Growth effects of education and social capital in the OECD countries

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1. Introduction

Public and private expenditure on educational institutions accounts for about 6% of the collective GDP of the OECD member countries, or roughly $1,300 billion dollars each year.[1] This figure understates the true opportunity cost of educational investments, since it does not take into account forgone earnings. Overall, it should be clear that the provision of education represents a major commitment of resources within the OECD, and so measuring the associated welfare benefits is an important task.

One aim of this survey is to examine the available evidence on the benefits of education in developed countries. The main focus is restricted to the productivity benefits, a topic for which there is a considerable body of evidence, admittedly indirect. I will draw on research from two fields in particular, labour economics and empirical research on growth. An underlying argument will be that, although the labour economics literature does an impressive job of measuring the private returns to education, it remains the case that macroeconomic studies have a complementary role to play in gauging the overall contribution of education.

The emphasis throughout is very much on education, rather than on any broader concept of human capital. The chief omission is any consideration of vocational training. This does not

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reflect my view of its relative significance, but rather the focus of the present survey on cross-country evidence. The nature of vocational training varies considerably across countries, and is tightly connected to production strategies (Broadberry and Wagner 1996). It is difficult to capture these differences in ways that lend themselves to empirical modelling. This means that, in explaining productivity differences across OECD countries, the cross-country evidence has little to say about the role of training, despite its potential importance.[2] This is one area in which answers should be sought from labour economics and detailed comparisons of practices in individual countries, rather than from the cross-country empirical work surveyed here.

A second theme of the survey is the relation between growth and what has come to be known as 'social capital'. It is difficult to arrive at a precise definition of this term, and I will discuss this issue in more detail later on. For now, it can be thought of as capturing such things as the extent of trustworthiness, social norms, and participation in networks and associations. In the last few years, some prominent academics and commentators have argued that these qualities of societies are potentially valuable not only in themselves, but also because they make a contribution to economic success. This is another area in which cross-country evidence may have something worthwhile to contribute, and in the latter half of the paper, I will review the small but growing literature on the correlation between growth and measures of social capital.

Empirical work on social capital and growth is a very recent development, and with this in mind, I devote the majority of the survey to research on education and growth. Section 2 provides the theoretical background, and shows that recent models provide some good reasons for seeing education as a central determinant of growth. Section 3 turns to the empirical evidence. It starts

[1] The expenditure share is taken from OECD (1997, 51). The figure for collective GDP is based on total OECD GDP in 1997, at 1997 prices and exchange rates (OECD 1999, 143). The figure excludes Korea, the Czech Republic, Hungary and Poland.

[2] One consequence of this omission is that I will have to ignore the interaction between education and training. To the extent that education is about 'learning how to learn', it may have

with a brief account of research in labour economics, an essential step in understanding where the cross-country evidence may be relatively useful. The rest of the section, perhaps the heart of the survey, covers evidence from growth accounting and growth regressions, and then some recent attempts to measure externalities to education.

Section 4 turns to social capital and growth. The section discusses the definition of social capital, reviews the macroeconomic evidence on its growth effects, and briefly discusses the prospects for further research in this area. Finally, section 5 rounds off with a summary and some conclusions.

2. Education and growth

In thinking about education, both theoretical and applied economists have usually taken a rather narrow view of its benefits. This section begins by setting the narrow view in a wider context, something that is important in forming an overall opinion on policy. The remainder of the section discusses some recent theoretical models. The aim here is to see whether formal models shed any light on the possible connections between education and growth.

Education makes a fundamental contribution to personal development, and probably to the health of societies more generally. In thinking about policy, it is crucial to remember that education may have significant welfare benefits that are not captured in the models and data typically analysed by economists and governments. Given that economics is often defined as the study of the relation between the allocation of scarce resources and human welfare, the wider benefits are clearly within the remit of the subject. Benefits could include effects on public health, crime, the environment, parenting, and political and community participation; they are discussed in more detail in OECD (1998, ch. 4), Behrman and Stacey (1997) and Wolfe and Haveman (2001).

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consequences for the value of subsequent on-the-job training. Also note that some international

Even the benefits of education that accrue directly to individuals are not always well captured in economic theory and empirical analysis. It is plausible that education has both an immediate consumption benefit and a long-term effect on life satisfaction, other things equal. The difficulty here is that it is much harder to measure well-being in a meaningful way than it is to measure output of goods and services, and economists are only just starting to investigate well-being and its determinants. In an innovative paper, Blanchflower and Oswald (2000) report estimates of 'happiness equations', regressions that relate survey measures of well-being to individual characteristics. They find that educational attainment is associated with greater happiness, even when controlling for family income.

Such findings could have important implications for education policy. For example, it is quite possible that the extent of an individual's education has a positive effect on the well-being of others, in which case self-interested individuals may tend to under-invest in education from society's point of view. Alternatively, education may affect happiness because it influences perceptions of status relative to others, in which case the results of Blanchflower and Oswald could overstate the effect on well-being of an expansion of educational provision.

Since not much is known about these effects, I will follow the literature in examining the consequences of education for productivity. A natural starting point is to think about the direct benefit, the difference a worker's education makes to his or her own output per hour worked. Under some assumptions, discussed below, the influence of education on productivity can be estimated using wage differentials between workers who vary in educational levels.

Compared to other measures of productivity, output per worker hour is often the best guide to welfare, not least because one benefit of an increase in hourly productivity may be that individuals choose to work fewer hours. Yet for some purposes, policy-makers are interested in output per worker and output per head, as well as output per worker hour. Education may also have

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comparisons of training programmes can be found in OECD (1998, ch. 3).

effects on these variables, and not simply through productivity. For example, education is often thought to affect labour force participation, particularly that of women.[3] It may also affect the non-monetary benefits associated with work and leisure, and so affect working hours.

Given that these effects are often less relevant to welfare than changes in hourly productivity, I will usually restrict attention to the latter. Even then, there are some other indirect effects of education to take into account. It is quite plausible that the extent of a worker's educational attainment will have an effect on the productivity of others that is not fully captured in the individual's own wages. Capturing such externalities to education using micro data is difficult, but significant progress has been made, and this work will be covered below. Externalities are also an important motivation for looking at the relation between education and growth at the level of countries.

Before turning to a detailed review of the evidence, I will briefly summarise theoretical work on the connection between education and growth.[4] It is often claimed that education plays a central role in growth. Can this argument be given a secure foundation in terms of economic theory? How plausible are the necessary assumptions? Do the models capture the growth effects of education, as it is generally defined and understood, or of something else?

One of the most prominent and influential contributions is that of Lucas (1988), which is in turn related to previous work by Uzawa (1965). In these models, the level of output is a function of the stock of human capital. In the long-run, sustained growth is only possible if human capital can grow without bound. This makes it difficult to interpret the Uzawa-Lucas conception of human capital in terms of the variables traditionally used to measure educational attainment, such as years

[3] The interaction between growth, human capital and female labour force participation is discussed in more detail by Mincer (1996). For evidence on female labour force participation in the OECD, see OECD (1998, ch. 4).

[4] A more detailed and rigorous summary can be found in Aghion and Howitt (1998, ch. 10).

of schooling. Their use of the term 'human capital' seems more closely related to knowledge, rather than to skills acquired through education.

One way to relate the Uzawa-Lucas model to the data is to suggest that the quality of education could be increasing over time (Bils and Klenow 1998). In this view, the knowledge imparted to schoolchildren in the year 2000 is superior to the knowledge that would have been imparted in 1950 or 1900, and will make a greater difference to their productivity in later employment. On this interpretation, even if average educational attainment is constant over time, the stock of human capital could be increasing in a way that drives rising levels of output.

Yet this argument runs into difficulties, even at the level of university education. There may be some degree courses in which the knowledge imparted currently has a greater effect on productivity than before (medicine, computer science, perhaps economics), but there are other, less vocational qualifications for which this argument is less convincing. At the level of primary and secondary schooling, with their focus on basic skills such as literacy and numeracy, the idea that increases in the quality of schooling drive sustained growth seems even harder to support. Finally, note that these models are typically silent on exactly how the increase in the quality of schooling is brought about: individuals can raise the stock of human capital, or knowledge, simply by allocating some of their time to its accumulation.

An alternative class of models places more emphasis on modelling the incentives that firms have to generate new ideas. Endogenous growth models based on the analysis of research and development, notably the landmark contribution of Romer (1990), yield the result that the steady-state growth rate partly depends on the *level* of human capital. The underlying assumption is that human capital is a key input in the production of new ideas. In contrast with the Uzawa-Lucas framework, this opens up the possibility that even a one-off increase in the stock of human capital will raise the growth rate indefinitely. Indeed, in many endogenous growth models, human capital must be above a threshold level for any innovation to take place at all.

In practice, the generality of these results, and the contrast with the Uzawa-Lucas model, should not be overdrawn. The Uzawa-Lucas framework can be seen as a model of knowledge accumulation in a similar spirit to that of Romer, but easier to analyse; and restrictive assumptions are needed to yield the Romer result that the long-run growth rate depends on the level of human capital (Jones 1995). But even under more general assumptions, a rise in the level of human capital is likely to be associated with a potentially substantial rise in the level of output and welfare, brought about through a transitional increase in growth rates.

