

**BARRINGTON LECTURE 2001/02**

**SCHOOLING RETURNS, SCHOOLING DECISIONS AND EDUCATIONAL  
FINANCE\***

COLM HARMON

*University College Dublin and CEPR*

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*Abstract:* Given the presence of significant returns to education, it would seem logical to query why individuals choose to leave school early. This paper examines the evidence on this issue, dealing with both methodological and evidence-based findings. Drawing on existing research in the area of schooling returns, the evidence and deficiencies of the literature are explored in an effort to quantify the scale of the private return to education. The schooling decision is subsequently examined more closely, so as to investigate the effect of variables such as family income on that choice. Proposed educational finance solutions are then surveyed. Specifically, this paper reports on an experimental approach in the UK, which pays allowances to households and individual students for participation in education, thus reducing the opportunity cost of staying on at school. Finally, estimates are presented, based on an analysis of non-experimental UK data, of the probability of early school leaving, conditional on named variables.

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## 1. INTRODUCTION

A child is defined as in poverty if they are living in a household which is below the median (equivalised) net income. Figures from the OECD, for example, show that in 1998 some 11 per cent of French children met this criteria, as opposed to Denmark, where only 6 per cent of children were recorded as in poverty in 1999, a relatively small problem by comparison. In the United Kingdom some 14 per cent of children were in such households in 1979. By 1999, this had risen to approximately 19 per cent. In 1997, 17 per cent of Irish children were in severe or “consistent” poverty (i.e. in households both below 60 per cent of average household income and experiencing basic deprivation). The rate in 1994 was 24 per cent. A number of major factors may lie behind these child poverty rates, including for example a high proportion of lone parents excluded from the workforce. Moreover, low levels of child support compliance and the independence, in some countries, of unemployment benefit from the number of children in the household both serve to exacerbate the problem.

Government policies are beginning to target this general issue; the UK government, for example, recently committed to “*eliminating child poverty within a generation*”. Indeed, the UK response has been clear and distinct, with a number of policies introduced to deal directly with this issue by way of income transfers. Higher Child Benefit and Working Families’ Tax Credit fall into this category. However, all such policy direction rests on a premise, given the evidence that child poverty is associated with growing up in a poor household, that giving poor parents more money makes for improved prospects for their children. This is very difficult to show in the empirical literature. Recent work looked at a South African “natural experiment” where social welfare payments were extended to the black population; rather than extending direct income transfers to the parents, it was found that by supporting grandmothers, the circumstances of their granddaughters might be improved (Duflo, 2001).

Kesselman (1994) examined changes in Canadian tax law for the treatment of children and illustrated how the alternatives to direct income transfers for tackling child poverty should include methods to enhance the employability of poor parents through education. The effectiveness of enhanced income transfers was questioned, as they seem to have had less impact on the lifetime prospects of poor children.<sup>1</sup>

Early school leaving also seems likely to be an important part of the transmission mechanism which induces a strong correlation of incomes and poverty between successive generations. For example, O'Neill and Sweetman (1998) provide direct evidence that parental background is an important determinant of a child's future welfare. A son whose father was unemployed twenty years earlier is almost twice as likely to be unemployed as a son whose father was not unemployed. This dependency remains significant after controlling for a range of the son's

characteristics, including education, ability and family composition. Dearden, Machin, and Reed (1997), Dustmann (2001) and Ermisch and Francesconi (2001) report a clear intergenerational correlation between fathers and both sons and daughters in terms of labour market earnings and years of schooling, based on analysis of the NCDS cohort.

Another strand of the literature examines the economic return for the individual of schooling. This literature is based on the concept of schooling as an investment decision, similar to any other investment choice that could be made. Despite a well-developed theoretical foundation, the estimation of the return to a year of schooling has been the focus of considerable debate in economics literature. The consensus remains that education is as good an investment as one can make in terms of the financial return on that investment.

Given these two literatures – one which stresses the financial return to an individual from schooling and one which discusses the school-to-work transition and the impact of schooling on life choices – it would seem logical to query why individuals choose to leave school early in the presence of significant benefits to education. Specifically, one must determine whether it is the case that the returns to education are simply not known to the individual in advance of making their schooling choices whereas, for low income families in particular, the returns to the alternatives are well known and easily observed in the job market. Put differently, the returns to schooling are captured at some future point, whereas the returns from quitting school and starting employment are captured now.

This paper examines the evidence on this issue, dealing with both methodological and evidence-based findings. Drawing on existing research in the area of schooling returns, the paper examines the evidence and deficiencies in this important literature to quantify the scale of the private return to education. Against the backdrop of these returns, we will examine the schooling decision more closely and explore the effect of variables such as family income on that choice. We survey a number of proposed educational finance solutions and, specifically, report on an experimental approach in the UK which pays allowances to households and individual students for participation in education, thus reducing the opportunity cost of staying on at school. We also present estimates, based on our analysis of non-experimental UK data, of the *probability* of early school leaving conditional on parental incomes and education, on family and individual characteristics (such as siblings, gender, race and region) and on wealth and area affects. Calculations are also made conditional on other characteristics (employment status, working mother) and, finally, on schooling contingent income, such as child support and child benefit.

## 2. RETURNS TO SCHOOLING<sup>2</sup>

### 2.1. *Schooling as a choice variable*

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 was derived by Mincer (1974). Typical human capital theory (Becker, 1964) assumes that education is chosen to maximise the expected present value of the stream of future incomes up to retirement, net of the costs of education. Thus, at the optimum level of schooling, the present value of the last chosen year of schooling equals the costs of that year.

Optimal investment decision-making would imply that one would invest in a year of schooling if the return for that year exceeds some alternative asset return, say the market rate of interest. The empirical approximation of the human capital theoretical framework is the familiar functional form of the earnings equation:

$$\log w_i = X_i\beta + rs_i + \delta x_i + \gamma x_i^2 + u_i, \quad (1)$$

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;

$X_i$  is a set of other variables assumed to affect earnings;

$u_i$  is a disturbance term representing other forces which may not be explicitly measured, assumed independent of  $X_i$  and  $s_i$ .

The availability of microdata and the ease of estimation have resulted in many studies estimating this 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 left full time schooling). It was found that the returns to schooling were 10 per cent, with returns to experience of around 8 per cent.

In this empirical implementation, the schooling measure is treated as exogenous, although education is clearly an endogenous choice variable in the underlying human capital theory. It is useful at this point to consider the implications of endogenous schooling. As suggested above, within the human capital framework on which the original Mincer work was based, schooling is an optimising investment decision based on future earnings and current costs. In other words, the investment

decision for schooling is 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) is the discount rate that equates the present value of benefits to the present value of costs. If the IRR is greater than market rate of interest (assuming an individual can borrow against this rate) more education is a worthwhile investment for the individual. In making an investment decision an individual who places more (or less) value on current income than future income streams will have a higher (or lower) value for the discount rates. Accordingly, then, individuals with high discount rates are *less* likely to undertake education. Secondly, the net benefits of schooling are lowered by direct education costs. 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 should the opportunity cost of schooling 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, if formal education and on-the-job training are complements (Blundell, Dearden, and Meghir, 1996). 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.<sup>3</sup> Finally, Heckman, Lochner, and Taber (1999) point to the difference between partial and general equilibrium analysis; 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, 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.

The empirical implications of this extension to the basic theory are most clearly outlined in Card (2000). Card revisits 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. Optimal schooling, however, is allowed to vary across individuals for a further reason: not only can different returns to schooling arise from variation in ability, so that those of higher ability “gain” more from additional schooling, but individuals may also have different marginal rates of substitution between current and future earnings. That is, there may be some variation in the discount rate across individuals.

