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 **Foresight**

**Mental Capital and Wellbeing:  
Making the most of ourselves in the 21st century**

**State-of-Science Review: SR-A7  
Estimating the Effects of Learning**

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## Summary

**Most literature on learning benefits has focused on associations of schooling attainment with labour market outcomes. But fuller understanding of learning impacts needs to consider: (1) broader definitions of learning than just schooling; (2) more benefits than just labour market outcomes; (3) that both learning and these outcomes are determined by behavioural choices within life-cycle contexts, which makes inferences about causality challenging; and (4) evidence for public support for types of learning in terms of basic policy motives of efficiency and distribution. This review of selected literature in light of these four points leads to a more nuanced understanding of the wider benefits of learning, the need for defining learning by what people know rather than how long they spent in school, caution about misinterpreting associations as necessarily reflecting causality, and concern regarding the limited evidence on the efficiency motive for public subsidies for learning.**

### 1. Introduction

Most literature on learning benefits has focused on associations of schooling attainment with labour market outcomes. But fuller understanding of learning impacts needs to consider:

- 1) Broader definitions of learning than just schooling;
- 2) More benefits than just labour market outcomes (see Wolfe and Haveman, (2003) and their well-known earlier reviews; Alheit and Kreitz, 2000; and Centre for Research on the Wider Benefits of Learning at [www.learningbenefits.net](http://www.learningbenefits.net));
- 3) That both learning and these outcomes are determined by behavioural choices within life-cycle contexts, which makes inferences about causality challenging;
- 4) Evidence for public support for types of learning in terms of basic policy motives of efficiency and distribution. Specific frameworks are necessary for interpreting empirical evidence on wider learning benefits, including policy implications.

### 2. Learning and learning benefits within life-cycle frameworks

Following is a sketch of a stylised life-cycle model with sequential decisions about learning. Because of prior concentration on schooling as an input into learning (indeed, often equated with learning, though this review argues that that is misleading), for illustrative purposes Stage 2 refers to school-age years and the other stages to pre-school and post-school years (but similar structures apply for other learning experiences):

Stage 1: pre-school learning determined by nutrition, health, infectious diseases and stimulation from families, neighbourhoods and human capital service providers (e.g. pre-school programmes).

Stage 2: school-age learning determined in part by schooling and school characteristics but also by out-of-school experiences ranging from homework to labour market work, all conditional on stage 1 outcomes and on individual, family, market and institutional characteristics.

Stage 3: post-school learning determined in part by formal programmes (e.g. training and adult learning programmes) and experiences in labour markets, household production and other activities.

Decision-makers (in early life, parents, but increasing the individual in whom learning investments are made) can be viewed as pursuing their own wellbeing and that of their children (and perhaps others) given their resources, market and policy options that they face, and production technologies for producing various outcomes of interest (e.g. 'learning' as produced by time spent in learning programmes, programme characteristics, other experiences, etc.). This sequential process, over each life-cycle stage, results in reduced-form demand relations that capture responses of learning, learning inputs and dimensions of wellbeing to changed incentives, both on the demand side (i.e. through families and expectations regarding future returns to learning) and on the supply side (through availability and quality of learning-related institutions). In stage 2, for example, parents and their children determine child learning through schooling –  $e_2$  – by their efforts to obtain their objectives (including forward-looking objectives related to expected impacts of school learning) conditional on:

- 1) determinates of outcomes of stage 1, including family (f), community (c), service-provider (s) and stochastic factors (u) for stage 1 and initial child ( $e_0$ ) and family ( $e_{f_0}$ ) endowments;
- 2) parallel factors for stage 2; and
- 3) expected values of those factors for stage 3:

$$(1) e_2 = e_2 (f_1, c_1, s_1, e_0, e_{f_0}, f_{12}^e, f_{13}^e, c_{12}^e, c_{13}^e, s_{12}^e, s_{13}^e, f_2, c_2, s_2, f_{23}^e, c_{23}^e, s_{23}^e, u_1, u_2, u_{12}^e, u_{13}^e, u_{23}^e),$$

where first subscripts denote stages in which variables are experienced and, for expectational variables (denoted by superscripts e), second subscripts denote periods for which expectations are held (i.e.  $c_{13}^e$ , denotes in stage 1 the expected community characteristics for stage 3).