In most endogenous growth models based on research and development, the stock of human capital is taken to be exogenously determined. More recent papers, notably Acemoglu (1997) and Redding (1996), have relaxed this assumption, and considered what happens when individuals can choose to make investments in education or training, while firms make investments in R&D. For some parameter values, multiple equilibria are possible, since the incentives of workers to invest in human capital, and those of firms to invest in R&D, are interdependent. This provides a way of formalizing earlier ideas about the possible existence of a "low-skill, low-quality trap" in which low skills and slow rates of innovation reflect a coordination failure (Finegold and Soskice 1988). The models suggest that, at the aggregate level, greater investments in education or training might raise expenditure on R&D, and vice versa.

Another interesting aspect of recent growth models is their suggestion that individuals may under-invest in education. Rustichini and Schmitz (1991) examine this argument in some detail. They present a model in which individuals divide their time between production, original research, and the acquisition of knowledge. Each individual knows that acquiring knowledge (through education) will raise his or her productivity in subsequent research, but since individuals do not fully capture the benefits of research, they will tend to spend too little time acquiring knowledge

relative to the socially optimal outcome.[5] Rustichini and Schmitz calibrate a simple model, and find that although policy intervention has only small effects on the allocation of time to education, it can have a substantial effect on the growth rate.[6]

In summary, these models are important for several reasons. First, they see human capital as an important input in the creation of new ideas, and this mechanism provides a relatively appealing justification for viewing education as a central determinant of growth rates, even over long time intervals. Second, they sometimes yield the result that the laissez-faire outcome delivers slower growth than is socially optimal. Third, the models suggest that policy-makers wishing to raise the level of welfare have several options: not just subsidies to R & D - which may be difficult to implement and monitor - but also subsidies to certain kinds of education, perhaps especially those which could lead to later work in research and development. Overall, the models suggest that, in searching for the determinants of growth, education is one of the first places to look.

3. Education and growth: the evidence

As described above, recent theoretical models suggest that educational attainment is potentially a key determinant of growth. In this section, I will turn to the attempts of economists to quantify this effect. The main focus will be on the macroeconomic evidence: the body of research which measures, or attempts to measure, the productivity benefits of education using the variation in educational attainment and growth rates across countries.

It would be a mistake, however, to review this evidence without first discussing the work on education and earnings by labour economists. Many of the arguments that education can affect

[5] The assumption that it is difficult to fully capture the benefits of research is uncontroversial. The presence of substantial research spillovers is intuitively plausible, and supported by empirical evidence. Griliches (1992) provides a survey.

growth ultimately depend on the relation between individuals' education and their levels of productivity. Evidence for this effect, even measured indirectly using data on earnings, would obviously strengthen the overall case for seeing education as a determinant of growth outcomes.

Moreover, an understanding of the strengths and weaknesses of findings in labour economics helps place the macro evidence in context, clarifies the areas in which the macro approach may have something worthwhile to contribute, and also points to the areas in which micro evidence is much more likely to be fruitful. With this in mind, section 3.1 reviews studies of the effects of education based on earnings surveys. Later sections cover growth accounting (3.2), the evidence from cross-country regressions (3.3), and recent work on externalities to human capital (3.4). Finally, section 3.5 attempts to tie together the various pieces of evidence.

It will be argued that each approach to measuring the productivity effects of education has its own important weaknesses and areas of uncertainty. Yet taken together, the various methods tend to agree in pointing to quite substantial effects of education. These effects are probably large enough by themselves to justify current expenditure on education within many OECD countries, even before consideration of wider benefits. Broadly speaking, this work might also justify an expansion of educational provision in at least some countries, with a consequent effect on the growth rate that can be assessed using the methods of growth accounting.

3.1 Evidence from labour economics

Labour economists typically study the link between education and productivity using survey data on the earnings and characteristics of large numbers of individuals. The techniques used to analyse these data have become increasingly sophisticated, and we will see that evidence from 'natural experiments' provides measures of the private return to education that are probably quite

[6] A complete welfare analysis of policy intervention would need to consider the effects on the level

accurate. There is much greater disagreement on the extent to which labour economists have identified the *social* return to education. A particularly important argument is that educational wage differentials (and hence measures of the private return) may largely reflect the value of educational qualifications as a signal of ability. As a result, even the high private returns found by labour economists are potentially consistent with the view that education does not affect productivity. Later in this section, I will discuss the extent to which even the most recent evidence is vulnerable to the signalling critique.

The standard empirical approach is to explain the variation in wages across individuals using regressions, where the explanatory variables include years of schooling, either age or a simple proxy for experience, and other characteristics. The most popular specification draws heavily on the work of Mincer (1974), and earlier contributions on 'human capital earnings functions'. The starting point is typically a specification that looks something like this:

$$\ln w = \alpha + \beta_0 S + \beta_1 E + \beta_2 E^2 \qquad (1)$$

which relates the natural logarithm of wages (w) to years of schooling (S) and a proxy for labour market experience (E). Under some assumptions, and given the semi-logarithmic formulation, the coefficient on schooling can be interpreted as the private return to education. Under the assumption that workers are paid their marginal product, this coefficient may also capture the social return to education. Empirical estimates of the return typically have a relatively small standard error and lie somewhere between 5% and 15%, depending on the time and country.

The evidence that earnings are positively associated with schooling is robust and uncontroversial; the obvious difficulty lies in giving this association a causal interpretation. Over time, labour economists have built up considerable knowledge of the problems in using estimated

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of the output path, as well as its growth rate.

earnings functions to draw inferences about the direct contribution of education to productivity. Card (1999) provides an excellent review and synthesis of this literature, and I will give only a broad overview of the relevant issues.[7]

One of the most easily understood problems is that, through lack of suitable data, the econometrician estimating (1) inevitably has to omit important variables that are likely to be correlated with both schooling and earnings. Family background and traits such as innate ability or determination are notable examples. The basic problem, from the econometrician's point of view, is that the group of people with a relatively advanced level of educational attainment is not a random selection from the population as a whole. For example, if more able individuals have relatively high earnings regardless of extra education, and also choose to spend more time in school, then the estimated return to schooling overstates the effect of education on productivity. If ability is not observed by employers, then the regression estimate may still capture the private return to schooling, but it will not capture the social return that is ultimately our main interest.

Unfortunately the problems do not stop there. It seems probable that the costs and benefits of education vary across individuals, perhaps substantially. Indeed, this is likely to be the principal cause of the variation in completed schooling that the econometrician uses to identify the effects of education. The heterogeneity will typically mean that the private returns to education vary across individuals. In the unlikely case where the returns vary independently of the explanatory variables, the least squares estimate should recover an unbiased estimate of the average return. More generally, the heterogeneity problem will lead to biased estimates. For instance, imagine that there are differences in the quality of schools: it seems quite likely that students in a higher-quality school will achieve a higher return to their education, and also choose more schooling. This

[7] Another very useful survey is that of Ashenfelter et al. (1999), which includes a detailed investigation of problems associated with publication bias.

correlation between the estimated parameters and the explanatory variables will give rise to a bias in the estimate of education's effect.

The recent focus of the literature on education has been on identifying natural experiments, in the hope that these will allow stronger claims about causality to be made. Researchers look for situations in which the level of schooling varies across individuals for reasons that are likely to be independent of the unobserved characteristics of those individuals (ability, determination, and so on).

The idea is best explained by means of an example. A good starting point is one of the most influential papers, by Angrist and Krueger (1991). The paper starts from the observation that, when it is compulsory to stay in school until a certain age, individuals born earlier in the calendar year will reach the legal minimum age for school-leavers at an earlier stage in their education. As a direct result, there is likely to be a correlation between an individual's quarter of birth and their length of schooling. The correlation means that quarter of birth can potentially be used to identify exogenous variation in schooling - that is, variation independent of unobserved characteristics like ability or determination. In econometric terms, quarter of birth can be used as an instrument for schooling, under the maintained assumption that characteristics other than schooling are independent of quarter of birth. Somewhat surprisingly, Angrist and Krueger find that the instrumental variable estimates of the return to schooling are similar to the least squares estimates, supporting the idea that conventional estimates are reasonably accurate.

Another much-discussed natural experiment is provided by identical twins who have different levels of schooling. Given that such twins have the same genes, and will usually share the same family background, the wage differential between twins with different years of schooling may provide useful information on the productivity effect of education. Finally, other natural experiments are provided by the possible connection between the geographical proximity of colleges to individuals, and their choice of schooling (see Card 1999).

Research of this kind has considerably strengthened the case for productivity effects of education, but even these studies retain an important weakness. It has long been understood that the private return to education may exceed the social return. The theoretical work of Spence (1973) indicated that educational attainment may be valued by employers mainly because it acts as a signal of innate ability, and not because it has an effect on productivity.

Models of signalling start from the observation that individuals have traits which employers value but do not observe at the time of hiring (ability, determination, and so on). If there is a systematic association between these traits and the costs and benefits of education, this may lead to an equilibrium in which high-ability individuals stay in school for longer because this decision signals their ability to employers. This argument provides a plausible reason for a correlation between ability and years of schooling, and suggests that earnings may be correlated with schooling even if schooling has no effect on productivity.

Few doubt that signalling plays some role in explaining educational wage differentials, but its overall importance remains controversial. Weiss (1995) and Quiggin (1999) provide very different perspectives on the theoretical generality and empirical validity of signalling models. There are two main arguments against such models, which note the implications of the assumption that education has no effect on productivity. First, given the wage premium earned by those with more years of schooling, employers would probably have strong incentives to conduct their own tests of ability and other characteristics, and use this direct information rather than the somewhat indirect signal provided by the schooling decision. This view is supported by evidence that measured performance in school and universities is correlated quite strongly with the outcomes of tests carried out at an earlier stage (see Quiggin 1999 for references). Yet the argument is not conclusive, mainly because employers may not be able to appropriate the returns to acquiring more information about their employees; other firms could bid away those workers found to have higher ability (Stiglitz 1975).

