Education for Growth in Sweden and the World

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ABSTRACT

This paper tries to reconcile evidence on the effect of schooling on income and on GDP growth from the microeconometric and empirical macro growth literatures. Much microeconometric evidence suggests that education is an important causal determinant of income for individuals within countries as diverse as Sweden and the United States. At a national level, however, recent studies have found that increases in educational attainment are unrelated to economic growth. This finding is shown to be a spurious result of the extremely high rate of measurement error in first-differenced cross-country education data. After accounting for measurement error, the effect of changes in educational attainment on income growth in cross-country data is at least as great as microeconometric estimates of the rate of return to years of schooling. We also investigate another finding of the macro growth literature -- that economic growth depends positively on the initial stock of human capital. We find that the effect of the initial level of education on growth is sensitive to the econometric assumptions that are imposed on the data (e.g., constant-coefficient assumption), as well as to the other covariates included in the model. Perhaps most importantly, we find that the initial level of education does not appear to have a significant effect on economic growth among OECD countries. The conclusion comments on policy implications for Sweden based on the human capital literature.

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[W]hat was rather jarring is the repeated finding, in these international data, that changes in the estimated levels of schooling or human capital do not contribute positively to growth, at least measured over the 1965-85 period.

Zvi Griliches, 1997

Research on the economic effects of education was marked by two contradictory sets of findings in the 1990s. On the one hand, the micro labor literature produced several new estimates of the monetary return to schooling that exploit natural experiments in which variability in workers' schooling attainment was generated by some exogenous and arguably random force, such as quirks in compulsory schooling laws or students' proximity to a college. These studies tended to find that education is an important determinant of income. On the other hand, the macro growth literature has found that changes in average schooling levels across countries are unrelated to the speed of economic growth, although the initial level of schooling is related to the countries' subsequent GDP growth rate. This paper tries to reconcile these two disparate but obviously related lines of research.

The next section reviews the theoretical and empirical foundations of the Mincerian human capital earnings function. Our survey of the literature indicates that Mincer's (1974) formulation of the log-linear earnings-education relationship fits the data rather well. Each additional year of schooling appears to raise earnings by 5 to 15 percent, depending on the country, with the United States on the high end and Sweden on the low end of the distribution. The rate of return to education varies over time as well as across countries. Perhaps surprisingly, there is little evidence that unobserved variables (e.g., inherent ability) that might be correlated with earnings and education cause simple OLS estimates of wage equations to significantly overstate the return to education in most countries. Indeed, consistent with Griliches's (1977) conclusion, much of the modern literature finds that the upward "ability bias" is of about the same order of magnitude as the downward bias caused by measurement error in educational attainment. Evidence on possible differences in the payoff to investments in human capital across subgroups of the work force is also discussed.

Section 2 considers the empirical macro growth literature. First, we relate the Mincerian wage equation to the macro growth model. The Mincer model implies that the change in a country's average level of schooling should be the key determinant of income growth. The macro growth literature, by contrast, typically specifies growth as a function of the initial level of education, not the change in education. Moreover, we show that if the return to education changes over time (e.g., because of exogenous skill-biased technological change), the macro growth models are unidentified. Much of the empirical growth literature has eschewed the Mincer model because studies such as Benhabib and Spiegel (1994) find that the change in education is not a determinant of economic growth. We show, however, that Benhabib and Spiegel's finding that the growth in education is unrelated to economic growth results because there is virtually no signal in their education data conditional on the growth of capital.

The macro growth literature has devoted only passing attention to potential problems caused by measurement errors in education. Despite their aggregate nature, available data on average schooling levels across countries are poorly measured, in large part because they must often be derived from enrollment flows. The reliability of country-level education data is no higher than the reliability of individual-level education data. For example, the correlation between Barro and Lee's (1991) and Kyriacou's (1991) measure of average education across 68 countries in 1985 is 0.86, and the correlation between the *change* in schooling between 1965 and 1985 from these two sources is only 0.34. Additional estimates of the reliability of country-level education data based on our analysis of comparable micro data from the World Values Survey for 34 countries suggests that measurement error is particularly prevalent for years of secondary and higher schooling. We find that measurement errors in education severely attenuate estimates of the effect of the *change* in schooling on GDP growth. Nonetheless, we conclude that measurement errors in schooling are unlikely to cause a spurious positive association between the initial level of schooling and GDP growth across countries, *conditional on* the change in education. Thus, like Topel (1998), we conclude that both the change and the initial level of education are positively correlated with economic growth.