Similarly, for outcomes that affect wellbeing  $w_3$  in stage 3, there are reduced-form relations that are conditional on learning through schooling attainment in stage 2  $e_2$ , various family (f), community (c), service-provider (s) and stochastic factors (u) for stage 3 and initial child ( $e_0$ ) and family ( $e_{f_0}$ ) endowments:

$$(2) w_3 = w_3 (e_2, f_3, c_3, s_3, e_0, e_{f_0}, u_3).$$

Good estimates of relation (2) allow us to answer important questions about how increased learning, in this case through increased schooling, affects important components of wellbeing. Various data limitations, however, make obtaining good estimates challenging. The absence of information on endowments, for an important example, means that estimated coefficients of impacts of stage 2 learning in relation (2) are likely to be biased because indicators of learning represent in part correlated endowments – and relation (1) indicates that learning is likely to be correlated with endowments.

A further common problem is that often no direct measurements of learning or knowledge are available, only time spent in school or other programmes that affect learning. Time spent in such programmes does not control for programme qualities and other experiences that affect learning. The results are likely upward biases in estimated impacts of time spent in programmes and no understanding of how programme quality and other experiences affect wellbeing (Behrman and Birdsall, 1983). Relations (1) and (2) emphasise impacts of demand-side and supply-side characteristics on individual demands for learning and on possible impacts of individual learning. But, if there are policy or other changes that alter incentives for learning for many individuals, their increased demand for inputs into learning processes and resulting increased supplies of individuals with greater learning may have impacts on markets, perhaps through altered prices. Increased demands for adult learning programmes, for example, are likely to result in increased demands for relevant teachers and staff.

### 3. Estimation issues and possible resolutions

Good estimates of relations (1) and (2) are informative regarding wider learning benefits. But obtaining such estimates is challenging because learning reflects behavioural choices in the presence of persistent unobservables such as endowments. Therefore, associations between learning and wellbeing are not likely to reflect causal effects of learning alone, but also correlated effects of various unobservables. The following section discusses two prominent estimation approaches for dealing with these problems. Better data, of course, always helps. The section on 'Data quality' below addresses five important data characteristics.

#### 3.1. Estimation methodologies

**Instrumental variable (IV) or two-stage least squares (2SLS) estimates:** To break the correlation between observed right-side variables such as indicators of learning in relation (2) and the compound disturbance terms that include unobserved determinants in addition to stochastic terms, one strategy is to use instrumental variables (IV) or two-stage least squares 2SLS. Good instruments must: (1) predict well those variables being instrumented and; (2) not be correlated with disturbance terms in the second-stage relation of basic interest (i.e. relation 2). The model should indicate potential instruments. The reduced-form demand relations for stage 2 learning experiences in relation (1), for example, gives potential instruments to be used to identify life-cycle stage 2 learning experiences in relation (2). Note that these right-side variables in relations such as relation (1) may include experiments and so-called 'natural experiments' such as natural events (e.g. weather) and policy changes (e.g. compulsory schooling laws).

Finding good instruments, however, is not easy. Not all potential instruments suggested by the model structure, for example, are likely to be independent of the second-stage disturbance term. For estimation of relation (2), for example, relation (1) suggests that family background characteristics are potential instruments. But, if there are significant correlations between parental and child endowments, then parental schooling attainment and income may not satisfy the second condition for good instruments. It may also be difficult to find instruments that predict sufficiently well the second-stage right-side variables. The econometric literature recently has developed better diagnostic tests for good instruments.