The second argument is that, if education does not affect productivity, one would expect to see the education differential decline with job tenure, as employers acquire direct knowledge of the characteristics of their employees. This does not seem to be observed in the data, although this question has not received the sustained attention it probably deserves.

More generally, there is clearly room to develop and test signalling arguments in more detail. This is important not least because, as Weiss (1995) has pointed out, even the results of natural experiments are not necessarily inconsistent with the signalling view of education. To see this, recall that employers may use years of schooling to gain information about unobserved characteristics. The results from the Angrist and Krueger quarter-of-birth study and the work on twins can easily be interpreted in terms of these signalling effects, and so their findings do not contradict even the extreme view that productivity is entirely independent of education.

For example, using wage differentials between differently-educated twins only tells us about productivity effects of education if employers observe the other characteristics of their employees, and reward them appropriately. If we are prepared to assume that employers do not observe these characteristics, and that a signalling model applies, then even the native ability of the twins will appear different to prospective employers, since they have different schooling. To assess fully the signalling argument using evidence from twins, the relevant natural experiment is not whether twins with different schooling are paid differently. The relevant experiment, much harder to find, is whether twins with different schooling but the same employer are paid differently.[8]

In summary, there is an ingenious and persuasive body of work which measures the returns to schooling while making plausible assumptions about the variables not observed by the econometrician. More accurate estimates of the private return to schooling will be extremely hard to achieve. Unfortunately, this does not tell us all we need to know about social returns, or

[8] Alternatively, the twins evidence remains informative in the unlikely case where each employer of a twin knows the wage and schooling of the other twin.

productivity effects. To interpret even the latest evidence as telling us something about productivity effects, we have to make some potentially unattractive assumptions about the extent to which certain variables are observed by employers. As a result, the current estimates of private returns, however accurate, may remain quite misleading about social returns.

For now, assume that employers fully observe all relevant characteristics, and hence do not infer any information about them from schooling decisions. Even in this case, as Card notes, not much is presently known about the mechanisms by which education might contribute to higher wages. The simplest interpretation of the evidence from earnings functions is that more educated individuals are more productive, whatever their chosen occupation. In practice, a college degree is unlikely to make one a better postman or roadsweeper.

It seems likely that more complex mechanisms are present, and these may again have implications for the interpretation of earnings functions. For instance, more educated workers may have better access to those jobs in which workers share some of the rents earned by imperfectly competitive firms. If mechanisms like this are at work, there would again be less reason to believe that the observed correlation between schooling and earnings represents solely a direct productivity effect.

There are other ways in which private and social returns could differ. In some countries, especially poorer ones, the public sector is a major employer of the well-educated. As Pritchett (1996) emphasises, the assumption that wage differentials reflect differences in marginal products is much harder to sustain in this context. If educational credentials are used as a means of determining access to rationed high-paying jobs in the public sector, estimated earnings functions may detect an effect of education even when it has little or no effect on productivity.

The general problem is that estimates of earnings functions capture, at best, the private return to education, yet it is the social return which is of most interest to policy-makers. The two

may diverge for a number of reasons, including the possibility that education functions purely (or mostly) as a signalling device. The arguments discussed above imply that the social return to education is less than the private return, and as we have seen, even just a lower bound on the social return is difficult to establish.

It remains worth bearing in mind that other mechanisms will have a positive effect on the social returns to education. It is plausible that individuals do not fully capture some of the benefits to society of their schooling, and I will review some of the empirical evidence on externalities in section 3.4 below. Another important argument is that educational provision may play a valuable role in allowing a more efficient matching between workers and jobs (Arrow 1973, Stiglitz 1975). In other words, even if education does act mainly as a signal, there should not be a presumption that education is, therefore, socially wasteful.

Overall, labour economists appear keen to note the extent to which recent studies, based on variation in institutions or differences in schooling between twins, continue to support the view that schooling has a causal role in raising productivity and earnings. Card (1999) concludes that the average marginal return to education is unlikely to be far below the estimate that emerges from simple least squares estimates of earnings functions.

The view that this represents a productivity effect is far from universally accepted, however. As we have seen, Weiss (1995) argues that even the most recent results can be interpreted in the light of signalling models, and that labour economists have been strangely reluctant to acknowledge the potential relevance of this approach. This viewpoint suggests two lines of enquiry that might be particularly fruitful. The first is further theoretical examination (and perhaps calibration) of signalling models, with a particular focus on the extent to which they can incorporate the direct productivity effects envisaged in the traditional theory of human capital. Second, more evidence on the extent to which educational wage differentials evolve with job tenure could be of great interest in advancing the debate.

3.2 Growth accounting

The labour economics literature provides a wealth of evidence on the private returns to schooling. As we have seen, the contribution made to productivity by education is uncertain but may be worthwhile, even before we start to think about possible externalities. Making similar assumptions to those of labour economists, the 'growth accountants' have set about the complex task of evaluating this contribution relative to other sources of growth. Again, we will see that the degree of uncertainty is considerable. Even the most careful and rigorous studies may substantially mismeasure the overall contribution of education.

Growth accounting essentially divides output growth into a component that can be explained by input growth, and a 'residual' which captures efficiency change, partly reflecting changes in technology. In explaining the change in output, the change in the quantity of each input is weighted by its marginal product, proxied by its market reward. This principle can be extended to any number of inputs, and where sufficiently detailed data are available, it is possible to disaggregate the labour force into various categories, where each type of worker is weighted by the average wage of that type.

For instance, in analysing the contribution of changes in educational attainment, the researcher disaggregates the labour force by level of schooling, and often by other available characteristics such as age and gender. Changes in the number of employees at each level of schooling are then weighted by their marginal products, proxied by the mean income associated with each schooling level, to give the overall change in an index of 'effective' or quality-adjusted labour. This ultimately allows the researcher to quantify the proportion of output growth that can be directly attributed to increases in educational attainment.

Griliches (1997) provides a brief but useful survey of this literature, and points out the two major assumptions, both of which will have a familiar ring to readers of the previous section. First, it is assumed that differences in observed market rewards correspond reasonably closely to differences in marginal products. Secondly, the calculations assume that differences in market rewards across schooling levels originate in schooling, and not in other factors such as native ability or family background that may be correlated with schooling.

The advantage of the first assumption, that market rewards correspond to marginal products, is that it allows the growth accountant to weight the growth rates of factor inputs using available data on factor shares, under the assumptions of constant returns to scale and perfect competition. Less restrictive frameworks are possible, but will generally tend to require assumptions about other parameters for which data are not readily available. It should also be clear that conventional growth accounting, imputing output elasticities based on market rewards, will not shed any light on the possible extent of externalities. This is a major limitation, and an important motivation for the cross-country empirical studies that will be considered further below.

What of the second assumption, that differences in wages originate in schooling? The danger here can be seen from considering an extreme scenario, in which education has absolutely no effect on an individual's productivity, but more able individuals both stay longer in school and earn more while in employment.[9] This scenario clearly implies that educational attainment and earnings are positively correlated. Now consider an exogenous increase in the proportion of individuals with the highest level of education: since the index of labour quality weights the numbers in each education class by the mean income of that class, the index must increase. As a result, the growth accountant will attribute some portion of growth to educational improvement, even though education plays no role in productivity gains.

[9] For example, more able individuals may choose to stay longer in school because they derive a greater consumption benefit.

This assumption clearly brings us back to the labour economics literature, perhaps looking for reassurance that education plays at least some role in raising productivity. As we saw in the previous section, many prominent labour economists seem to regard the observed correlation between earnings and schooling as largely reflecting a genuine productivity effect of education. If they are right, growth accounting provides a rough lower bound for the overall contribution of education to growth.

Growth accounting exercises vary widely in the extent to which they disaggregate labour input. Nearly all the studies which carry out a detailed disaggregation by level of schooling are restricted to the United States; a prominent example is Jorgenson, Gollop and Fraumeni (1987). For the period 1948-79, they find that growth in labour input has contributed about a third of growth in aggregate value added, where the measure of labour input takes into account both hours worked and the quality of labour. Changes in their aggregate index of labour quality are based on changes in the composition of total hours worked by age, sex, education, employment class and occupation. They find that a favourable shift in labour quality is responsible for about a tenth of the growth in value added, or about a fifth of the productivity residual that remains after accounting for the contribution of growth in physical capital (see their Table 9.5).

In interpreting the results of Jorgenson, Gollop and Fraumeni, it is important to note that some of the compositional shifts within the labour force have a *negative* effect on the index of labour quality over the 1948-79 period, which partly offsets the benefits of improvements in educational attainment. As previously noted, the calculation of the labour quality index assumes that differences in market rewards reflect genuine differences in marginal products. One consequence is that the increasing entry of women and young workers into the labour market, mainly into low-paying jobs, has a negative effect on the aggregate index of labour quality.

Over the 1948-79 period, the negative effect on the index of labour quality is more than offset by positive changes in the composition of the labour force by educational attainment and

occupation. One implication is that the latter effects are likely to be responsible for more than a fifth of the productivity residual, since the favourable shift in labour quality would have been larger in the absence of the change in composition by age and sex.

