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Economic Returns to Education: What We Know, What We Don't Know, and Where We Are Going – Some Brief Pointers*

The estimation of the economic return to education has perhaps been one of the predominant areas of analysis in applied economics for over 50 years. In this short note we consider some of the recent directions taken by the literature, and also some of the blockages faced by both science and policymakers in pushing forward some key issues. This serves by way of introduction to a set of papers for a special issue of the Economics of Education Review.

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1. Introduction

Economics has invested much energy in identifying the value of educational investment, to determine whether governments and individuals are investing optimally. Much of this work stems from, *inter alia*, the work of Becker (1962) that introduced the concept of treating investment in education as a capital investment. Since then hundreds of papers have been published estimating the return to education investment (see, for example, the reviews by Card (1999), Harmon, Oosterbeek, and Walker (2003), and the meta analysis of Ashenfelter, Harmon, and Oosterbeek (1999) for research on private returns to schooling; la Fuente and Ciccone (2003) for research addressing the impact of education on the so-called 'knowledge economy' through growth models; and Acemoglu and Angrist (2001) or Oreopoulos and Salvanes (2011) for research on wider externalities associated with education).

However, estimates of this return vary significantly, depending on the data sets used, the assumptions made and the estimation techniques. In terms of broad methodologies, the focus on the issue of endogeneity often requires identifying assumptions that cannot be empirically tested or are, at best, somewhat fragile in estimation. Furthermore, attempts at estimating a single rate of return may not be very informative if returns to education differ by education level, or differ across populations (including by social strata). This may be particularly important for policy responses, but ironically gets masked by methodological debates. Similarly, economists often fail to take into account the risk associated with education investment decisions. Risk may play an important role in an individual's education investment decision, and indeed a government's educational investment level, and should be taken into consideration when testing rationality and optimality of education investment (see Heckman, Lochner, and Todd (2008) and the comprehensive review in Heckman et al. (2006)). In addition, as most cogently argued by Oreopoulos and Salvanes (2011), the return to education may be much wider than the private financial returns that is the focus of so much of the economics literature, and perhaps economics as a profession has allowed a major body of research on the non-pecuniary returns (which may create private returns through externalities that are as great – if not greater – than the direct effect of education on earnings) to become dominated by the other social sciences.

2. Brief thoughts on current directions

There appears, to me, a welcome trend in the current literature to consider (a) a broader concept of monetary private returns to education that considers earnings variance as much as average earnings, and considers variation in returns across the distribution of education; and (b) a wider consideration of non-monetary returns for both the individual and likely social returns.

In addressing the first of these issues our sense is that the key requirement is methodological. Even though Instrumental Variables (IV) has been used most frequently to causally estimate the returns from education, there has been a debate recently in the literature about the appropriateness of this approach. Heckman and Urzua (2009) outline a number of potential problems associated with IV estimation; weak instruments can give biased estimates; IV estimates rest on strong, *a priori* data assumptions; in a heterogeneous model, different instruments will give different estimates; and finally, the IV estimate, depending on the instrument used and assumptions made will give different estimates of the return to education, which are often incorrectly interpreted. Advances have been made recently in this area, with Heckman and Vytlacil (2005) proposing the estimation of marginal treatment effects whereby different treatment effects typically estimated by researchers (local/average treatment effects (L/ATE); treatment on the treated (TT); treatment on the untreated (TUT); policy relevant treatment effect (PRTE); IV and OLS estimates) can all be estimated from different weighted averages of the marginal treatment effect[1].

It is also, of course, possible to relax and modify the standard Mincerian approach. Henderson, Polachek and Wang (2011) relax the assumption of homogeneous rates of return to schooling by employing nonparametric kernel regression allowing the examination of the differences in rates of return to education both across and within groups – for example, they find that Blacks have higher returns to education than Whites, natives have higher returns than immigrants and younger workers have higher returns than older workers. Park (2011) explores nonlinearity in the rate of return to education, exploiting respondents in the National Longitudinal Survey of Youth (NLSY) who change jobs with an intervening period of education reinvestment. The conventional assumption of linearity of log wages in years of schooling is

[1] Other research has developed the traditional two choice model to ordered choice models and to general unordered choice models which allow heterogeneity of response to treatments (Heckman and Vytlacil (2005); Heckman, Tobias, and Vytlacil (2001)).

strongly rejected: a typical reinvestment for the 1980 through 1993 period is associated with a rise of about 3.5 percentage points in the estimated return to an additional year of schooling. The estimated marginal rate of return generally rises in the former education level, and reaches the maximum where an additional year of investment is associated with a rise in real hourly rate of pay by approximately 20 percent. Park (2011) shows that neither sheepskin effects nor sample selectivity is responsible for the finding, nor concurrent technological change.