This variation in discount rates may arise, for example, from variation in access to funds, or taste for schooling. If ability levels are similar across 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, 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, 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 of the potential bias in the least squares estimate of the return to schooling to be derived. The bias is 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; 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. 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.

## *2.2. Comparative analysis*

Table 2.1 presents estimates of the rate of return to education based on multivariate (OLS) analysis from the International Social Survey Programme (ISSP) data, drawn together from national surveys designed to be consistent with each other. The British data in ISSP, for example, is taken from the British Social Attitudes Surveys. In Table 2.1 we apply exactly the same estimation methods to data which has been, in turn, constructed to be closely comparable across countries. The results show wide cross-country variation.

**Table 2.1: Cross Country Evidence on the Returns to Schooling**

Country	Male		Female	
Australia	0.0509	<i>0.0042</i>	0.0568	<i>0.0071</i>
West Germany	0.0353	<i>0.002</i>	0.0441	<i>0.0036</i>
Great Britain	0.1299	<i>0.0057</i>	0.1466	<i>0.0069</i>
USA	0.0783	<i>0.0045</i>	0.0979	<i>0.0058</i>
Austria	0.0364	<i>0.0033</i>	0.0621	<i>0.0049</i>
Italy	0.0398	<i>0.0025</i>	0.0568	<i>0.0036</i>
Hungary	0.0699	<i>0.0053</i>	0.0716	<i>0.0051</i>
Switzerland	0.0427	<i>0.0065</i>	0.0523	<i>0.0143</i>
Poland	0.0737	<i>0.0044</i>	0.1025	<i>0.0046</i>
Netherlands	0.0331	<i>0.0025</i>	0.0181	<i>0.005</i>
Rep of Ireland	0.1023	<i>0.0051</i>	0.1164	<i>0.0081</i>
Israel	0.0603	<i>0.0069</i>	0.0694	<i>0.0077</i>
Norway	0.0229	<i>0.0025</i>	0.0265	<i>0.0032</i>
N Ireland	0.1766	<i>0.0111</i>	0.1681	<i>0.0127</i>
East Germany	0.0265	<i>0.0032</i>	0.045	<i>0.0041</i>
New Zealand	0.0424	<i>0.005</i>	0.0375	<i>0.0058</i>
Russia	0.0421	<i>0.0042</i>	0.0555	<i>0.0043</i>
Slovenia	0.0892	<i>0.0104</i>	0.1121	<i>0.0091</i>
Sweden	0.0367	<i>0.0047</i>	0.0416	<i>0.0047</i>
Bulgaria	0.0495	<i>0.01</i>	0.0624	<i>0.0091</i>
Canada	0.0367	<i>0.0072</i>	0.0498	<i>0.0083</i>
Czech Rep	0.0291	<i>0.0069</i>	0.0454	<i>0.0077</i>
Japan	0.0746	<i>0.0066</i>	0.0917	<i>0.0151</i>
Spain	0.0518	<i>0.0071</i>	0.0468	<i>0.0099</i>
Slovakia	0.0496	<i>0.007</i>	0.0635	<i>0.0078</i>

Source: International Social Survey Programme (ISSP) data (1995).

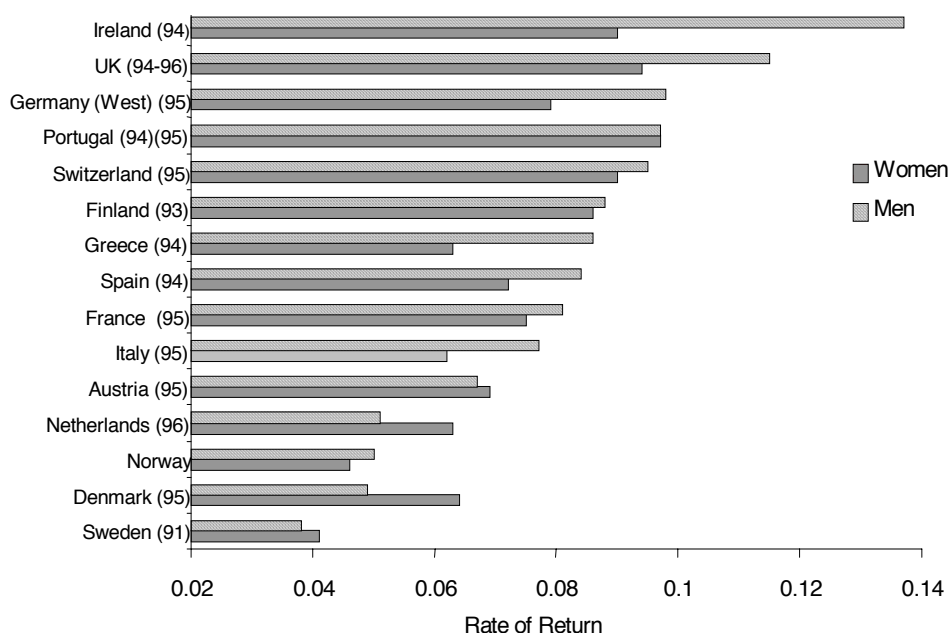
Note: Standard errors in italics. Regression specification includes controls for age and age squared, and union status.

These estimates have the advantage that they are all derived from common data, making them broadly comparable. However, they do so at the cost of simplicity. In particular, the estimated models only contain controls 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; 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.<sup>4</sup> Therefore, it might be interesting to consider cross-country rates of return derived from national surveys, rather than from a single consistent source such as the ISSP.

Recent results from a pan-EU network of researchers (entitled Public Funding and Private Returns to Education, or PURE) did precisely this. Estimates were derived from national data sets in a way that exploited the strengths of each country's data. The main objective was to evaluate the private returns to education by estimating the relationship between wages and education across Europe. In a cross-country project, it is preferable that data be reasonably comparable across countries; 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.<sup>5</sup>

Figure 2.1 is a summary of the returns broken down by gender. These were 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 characterised by relatively low returns. Ireland and the UK are close to the top of the estimated returns in this cross-country review.

**Figure 2.1: Returns to schooling in Europe: men and women (year closest to 1995)**



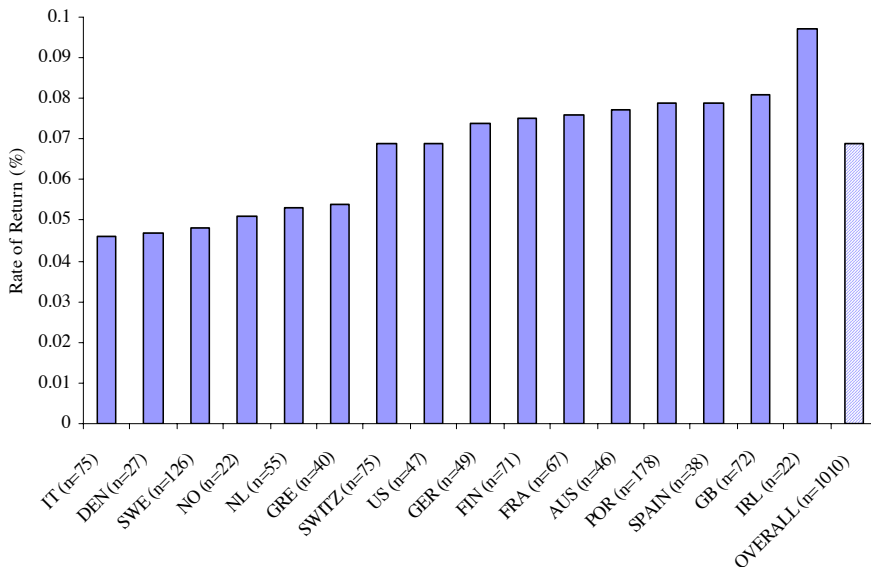
Source: Harmon, Walker and Westergaard-Nielsen (2001).