Finally, we explore the robustness of the impact of the initial level of schooling on economic growth. First, we estimate a variable-coefficient model that allows the coefficient on education to vary across countries (as is found in the micro data). Second, we relax the linearity assumption of the initial level of education. Finally, we estimate growth equations for the subset of OECD countries. These extensions show that the positive effect of the initial level of education on economic growth is sensitive to econometric restrictions that are often rejected by the data.

Our main conclusion is that while support for the micro Mincerian wage equation is strong, the evidence of a positive effect of the stock of education on a country's growth rate is less robust. Moreover, if one accepts the assumptions necessary to interpret the coefficient on the initial level of education in cross-country growth regressions as identifying externalities from education, the results most likely do not apply to the OECD countries.

1. The Microeconomics of the Private Return to Education

> *The difference between the most dissimilar characters, between a philosopher and a common street porter, for example, seems to arise not so much from nature, as from habit, custom and education.*
>
> *Adam Smith, 1776*

Adam Smith suspected that education and other environmental factors were more important determinants of economic success for individuals than their natural ability. Since at least the beginning of the century, economists and other social scientists have sought to estimate the economic rewards individuals receive from completing more schooling.[1] It has long been recognized that workers who attended school longer may possess inherent abilities that would lead them to earn higher wages irrespective of their level of education. If these other characteristics are not accounted for, then simple comparisons of earnings across individuals with different levels of schooling would overstate the rate of return to education.

[1] Early references are Gorseline (1932), Walsh (1935), Miller (1955), and Wolfle and Smith (1956).

Early attempts to control for this "ability bias" included the analysis of data on siblings and twins to difference-out unobserved family characteristics (e.g., Gorseline, 1932 and Taubman, 1976), and regression analyses which included as control variables observed characteristics such as IQ and parental education (e.g., Griliches and Mason). By now this literature has been amply surveyed in Griliches (1977), Rosen (1977), Willis (1986), and Card (1998). Below we briefly review evidence on the Mincerian earnings equation, emphasizing recent studies that use exogenous variation in education to estimate the Mincerian earnings equation.

1.1 The Mincerian wage equation

Mincer (1974) showed that if the only cost of attending school an additional year is the opportunity cost of students' time, and if the proportional increase in earnings caused by this additional schooling is constant over the lifetime, then the log of earnings would be linearly related to individuals' years of schooling, with the slope equal to the rate of return to investment in education. He augmented this model to include a quadratic term in work experience to allow for returns to on-the-job training, yielding the familiar Mincerian wage equation:

(1) $\ln W_i = \beta_0 + \beta_1 S_i + \beta_2 X_i + \beta_3 X_i^2 + \varepsilon_i,$

where $\ln W_i$ is the natural log of the wage for individual i, S_i is years of schooling, X_i is experience (usually measured as age minus education minus 6), X_i^2 is experience squared, and ε_i is a disturbance term. With Mincer's assumptions, the coefficient on schooling, β_1, equals the discount rate, because schooling decisions are made by equating two present value earnings streams: one with a higher level of schooling and one with a lower level. An attractive feature of Mincer's model is that time spent in school (as opposed to degrees) is the key determinant of earnings, so data on years of schooling can be used to estimate a comparable return to education in countries with very different educational systems.

There are, of course, other theoretical models that could yield a log-linear earnings-schooling relationship. For example, if the underlying production function between human capital and earnings is log-linear, and individuals randomly choose their schooling level (e.g., optimization errors), then the coefficient from equation (1) would uncover the educational production function. The slope of the earnings-education gradient would then vary with the quality of education (see Behrman and Birdsall, 1986 and Card and Krueger, 1996).