In reviewing the evidence as a whole, Griliches (1997) writes that increases in educational attainment seem to have accounted for perhaps a third of the productivity residual in the US over the post-war period. In the 1950s and 1960s, this would correspond to an effect on the annual growth rate of aggregate output of around 0.5 percentage points; during the 1970s productivity slowdown, the effect of educational improvement would have been lower, perhaps raising the growth rate by 0.2 or 0.3 percentage points.

For other OECD members, there are few studies that cover recent experience in the same degree of detail as Jorgenson, Gollop and Fraumeni. The best known work in this respect is that of Maddison (1987, 1991). I will first consider the general trends in educational attainment that are highlighted by his work, and then measures of the impact that are derived from growth accounting. More detail on the general trends can be found in OECD (1998, ch. 2).

Maddison (1991, 138) argues that the 20th century saw a fairly steady improvement in educational attainment for the six countries he considers (France, West Germany, Japan, the Netherlands, the UK and the US). One implication is that changing trends in educational attainment are unlikely to provide a satisfactory explanation for the transition from Europe's 'Golden Age' of rapid growth (1950-73) to the productivity slowdown after 1973.

Examining trends in more detail, Englander and Gurney (1994a) note that tertiary education in particular has expanded rapidly in many OECD countries since 1960. Even in the absence of further increases in tertiary enrollment, the average educational attainment of the labour force will continue to increase for some time as older, less well qualified workers retire from employment. It

may be too early to tell whether or not this continuing increase in average attainment has resulted in a significant growth payoff.

Maddison (1987, 1991) estimates the growth impact of changes in educational attainment for six countries, by disaggregating the labour force into those with primary, secondary and higher qualifications. He then combines these three different types of labour using weights that are the same across countries and over time. Perhaps more importantly, in selecting the weights, he follows Denison (1967) in assuming that observed educational wage differentials overstate the contribution of education to productivity, because the differentials are also affected by other characteristics that are correlated with schooling. Inevitably, adjustments for the size of this effect are somewhat arbitrary. They highlight, rather than eliminate, the uncertainty inherent in using growth accounting to measure the impact of changes in educational attainment. The other point to note is that, because of these adjustments, the estimates of Denison and Maddison are not directly comparable with those of other studies.

With all this in mind, we can turn to Maddison's results on the contribution of increases in labour quality to output growth in France, West Germany, Japan, the Netherlands, the UK and the US. His figures suggest that changes in the quality of the labour force typically added between 0.1 and 0.5 percentage points to annual growth rates between 1950 and 1984 (his Table 20). The Maddison index of labour quality takes into account changes in the male/female composition (though not age composition) of the labour force, as well as changes in educational attainment. In countries where the proportion of women in the labour force has noticeably risen, such as the UK and the US, the contribution of education to growth will be slightly higher than the reported contribution of growth in labour quality. For other countries, the difference between the two figures will be very small, and certainly dwarfed by the other sources of uncertainty that surround the approach.

More recent studies include that of Jorgenson and Yip (1999), who have recently carried out a detailed growth accounting exercise for the G7, and present estimates of growth in labour quality for 1960-95 (their Table 7). These estimates suggest that labour quality has grown particularly quickly in Japan, and to a lesser extent, relatively quickly in France and the US. The Jorgenson-Yip disaggregation of the labour force is slightly finer than that adopted by Maddison, and this makes it harder to assess the role of education within changes in the overall index of labour quality.

A useful survey by Englander and Gurney (1994b) draws together the results of a number of studies for the G7, although some of this evidence is based on regressions rather than growth accounting. Their summary suggests that for the 1960s to 1980s the growth of labour quality (sometimes including demographic effects of the kind discussed above) typically accounts for 10%-20% of growth in total output. In some ways it is more informative to look at the fraction of growth in output per worker that is explained. Growth in output per worker is lower than output growth in those countries, like the US, where there has been a rapid increase in employment. In such countries, the increase in educational attainment will sometimes account for a proportion of growth in output per worker slightly higher than the 10%-20% suggested by the survey of Englander and Gurney.

Another OECD country for which detailed growth accounting results are available is Korea. The most influential contribution is that of Young (1995), who examines and compares the growth performance of four East Asian economies. For the purpose of the present survey, the case of Korea is particularly interesting in that the country has seen a dramatic increase in the educational attainment of the labour force. Between 1966 and 1990, the proportion of the working population with secondary level education or higher roughly trebled, from 27% to 75%. Yet this dramatic expansion does not translate into an equally dramatic effect on the growth rate, at least under the assumptions of growth accounting. For each of the four economies he considers, Young finds that

the improving educational attainment of the workforce raises the annual growth rate of effective labour input by about one percentage point (Young 1995, p. 645).

I end this section by noting a crucial qualification to all growth accounting results. Findings in this area require careful interpretation, because the approach does not tell us everything we need to know about the relevant counterfactual.[10] As an example, consider a claim that X percentage points of growth in a given country is due to a change in the quality of the labour force. This does not imply that, in the absence of the change in labour force quality, the growth rate of output would have been precisely X percentage points lower. The problem is that educational attainment may have other, indirect effects on output through labour force participation, investment, and even R&D and the growth of total factor productivity. Growth accounting does not capture these indirect effects, and so it is necessarily silent on the overall importance to growth of variables like education.

3.3 Evidence from growth regressions

Although growth accounting exercises are informative and often useful, it is clear that they are not a complete substitute for other forms of investigation, given the necessary assumptions. Griliches (1997, S333) writes that "the main, and possibly only, approach to testing the productivity of schooling directly is to include it as a separate variable in an estimated production function". Such estimates could be at the level of firms or regions, but much of the evidence uses the variation in education across countries, and it is to such estimates that I turn next.

As the quotation from Griliches makes clear, the key attraction of growth regressions is that they provide a way of testing directly for productivity effects of education. Similarly, Arrow (1973, 215) pointed out that the use of macroeconomic evidence would be one way of testing the

[10] Barro and Sala-i-Martin (1995, 352) make this point in greater detail.

signalling arguments, although he also expressed scepticism about the usefulness of this empirical approach.

Recent work on these issues has led to a better understanding of precisely when and where scepticism might be justified. In what follows, I will review the most important problems associated with measuring growth effects of education at the macroeconomic level. An underlying theme is that, despite these problems, there are some grounds for optimism that this research can make a valuable contribution.

This may seem surprising, given that several well-known papers in this field take very different views on the importance of education. Now a more coherent story is emerging, one which is consistent with the effects identified by labour economists, and which can also explain why some cross-country studies have failed to detect any significant effect of education using aggregate data. As we will see, as the treatment of measurement and specification issues has improved, stronger findings have started to emerge.

One of the best known and most influential contributions to the empirical growth literature is that of Mankiw, Romer and Weil (1992) or MRW. Their parameter estimates for an OECD sample can be used to illustrate the potential importance of education. If taken at face value, their estimates imply that if human capital investment (as a share of GDP) is increased by a tenth, output per worker will rise by 6%; if investment in human capital is doubled, output per worker will eventually rise by about 50%. (The details of this calculation are described in the Appendix.)

It is essential to emphasise that these figures should not be taken too seriously. All growth regressions share a number of important statistical problems. In the case of MRW, even quite simple extensions, such as the inclusion of equipment investment in the regressions, mean that it can be difficult to get precise estimates of the relevant parameters. Sometimes the hypothesis that

education has no effect cannot be rejected (as in Temple 1998, Tables 2-4).[11] In the present context, another drawback of most regression studies is their focus on a large sample that includes less developed countries as well as OECD members. One should clearly be rather wary about drawing conclusions for OECD policy based on samples that are often dominated by developing countries. I will usually concentrate on the few studies that include separate estimates of regressions for OECD members (or alternatively, samples of rich countries).

Researchers have generally used one of two specifications in modelling growth and education. In the first, and most common, the researcher chooses to regress growth on control variables and the initial level of an education measure, such as the secondary school enrollment rate or (preferably) average years of schooling. The underlying idea is that the stock of human capital could affect subsequent growth in a variety of ways, notably by influencing a country's ability to adopt technology from abroad.[12]

Those working along such lines typically find an effect of schooling that is both large and precisely estimated, at least when initial output per worker is also included as an explanatory variable (Barro 1991). Yet it is not clear that these results are applicable to OECD members. In an interesting exercise, Englander and Gurney (1994a) re-estimate growth regressions based on four influential papers, including Barro (1991), but restricting the sample to the OECD. Three of the four sets of regressions include human capital variables, typically primary and secondary school enrollment rates.[13] These variables turn out to perform relatively well, but are still far from robust.

[11] Other papers which discuss the robustness of the MRW results for the OECD sample include Nonneman and Vanhoudt (1996) and Vasudeva Murthy and Chien (1997). A more general discussion of statistical problems associated with growth regressions can be found in Temple (1999a).

[12] There is also important work on human capital as a determinant of technological catch-up using data at the sectoral level. For example, Cameron, Proudman and Redding (1998) investigate the role of human capital and openness to trade in explaining catch-up by UK manufacturing sectors.

[13] Of the two measures, only the secondary school enrollment rate is likely to be relevant in explaining growth within the OECD. Englander and Gurney (1994a) report that average secondary

In further work, it may be valuable to repeat this exercise, drawing on more recent data sets that allow one to use average years of schooling rather than enrollment rates.

Another interesting paper that includes results for OECD samples is Gemmell (1996). He emphasises the problems of using enrollment rates, and constructs alternative measures of human capital based on attainment at the primary, secondary and tertiary levels. For a sample of 21 OECD countries, he finds a correlation between the number of people with tertiary qualifications and subsequent growth. He also finds some evidence that investment in OECD countries is positively correlated with the extent of secondary schooling in the labour force.