To summarise the various issues discussed above, we use methods common in meta-analysis to provide both a structure to our survey of returns to schooling and a framework to determine whether our inferences are sensitive to specification choices. Meta-analysis combines and integrates the results of several studies, which share a common aspect allowing them to be “combinable” in a statistical manner. The methodology is typical in clinical trials in medical literature. In its simplest form, the computation of the average return across a number of studies involves weighting the contribution of an individual study to the average, on the basis of the standard error of the estimate.<sup>6</sup>

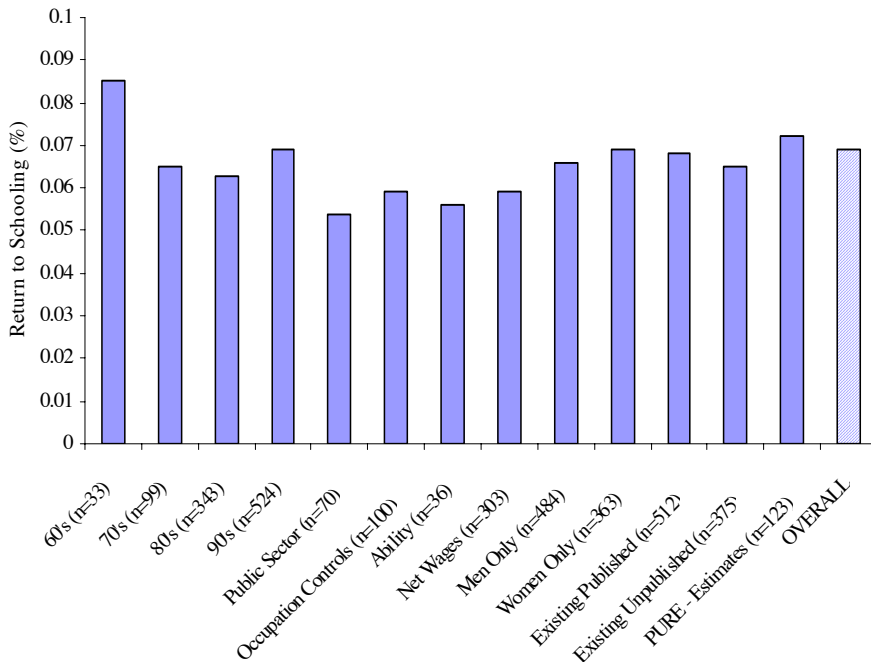
In Figure 2.2(a) and Figure 2.2(b), we present the findings of a simple meta-analysis. The analysis was based on the collected OLS estimated rates of return to schooling from the PURE project, supplemented by a number of findings for the US. Some 1010 estimates were generated across the PURE project<sup>7</sup> 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 sharing a particular characteristic. “US”, for example, indicates only studies based on US data; “Net wages” indicates that the dependent variable was net rather than gross wages; “Ability” indicates that ability controls were included.

**Figure 2.2 (a): Meta analysis: cross-country**



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

**Figure 2.2 (b): Meta analysis: varying the samples of estimated returns**



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

The classifications “60s”, “70s”, “80s” and “90s” are the time periods used to estimate the models. “Public Sector” refers to estimates solely from public sector workers; “Occupation Controls” refers to estimates where controls were included in the specification; “Ability” denotes where explicit ability controls were specified. Under “Net Wages”, the sample data refers to net rather than gross wages, while “Men Only” and “Women Only”, as expected, represent specifications based exclusively on male and female samples. Finally, “Existing Published” and “Existing Unpublished” refer to estimated returns to schooling from existing published and unpublished work across different countries; “PURE Estimates” were generated exclusively for PURE, based on a simple schooling/age/age-squared specification.

A number of points emerge from the figure. 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 per cent across the majority of countries and model specifications. Some notable exceptions are observed. 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 per cent. Estimates produced using samples from the 1960s also seem to have produced higher than average returns.

### *2.3 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, ideally an individual's earnings with  $N$  years of schooling should be compared 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 is observed, while the other is unobserved. The problem is analogous to those encountered in other fields, such as medical science: either a patient receives a certain treatment or not, so that observing the effectiveness of a treatment is difficult. All we actually observe is the outcome. In medical studies, the usual solution to this problem is to provide treatment to patients on the basis of random assignment. In the context of education, this is rarely feasible. However, the problem, that the treated differ from the untreated in unobservable ways, may yet be tackled. Labour economists have made significant progress in this area over the past ten years. The key idea is to look for real-world events (as opposed to real experiments) which, arguably, could be considered to assign individuals randomly to different treatments. "Random" here has a precise definition: 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; in doing so, a close approximation is provided to a randomised trial, such as might be done in an experiment for a clinical study.

The effect of an additional year of education on wages may be addressed in a very direct way by examining the effect on wages when the minimum school-leaving age was raised to sixteen. The wages of people who left school at sixteen, after the minimum was raised, are compared to the wages of those who left school at fifteen, before the minimum was raised. The FRS data are large enough for us to select the relevant cohort groups and thus allow the comparison. Table 2.2 shows the relevant wages.

**Table 2.2: Wages and minimum school-leaving ages (£/hour)**

	(1) Left at 15 pre RoSLA	(2) Left at 16 pre ROSLA	(3) Left at 16 post ROSLA	(4) % difference between (3) and (1)	(5) % difference between (2) and (1)
Men	7.66	9.56	8.9	14.9	24.8
Women	2.25	6.25	5.81	10.7	19

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

The treatment effect 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 who left at sixteen post RoSLA would have left at fifteen had they been pre-RoSLA, we get a lower bound to the effect of the treatment: that is, 14.9 per cent for men and 10.7 per cent for women. The former figure is very close to that obtained in Harmon and Walker (1995) using more complex multivariate methods. By contrast, the upper bound of the treatment effect is the impact of *choosing* an additional year of schooling: this earned a larger premium of 24.8 per cent for men and 19.0 per cent for women. This reflects the fact that those people who chose to leave at sixteen are different from those who left at fifteen in terms of other characteristics.

More formally, the treatment group is chosen, not at random, but independent of any characteristics that affect education. Thus, one could not, of course, group the data according to ability; grouping by cohort, however, to capture a before- and after-effect 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. In other words, 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 – the treatment group may, for example, be taller than the control group; 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. To calculate a Wald estimate, the differences in wages

across the groups is divided 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 Great Britain, for even years from 1978-96. Table 2.3 shows that by examining these differences between groups, the estimated return to schooling is around 16 per cent for men and 18 per cent for women.