Leaving aside the 'deeper' aspects of the econometric debates, it is good to see new research which, while in the tradition of papers which use educational reform as instruments to deal with endogeneity, focusing on particular aspects of the educational distribution or on the broader impact of the policy change on wage distributions or on multiple cohorts. For example, Devereux and Fan (2011) use IV to uncover the causal effects of education on earnings, studying the effects of a major expansion in education levels that occurred for persons born between 1970 and 1975 in the United Kingdom where, following a long period of stagnation in educational levels, the average age of finishing education increased by about one year over the space of these cohorts. While this period has been considered one of higher educational expansion, the authors demonstrate that large increases in educational attainment occurred throughout the educational distribution, with a higher proportion of persons staying in school beyond age 16 and obtaining high school completion qualifications. Moreover, hourly wages and weekly earnings also increased over this expansion period, and did so in a way that is highly correlated with the educational increases. This suggests that people born late enough to be able to take advantage of the educational expansion also benefited from higher wages as a result of the increased education. When the authors use the educational expansion to estimate the return to education by instrumental variables, they find that the return to an extra year of education is about 6% for both men and women. The findings suggest that, consistent with human capital models, policy-induced increases in educational attainment are of benefit to affected cohorts.

Other work focuses on specific 'ranges' of the education distribution such as higher education including, *inter alia*, Walker and Zhu (2008, 2011), Green and Zhu (2010) and Chevalier (2011). Walker/Zhu (2011) examines the impact of a rise in student fees on the internal rate of return to English undergraduate degrees. Since repayments occur in the future and depend on earnings, it is important to get good estimates of the lifecycle pattern of wages. They exploit the short panel nature of their data to obtain estimates of the lifecycle that are

(under certain conditions) uncontaminated by cohort effects. They also recognize that the lifecycle pattern of wages differs by college major so estimate separate equations by college major. They use these estimates to simulate the impact of changes in the level of fees (which are set to treble in England) and the structure of the student loan scheme. They also take account of income tax and social security contributions. They find large internal rates of return to some subjects (like economics) and low to others (like arts). Their simulations suggest that even the dramatic increases in fees proposed by the UK Government beginning with the 2011 intake will have only small effects on rates of return. Chevalier (2011) makes an important extension of the Walker and Zhu (2011) paper. The range between subjects reaches 0.26 log points even after excluding medical degree graduates who are clear outliers. However, these differences in mean wage between subjects are dwarfed by larger differences within subjects. In the view of the current debate on the 'marketisation' of higher education, Chevalier (2011) computes a graduate tax to approximate a willingness to pay for these wage differentials. Assuming perfect forecast, he concludes that tuition fees could range from £1,900 to £5,300 by subject, which is considerably less than the planned tuition charge at most institutions of £9,000.

While extensions, the papers above are still largely focused on the concept of a single return to education, albeit sometime for different sub-populations. However, if the simplest, so-called Mincer, coefficient is assumed to be the rate of return that individuals consider when deciding their educational attainment levels, then returns to education must be risk free or the individual is risk neutral. However, returns to education are not risk free, and, typically, individuals are not risk neutral. Borrowing directly from the finance literature, Palacios-Huerta (2003) computes the Sharpe ratio of human capital investments for different demographic groups and education levels (the Sharpe ratio of an investment is the ratio of the expected returns to the standard deviation of returns), and compares this to the Sharpe ratio available in financial markets – for many demographic groups and education levels the human capital Sharpe ratio was greater than that available in financial markets.

The risk associated with different educational attainment levels and the degree of individual risk aversion will impact upon educational attainment choices. Harmon, Hogan, and Walker (2003), using UK Labour Force Survey data from 1993-2000, estimated that mean returns to education of 7 percent are associated with a 4 percent standard deviation. Melnik, Pollatschek, and Comay (1973) and Heckman, Lochner, and Todd (2008) consider the option

value associated with an additional year of education. This option value arises for two different reasons. One is due to the non-linearity of returns to different years of education - an additional year of education confers the option to go on to further levels of education that might be associated with higher returns. However, an additional year of education also gives you more information about labour market returns and your own ability. This additional information reduces the uncertainty of returns to future levels of education, and might enable you to make better-informed decisions about your educational attainment levels, resulting in better outcomes.

With respect to the estimation of wider returns, the basic Mincer model does not allow for non-monetary returns. If non-monetary returns to education exist, then the Mincer estimate will underestimate the public/private returns from education. Many papers have been published linking education and non-market benefits. Haveman and Wolfe (1984) outline many private non-monetary benefits including own health, spouse and family health, fertility (achieving desired family size and changing of family size preferences), broadened enjoyment of other activities, consumer choice efficiency, higher saving rates and improved marital sorting. They also review public non-monetary benefits including crime reduction, social cohesion, technological change and charitable giving. This work has been extended and refined over the years - Lochner (2004) finds that education reduces criminal behaviour, and the social value of this return equates to between 14 and 26 percent of the private return to schooling.