One drawback of most cross-country work is the likelihood of important differences in the nature and quality of schooling across countries, which could undermine the usefulness of international comparisons. Even such things as the length of the school year can show a surprising degree of variation across countries. An alternative data set, which may overcome these problems to some extent, has been introduced by Hanushek and Kim (1995). They propose measuring educational attainment using scores in international tests of cognitive skills in maths and science. Their results support the idea that education has an important effect on growth. Again, the sample includes less developed countries, and it would be interesting to examine the robustness of the results in a sample restricted to OECD members.

The lack of studies with direct relevance to the OECD is not the only dilemma for those who wish to draw policy conclusions for developed countries. The rather atheoretic approach of the macroeconomic literature on education and growth has attracted a certain amount of criticism, notably from labour economists. One argument, used by Topel (1999), is that the measured effect of the initial level of human capital is simply too large to be credible. This claim ultimately rests

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enrollment in the OECD was about 70% in 1960, so there may be enough variation across countries for regression evidence to be informative.

on the assumption that traditional earnings functions roughly capture the social returns to education, which may take too narrow a view of the potential growth benefits.

Another argument perhaps has more force. Starting with Pritchett (1996), researchers have noted the implications of traditional earnings functions for analyses at the cross-country level. If an individual's education contributes directly to their productivity, in the manner envisaged by labour economists, we should expect to observe a cross-country correlation between the change in output per worker and the *change* in average educational attainment, at least after controlling for other variables. Furthermore, it should be possible to detect this effect regardless of whether or not the initial *level* of educational attainment determines growth.

This argument has shifted the focus of research, towards regressions that relate growth to the change in educational attainment, rather than its level. Several well-known studies have found the correlation to be surprisingly weak; Benhabib and Spiegel (1994) and Pritchett (1996) both come to this conclusion for a large sample of countries.[14] Benhabib and Spiegel do find a statistically significant correlation between the level of educational attainment and growth for the wealthiest third of the sample (their Table 5, model 2) but no connection between the change in attainment and growth in a larger sample.

It is possible to question this latter result, as in Temple (1999b), since a strong relation can be discerned when some influential outliers are eliminated. There are a number of other problems that dictate caution in reading these papers. One is the specification chosen for the relation between years of schooling and output. The specification adopted by Benhabib and Spiegel, and by Pritchett, implicitly assumes that the returns to an extra year of schooling are much higher at low levels of schooling than at high levels. As Topel (1999) points out, this runs contrary to the

[14] This finding is also associated with a number of panel data studies using fixed effects, but these results should almost certainly be discounted. Researchers using panels typically do not allow for lags in the effect of variables like enrollment rates. In any case, given the way the education data are constructed, the time series variation is probably too noisy to draw sensible conclusions.

standard semi-logarithmic formulation for earnings functions, which in its simplest form assumes that the returns to an extra year of schooling are independent of the level of schooling. When growth regressions are specified in a way more compatible with this idea, the evidence for a growth effect of changes in human capital is rather stronger.[15]

Krueger and Lindahl (1999) have argued convincingly that another important problem is likely to be measurement error. The difficulty is that a specification based on an aggregate production function (as in Benhabib and Spiegel) typically seeks to explain growth using the change in educational attainment, but first-differencing the education variable in this way will usually exacerbate the effect of any measurement errors in the data.

To support this argument, Krueger and Lindahl examine the correlation between two different measures of the change in average years of schooling that have been used in the literature. The correlation is low enough to suggest that a substantial component of the measured change in educational attainment is uninformative noise. As a consequence, regressions that use the change in education to explain growth will tend to understate its importance.[16]

The case for seeing measurement error as an important part of the story has been considerably strengthened by the careful and impressive work of de la Fuente and Domenech (2000). Unusually, they restrict attention throughout to OECD members. Their close examination of standard data sets reveals that schooling levels for some countries appear implausible; some of the figures for average years of schooling display surprising short-run volatility; and others appear

[15] At this point, it is worth noting that the assumption of stable marginal returns is based mainly on evidence from developed countries. In such countries, given compulsory schooling laws, the range of variation for (measured) educational attainment is relatively low. That is not true for the poorer countries included in these samples, and the appropriate specification for marginal returns at very low levels of schooling remains an open question.

[16] Note, though, that measurement error in other explanatory variables (notably physical capital) could bias the coefficient on education in the opposite direction.

to give a misleading view of trends. Other writers, notably Steedman (1996), have also noted inconsistencies in the way data on human capital are collected and compared.

By drawing on national sources and more recent figures compiled by the OECD, de la Fuente and Domenech compile a new and more reliable data set of education measures for OECD countries. In their empirical work, they find that changes in output and educational attainment are positively correlated, even in panel estimates that include country and time fixed effects. This supports the idea that, where previous researchers have failed to detect an effect, this may be due to measurement error.

Overall, this literature is beginning to suggest that there is a correlation between changes in education and growth, of the kind that most labour economists would expect to observe. This is reassuring, but there are a number of interesting open questions. One obvious question mark surrounds the interpretation of the earlier results that related growth to the initial level of attainment, rather than the change in attainment. Growth studies for the OECD that allow a role for both possibilities simultaneously are yet to appear. This may be an important omission, especially when one recalls the possible role for human capital in the creation of new ideas, and thereby the possible connection between the level of education and subsequent growth.

There is another reason why the effect of the initial level of education remains of some interest. Studying the relation between the change in output and the change in education remains somewhat vulnerable to the charge that causality runs from output (or anticipated output) to education, and not simply vice versa.[17] To a large extent, changes in educational attainment are driven by government policy. It seems plausible that as output and tax revenues increase, governments will often allocate more resources to education, and attainment will rise.

[17] The two-way interaction between growth and education is discussed in more detail by Mincer (1996). As Bils and Klenow (1998) argue, the direction of causality may be uncertain even when attention is restricted to the growth effect of the initial level of education.

Yet the argument that panel data results, such as those of de la Fuente and Domenech (2000), are driven by reverse causation is rather less strong than it may appear at first. This is a key advantage of their use of data on average years of schooling in the population, rather than enrollment rates. Given that new entrants are typically a small fraction of the labour force, average attainment will change only very slowly in response to any change in educational provision. It, therefore, seems rather unlikely that reverse causation explains the de la Fuente and Domenech results.

Where does this leave us? Earlier in the survey, we saw the important qualifications that surround micro estimates of the social returns to schooling. Ultimately we would like the cross-country evidence to shed light on the accuracy of these estimates. In practice, we are likely to remain some way short of this goal, at least in the absence of better data. The aggregate evidence is currently too fragile to draw any strong conclusions about the possible extent of social returns.

Even so, the results we have provide some grounds for optimism, and it is reassuring that several recent studies find education to be important despite the likely presence of measurement error. This suggests that better data, and more sophisticated methods, may yet lead to a steady improvement in the precision of our estimates of the growth effects of education. The prospects for this should not be exaggerated, but there is certainly more reason to be hopeful now than in the early days of the literature, when the various sets of estimates were hard to reconcile into any kind of coherent story.

Another advantage retained by the macroeconomic approach, compared to micro estimates, is that we can explore indirect effects of education, notably those working through investment. These effects appear in the model estimated by MRW, and may have wider relevance. Two-sector models of endogenous growth, such as those reviewed in Barro and Sala-i-Martin (1995, ch. 5), typically yield a steady-state in which there is an equilibrium ratio of human capital to physical

capital. An immediate consequence is that a rise in educational attainment will eventually be met with a corresponding rise in the stock of physical capital.

Analysing the consequences for welfare is not wholly straightforward. The distinction between output and welfare matters, since the extra output directly associated with education could be allocated to consumption rather than increasing the capital stock. Growth economists have not yet developed and calibrated a model which derives overall output and welfare effects of education based on sensible microfoundations for investment.[18] This may explain why the effect is ignored by most interpretations of the empirical literature on education and growth. For now, it is important to be aware that growth accounting and growth regressions, by using capital investment as one of the conditioning variables, may understate the total impact of an increase in educational attainment on output per worker. The probable magnitude of this effect, and its significance for welfare, remain uncertain.

3.4. Human capital externalities

As we have seen, one important motivation for looking at the macro data is the possible presence of externalities to human capital. In this section, I will briefly review recent theory and evidence on this topic, before section 3.5 draws together the various strands of evidence on education and growth.

Interest in human capital externalities was revived by Lucas (1988, 1990). One of his arguments was that, in the absence of such externalities, it is difficult to reconcile observed pressures for migration from poor to rich countries with the absence of massive capital flows in the

[18] Although some theoretical work has started to appear: Masters (1998) analyses the efficiency of investments in human and physical capital in a bilateral search context.

other direction.[19] He also drew on the work of Jacobs (1969) to argue that such externalities are a natural explanation for the existence of cities.

In more recent work, Acemoglu (1996) has provided an ingenious justification for the presence of externalities. His theory is based on microeconomic foundations, and so is particularly worthy of attention. In his model, firms and workers make investments in physical capital and human capital respectively, before production begins. Production requires a partnership between a firm and a worker, but when firms or workers make their respective investments, they do not know the identity of their future partner. A key assumption of the model is that firms and workers are then brought together via a matching process that is imperfect, perhaps because searching for partners is costly.