**Table 2.3: Wald estimates of the return to schooling, grouped by smokers, non-smokers**

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 = <b>0.164</b>
	Educ Yrs	12.11	13.08	0.97	
Women	Log Wage	2.01	2.18	0.17	0.17/0.90 = <b>0.188</b>
	Educ Yrs	12.52	13.42	0.9	

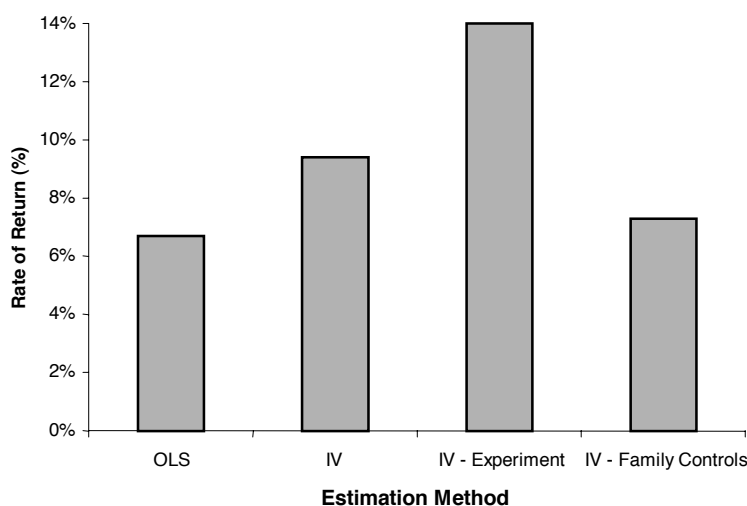
A closely related way of controlling for the differences in observable characteristics is to control for them using multivariate methods. This is the basis of the instrumental variables (IV) approach. Essentially, the variable that is used for grouping could be used as an explanatory variable in determining the level of education. This is useful since it allows the use of multivariate methods to control for other observable differences between individuals with different levels of education. It is also useful in cases where the variable is continuous: the research can exploit the whole range of variation in the instrument, rather than simply using it to categorise individuals into two (or more) groups. By exploiting instruments for schooling that are uncorrelated with earnings, the IV approach will generate unbiased estimates of the return to schooling. In order, therefore, to model the relationship between schooling and earnings, we must use the schooling equation to compute the predicted or fitted value for schooling. We then replace schooling in the earnings function with this predicted level. As predicted schooling is correlated with actual schooling, this replacement variable will still capture the effect of education on wages. However, there is no reason for predicted schooling to 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, there must exist a variable which is a determinant of schooling and which can legitimately be omitted from the earnings

equation. In essence, this amounts to examining how wages differ between groups whose education varies for exogenous reasons. For example, some individuals may have faced a minimum school leaving age that differed from that faced by others, or may have started school at an earlier age for other random reasons (i.e. reasons that are uncorrelated with the wages eventually earned); smoking when young, as we suggest above, falls into this category as it is associated with one's rate of time preference. In Denny and Harmon (2000), we used the fundamental changes in the educational system in 1960s Ireland, changes which affected the entire population of school-age individuals, but in ways that differed across socio-economic backgrounds.<sup>8</sup> A series of papers (Harmon and Walker 1995; Harmon and Walker 1999; Harmon and Walker 2000) use changes in the compulsory school leaving age laws in the 1950s and 1970s as instruments, along with other educational reforms (such as the 1960s Robbin's Act) and peer effects. Across a number of datasets, a robust finding emerges. Compared to OLS estimates of the order of 5-7 per cent per year of schooling, the IV estimated returns were significantly higher.

Figure 2.3 presents the results of a meta-analysis of studies which treat schooling as endogenous, based on the PURE dataset of results used earlier. Compared to an average from OLS of 6.5 per cent, we see much larger returns to schooling in IV studies generally (of about 9 per cent) and from IV studies based on education reforms in particular (of around 13 to 14 per cent). 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.

**Figure 2.3: Meta-analysis of models with endogenous schooling**



#### 2.4 Why are the IV estimates higher than OLS?

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

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 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 – Imbens and Angrist, 1994)<sup>9</sup>.

Bound, Jaeger, and Baker (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 essence of their argument is that the consistency of IV assumes such instrumental variables are correlated with the schooling decisions of individuals, but not with the earnings outcomes for individuals. If this is not the case, then estimation by IV will lead to large inconsistencies.

### 3. EDUCATIONAL CHOICES AND EDUCATIONAL FINANCES

The evidence presented in the previous section 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. Yet it is still the case that for many individuals these returns are not being captured – by choice or by circumstance, they leave education and therefore do not capture the returns, both financial and non financial, that education confers. In Ireland, for example, some 3,500 individuals leave school without the Junior Certificate and some 10,000 leave without a successful performance in the Leaving Certificate. In this section, we restrict our attention to

the specific issue of family influences and, in particular, family income in the educational decision.

### *3.1 Investment in education: does money matter?*

Knowing how educational enrolment responds to family income (or the income elasticity of education decisions) is clearly a key issue in addressing the reasons behind low enrolment. Acemoglu and Pischke (2001) found robust and large income elasticities, based on an identification strategy which used variations in family income over time due to changes in the overall income distribution. According to their research, a 10 per cent increase in family income leads to an increase in college enrolments of between one and two percentage points. Recently Cameron and Heckman (1998) used the US NLSY panel, finding weak current parental income effects and strong parental wealth effects in schooling choices. For the UK, Micklewright (1989) showed that the proportion of sixteen-year olds in Britain who stayed on at school was low by OECD standards and, based on the NCDS cohort, examined the probability of completing education at the minimum legal age. Family background, in the form of class and parental education, was shown to have a large effect even when ability and school type were controlled for. Chevalier and Lanot (2000) attempted to disentangle the effect of family income on the child's educational attainment from other characteristics, which might also affect schooling decision, such as parental education. They found that the direct effect of family income on a child's schooling attainment was overstated by the raw correlation in the data, and that a policy that relies on increasing parental incomes to increase post-compulsory education is unlikely to be effective, and would be expensive. Plug and Vijverberg (2001) showed that children living in families where resources are lacking are severely restricted in their educational opportunity.

#### *3.1.1 Experimental data: the Educational Maintenance Allowance<sup>10</sup>*

The Education Maintenance Allowance (EMA) pilots were introduced in September 1999 in fifteen pilot areas of the United Kingdom. The scheme aims to provide assistance to young people from low-income families who are entering post-compulsory education, through the provision of a weekly allowance and bonuses for retention and achievement, based on a financial assessment of parental income. The allowance ranges from a maximum of £30/£40 per week for those with a gross taxable income of up to £13,000, to a minimum of £5 per week for those with incomes nearing £30,000. No EMA is payable for incomes of more than £30,000. The pilot has subsequently been extended to a further 41 LEA areas, including five areas providing financial assistance for transport costs. Flexibilities to the regulations are also being piloted in some areas to examine the extent to which these cater for the needs of more vulnerable groups of young people – including those who have disabilities, who are homeless or who are teenage parents. Four core variants of EMA are being tested in the original fifteen pilot areas, based on differences in the amounts of weekly allowance, level of bonuses and delivery of the allowance (either to young people or to parents).



**Table 3.1: Variants of the Educational Maintenance Allowance, currently being tested**

	Weekly Allowance	Retention Bonus	Achievement Bonus
Variant 1	£30	£50	£50
Variant 2	£40	£50	£50
Variant 3	£30 (paid to the parent)	£50	£50
Variant 4	£30	£80	£140

A random sample of young people aged either sixteen or seventeen (born between 1 September 1982 and 31 August 1983) was drawn from child benefit records. This sampling frame was designed to be representative of young people leaving school at the end of the 1998/99 academic year, in the pilot and control areas. The parents of these young people were then contacted and given the opportunity to “opt-out” of the survey if they wished to do so. Over 10,000 face-to-face interviews with young people and parents or guardians were then carried out in ten of the pilot areas and in eleven matched control areas. Weights were constructed to correct for potential sources of bias arising from differences in the distributions of key characteristics between the achieved sample and the sampling frame. Questionnaires were designed which covered issues ranging from household composition, education decisions and current activities, parents’ occupations and attitudes to education, to sources of funding for post-compulsory education and issues relating to childcare and other caring responsibilities.