**Fixed effects (FE) estimates:** Some unobserved variables likely to cause biases if they are not controlled are fixed across observations in the data. From a longitudinal perspective (i.e. fixed over time) these include variables such as individual and parental genetic ability and innate health endowments, some aspects of community culture and environment, and longer-run fixed characteristics of learning service providers. From a cross-sectional perspective (i.e. fixed across observations in some group such as members of the same family or the same community), these include family and community environments and endowments shared by siblings, and learning institution environments shared by students in the same learning programmes. Such factors that are fixed across observations can be controlled so that they do not bias estimates of observed variables by using dummy variables for each group of observations for which the control is desired (i.e. individuals or families over time, siblings or community members at a point of time). Numerous studies suggest that controlling for FE changes estimates substantially.

FE estimates have limitations. They do not control for unobserved varying characteristics (e.g. time-varying prices that may affect behaviours), for which reason in some studies they are combined with IV estimates. Also, they increase the importance of noise relative to the signal, which tends to cause a bias towards zero, for which reason FE-IV estimates have been used in some studies (e.g. using other respondents' reports for schooling attainment in Ashenfelter and Krueger, (1994); Behrman, Rosenzweig and Taubman, (1994)). And lastly, they do not permit estimates of first-order impacts of observed-fixed variables, but only of variables that vary across observations for which FE are used. They do not, for example, allow us to estimate impacts of parental schooling on child schooling unless parental schooling varies over time.

### 3.2. Data quality

Critical aspects of the quality of quantitative and qualitative data include:

- 1) **Representativeness:** Can inferences be made for some population of interest beyond the sample?
- 2) **Sufficient power:** Is the sample large enough to identify effects of interest at given significance levels?
- 3) **Coverage of relevant variables:** Is information available on variables that capture critical elements of relations (1) and (2)?
- 4) **Measurement errors:** Random measurement error in right-side variables tends to cause biases towards zero, which tends to be exacerbated in FE estimates. Random measurement error bias can be eliminated if there are multiple reports and measurement errors across the reports are not correlated (e.g. multiple reports on schooling attainment from various respondents, as in Ashenfelter and Krueger, (1994) or Behrman et al. (1994)). More generally, IV estimates may also eliminate this bias. However, measurement error also may have systematic components, which may make inferences difficult.
- 5) **Possibility of controlling for endogeneity of learning:** Estimates of relation (2) can be obtained with control for FE endowments common to siblings if there is information on siblings at the appropriate life-cycle stage (or better yet, identical twins given evidence of the importance of individual-specific endowments, e.g. Behrman et al. (1994)). If there are exogenous (to individual families) changes in right-side determinants of learning in relations such as (1) due to controlled experiments or 'natural' policy experiments (e.g. compulsory schooling laws), these may permit IV estimates to control for behavioural choices determining right-side learning in relation (2).

## 4. Selected examples of empirical estimates of wider learning benefits

Most literature on wider learning benefits, such as those referred to in the references in the Introduction, simply presents associations and is, therefore, not sensitive to the estimation and interpretation issues discussed in the preceding two sections. These associations may be upward biased estimates of learning impacts, if both learning and the benefits are positively determined by characteristics such as ability endowments or motivation. The opposite may be the case if characteristics such as innate sociability negatively affect learning but positively affect indicators of wellbeing such as participation in social and political groups. A relatively small, but growing, number of studies attempt to deal with these issues and, in some cases, present estimates that differ importantly from the standard associations in the literature.

*School learning impacts on adult happiness:* Schnittker (2008) uses the 1995-6 US National Survey of Midlife Development, which includes samples of unrelated persons and siblings (including twins) to examine relationships between schooling and happiness. His cross-sectional estimates imply that happiness increases significantly for additional schooling. But control for family endowments (sibling FE) or for individual endowments (identical twins FE) results in insignificant coefficients. These estimates suggest, therefore, that both schooling attainment and adult happiness are determined by aspects of family background possibly including genetic endowments, so that cross-sectional associations between them do not reflect a causal impact of school learning on adult happiness.