Acemoglu shows how the structure of the model yields an important result: an increase in the average level of human capital can have a positive effect on the private return to human capital, at least over some region. The intuition is as follows: say that a subset of workers decides to acquire more human capital. This will raise average human capital, and anticipation of this encourages firms to make greater investments in physical capital. Since the matching process is inefficient, the firms who have invested more are not necessarily matched with the workers who have invested more in human capital. As a result, some of the other workers will gain from the increase in average human capital, since they are matched with firms using more physical capital than before, and in this sense the average level of human capital has an external benefit.

Work of this kind has helped to motivate the recent search for externalities in the data. As we have seen, the empirical growth literature gives rather imprecise answers about the social returns to education. Some researchers have pursued an alternative approach which may be more informative, based on survey data sets that include individuals who live in different cities. The idea is to estimate human capital earnings functions in the normal way, but with one important addition:

[19] Lucas (1990) sets out the details of this argument.

for each individual, they also include the average level of schooling in that individual's city. The central idea is that, if there are significant externalities to human capital, individuals should earn more when they work in those cities with a higher average level of schooling. The exercise will miss externalities that work at the national level, perhaps through social structures or institutions, but it remains of considerable interest.

Several studies based on this idea have been carried out for the US. The initial results of Rauch (1993) appeared promising. Consider otherwise similar individuals living in two different cities, the second city with a population that has an extra year of average schooling. His estimates suggested that each individual living in the second city could expect to gain a wage premium of around 3%, an effect large enough to be worthy of further investigation.

Unfortunately, as Ciccone et al. (1999) point out, there is an important argument against interpreting the observed wage premium as solely driven by externalities. Differences in average years of schooling across cities are likely to be associated with differences in the relative supplies of skilled and unskilled labour. These relative supply effects may give rise to an apparent wage premium for average schooling even in the absence of externalities.

The empirical work of Ciccone et al. (1999) supports this proposition. When they follow Rauch and do not allow for relative supply effects, they are able to obtain a high and precise estimate of the social return to education. In a more general approach, which builds in a role for supply effects, the measured externalities are greatly reduced; indeed it is not possible to reject the hypothesis that externalities are absent altogether. Related work by Acemoglu and Angrist (1999) also indicates that the overall social returns to education may be close to the private returns, this time using the variation in average schooling across US states to capture the effects of externalities.

3.5 A tentative summary of the evidence

At this point, one may be left wondering what the evidence ultimately achieves in terms of lessons for policy. The most useful perspective is probably to combine the various strands of evidence and see whether they form a coherent whole, despite the problems inherent in each.

Labour economists seem to be agreed that the private rate of return to a year's extra schooling is typically between 5% and 15%. Working under similar assumptions, growth accountants find that increases in educational attainment account for perhaps a fifth of growth in output per worker.

Labour economics and growth accounting have a relatively long history, and the strengths and weaknesses of the available evidence are well understood. It is possible that both approaches overstate the social benefits of education because of signalling effects, or a correlation between education and unobservable characteristics. Acting in the other direction, the estimates provided by this research may understate the role of education, because they rarely allow measurement of externalities, or quantify the importance for productivity of an improved matching between workers and jobs.

The great strength of the emerging macroeconomic literature is that, at least in principle, it could provide a direct test of the productivity benefits. As we have seen, however, this field has significant weaknesses of its own. Answers that are sufficiently accurate and robust to allow confident conclusions are some way off. They may have to wait until growth economists have longer spans of data to work with, and greater skill at matching a variety of possible statistical techniques to the question at hand.

With these caveats in mind, a brief summary of the macroeconomic literature may be useful. Although in some ways such an exercise is rather premature, it should at least prevent the unwary from jumping to an over-hasty conclusion based on the reading of one or two papers alone. That would be an easy mistake to make. Over the last ten years, growth researchers have bounced

from identifying quite dramatic effects of education, to calling into question the existence of any effect at all.

More recent research is placed somewhere between these two extremes, but perhaps leaning closer to the original findings that education has a major impact. In examining the studies that have not detected an effect, we have some convincing reasons (measurement error, outliers, incorrect specification) to doubt such results. The balance of recent evidence points to productivity effects of education which are at least as large as those identified by labour economists. This should reassure us that most countries are not over-providing education, especially as educational wage differentials have shown little sign of narrowing over time. I will return to this topic, and the implications for policy, in the final conclusions.

4. Social capital and growth

This section moves away from education, and concentrates on the idea of 'social capital', and its role in growth. Before describing the underlying ideas in more detail, it may be helpful to discuss their role in the wider context of empirical growth research. One reason for this is that a focus on social capital is relatively controversial: certainly compared to education, where there is general agreement that education is likely to be important, even if our measurements of its effect are imprecise.

Ideally, researchers studying development and growth would like to find a set of policy interventions sufficient to raise living standards and welfare. It is sometimes argued that this is an impossible goal, partly because the circumstances of each country are unique. A less extreme position is that growth research can give us some insight into possible generalisations by telling us

about the average pattern; at the same time, it should be recognised that any proposed set of 'sufficient' conditions will never be universal.

One way of making our generalisations more widely applicable is to discriminate more finely between societies, by introducing extra dimensions into our analysis of the growth process. This cannot be pushed too far, since we only have a limited set of countries, and a limited time span, from which to draw evidence. The central challenge for growth researchers is to identify the dimensions which are most relevant for growth, without endlessly multiplying the possibilities in such a way that we ultimately ask too much of the data. At the moment, the hope appears to be that a coherent picture will ultimately emerge through a gradual accumulation of evidence, as empirical researchers both introduce new variables and indicate that some earlier proposals should be discarded. The fundamental problem here is that the most general model, which in principle would allow us to discriminate easily between the competing hypotheses, has already become too large to be informative (Levine and Renelt 1992).

In this context, in explaining growth, it makes sense to concentrate on those dimensions of societies which have a strong prior claim on our attention. Among the dimensions recently proposed for further investigation, one stands out as both promising and - in terms of its prior claim - relatively controversial. The concept of 'social capital' appears to be a potentially formidable way of discriminating between countries and their growth prospects. It provides a useful umbrella term for those aspects of societies which, though difficult to measure and incorporate into formal models, are widely thought to be an important determinant of long-run economic success. For some economists (not all) the intuition that 'society matters' is strong enough to outweigh the current absence of much in the way of a theoretical underpinning.

There is a long academic tradition that something is not fully understood until it can be measured, and the concept of social capital presents serious problems of definition, let alone measurement. But in this respect, it is interesting to note the comment of Lucas (1988, 35) about

the early days of human capital theory. He wrote that "the idea of human capital may have seemed ethereal when it was first introduced - at least, it did to me - but after two decades of research applications of human capital theory we have learned to 'see' it in a wide variety of phenomena". The possible analogy with the present and future status of social capital should be clear.

Overall, it is easy to see why growth economists and others have started to emphasise social capital only very recently, even though the basic ideas have a long intellectual history. In this part of the survey, I will discuss some of the most recent work, starting with a discussion of the nature of social capital (section 4.1). This provides a necessary backdrop for section 4.2, which covers the limited cross-country evidence so far available, most of it based on survey evidence on willingness to trust. The implications for policy may seem rather meagre, but it should be remembered that this literature is still in its early stages. Section 4.3 will discuss some of the questions that remain to be answered.

4.1 What is social capital?

It is widely acknowledged that social capital needs to be carefully defined, if it is to prove anything more than suggestive in thinking about growth. Woolcock (1998) provides a brief history and a very useful exploration of the forms of social capital, from which this discussion will draw heavily.

One of the best known and most representative definitions can be found in the highly influential work of Putnam (1993): "social capital...refers to features of social organization, such as trust, norms, and networks, that can improve the efficiency of society by facilitating coordinated actions" (167). As Woolcock and others have noted, this is useful but comes close to defining social capital in terms of its function, so that it becomes difficult to separate analytically the sources of social capital from its consequences. As an example, social capital in the form of trust may be

created by participation in civic associations, but these associations could themselves be regarded as an important form of social capital. The importance of this point is reinforced when one considers that social capital may also have costs: one person's valuable network may be another's restrictive interest group.

Many discussions of social capital, including those of Putnam, Schuller (2001) and Woolcock (2001), associate it with a resource that is useful in achieving common objectives. For example, the suggested definition of Woolcock (2001, section 2) is that "social capital refers to the norms and networks that facilitate collective action". This emphasis on collective action may be problematic for economists who wish to make wider use of the idea. As I will discuss later, an understanding of the formation of social capital is likely to require an understanding of its value as a resource to individuals (Glaeser 2001). This can easily conflict with a definition of social capital that emphasises its role in collective action, in the usual sense of the latter term. For example, an entrepreneur who gains knowledge from participating in various networks is arguably benefiting from social capital, and this benefit occurs, and may be worthy of analysis, even if the entrepreneur does not share goals, objectives or outcomes with others.

A broader exploration of the term can be found in Woolcock (1998). He proposes a scheme in which it has four dimensions, roughly corresponding to (i) the extent of horizontal associations; (ii) the nature of social ties within communities; (iii) the nature of the relation between civil society and the state; and (iv) the quality of governing institutions. Independently of the social capital literature, economists have made some progress under category (iv), in analysing the growth impact of the quality of institutions (for instance Knack and Keefer 1995). At least for present purposes, it is not clear that bringing this work under the umbrella of social capital will yield extra insight.[20] In any case, measuring the benefits of good institutions is arguably a less urgent task than formulating

practical advice on how to improve bad ones, and the growth literature does not have much to offer here.