The selection of LEA areas to participate in the pilot was not random. Urban areas were chosen that were known to have relatively high levels of deprivation, low participation rates in post-sixteen education and low levels of attainment in Year 11 examinations. In short, areas were chosen where there was most scope for EMA to have a positive impact. In addition, one pilot area was chosen to examine the potential impact of EMA in rural areas. In order to fully evaluate the effect of EMA, the process of matching was used. Pilot areas were matched as closely as possible to control areas and interviews conducted in both. The outcomes of the interviews provide a unique statistical account of young people in the initial period following the end of compulsory schooling, and allow a comparison of individuals in the pilot and control areas. Each individual in a pilot area was matched with an individual in the control area on a range of characteristics, information about which was collected from young people and parents during the face-to-face interviews. This was necessary because, despite the LEA matching, there remained the possibility that the areas were different in ways which might effect young people’s decisions about their future, such as differing labour market opportunities.

There were equal numbers of young men and women in both pilot and control samples. All were either sixteen years (64 per cent) or seventeen years (36 per cent) old. Approximately 8 per cent were from non-white ethnic backgrounds; the largest proportion of these were of Indian or Pakistani origin. 13 per cent of the sample had some form of special educational needs. Most young people were still living with their parent(s) or a parental figure (e.g. grandparent); of these, almost three-quarters lived with two parents and more than a quarter with a lone parent. Among the 147 young people not living with a parental figure, the largest proportion (60 per cent) were living without a partner or child and 10 per cent were lone parents.

There were four times more lone teenage parents in the control than pilot areas. Just under one-third of young people not living with their parents had formed new relationships and were living with a partner, with or without a child. Of the total sample, almost half of young people were living in families with annual incomes of £13,000 or less (46 per cent); they therefore met the criteria for maximum award of EMA. Just under one-third (32 per cent) were eligible for a partial EMA award, and less than one-quarter (23 per cent) were not eligible for EMA on the grounds of income. The income profile shows small differences between areas, with families in control areas being slightly better off than in pilot areas. A similar pattern of slightly lower deprivation in control areas also emerged for housing tenure (less people were living in rented housing in control areas). The young people in the sample also experienced relatively high levels of labour market inactivity among their parents. Only just over half of young people lived with two parents where at least one was in full-time work. Almost one-quarter of the young people's parents had no educational qualifications.

Amongst all sixteen and seventeen year olds – pilot and control – approximately three-quarters (73 per cent) said that they were in full-time education at the time of interview. 17 per cent said they were in work and/or training, and the remaining tenth described themselves as either unemployed or looking for work or involved in some other activity. Variations in the destinations of young people emerged when comparisons were made between those eligible or ineligible for EMA, in rural and urban areas and in different variants. Across variants, participation rates in full-time education in the pilot areas among EMA eligible young people varied from a high of 81.4 per cent in the Variant 1 (rural) pilot area to a low of 64.4 per cent in Variant 2. Across the urban variants, there was little variation in the control area participation rates.

Descriptive statistics show two patterns in the data that are consistent with a possible EMA effect. Firstly, participation rates in post-sixteen education amongst income eligible young people in those areas where EMA is being piloted are higher than amongst young people in the control areas, who would have been eligible on income grounds if EMA had been available. Secondly, this pattern is reversed amongst those young people who are not eligible for EMA.

Young people not in education were asked whether or not a weekly payment would make them more likely to consider post-sixteen education. Of eligible young people in this group, 52 per cent said that a financial payment would make them more likely to consider post-sixteen education. Of those in work or training, half were unwilling to consider post-sixteen education even with a weekly payment, although 39 per cent would definitely consider it. Respondents who said that a weekly payment would make them more likely to consider post-sixteen education were asked at what level this should be. EMA eligible young people in work or training would require the highest levels of payment. Just over half of young people in work or training would want amounts of over £40 over week. This coincides approximately with current training allowances payable to young people in the UK. However, it is not known if young people set this level because of the experience of pay from current work, or if such high inducements would have been required prior to their entry into work or training.

The main set of estimates of the impact of EMA are derived using a matching approach. Carefully matching pilot area individuals to their closest counterparts in the controls solves the problem that the pilot and control areas may be quite different in their demographic composition. This approach is valid so long as all relevant differences between pilots and controls can be accounted for by characteristics of individuals which are observable in the survey data. Once individuals have been matched, the impact of EMA is measured by taking the difference in participation rates between pilots and their relevant controls. A regression model is used to obtain a further understanding of important policy issues relating to the design of EMA, such as the effectiveness of each of the different variants being tested. This also allows estimates to be obtained of the impact of incremental changes in the amount of EMA offered. The final methodology contends with the problem that the pilot and control areas may differ from one another in ways that are not directly observable in the survey data, but which may be important for explaining differences between them in post-sixteen participation in education. Thus, the staying-on decisions in pilot and control areas are compared among individuals who would never be eligible for the EMA. Any observed differences among ineligible in controls and pilots gives an estimate of unobserved area-specific effects, which are ignored when using our simple one-way matching techniques.

The first set of results used one-way matching techniques to show the overall impact of EMA on participation, among EMA-eligible young people in post-sixteen full-time education. The results showed that, before taking into account any possible area-specific effects, there is a gain in participation rates amongst eligible young people in pilot areas, compared to those in the control areas, of around 5.0 percentage points. Moreover, the estimated impact of EMA is larger in rural areas than in urban areas. In rural areas, the estimated gain in participation in post-compulsory education is 9.2 percentage points, compared with an estimated gain of 3.8 percentage points in urban areas. EMA is estimated to have had a larger effect on young men than on young women – overall, these techniques found that participation gains among young men is estimated to be 6.0 percentage points; for

young women, the estimated gain is 3.9 percentage points. The greater effect on young men than young women holds for both urban and rural areas. In urban areas, the EMA effect on males is found to be 4.6 percentage points, compared to 2.9 percentage points for females. In rural areas, the effect is 10.9 percentage points for males and 7.4 percentage points for females. These findings therefore suggest that EMA may go some way towards closing the gap between males and females in participation in post-sixteen education. Finally, EMA has had a significantly larger effect on young people who are eligible for the full amount of EMA available, compared to those who are eligible for an amount on the taper (however, these results vary between urban and rural areas and by gender). Taking all areas together, the overall effect of EMA is a gain in participation in pilot areas amongst those eligible for the full EMA of 7 percentage points, compared to 2.9 percentage points for those on the taper.

Regression analysis was used to estimate the effects on participation in education of each additional £1 per week of EMA offered. This found that each additional £1 per week of EMA is associated with a gain of 0.36 to 0.42 percentage points in post-sixteen school participation for rural men, and a 0.11 to 0.12 percentage points gain for rural women. For urban males, this figure is between 0.18 and 0.21 percentage points, and for urban females between 0.13 and 0.15 percentage points. The estimated impact for the urban Variant 3, where EMA is paid directly to the parent, is not significantly different to the estimated effect in the other urban variants. If the more generous EMA offered in the urban Variant 2 had been made available to all the urban pilot areas, this would have led to a gain in the overall participation rate by an additional 1.2 percentage points amongst eligible young people, over and above the participation rate obtained under Variants 1 and 3.

