in the General Household Survey for the UK, for even years from 1978-96, and Table 1.7 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. By constructing instruments for schooling that are uncorrelated with the earnings equation the instrumental variables (IV) approach will generate unbiased estimates of the return to schooling. Consider the following model

\[ y_i = \mathbf{X}_i' \delta + \beta S_i + u_i \]

where

\[ S_i = \mathbf{Z}_i' \alpha + \nu_i \]

where in addition to those variables described earlier \( \mathbf{Z} \) is a vector of observed attributes. We assume

\[ E(\mathbf{X}_i u_i) = E(\mathbf{Z}_i \nu_i) = 0. \]

We can, as before, interpret \( \beta \) as the return to schooling. Estimation by OLS will yield an unbiased estimate of the return only if schooling is exogenous, so that there is no correlation between the error terms. If this condition is not satisfied alternative estimation methods must be employed. The correlation might be non-zero because some important variables related to both schooling and earnings are omitted from the vector \( \mathbf{X} \). Motivation, or other ability measures, besides IQ are examples. It is important to note that even a very extensive list of variables included in the vector \( \mathbf{X} \) will never be exhaustive. An estimate of the return to schooling based on OLS will not give the causal effect of schooling on earnings as the schooling coefficient \( \beta \) captures some of the effects that would otherwise be attributed to the omitted ability variable. For instance, if the omitted variable is motivation, and if both schooling and earnings are positively correlated with motivation, the estimation ignores that more motivated persons are likely to earn more than less motivated persons even when they have similar amounts of schooling.

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

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 $Z_t$ that are not contained in $X_t$. That is, there must exist a variable which is a determinant of schooling that can legitimately be omitted from the earnings equation. This variable is provided by the natural experiment. 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 schooling-leaving age that differed from that faced by others, or may have started school at an earlier age for random reasons (i.e. reasons that are uncorrelated with the wages eventually earned).

Results from IV Studies – International Evidence

In Figure 1.4 we present results of a meta-analysis of studies which treat schooling as endogenous based on the PURE dataset of results used in Chapter 2. Compared to an average from OLS of 6.5% we see much larger returns to schooling in IV studies (of about 9%) and from IV studies based on interventions in particular (of around 13% to 14%). IV studies based on family background sourced instruments have returns an average close to the OLS estimate.

*Figure 1.4 Meta-analysis of models with endogenous schooling*
Table 1.8 outlines the key results in this literature for the non-UK studies. Angrist and Krueger (1991) use the presence of compulsory schooling law variation across US states and the quarter of the year in which a person was born as the basis of their instruments. The underlying idea here is that a person who has been born early in the year (the first quarter) reaches the minimum school leaving age after a smaller amount of schooling than persons born later in the year. The actual amount of schooling attained is directly related to the quarter in which they were born while at the same time there seems no reason to believe that quarter of birth has an own independent effect on earnings. Direct estimation by OLS gives an estimate of the return to schooling of 0.063 whereas the IV method gives an estimate of 0.081. The study of Angrist and Krueger has been criticized by Bound et al., 1995. They argue that quarter of birth may have an impact on earnings other than only through the effect on schooling. Studies from other social sciences indicate that the timing of births over a year is related to social background. Parents with lower social backgrounds tend to get children spread evenly over the year, while parents from higher social classes get children during more concentrated in particular seasons.

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

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 programme in education in Indonesia in the early 1970s. Individuals were assigned to the treatment on the basis of their date of birth (pre-and post- reform) and the district they lived in (as investment was a function of local level needs assessment). Meghir and Palme (1999) pursue a similar strategy in their analysis of reforms in Sweden in the 1950s 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 or ‘natural experiments’ whose effect differed
across individuals. A similar approach is used in Denny and Harmon (2000) in looking at a fundamental change in the educational system in 1960s Ireland which not only affected the entire population of school-going individuals but in a way which differed across socio-economic backgrounds.