There have been a number of more recent papers investigating the causal impact of education on non-monetary outcomes[2]. Oreopoulos (2007) finds that in addition to increasing lifetime wealth by approximately 15 percent, an additional year of schooling reduces the likelihood of being in poor health, being unemployed and being unhappy. Dickson and Smith (2011) explore the return to education in terms of employment outcomes as well as log wages. Delaney *et al* (2011) allows the return to education to be felt via the impact education has on non-cognitive outcomes, using a particularly rich and innovative dataset of Irish university students that includes a myriad of controls of a psychological nature, which in effect allow for a control of issues such as time preferences. They show how an observed gap in educational attainment by social class at the time of entry to university is all but eliminated by the

[2] This extends to political and civic behaviour. For example, Dee (2004) and Milligan *et al* (2004) find that education increases voter turnout in the United States, leads to more politically informed citizens, increases demand for free speech and civic knowledge.

graduation, but there persists a large and significant gap in the expectations of students whereby poorer students have lower wage expectations.

3. Implications

Oreopoulos and Salvanes (2011) put the key issues very well – while increasing income and wealth is a key motivator for educational choice, it is arguable whether it is THE motivator. Economics has perhaps been somewhat 'underpowered' in terms of research that recognises how education can define major life outcomes – occupation, marriage/relationships and so on – but also can change you as a person – increasing your sense of self esteem, self awareness and consideration of the future. It changes key skills that are not often captured in standard models – education may make you more opinionated, more decisive. It may promote trust, civic engagement. You may also have the skills to avoid lifetime 'traps' – making you better at running your household budget, managing your time and your allocation of time to the benefit of others such as your children. This is changing – and this is welcome.

Some final observations flow from these developments. Firstly, perhaps greater dialogue and understanding between the policy community and the research community would lead to mutually beneficial outcomes? Some things are clear – the focus of economics on causal outcomes is vital and a necessary condition, and is, by and large, what economists do in this field! However, estimating the economic return to schooling has been very focused on the estimation of a point estimate – and moreover, on explaining what the point estimate means through, for example, the extensive debates in the IV/LATE literature. This is a vital scientific argument but perhaps can lead us up a pathway that may be throwing the proverbial baby out with the bathwater by ignoring what we as scientists might see as irrelevant. For example, a LATE effect may be a very important effect. For many countries, the 'local' effect – for example where the population impacted is the group who have strong preference to leave school early – is actually a large cohort. The drive to find a single-number return to education also belongs, I would suggest, to an era where policy was more focused on actions that are mainly universal in impact. For many reasons, not least the reality that fiscal positions of governments have become ever tighter, the era of universality of policy design may be over and targeted action (at the 'local' population) will become the norm. I see this as a welcome development.

Secondly, expanding the point made above, examination of specific parts of the education/wage distribution, returns to specific qualifications, the motivations of high-and-low achieving children and other aspects of the education/earnings relationship will become more prevalent. A conventional policy response – throw money at the issue – has been largely unsuccessful in addressing important and persistent policy blockages like low intergenerational educational attainment, suggesting that a more nuanced understanding of the economic returns that include wider external benefits could be vital. The developments of methodological and theoretical underpinnings (such as the increased role of non-cognitive skills in a formal model of human capital skill formation in the work of Heckman and others, or the increasing role of risk in modelling educational choices and outcomes) have important policy implications that need to be thought through in more detail. For example, the presence of risk in education returns provides an interesting alternative justification for government intervention in education markets. If individuals are choosing socially sub-optimal levels of education due to high levels of risk aversion, then the government, by initially subsidising the individual cost of education, and claiming it back through future progressive labour market taxation, could increase a society's educational attainment levels by diversifying individual risk of education investment. In general, the emergence of this literature, that both widens the concept of the return as well as the ways of estimating it, can widen the policy choice set substantively and more research will be needed to map that set.

Understanding wage expectations is at an early stage, as in my work with Liam Delaney and Cathy Redmond (Delaney *et al*, 2011). I think this is perhaps as critical as understanding actual distributions of outcome. The implication of this is important – if the minority of poorer background students who actually make it to University have such different expectations of earning when objectively they are as talented as their richer peers, the gaps in earnings expectations for those more marginally attached to education who never made it to University must be very much larger. In the context of a Mincer style model where participation is largely determined by potential earning returns, this is a critical issue and the policy responses thus far – again largely about monetary incentives to encourage participation – may be missing the point totally.

Finally, all of the above can only be turned into empirical application with the right data sources and three main data challenges remain. We need to progress further the linkages

between administrative and survey sources, and national agencies need to do this as a matter of course. In the same vein, statistical agencies need to focus less on their concerns on access and ethical/privacy consideration and recognise that sharing data as a public good is vital and also efficient for all sides – bringing the academic research community inside the policy tent leads to a shared risk/shared return perspective on data collection and analysis. As an aside I do not wish to downgrade issues of privacy etc – US colleagues will agree to terms of access to datasets, and do so ethically and with due regards for privacy concerns, as to ignore these would risk massive fines, career ruin and possible incarceration. Agencies should raise the penalty if breach of conditions, and the probability of being audited for good practice, instead of just having a blanket ban on access or cumbersome processes (such as very restrictive IT access conditions (such as disabling network access or USB ports) or requirements to be physically located at statistical agencies for the analysis). We need to use social experiments in education more substantively, ideally against a backdrop of the administrative data linkages. Finally, the embedding of measures from the other human sciences (like psychology) can be incredibly powerful in adding to our understanding of matters, but add little in terms of burden or cost to the data collection processes.

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