### *3.1.2 Non-experimental data: the Family Resources Survey*

An alternative approach might be to use non-experimental data and exploit the correlation between education and schooling-contingent parental income with the focus on the two sources of such income: child support from absent parents and Child Benefit. This section, drawn from my work with Ian Walker for the UK Department for Education and Employment, is based on UK Family Resources Survey (FRS) pooled for 1994, 95, 96 and 97. The FRS is a large-scale survey which produces very reliable income data, as it is a diary-based survey like the Irish Household Budget Survey and the UK Family Expenditure Survey. We drop over-eighteens because of censoring by leaving home – post-eighteen children in higher education or further education (HE or FE) are recorded as external children. For this reason, we confine our attention to the choice of staying on at age sixteen. The pooled data contains 4416 households with sixteen- to eighteen-year-old children.

The raw data for sons show: strong social class effects – non-manual sons about 30 per cent points higher than manual sons; strong regional effects – North and Midlands about 10 per cent lower than South; strong effects of father's education at low levels – leaving at eighteen rather than fifteen adds about 30 per cent to staying

on rate; strong effect of current income – staying on rises at about 3 per cent per decile across deciles of the net income distribution. The data contains information on the Council Tax (CT) band of the house. For renters, this picks up area effects that might reflect peer group influences; for owner-occupiers, the CT band picks up both this effect and an effect of wealth. Thus, we can net out the area effect by looking at the effect of the CT band on the children of owner-occupiers, minus the effect of the CT band on the children of renters. We find weak wealth effects and strong area effects in the raw data for sons.

The data for daughters show: weak social class effects – non-manual daughters about 15 per cent points higher than manual daughters; strong regional effects – North and Midlands about 10 per cent lower than South; some effect of father’s education at low levels – father leaving at eighteen rather than fifteen adds about 20 per cent to staying on rate; some effect of current income – staying on rises at about 2 per cent per decile; weak area (peer group) effects; weak wealth effects.

Parental income and parental education (and other variables) are correlated, so we cannot make inferences about the effect of either without controlling for both (see Plug and Vijverberg, 2001). Thus, we need to model both (all) effects simultaneously. Moreover, unobservable effects are also likely to be correlated with income – for example, parental income may be directly correlated with both school leaving and with the provision of a good home working environment that may also be reflected in later school leaving. Thus, we cannot make inferences about the effects of income without controlling for unobservable factors that affect school leaving. As such, we need to use multivariate methods and we need to instrument parental income to allow for the correlation with unobservables that affect child education.

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

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

In order to overcome the potential endogeneity of parental income, we replace incomes by predicted incomes. The latter depends on education, work experience etc. and on two possible *instrumental variables*: a variable that captures the effect of

the 1974 increase in the minimum school leaving age, and union membership. The predicted income variable then picks up the effect of exogenous differences in income across individuals. The maintained hypothesis in the methodology is that school-leaving decisions are uncorrelated with parental union status and with whether the parents were born before or after the raising of the school leaving age (from 14 to 15): these variables only affect parental income but not child school leaving.

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

Since the analysis is based on estimating a model of the *probability* of leaving school at sixteen, the estimated coefficients are transformed into graphs showing the effects of each of the variables on an individual who has a (relatively high) probability of leaving school of 50 per cent. That is, we fix the characteristics of the individual and then choose a value for the error term, so that the model predicts a probability of early school leaving of 50 per cent. We then compute the effect on the probability of changing each variable in turn. For example, Figure 3.1 shows the effects of parental income on the school-leaving for each of the specifications. The figure shows the predicted effect and the 95 per cent confidence interval around that prediction. With no controls, this (large) increase in income has a large and statistically significant effect, raising the probability to over 70 per cent. However, this prediction is not robust to including other controls. With a full set of control variables, for example, we find only a small and insignificant effect (indicated by the confidence interval bar crossing the horizontal axis).

**Figure 3.1: The effects of father's income: extra £100 pw  
(evaluated at 50 per cent probability)**

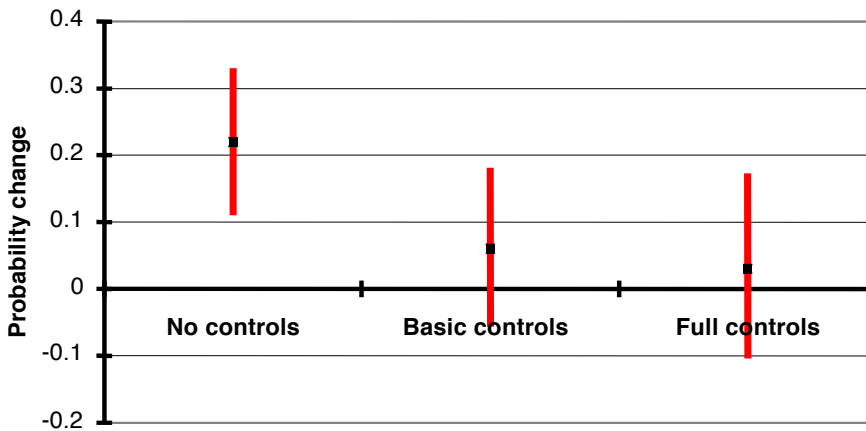
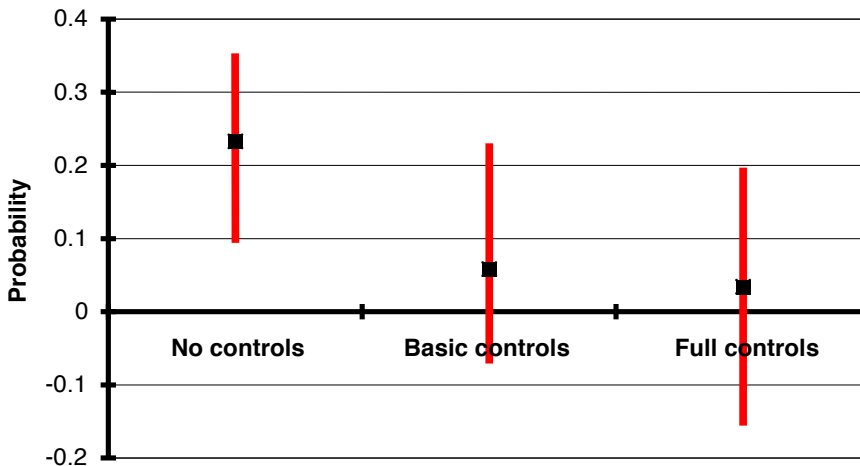


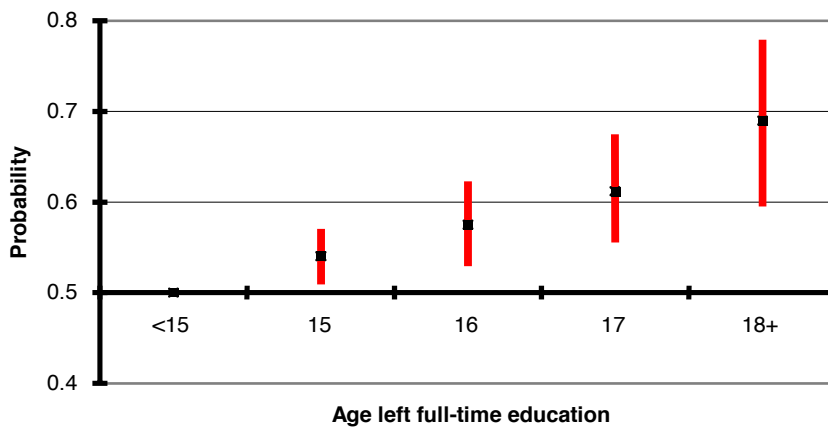
Figure 3.2 shows the effect of an increase in schooling-contingent income. Here, we simulate the effect of a transfer of £25 per week. Again we find that, although we can easily generate a large effect, the effect becomes smaller and less precisely estimated when additional control variables are included. Thus, with full controls, the effect of such a change, akin to an EMA of realistic magnitude, is just a 3.4 per cent increase in the probability, although this figure is estimated with a high standard error. As such, our results do not provide robust support for a policy of using EMAs to promote later school leaving. However, our estimates depend on variation in schooling contingent income in the FRS data and this is essentially a function of household characteristics. It is difficult, therefore, to untangle the effects of the income from the effects of the characteristics on which that income depends, based on the non-experimental data.