The Mincerian earnings function is one of the great success stories of empirical economics. Equation (1) has been estimated for most countries of the world by OLS, and the results generally yield estimates of β_1 ranging from .05 to .15, with slightly larger estimates for women than men (see Psacharopoulos, 1995). A coefficient of .05, for example, should be interpreted as meaning that acquiring an additional year of education is associated with 5 percent higher earnings, other things being equal. The log-linear relationship also provides a good fit to the data, as is illustrated by the plots for the U.S., Sweden, West Germany, and East Germany in Figure 1.[2] These figures display the coefficient on dummy variables indicating each year of schooling, controlling for experience and gender, as well as the OLS estimate of the Mincerian return. It is apparent that the semi-log specification provides a good description of the data even in countries with dramatically different economic and educational systems.[3] Notice also that in Sweden the slope of the relationship between earnings and education is relatively flat, probably a result of institutional forces that compress wage dispersion in Sweden.

Much research has addressed the question of how to interpret the slope of the education variable in equation (1). Does it reflect unobserved ability and other characteristics that are correlated with education, or the true reward that the labor market places on education? Is education rewarded because it is a signal of ability (Spence, 1973), or does the labor market value education because it increases productive capabilities? Is the social return

[2]The German figures are from Krueger and Pischke (1995). The American and Swedish figures are based on the authors' calculations using the 1991 March Current Population Survey and 1991 Swedish Level of Living Survey. The regressions also include controls for a quadratic in experience and sex.

[3]Evaluating micro data for states over time in the U.S., Card and Krueger (1992) find that the earnings-schooling relationship is flat until the education level reached by the 2nd percentile of the education distribution, and then becomes log-linear. There is also some evidence of sheep-skin effects around college and high school completion (e.g., Park, 1994). Although statistical tests often reject the log-linear relationship for a large sample, the figures clearly show that the log-linear relationship provides a good approximation to the functional form. It should also be noted that Murphy and Welch (1990) find that a quartic in experience provides a better fit to the data than a quadratic.

to education higher or lower than the coefficient on education in the Mincerian wage equation? Would all individuals reap the same proportionate increase in their earnings from attending school an extra year, or does the return to education vary systematically with individual characteristics? Definitive answers to these questions are not available, although the weight of the evidence clearly suggests that education is not merely a proxy for unobserved ability. For example, Griliches (1977) concludes that instead of finding the expected positive ability bias in the return to education, "The implied net bias is either nil or negative" once measurement error in education is taken into account. The more recent evidence from natural experiments also supports this conclusion.

Table 1 summarizes estimates of the return to education based on natural experiments. A hallmark of these studies is that the variations in educational attainment used to identify the return to education stem from a known and arguably exogenous source. For example, Angrist and Krueger (1991) observe that the combined effect of school start age cutoffs and compulsory schooling laws produces a natural experiment, in which individuals who are born on different days of the year start school at different ages, and then reach the compulsory schooling age at different grade levels. If the date of the year individuals are born is unrelated to their inherent abilities, then, in essence, variations in schooling associated with date of birth provide a natural experiment for estimating the benefit of obtaining extra schooling in response to compulsory schooling laws.

Using a sample of nearly one million observations from the U.S. Censuses, Angrist and Krueger find that men born in the beginning of the calendar year, who start school at a relatively older age and can dropout in a lower grade, tend to obtain less schooling. This pattern only holds for those with a high school education or less, consistent with the view that compulsory schooling is responsible for the pattern. They further find that the pattern of education by quarter-of-birth is mirrored by the pattern of earnings by quarter-of-birth: in particular, individuals who are born early in the year tend to earn less, on average.[4] Instrumental variables (IV) estimates that are identified by variability in schooling associated

[4]Again, no such pattern holds for college graduates.

with quarter-of-birth suggest that the payoff to education is slightly higher than the OLS estimate.[5] Angrist and Krueger conclude that the upward bias in the return to schooling is about the same order of magnitude as the downward bias due to measurement error in schooling.

The other studies listed in Table 1 use a variety of other sources of variability in schooling. Harmon and Walker (1995), for example, more directly examine the effect of compulsory schooling by studying the effect of changes in the compulsory schooling age in the United Kingdom, while Card (1994) exploits variations in schooling attainment owing to families' proximity to a college in the U.S. The evidence summarized in the table is drawn from several countries, and generally supports the conclusion that the private return to education is at least as great as simple OLS estimates would suggest.