With these points in mind, this survey will mainly restrict itself to recent empirical work that uses the extent of trust in a society as an indicator of its underlying social capital. It should already be clear that this is an imperfect and simplistic way of capturing the ideas of Putnam and others. Trust may be determined by social capital, but also by other aspects of societies; and the extent of trust may be influenced, in very different ways, by all four of the dimensions of social capital identified by Woolcock.

In the present context, the focus on trust retains two key advantages. First and most importantly, trust can potentially be measured in a way that is comparable across countries, as we will see below. Second, although a focus on trust does not allow us to discriminate between the growth effects of different forms of social capital, it may be a valuable way of collapsing the various aspects of social capital into one quantifiable variable. As emphasised earlier, given the nature of our cross-country data sets, it can be valuable to limit the number of dimensions that we seek to explore, at least in preliminary work.

4.2 Empirical evidence

The most important macroeconomic evidence on social capital takes the World Values Survey as its starting point. The 1981 survey is based on responses from thousands of individuals across 21 market economies, while the 1990-91 survey covers 28 market economies. Overall, 29 market economies are covered at least once. The selection of respondents is not completely

[20] Following Abramovitz (1986), Temple and Johnson (1998) have suggested the use of the term "social capability" rather than social capital for social arrangements and institutions defined more broadly.

random, but adjustments to take this into account are available.[21] Among the issues addressed in the surveys, economists have mainly focused on a question designed to capture willingness to trust. Respondents were asked "Generally speaking, would you say that most people can be trusted, or that you can't be too careful in dealing with people?". The percentage of respondents in each nation replying "most people can be trusted" forms a potentially useful index of trust.[22] Table 1 shows values for this index, TRUST, for those OECD countries covered in the survey, and also for a small selection of less developed countries.

Clearly, measurement error is potentially a major problem in using such data. Interestingly, for the twenty countries with TRUST values for both 1981 and 1990, the correlation between the two is 0.91. Knack and Keefer (1997) also report on an interesting experiment, in which a large number of wallets containing $50 were deliberately 'lost' in a number of cities. The percentage of 'lost' wallets that are returned to their owners in each country has a correlation with TRUST of 0.67, providing a tentative indication that people are genuinely more trustworthy in countries with high values of the TRUST index.

[21] For a more detailed discussion, see Knack and Keefer (1997).
[22] Note that someone's response to the survey question may tell us more about their own trustworthiness, rather than a view of trust in their country as a whole. Even then, the pattern of responses may form a useful guide to the prevalence of trust in a particular country (see for instance Glaeser 2001).

Table 1
A measure of trust

Data for some OECD members

Norway	61.2	Ireland	40.2
Finland	57.2	Korea	38.0
Sweden	57.1	Spain	34.5
Denmark	56.0	Austria	31.8
Canada	49.6	Belgium	30.2
Australia	47.8	Germany	29.8
Netherlands	46.2	Italy	26.3
United States	45.4	France	24.8
United Kingdom	44.4	Portugal	21.4
Switzerland	43.2	Mexico	17.7
Iceland	41.6	Turkey	10.0
Japan	40.8		
Non-OECD members			
India	34.3	Nigeria	22.9
South Africa	30.5	Chile	22.7
Argentina	27.0	Brazil	6.7

Source: Knack and Keefer (1997).

Knack and Keefer (1997) also construct a second index, CIVIC, designed to capture the strength of norms of civic cooperation. The index is constructed by averaging across five questions, addressing the attitudes of the respondents to such things as fraudulent benefit claims and avoidance of fares on public transport. Perhaps surprisingly, this index shows relatively little variation across OECD countries, although it is positively correlated with TRUST. In what follows I will concentrate on the empirical evidence relating to the TRUST variable; Knack and Keefer note that results are broadly similar when CIVIC is used in its place.

Before turning to the evaluation of the results, it is worth examining the data in Table 1 more closely. Most of the empirical work is based on samples that contain a small number of less developed countries, as well as OECD members; as a result, one might be concerned that poorer

countries are responsible for most of the identifying variation in the TRUST variable. The evidence of Table 1, however, suggests that there is substantial variation in TRUST across OECD members.[23]

The index of trust is used by both Knack and Keefer (1997) and La Porta et al. (1997). Both these studies report cross-country regression evidence relating a wide variety of dependent variables to trust and a number of controls. In many cases, the results should be regarded as indicating the existence of associations, rather than establishing a causal relationship.

The evidence for causality is arguably strongest in the regressions seeking to explain growth in output per head. La Porta et al. (1997) report that the trust index is weakly associated with growth over 1970-93, although the explanatory power of their growth regression is low and the sample includes some countries that were centrally planned during this period. This suggests that one should be quite careful about drawing conclusions for OECD members.

Knack and Keefer exclude socialist countries and focus on a shorter period, 1980-92. They find stronger results. Controlling for initial income per head, a human capital variable, and the relative price of investment goods, they find that a one standard deviation change in the trust index is associated with a change in the growth rate of 0.56 of one standard deviation. In alternative terms, a level of TRUST that is ten percentage points higher (slightly less than one standard deviation) is associated with an annual growth rate that is higher by 0.8 percentage points.

This is an effect large enough to be of great interest, so Knack and Keefer carry out a number of robustness tests. When influential outliers are deleted, or growth analysed over longer periods (1960-92 and 1970-92), the point estimate of the growth effect is roughly halved, but remains statistically significant (see their Table II). They do note that, over the longer time span,

[23] The scatter plots presented in Knack and Keefer are also reassuring in this respect, as they suggest that the partial correlations between growth, investment and TRUST that will be discussed later are not simply driven by the inclusion of a few less developed countries.

the effect of TRUST is not always robust to the inclusion of other explanatory variables in the growth equation.

The evidence suggests that the effect of TRUST is large enough to be worthy of further investigation. It is important to note, however, that results are typically less strong when attention is restricted to a sample of OECD countries. Also using World Values Survey data, Helliwell (1996) found a negative effect of trust on growth in a sample of 17 OECD members. Knack (2001) reports that in a sample restricted to 25 OECD members, the effect of trust is imprecisely measured, and the hypothesis that it has no effect cannot be rejected at conventional significance levels.

These are quite small samples, so in a sense it is not surprising that trust is insignificant when attention is restricted to the OECD. Results are probably sensitive to the choice of conditioning variables, while outliers may play a role in hiding a relationship. Knack (2001) makes two other useful points in relation to the OECD results. First, as in Knack and Keefer (1997), there is evidence that the effect of trust is greater in low income countries, based on an interaction term in the growth regressions. Even if one is sceptical that trust matters for the high income members of the OECD, it may still play an important role in poorer countries like Mexico and Turkey. Secondly, Knack (2001) reports a statistically significant and positive correlation between investment and TRUST within an OECD sample, supporting the idea that trust plays some role even for richer nations.

Both La Porta et al. (1997) and Knack and Keefer (1997) report evidence on other interesting associations between TRUST and indicators of performance. La Porta et al. find strong positive associations between TRUST and a number of measures of government performance, including the effectiveness of the judiciary and the quality of the bureaucracy (their Table 2). Knack and Keefer present very similar results (their Table V). They also provide some evidence that the effect of trust works through raising the share of investment in GDP.

These results are intriguing, but one should be careful to avoid jumping to strong conclusions about the importance of trust. A central problem is that the extent of trust may well be determined by, or correlated with, other aspects of societies that are omitted from the growth regressions. For instance, it may be that corruption or weak legal enforcement lowers trust and, for quite independent reasons, the growth rate. As Knack and Keefer note, one could even tell a story in which trust is a product of optimism in societies that are performing well in economic terms.

In regarding trust as potentially an endogenous variable, the role of education is particularly interesting from the point of view of the present survey. La Porta et al. (1997, 336) write that trust has a positive effect on educational achievement, but it should be clear that causality may run in the opposite direction. Knack and Keefer report a strong correlation ($r = 0.83$) between TRUST and an estimate of average years of schooling for 1980, and note that "education may strengthen trust and civic norms, for example, if ignorance breeds distrust, or if learning reduces uncertainty about the behavior of others, or if students are taught to behave cooperatively" (1270). If we see trust as endogenous to the extent and quality of education, we have the beginnings of a potentially important story about externalities to education, of the kind briefly discussed earlier.

4.3 The future for social capital research

Given that interesting and suggestive evidence for the importance of social capital has been compiled in so short a time, further research on social capital appears to have a bright future. To live up to this promise, however, there are at least two potentially difficult questions that will need to be addressed. The first question concerns the origins and formation of social capital; the second, the precise mechanisms by which social capital, once formed, gives rise to particular microeconomic and macroeconomic outcomes.

It should be clear that, to incorporate the ideas of the social capital literature in policy advice, we will often need to understand how social capital is created, and how it might sometimes be undermined. In line with the usual methods of economists, Glaeser (2001) has convincingly argued that we need to give more attention to the value of social capital as a resource for individuals, as well as for communities as a whole. It seems unlikely that social capital is best understood as simply an unintended by-product of other decisions. With this in mind, we need a model that captures the incentives of individuals to form or undermine social capital, and which also shows how these incentives are affected by policy. Without such a model, our knowledge of policy implications will remain incomplete, however strong our intuition and evidence that social capital matters.