**Figure 3.2: Schooling contingent income: extra £25 pw  
(evaluated at 50 per cent probability)**



The effect of the mother's education is explored in Figure 3.3. The specification includes a full set of control variables. We find a strong effect: an additional year of education for a mother induces a predicted increase in participation of approximately 4 percentage points.

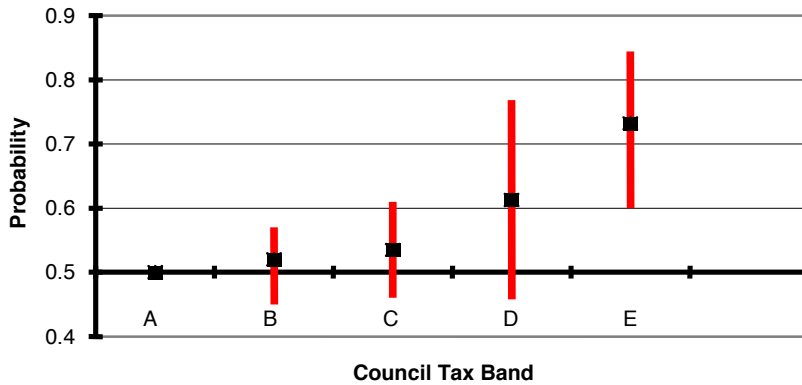
**Figure 3.3: Effect of mother's education (evaluated at 50 per cent probability)**



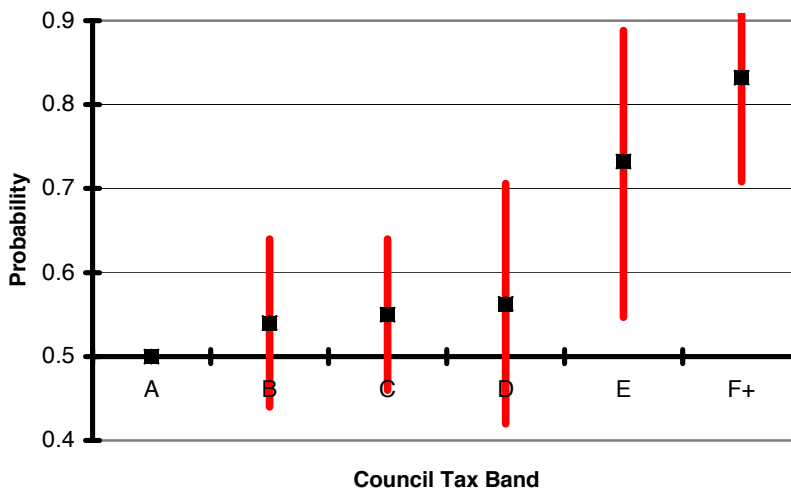


The CT band information is used as a crude control for area. For renters, this is the only effect that this variable would incorporate, while for owner-occupiers it would pick up both area and wealth effects. Figure 3.4 shows that for renters, children in E Band houses have significantly later school leaving than Band A children. Figure 3.5 shows the effect for owner-occupiers *minus* the effect for renters and so could, arguably, be thought to have factored out the area effect, to leave only the effect of wealth.

**Figure 3.4: Area effects (renters), evaluated at 50 per cent probability**



**Figure 3.5: Area effects (owners minus renters) , evaluated at 50 per cent probability**



### *3.2 Policy responses*

As discussed in Plug and Vijverberg (2001), if the financial constraints to education from parental income are a severe constraint, parents have two main options. The first option is to borrow, which of course is unlikely as capital markets are hard to access for poor parents, particularly when effectively borrowing against the expected human capital of their children. The second option might be to save; parents may have an incentive to save when their children are young, if the bottle-neck at later stages of the child's life are perceived at an early enough stage. This is unlikely – apart from the difficulty in realising savings when total income is low, typically young parents will be net consumers when forming families, even if in subsequent years they can generate enough income to change this situation.

In the absence, therefore, of any direct intervention, financial constraints from parents may be very decisive in explaining the school career of the children. Educational policy should be designed to benefit the poor; society potentially benefits from the alleviation of constraints keeping able, but low-income, students from attaining higher levels of education. Moreover, the evidence on endogenous returns to education suggests that this group stand to capture the largest private returns as well. The intergenerational effects also hold. Unblocking the logjam caused by poor parents will not only benefit the children but, crucially, also benefit (via transfer of ability both financial and psychic) the grandchildren, who will no longer face the same constraints (see Denny and Harmon, 2000).

Which policy might be appropriate in this context? A number of alternatives are in play in other countries. Emmerson and Wakefield (2001) discuss the various options in play in the United Kingdom and internationally. In April 2001, the UK government announced two proposed reforms. Firstly, a Child Trust Fund was put forward, consisting of a lump-sum payment made for every child at birth and locked away until adulthood, with children from lower-income families receiving a larger payment. Secondly, a Saving Gateway, or savings account for those on lower incomes, was announced. Like the recent Irish SSIA, the government would match individuals' contributions so as to provide an incentive for account holders to place funds in these accounts. To justify these policies, the government has focused on the benefits to individuals of a "saving habit" and of holding financial assets, including the fact that people without assets are less likely to enter higher education. With the Child Trust Fund in particular, the government has a manifesto pledge to provide incentives for extended family, friends and parents to contribute.

The extent to which individuals born into low-income families remain in a low-income family throughout their childhood is important to determining how well the progressive element of the Child Trust Fund is targeted. Data from all nine waves of the British Household Panel Survey are used by Emmerson and Wakefield (2001) to look at how the incomes of mothers and mothers-to-be change over time, based on a sample of women who either have a child aged under eleven at the time of interview, or have a child at some point over the next four years of the panel. They show that,

for example, the median income of those women who, two years before the birth of a child, do not have any other children aged under eleven is 14.9 per cent higher than the average for the sample. Among those with a youngest child aged one, it is 92.2 per cent of the average. It is also the case that, among this group of women, average incomes are relatively lower when the youngest child is aged under four; this is likely to be caused, in part, by the difficulty of working full time with a pre-school-age child in the household. The figures could be used to support arguments that we should redistribute more towards groups with younger children, since this is a time when family incomes appear to be relatively lower, although families might plan for this in advance and might also choose the number of children that they have.

Again using the BHPS, children are placed into income quartiles depending on their family income and family size at the age of one; this is then compared with the income quartile they are in when aged five. If the ranking of these children did not change between ages one and five, then everyone would remain in the same quartile. In fact, of those in the poorest income quartile at the age of one, only 58.2 per cent were in the same income quartile aged five, with 24.8 per cent moving up to the second quartile, 13.3 per cent moving up to the third quartile and the remaining 3.6 per cent moving up to the richest quartile. Mobility was found to be slightly higher in the second and third quartiles, with less than 50 per cent being in the same quartile aged five as they were aged one. While lower levels of mobility were found among those who were in the richest income quartile at the age of one, it is still the case that 11.6 per cent were in the bottom two quartiles aged five and a further 22.4 per cent in the third quartile. These movements across the income distribution of young children could be due to changes in family incomes, changes in family size or a combination of the two. This mobility is over just four years, rather than the entire duration that the Child Trust Fund will be held, implying that income when the child is born may not be a particularly good proxy for the child's family income during their entire upbringing. This suggests that providing larger Child Trust Funds to young adults whose families were on low incomes at birth is not equivalent to providing larger Child Trust Funds to young adults whose families were on low incomes throughout their childhood. The government has responded by offering the possibility of making top-up payments, based on family income, into the child's fund during their life – for example, at ages five, eleven and sixteen.