**Table 1.8 IV studies – Estimated Rate of Return to Schooling**

<table>
<thead>
<tr>
<th>Study</th>
<th>Sample</th>
<th>OLS</th>
<th>IV</th>
<th>Instruments</th>
</tr>
</thead>
<tbody>
<tr>
<td>Meghir and Palme (1999)</td>
<td>Sweden – males</td>
<td>2.8</td>
<td>3.6</td>
<td>Curriculum reforms</td>
</tr>
<tr>
<td>Duflo (1999)</td>
<td>Indonesian – males</td>
<td>7.7</td>
<td>9.8</td>
<td>School building project Peer effects, education reforms</td>
</tr>
<tr>
<td>Harmon and Walker (2000)</td>
<td>UK NCDS: men</td>
<td>5.1%</td>
<td>9.9%</td>
<td></td>
</tr>
</tbody>
</table>

**Notes:** See Card (2000) for further details.
Dearden (1998) repeats the idea in Butcher and Case (1994) by using sibling presence as an instrument for schooling. This study employed National Child Development Study (NCDS) data from the UK and found increased estimates of the return to schooling compared to the 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 1950s and 1970s as instruments, as well as other educational reforms and peer effects. Across a number of datasets a robust finding emerges that compared to OLS estimates of the order of 5% or 6% per year of schooling the IV estimated returns could be significantly higher.

Why are the IV estimates higher than OLS?

As discussed above Card (1995, 2000) presents a model of endogenous schooling which shows that individuals invest in schooling until the marginal return to schooling is equal to their marginal discount rate. Therefore less educated workers have either lower returns to schooling (less able) or higher discount rates (less taste for education, poorer backgrounds). If an intervention used as an instrument in an IV estimation induces those from the low-education group to participate further the associated return will reflect the marginal returns for the low-education group, which may well exceed the return for the population as a whole.

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

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

As noted by Dearden (1995) if the instrument(s) is correlated with the true measure of education but uncorrelated with any measurement error in schooling, the IV approach can be used, and the presence of measurement error should not affect the estimated IV return to education which will be consistent. What will differ is the interpretation placed on the difference between OLS and IV results. As such the difference can now be attributed to a combined effect of measurement error and the endogeneity of schooling. The research by Ashenfelter and Krueger (1994) calculates the reliability ratio (the ratio of variance of the measurement error to total variance in S) in years of schooling measures in survey data at 90%, suggesting that approximately 10% of the total variance in schooling is due to measurement error, but still finds large and significant downward bias in the least squares estimates. On this evidence measurement error appears an unlikely candidate for explaining the IV/OLS difference.

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

Instrument Relevance and Instrument Validity

Bound et al. (1995) urge caution in the use of IV. In this context IV can be simply explained as the splitting of the variance in schooling into an endogenous and exogenous component, with the exogenous component used in estimation of the first stage equation. The essence of their argument is that the consistency of IV assumes the instruments are correlated with schooling but not associated with the earnings outcome. Moreover, if this is not the case, and there is only a tenuous relationship between the instrument and schooling, estimation by IV will lead to large inconsistencies. Thus, we find two main results. Firstly, a weak relationship between schooling and the instruments will raise IV inconsistency. Secondly, a strong relationship between the instruments and the error in the wage equation will also raise the inconsistency, with this effect magnified by the presence of a poor schooling/instrument relationship. 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 which can result in IV being more biased that OLS.

A similar argument has been put forward for the case of invalid instruments. Again Bound and Jaeger (1996), based on a replication of the
original paper, find that quarter of birth does seem to have an effect on wages invalidating the case of Angrist and Krueger (1991). Family background variables come into this category.

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

Finally, publication bias is suggested by Ashenfelter et al. (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. (1999) estimate the probability of being observed in a sample of estimated returns as a declining function of the p-value on your result. In other words more significant results have a higher chance of being observed. When this is corrected about two-thirds of the gap between the average OLS estimated return and the average IV estimated return is accounted for.