It can seem that social capital resists the usual methods of analysis of economists, given that it is usually understood to be a property of groups rather than individuals. The Glaeser argument works well for the 'networks' aspect of social capital, since participation in networks can be modelled as the outcome of individual investment decisions; the argument is less clearly applicable to other aspects of social capital, such as social norms. Yet even for social norms, such as the value of trustworthiness, it is possible to analyse their creation and evolution in terms of individual decisions to observe (or not to observe) the prevailing norm. Economists have recently started to give greater attention to constructing models in which social norms are endogenous, and it seems probable that this work will yield some valuable insights.[24]

A second, and related, question concerns the precise mechanisms by which social capital, once in place, affects economic outcomes. Again, formal modelling may be useful. For example, Zak and Knack (1999) present a model in which agents divide their time between production and

[24] Many references to research in this area can be found in Zak and Knack (1999).

verifying the actions of those they transact with. Their model captures the simple idea that in low trust societies, some resources and time are diverted to verification, and this results in lower output.

It will be very difficult to discriminate between alternative theoretical models using macroeconomic data. Studies based at the micro level will often be rather more informative, and some interesting empirical work has already started to appear. Guiso et al. (2000) argue that one of the best testing grounds for the importance of social capital may lie in the financial sector, since it is here that trust may be especially relevant to economic activity. They study this effect within Italy, using a measure of civic engagement (essentially voter turnout in certain elections) as a proxy for social capital, as in Putnam (1993). Using large samples of households and firms, they find that their measure of civic engagement helps explain variation in financial practices across Italian regions, even when controlling for different levels of development.

Such studies are likely to play an increasingly important role in the wider debate on the importance of social capital. Sceptics will remain unconvinced by the economic importance of trust and other aspects of societies (networks, norms, participation) until we have a more complete and detailed story describing their connection to economic outcomes, supported by reliable evidence.

5. Summary and conclusions

Where does all this leave us in thinking about policy? I will again consider education first. Griliches (1997, S339) notes that for academic economists, an emphasis on the importance of education for economic growth "may be somewhat self-serving" and occasionally in the literature one does come across a paper which echoes to the sound of grinding axes. This is particularly true in reading opposing assessments of the signalling argument, where the lack of evidence does not seem to preclude strong views on the importance or otherwise of the central claims.

In assessing the empirical evidence for productivity benefits of education, it is quite possible that an overall judgement is frequently contaminated by a keen awareness of wider benefits of education that are not captured in economic data. After all, one could probably construct a viable case for much educational expenditure entirely based on its implications for personal development, independent of any productivity effects. It is worth quoting Weiss (1995, 151):

> Education does not have to be justified solely on the basis of its effect on labour productivity. This was certainly not the argument given by Plato or de Tocqueville and need not be ours. Students are not taught civics, or art, or music solely in order to improve their labour productivity, but rather to enrich their lives and make them better citizens.

Most economists, appropriately enough for practitioners of the 'dismal science', have concentrated on examining a rather more narrow case for education, in terms of its contribution to productivity growth. As we have seen, the weight of evidence points to significant productivity effects, but the degree of uncertainty is large, and even a lower bound is surprisingly difficult to establish.

The evidence from labour economics has the greatest weight of experience, time and academic firepower behind it, and this suggests that it would be a mistake to summarise the macroeconomic results in isolation. Although a reconciliation of these two literatures is in its early stages, the correlation across countries between measures of human capital and growth is arguably robust enough to support the belief that earnings functions pick up genuine productivity effects, and not simply the effects of signalling or omitted characteristics.

That is reassuring, but it leaves many questions open for policy-makers. There is likely to be pervasive heterogeneity in rates of return across individuals, let alone across countries. A greater understanding of the pattern of heterogeneity will lead to better policy decisions, but on this

subject the macroeconomic literature surveyed here is necessarily silent. Evidence compiled by labour economists will be far more useful in this respect.[25]

Other limitations of the macroeconomic evidence are worth noting. Growth regressions are best thought of as picking up an average effect of schooling, and should certainly not be used to conclude that every OECD member is currently under-providing education. Indeed, the results from growth accounting exercises suggest that, although increases in educational provision can yield a worthwhile increase in the growth rate, one should not necessarily expect an effect that is large relative to current rates of growth. For policy-makers who wish to raise the growth rate, policy on education remains a natural place to look, but it is by no means a panacea.

Some other evidence sheds important light on the extent to which current educational provision is appropriate. Particularly relevant is the evolution of educational wage differentials since the late 1970s. Even though the relative supply of skilled labour has increased, there has been a substantial and well-documented increase in educational wage differentials in the UK and the USA, with less pronounced changes in other OECD countries (see for instance Katz and Autor 1999, 1501-1503).[26] It seems difficult to explain the evidence for the UK and the US without a dominant role for a shift in the relative demands for different types of labour, favouring the more educated.

Much research has focused on the origins of the change in relative labour demand, but for policy-makers an equally important question is whether this change is generating a rising return mainly to education, or to other characteristics such as innate ability or initiative. Clearly the policy implications are very different under the alternative scenarios, yet disentangling the two effects is difficult. Existing research often finds that it is the return to ability which is rising; but

[25] A special issue of the journal *Labour Economics* (November 1999) includes studies that measure the returns to schooling for a variety of OECD member countries, and thus sheds light on the possible heterogeneity across countries.

the work of Cawley et al. (1998) suggests that, due to some important identification problems, these results are not robust to small changes in assumptions.

The recent shifts in differentials remind us, once again, that policy on education matters for more than productivity. There are also likely to be important consequences for inequality, as discussed by Topel (1997). Given that trade does not seem to equalise factor prices across countries, any increase in the relative supply of skilled labour is likely to lower the wage premium for the possession of skills. In turn this could make an important contribution to reducing income inequality.[27]

This survey began with the observation that OECD members spend about 6% of their collective GDP on education, or $1,300 billion each year. That is a major commitment of resources, but on the available evidence, including recent changes in wage dispersion, the arguments for cutting back on provision seem rather weak. In deciding the extent to which provision should be expanded, perhaps the key open question is the validity of the signalling arguments. More evidence on the signalling debate would be extremely helpful in judging the benefits of further educational expansion, and perhaps especially the benefits of expanding higher education, one of the main changes in provision within the OECD since the 1960s.

In exploring some of the details of such arguments, empirical evidence is not the only way forward. Theory and calibration exercises may also shed light on these issues. From a policy perspective, an interesting implication of new growth theory is that individuals may under-invest in education, because those who later go into research careers do not capture all the benefits of the new ideas that they help to create. This provides the beginnings of an argument for subsidising education in engineering and science, at least at those levels (perhaps PhDs, or post-docs) where a

[26] Evidence on recent trends in wage dispersion more generally can be found in OECD (1996, ch. 3).

high proportion of individuals subsequently go into research and development activity. As we have seen, measuring the benefits would be difficult, but some progress may be possible by calibrating simple theoretical models, as in Rustichini and Schmitz (1991).

The literature on social capital and growth is at an earlier stage than the macroeconomic evidence on education, and the policy implications are less clear. Indeed, one weakness of the social capital literature, at least in relation to richer countries, is that it is currently difficult to see what policy conclusions could ever be drawn. What can a policy-maker in Mexico or Turkey actually do, confronted with the evidence from the World Values Survey that they govern a low-trust society? Standard recommendations, such as attempting to eliminate corruption and improve the legal system, are nothing new, and make good sense quite independently of any emphasis on social capital.

Perhaps the best answer lies in drawing an analogy with the introduction of human capital theory into economics. In its early stages, as Lucas (1988) makes clear, this too seemed a rather ethereal concept, and presumably one with little immediate message for education policies. Work on social capital is still in its early stages, and as we learn more about what it is, where it comes from, and what it does, there may ultimately be implications and conclusions that leave our successors wiser in ways that we can only guess at.

[27] One has to be careful in making this argument, even in a simple model with just two types of labour. Inequality depends not only on the skill premium, but also on the relative supplies of skilled and unskilled labour.

Appendix: Growth effects of education in MRW

Mankiw, Romer and Weil (1992) estimate a simple model in which human capital is accumulated in the same way as physical capital. An interesting feature of this model is the way in which the presence of physical capital raises the overall impact of education. The rate of investment in physical capital is assumed to be a fixed proportion of output, and so anything which raises output, including extra investments in education, will raise the steady-state stock of physical capital per worker. This means that, in calculating the effect of education on growth, we also need to take into account its indirect effect via physical capital. MRW's specification of a complete growth model, simple though it is, allows us to gauge the magnitude of the total effect.

The aggregate production function is assumed to be:

$$Y = K^{\alpha} H^{\beta} (AL)^{1-\alpha-\beta}$$

where Y is output, K is physical capital, H is human capital, A is an index representing technical efficiency, and L is the labour force. Human capital is accumulated as follows:

$$\dot{H} = s_h Y - \delta H$$

where s_h is the fraction of output invested in human capital, and δ is the rate of depreciation of human capital. Using results in MRW, it is possible to show that if the human capital investment ratio s_h is multiplied by a factor θ, output per worker will ultimately rise by a factor:

$$\frac{\dot{Y}}{Y} = \theta^{\frac{\beta}{1-\alpha-\beta}}$$

The growth regressions in the MRW paper for the OECD sample yield estimates for the key parameters of α=0.38 and β =0.23. Taken at face value, these estimates imply that if human capital investment as a fraction of output is increased by a tenth (so θ=1.1) then output per worker will rise by 6%; if the human capital investment ratio is doubled, output per worker will eventually rise by about 50%. This illustrates the potential importance of education, although it should also be noted that the model lacks strong microfoundations. In a model with a more complete specification for the determination of investment, education might be rather less important.

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