*School learning impacts on hospitalisation and mortality:* Lleras-Muney (2005) examines whether schooling has causal impacts on mortality by following synthetic cohorts using successive US censuses and taking compulsory schooling laws as instruments for schooling. Her results suggest that schooling has causal impacts on mortality, and that these effects are at least as large as had been previously estimated. Behrman

et al. (2006) investigate causal impacts of schooling on hospitalisation in 1980-2002 and mortality by 2003, using data on men and women from 1921-1950 birth cohorts from the Danish Twins Registry linked to population-based registries. Their results indicate strong, significantly negative *associations* between schooling and both hospitalisation and mortality, but in these associations schooling is serving primarily as a marker for background family and individual-specific endowments, generally with no significant causal effects. The differences between these results and Lleras-Muney's may reflect differences in schooling-related health service access and health behaviours across countries.

*Intergenerational school learning impacts on children's schooling:* A small set of studies have estimated impacts of parental schooling on child schooling using identical twins, adoptees or 'natural' policy experiments to break correlations between parental schooling and endowments (Behrman and Rosenzweig, 2002; Black et al., 2005; Plug, 2004; Plug and Vijverberg, 2003). These studies find that such controls alter substantially estimated impacts of parental schooling on child schooling, generally lowering estimated impacts considerably, particularly for mothers' schooling.

*School learning impacts on schooling attainment of spouse:* Marriage is widely thought to confer benefits, particularly for males. The greater the learning of the spouse, the greater the benefits would seem to be. Behrman and Rosenzweig (2002) present cross-sectional estimates that, for every additional schooling grade of women born in Minnesota, US, in 1936-1955, their spouse had 0.67 additional schooling grades. But within-identical twins IV estimates, which control for adult sisters' common endowments, indicate a definitely smaller causal effect of about 0.4 additional grades.

*School learning impacts on citizenship:* Milligan et al. (2004) explore the effects of extra schooling induced through compulsory schooling laws on becoming politically involved. They find strong and robust relationships between schooling attainment and voting in the US, but not the UK. Their estimates indicate stronger significant effects of schooling when treated as endogenous than ordinary least squares (OLS) estimates in some cases. Specifically, in the US these effects are seen in following campaigns on TV and in newspapers, following public affairs and in trusting people; in the UK for discussing opinions and persuading others to share views, discussing political matters with friends, and considering oneself politically active. This suggests that unobserved characteristics that determine these civic behaviours are inversely associated with unobserved characteristics that determine schooling.

There also is a smaller set of civic activities for which IV estimates indicate less strong or less significant effects of schooling: for the US, registering to vote, working on community issues, attending community meetings, not minding jury duty; and for the UK, following news, considering oneself fairly or very close to a political party, being satisfied with how democracy works, voting – thus indicating that for these activities unobserved determining characteristics are positively correlated with those that determine schooling. The mix of effects of treating schooling attainment as endogenous relative to not doing so is consistent with different unobserved factors playing more important roles in various civic activities *versus* schooling attainment.

*School learning impacts on crime:* Lochner and Moretti (2004) estimate the effects of schooling attainment on participation in criminal activity, again using changes in state compulsory schooling laws over time to control for endogenous schooling. Using Census and FBI data, they find that schooling significantly reduces probabilities of incarceration and arrest. National Longitudinal Study of Youth (NLSY) data indicate that their results are caused by changes in criminal behaviour and not differences in probabilities of arrest or incarceration conditional on crime. Lochner and Moretti estimate that social savings from crime reduction associated with high school graduation for men is about 14-26% of private returns. Their estimated impacts of another school grade in some cases do not change much if schooling is treated as endogenous, but in some cases the changes are substantial.