Fixed Effect Estimators

Table 1.9 illustrates some recent findings from the literature based on samples of siblings or twins. This approach exploits a belief that siblings are more alike than a randomly selected pair of individuals, given that they share common heredity, financial support, peer influences, geographic and sociological influences etc. This literature attempts to eliminate omitted ability bias by estimating the return to schooling from differences between siblings or twins in levels of schooling and earnings, based on a belief that these differences represent differences in innate ability or motivation, a truer picture of ability bias than simple test scores. This approach received much attention in the schooling-earnings literature in the late seventies and early eighties, possibly as a result of the availability of suitable panel data or specialist studies. If the omitted variable, say ability (A), is such that siblings have the same level of A, then any estimate of β from within family data, i.e., differences in salary between brothers, will eliminate this bias. The survey by Griliches (1979) concludes that the estimated return to schooling where ability bias is purged via differencing is lower than the estimated return from the whole sample.

The research of Blanchflower and Elias (1999) argues that twins may represent a quite distinct population grouping, making generalizations to the population as a whole difficult. Bound and Solon (1998) point out that the US twins data seems to have larger differences in \( S \) than randomly matched unrelated invididuals would have casting doubt on the data. However more
fundamental criticisms of this approach have focused on the underlying assumptions. If ability has an individual component as well as a family component, which is not independent of the schooling variable, the within-family approach may not yield estimates which are any less biased. Also, although more desirable than the approach of ability 'proxies' outlined above the problem of poorly specified data may be particularly damaging to this more sophisticated approach, particularly if the measurement of schooling is prone to error both in the choice of measure and the reporting of the data, even in cross-sectional studies. If schooling is measured with error, this will account for a larger fraction of the differences between the twins than across the population as a whole. This would imply that the bias from measurement error in schooling is likely to increase by forming differences between twins.

### Table 1.9 Twins/Siblings Research on Schooling Returns

<table>
<thead>
<tr>
<th>Source</th>
<th>Measure</th>
<th>OLS</th>
<th>IV</th>
</tr>
</thead>
<tbody>
<tr>
<td>Ashenfelter and Rouse (1999)</td>
<td>Princeton Twins Survey</td>
<td>7.8%</td>
<td>10%</td>
</tr>
<tr>
<td>Rouse (1997)</td>
<td>Princeton Twins Survey</td>
<td>7.5%</td>
<td>11%</td>
</tr>
<tr>
<td>Miller et al (1995)</td>
<td>Australian Twins Register</td>
<td>4.5%</td>
<td>7.4%</td>
</tr>
<tr>
<td>Isacsson (1999)</td>
<td>Swedish same sex twins</td>
<td>4%</td>
<td>5.4%</td>
</tr>
<tr>
<td>Ashenfelter and Zimmerman (1997)</td>
<td>NLS Young Men</td>
<td>4.9%</td>
<td>10%</td>
</tr>
</tbody>
</table>

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 found against the conventional result of upward bias in OLS estimates. Moreover, correcting for measurement error in the self-reported schooling level generates a much larger estimate of the schooling return, in the order of 14%. The possible non-randomness of this dataset and the relatively small samples used led to criticisms. However, the findings of Ashenfelter and Zimmerman (1992) support this result. The recent paper by Miller et al. (1994), which uses a much larger sample of twins from a representative survey and employs the same technique as Ashenfelter and Krueger (1994), also finds strong evidence of downward bias in the least squares estimates.