Issues of targeting and interactions with other savings instruments are crucial to the design of the Child Trust Fund and the Saving Gateway. The provision by the government of a clearer set of aims for these policies would help enable further consultation to be of maximum benefit to the reform process.

An alternative strategy would be based on vouchers where, similar to the EMA experiment, results are often based on randomised trials. Angrist *et al.* (2001) show very compelling evidence based on a randomised voucher scheme used in Columbia for access to private education, which provides a substantial portion of education provision in the state. The recipients were more likely to attend school and “voucher schools” tended to outperform other schools. Moreover, as poor performance in

school means loss of vouchers, the pupils tended to perform better on average than the corresponding non-recipient. Peterson *et al.* (2001) show results from three trials in the United States which support this evidence.

#### 4. CONCLUSION

In facing the issue of early school-leaving, the economist is faced with conundrums. We have a good investment, an investment with above average returns, and clearly there is scope for these excess returns to be captured and therefore driven downwards towards market interest rates. The excess is not choked off, however, as individuals do not make the choices to remain in education. We therefore have something blocking the market taking effect in its full way. There are clearly a multitude of factors at work. This paper has investigated one element of the payoff from education (the private return) and one influence on the choice to educate, namely family background and family income. While this is clearly not the only influence on the decision by individuals to participate in education, it is one that has a distinct advantage: family income can be measured and family income can be influenced by policy in a national context. The same is not the case for area-specific and other community-based policy responses, which may be productive in their own right; they remain the focus for another study.

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 functions is that schooling is exogenous and, indeed, this has been the focus of recent research efforts. With respect to the returns from schooling for an individual, a number of conclusions can be drawn. The simple analysis of average earnings for different levels of education can mask a number of issues. The omission of additional controls assumes that variables that affect wages are uncorrelated with schooling – which seems implausible. Older people, for example, 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 suggests a return to a year of schooling of between 7 per cent and 9 per cent when a relatively parsimonious specification is used, based on controlling for schooling and experience.<sup>11</sup> This would appear to be at the upper end of returns to schooling in Europe, where Nordic countries in particular have low average returns to schooling. The returns to schooling are relatively stable to changes in this simple OLS specification, such as including controls for marital status/family size/union membership, but some differences are worth noting. Using different measures of experience (based on actual papered experience and so-called “potential” experience, or the difference between current age and the age left school) will tend to raise the return to schooling by approximately 1 per cent. Including occupational controls will tend to have the opposite effect, lowering the return by around 1 per cent. Basing the estimation on samples of employed persons may also bias the

returns to schooling downwards, at least for samples of women, but our evidence suggested that this effect, although significant, was small.

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

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

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

Nevertheless, 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. Yet it is still the case that for many individuals these returns are not being captured; by choice or by circumstance, they leave education and therefore do not capture the returns, both financial and non-financial, that education confers.

The evidence presented here on the impact of family financial considerations on the education decision stems from the experimental evidence of the EMA pilot in the UK and our own replication of these results based on non-experimental data. Preliminary findings from the EMA study show that amongst all sixteen- and seventeen-year-olds, 73 per cent said that they were in full-time education; 17 per cent in work and/or training and 10 per cent unemployed or looking for work or involved in some “other” activity (e.g. part-time education, looking after the home or family, waiting to start a job/training course, or not engaged in an activity due to illness, pregnancy etc). EMA appears to have raised participation in education. The statistical analysis, which controls for variation in the characteristics of individuals in the pilot and control areas, estimates an average participation gain in the pilot areas of around 5 percentage points. However, the impact is estimated to be different for different groups of young people. For example, the scheme has been found to have a greater impact on young men than young women, in rural areas and on those eligible for the full amount of EMA. Therefore the most realistic assessment of the effect is that gains in participation, amongst EMA eligible young people in the pilot areas, ranges from 3 to 11 percentage points. Receipt of EMA appears to have little relationship with the tendency to combine education and part-time work. Part-time work was more associated with socio-economic circumstances; EMA-eligible young people were less likely to work part-time than those ineligible. Qualifications attained at the end of Year 11 by young people in the sample were lower than the national average. Eligible young people in pilot areas were slightly less well qualified than those in control areas. This suggests that high participation rates in pilot areas were not simply the result of these young people having achieved more or better examination passes in Year 11. Young people were found to spend their EMA allowance in a variety of ways, including on housekeeping, transport, books and equipment for school and college, and on personal items such as clothes and shoes and entertainment. Overall, it was found that EMA was not being used to supplement young people’s spending on entertainment. The evidence based on non-experimental data is somewhat more inconclusive on the impact of family background and income on participation but nevertheless supports the main thrust of the EMA response.

A typical policy response might therefore be based around financially enabling the parents in a more structured way to value the education of their children. Responses in the UK (so called “baby bonds”) and US (based on voucher schemes) suggest that policy can work. However, policy needs to be carefully targeted at groups; the

voucher schemes, for example, seem to have the greatest impact on minority groups and groups most marginalised in terms of educational attainment.

The challenge of the policymaker is to gather the evidence and make the decision. If I might be allowed a plea to conclude this paper, it is that I would like to see more innovative and experimental approaches in the policy evaluation forum. The issue of evaluation and design of microeconomic policy, discussed by Dr. Donal O'Neill in his Barrington Lecture two years ago, requires good data and good practices in the collection of data. Supplement this with a willingness to design policy experiments (like the EMA trial) and I would see scope for real and lasting policy effects to be a genuine possibility.

## Endnotes

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1. Parental income and parental education are correlated, so we cannot make inferences about the effect of either without controlling for both. Thus, we need to model both effects simultaneously. Moreover, unobservable effects are also likely to be correlated with income – for example, parental income may be directly correlated with both school leaving and with the provision of a good home working environment that may also be reflected in later school leaving. Thus, we cannot make inferences about the effects of income without controlling for unobservable factors that affect school leaving.
2. This section draws heavily on work with Hessel Oosterbeek and Ian Walker, including our recent survey which can be downloaded from the ISSC website (see [www.ucd.ie/issc](http://www.ucd.ie/issc)).
3. The extent to which non-pecuniary benefits are reflected in lower wage rates turns out to be small in most empirical research.
4. Indeed, for the data for Great Britain and Northern Ireland, the information on schooling is top-coded at eighteen. This serves to bias the estimated return to schooling upwards.
5. Austria, Netherlands, Greece, Spain and Italy use net wages.
6. See Ashenfelter, Harmon and Oosterbeek (1999) for further details.
7. 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.
8. The natural experiment usually ignores “spillover” effects of the treatment. If, for example, the school-leaving age is raised, those who leave school just before the increase belong to a group of low educated people who have no competition from younger cohorts with the same low level of schooling. This may increase the wages of this group and lead to a bias in the estimated return to a year of schooling.
9. There are two arguments in the LATE literature: either unobserved heterogeneity in returns, or higher returns for specific groups, such as the disadvantaged for example.
10. This section draws on the reports by Ashworth *et al.* (2001). My thanks to Lorraine Dearden of the Institute for Fiscal Studies for her help.
11. Measured with age and its square to capture the potential for diminishing returns to experience.



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