Also, Lochner (2004) notes that increased schooling may reduce violent and property crimes through increasing opportunity costs of such activities and possible implications of apprehension, but that a human capital approach suggests that impacts on white collar crime of more schooling should be less negative and possibly positive (as are associations between white collar crime and schooling).

*Impacts of schooling and post-schooling work experience on adult cognitive skills:* Behrman et al. (2009) use longitudinal data collected over 35 years in Guatemala to investigate production functions for adult (i) reading-comprehension; and (ii) nonverbal cognitive skills and their dependence on behaviourally-determined pre-schooling, schooling and post-schooling experiences. Results include: (1) Schooling has significant and substantial impacts on adult reading comprehension (but not on adult nonverbal cognitive skills). But estimates are biased upwards substantially, with no controls for behavioural determinants of schooling and/or if family background factors that appear to be correlated with genetic endowments are included among first-stage instruments; (2) Both pre-schooling and post-schooling experiences have substantial significant impacts on one or both of the adult cognitive skill measures that tend to be *underestimated* if these experiences are treated as statistically predetermined, in contrast to upward biases for schooling; (3) The failure in most studies to incorporate pre- and post-schooling experiences in analyses of adult cognitive skills or outcomes affected by these skills is likely to lead to misleading over-emphasis on schooling relative to these pre-and post-schooling learning experiences.

*Impact of adult learning courses on changes in health-related behaviours, health and social capital:* The above studies use only schooling attainment to represent learning experiences, with the exception of the Guatemalan study. Feinstein and Hammond (2004) use the UK National Child Development Study to investigate effects of adult learning upon 12 outcomes that are proxies for health and social capital. To minimise selection bias and to control for unobserved fixed factors, they consider changes in outcomes rather than levels. They find that adult learning plays important roles in contributing to small shifts in attitudes and behaviours that take place during mid-adulthood. The results persist as controls are added for demographic, schooling and other background factors, as well as for changes in life circumstances during mid-adulthood. However, they are cautious about suggesting purely one-way causal relationships, given that they control for selection only to the extent that it is based on fixed characteristics.

## 5. Conclusion

Making inferences about wider learning benefits is difficult. To advance understanding of such possible effects, skilful combinations of better data and better analysis are needed. Much work confuses associations with causality and has substantial measurement problems. Some recent studies suggest that understanding of wider learning benefits may change substantially if estimation methods are used that are consistent with the framework outlined above. Some of these estimates suggest that impacts of schooling attainment may be greater than in previous studies because schooling is negatively associated with underlying factors (e.g. propensity to socialise?) that are positively associated with some activities such as participation in political and civic groups. But most suggest that standard estimates overstate schooling impact on the wider benefits considered, because schooling is determined in part by unobserved factors such as innate ability and family background that also may determine directly wider learning benefits.

However, these studies are only a beginning for enhancing our understanding of wider learning benefits. They, too, have some maintained assumptions that need further investigation (e.g. what determines timing of compulsory schooling laws or what determines different schooling between identical twins). They also depend on particular market and policy contexts, and effects may differ as these contexts change. They address only a limited subset of hypothesised wider learning benefits – without much attention, for example, to components of mental health. They almost all focus on schooling attainment, without considering impacts of variations in school quality, to say nothing of non-schooling forms of learning.

Further, some interventions or other changes that improve learning are likely to have market-wide effects, and thus have feedbacks through changing market prices that generally have not been considered.

Finally, more clarity regarding policy motives and more attention to critical benefits and costs are desirable. Most studies provide no information about efficiency motives for learning-related policy interventions. They usually provide little guidance about how impacts relate to distributional motives nor do they place policy interventions into distributional policy hierarchies. And most studies provide little or no information about costs, particularly private costs, which must be incorporated to place possible policy interventions into either efficiency or distributional policy hierarchies.

Future research, by dealing better with these issues, has potential for yielding important insights regarding behaviours and policies related to wider learning benefits.

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