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

SOCIAL RETURNS TO EDUCATION

Externalities from Education

A clear message of the previous section is the presence of a significant private return to education (which just includes the costs and benefits that flow to the student) of which the OLS estimates can be considered at least a lower bound. As noted in Sianesi and Van Reenen (1999) and Dutta et al. (1999) persistently high returns to individuals undertaking higher education suggests that individuals may be underinvesting in education for some reason. However, given this high private return, it is not clear why taxpayers resources should be invested in encouraging educational participation unless there are benefits to society over and above the benefits to the individual. Greenaway and Haynes (2000) discuss the possibility that graduates raise the productivity of non-graduates such that aggregate productivity is higher. Moreover, there may be socially cohesive benefits from education participation rates being increased through government interventions. Dutta et al. (1999) calculate social rates by comparing the earnings profiles for male university graduates and non-graduates who have A levels and using a baseline assumption for the cost of producing a graduate of £4790 per annum plus earnings foregone while studying. Social rates of return for three groupings of degree subjects are then estimated. These rates of return for graduates range from zero (for broadly humanities and biological sciences) to over 11% (for medicine, science and computing, business studies and social studies). These are lower than their private rate of return estimates but are still relatively high and crucially exceed the rate of return expected by the UK Treasury for support of investment projects. Evidence in OECD (1998) cited in Greenaway and Haynes (2000) suggests that social rates of return in
the OECD are around 10% and higher in countries where students make a contribution to costs (such as Australia, Canada and the USA). This analysis makes no allowance for wider benefits to the economy.

**Human Capital and Growth – Macroeconomic Evidence**

Aggregating a Mincer human capital earnings function to the economy level we get

\[
\ln \bar{w}_t = r_t \bar{S}_t + e_t
\]

where the overbar denotes country-specific means for schooling and incomes (although, for income, in practice, GDP per capita is used), for country \( j \) at time \( t \). Differentiating removes technological differences that are part of the error term terms to give

\[
\Delta \ln \bar{w}_j = \Delta r_j \bar{S}_j + r_j \Delta \bar{S}_j + \Delta e_j
\]

so the \( S \) coefficient shows how returns have changed over time, while the \( \Delta S \) coefficient gives the (social) rate of return in \( j \) at time \( t \). Psacharopoulos (1994) found that the Mincerian return fell on average by 1.7% over 12 years while O’Neill (1995) found that the (social) return rose by 58% in developed countries and 64% in less developed countries (LDCs) between 1967 and 1985. The implication is that the externality has been growing over time.

The idea that growth rates should converge is in a feature of many macrostudies – those below their steady-state growth rate should catch up with those above, that is

\[
\Delta W_j = \beta (W_{j,t-1} - W^*_{t-1}) + u_j
\]

where \( W = \log \) of \( w \) and \( W^* \) is the steady state level of GDP (per capita). Then the macro growth equation would become

\[
\Delta W_j = \beta W_{j,t-1} + r S_{j,t-1} + \ldots + e_j
\]

where variables such as “rule-of-law” index, inflation and capital are sometimes included. In addition an interaction

\[
W_{j,t-1} S_{j,t-1}
\]

may be included to capture the idea that the speed of convergence may be faster the higher is the level of education. Such growth equations are usually estimated from pooled cross-section data spanning five (or more) years.
Introduction

Classic examples are Barro and Lee (1993) and Benhabib and Spiegel (1994). However, there are some differences between what is usually estimated in the growth modelling literature and micro-work in the Mincer tradition. Much of the macro-growth literature excludes $\Delta S$, the change in schooling levels in the economy. The growth literature also typically includes controls to capture the steady state level of GDP. There are a number of empirical difficulties with this literature mainly related to the nature of the causal relationship between schooling and growth. The interpretation of the $S$ coefficient in

$$\Delta W_j = \beta W_{j-1} + rS_{j-1} + \ldots + e_j$$

could be interpreted as a return in terms of the ‘steady state’ growth of the economy - educated countries grow faster. However, more indirect effects are possible. Schooling may better enable the workforce to develop and adapt to new technologies which will also allow educated countries to grow faster. But paradoxically countries with low levels of average schooling might have better opportunities to grow by adopting technology developed abroad. The return to $S$ may have risen or fallen which can jeopardize the interpretation in these growth models. However anticipated growth in an economy could cause an increase in the demand for education. Indeed Topel (2000) has argued that “little can be learned” from macro-growth equations because either a positive or a negative coefficient on human capital is “consistent with the idea that human capital is a boon to growth and development”.

Human Capital and Growth – Microeconomic Evidence

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

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

A literature is beginning in this vein but unfortunately the evidence is already conflicting. Moretti (1998) examines US census information for otherwise similar workers within cities with higher and lower education levels. He differences out the potential draw of the city for particular workers as well as the endogeneity of the growth in education across cities. What is found is that a 1% increase in the share of college-educated workers raises the earnings of school dropouts by 2.2%, of high school graduates by 1.3% and college graduates by 1.1%. All gains are net of costs. In this paper Moretti instruments for average schooling - individual schooling is however left as exogenous. Acemoglu and Angrist (1999) consider implications of, like Moretti (1998), treating average schooling as endogenous. However, they also allow for the endogeneity of individual schooling on the basis that instrumenting average schooling if the OLS and IV estimates of the private return to schooling differ can raise more considerable specification problems. They use compulsory schooling laws in the USA to instrument individual schooling while basing their treatment of average schooling by exploiting differences in child labour laws across the US states. Compared to least square estimates of the private return to education of around 6% estimates based on IV range from 7% to just over 9%. However the social returns estimated in this paper are smaller at around 2% per year of average schooling. Acemoglu and Angrist conclude that their study offers little evidence for sizeable social returns to education, at least
over the range of variation in average state-wide education induced by changing the compulsory schooling laws.

**Other Externalities from Education**

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

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

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

**CONCLUSIONS**

Despite a well-developed theoretical foundation 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 simple human capital earnings function,
that schooling is exogenous, has in particular been the focus of recent research efforts. With respect to the returns for an individual from schooling a number of conclusions can be drawn.

Simple analysis of average earnings for different levels of education can mask a number of issues. The omission of additional controls assumes that variables that affect wages are uncorrelated with schooling – which seem implausible. For example older people are likely to have lower levels of education but higher levels of work experience giving very different 'returns' for a given level of schooling. Multivariate regression analysis based on OLS suggests a return to a year of schooling of between $[3\% \text{ and } 9\%]$ depending on the country of analysis when a relatively parsimonious specification is used based on controlling for schooling and experience (measured with age and its square to capture the potential for diminishing returns to experience).

The returns to schooling are relatively stable to changes in this simple OLS specification (such as including controls for marital status/family size/union membership) but some differences are worth noting. Using different measures of experience (based on actual reported experience and so-called 'potential' experience or the difference between current age and the age left school) will tend to raise the return to schooling by approximately 1%. Including occupational controls will tend to have the opposite effect, lowering the return by around 1%. Basing the estimation on samples of employed persons may also bias the returns to schooling downwards slightly, at least for samples of women.

The basic specification assumes that earnings are linear in education, or that each year of education adds the same amount to earnings irrespective of the particular year. This may seem implausible but it has been difficult to find examples in the literature that conclusively prove that linearity is not a valid assumption. There is limited evidence that some years of schooling carry 'sheepskin' effect – leaving school the year immediately following a credential awarding year for example may generate a lower return for that year generating a dip in the education/earning profile.

Returns to education may also differ across the wage distribution. Evidence based on quantile regression methods suggest 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.

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

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

The effect of this change in procedure can be considerable. Average returns to schooling from OLS are around 6% internationally and over 9% from these alternative 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. An alternative explanation is that the intervention actually has a weak effect on schooling which for econometric reasons can introduce or exaggerate bias in the estimated returns. In short, care should be taken in the interpretation of IV estimated returns to schooling as an indicator of the return to all individuals without careful knowledge of the effect of the interventions used in estimation of the return.

The evidence on private returns to the individual is compelling and despite some of the subtleties involved in estimation there is still an unambiguous positive effect on the earnings of an individual from participation in education. Given this high return unless there are benefits to society (social returns) over and above the private returns there is little argument for the taxpayer to subsidize individual study. There is a limited amount of evidence that suggests social returns to education may be positive. Direct macroeconomic evidence that links growth to education in confounded by the unclear nature of the causal relationship between average schooling levels and measures such as GNP growth. The microeconomic studies that are available confirm this and show how many important
findings linking education to growth are based on restrictive functional form assumptions. What is possibly needed in order to solve the issue of the wider impact of education on society is a parallel to the experiments approach adopted in the estimation of private returns which does suggest that within-country rather than between-country analysis may be the route to quantifying the externality from education.

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