The evidence specifically for Sweden is more limited, but suggests that the private payoff to education in Sweden is positive but lower than in most of the rest of the world. For example, Kjellström (1997) uses register earnings data to estimate the payoff to years of schooling in 1991 for men. Controlling for family background, experience, school grades, and test scores at age 12-13, he finds that the return to a year of education varies between .037 and .051, depending on the birth cohort. Using register data on earnings for identical twins in 1987, 1990, and 1993, Isacsson (1999) finds that the cross-twin OLS estimate of the return to education is .046, and that the within-twin estimate is .022. When he adjusts the within-twin estimate for measurement error in education, however, the return rises to .042, suggesting little downward ability bias. Similar to the U.S. literature, Ottersten, et al. (1996) find that the return to education in Sweden falls by about 10 percent when they estimate a parametric sample selection model. Using the Swedish Level of Living Surveys (LNU), Palme and Wright (1999) find that the payoff to education fell for both men and women from .08 in 1968 to .03-.04 in 1981, and stayed roughly constant between 1981 and 1991. Edin and Holmlund

[5]Bound, Jaeger and Baker (1995) argue that Angrist and Krueger's IV estimates are biased toward the OLS estimates because of weak instruments. However, Staiger and Stock (1997), Donald and Newey (1997), Angrist, Imbens and Krueger (1998), and Chamberlain and Imbens (1996) show that weak instruments do not account for the central conclusion of Angrist and Krueger (1991).

(1995) also find that the college-high school wage differential (both before, and especially after, tax) fell considerably between 1968 and 1984, and then rose gradually rose between 1984 and 1991. In sum, these studies paint a picture of education in Sweden that is broadly similar to the rest of the world: the OLS estimate of the return to education does not appear to be severely affected by ability bias, although the payoff workers gain from attaining additional education in Sweden is lower than in most other countries that have been carefully studied.

1.2 Differences in the payoff to human capital across groups

The studies listed in Table 1 typically find somewhat higher estimates of the return to schooling when variability in schooling from exogenous circumstances is used to estimate the return than when all variability is used. Although the difference between the OLS and IV estimates is not statistically significant in most of these studies, there is at least a hint that students who complete more schooling than they would ordinarily choose earn a higher return for that schooling than others do from the years they voluntarily selected. Ashenfelter, Harmon and Oosterbeek (1998) assemble estimates from many of the studies in the literature, and find that the average conventional OLS return to schooling is .065, whereas the average IV estimate is .086.

One possible explanation for the tendency of IV estimates to exceed OLS estimates is that IV estimates are more likely to be published when they obtain statistically significant, positive coefficients, since there is a presumption that the return to schooling should be positive. Because the IV studies tend to have relatively imprecise estimates, there may be a selection process at work which leads to an over-representation of IV studies with relatively large returns to education in the literature: a larger coefficient is required to have a significant t-ratio the larger the standard error. Ashenfelter, Harmon and Oosterbeek (1998) provide some evidence for this type of selection by showing that the return to education from various IV estimates is positively related to the standard error of the estimates; absent some form of selection, there is no reason to expect the true return to education to be correlated with its standard error. Once they adjust for this form of selection bias, however, they still find that the return to education is higher in the IV estimates on average than in the OLS estimates (.080 versus .065).

We tentatively conclude from this evidence that the return to an additional year of education obtained for reasons like compulsory schooling is more likely to be greater than, than less than, the conventionally estimated return to schooling. Because the schooling levels of individuals who are from more disadvantaged backgrounds tend to be those who are most affected by the interventions examined in Table 1, Card (1993) and others have concluded that the return to an additional year of schooling would be higher for individuals from disadvantaged families than for those from advantaged families.

Other related evidence for the U.S. suggests the payoff to investments in education might be higher for more disadvantaged youth. First, while studies of the effect of school resources on student outcomes yield mixed results, there is a tendency to find more beneficial effects of school resources on more disadvantaged students (see, for example, Summers and Wolfe, 1977, Krueger, 1998 and Rivkin, Hanushek and Kain, 1998). Second, evidence suggests that pre-school programs have particularly large, long-term effects for disadvantaged children in terms of reducing crime and welfare dependence, and raising incomes (see, Barnett, 1992). Third, several studies have found that students from advantaged and disadvantaged backgrounds make equivalent gains on standardized tests during the school year, but children from disadvantaged backgrounds fall behind during the summer while children from advantaged backgrounds move ahead (see Entwisle, Alexander, and Olson, 1997). And fourth, recent evidence suggests that college students from more disadvantaged families benefit more from attending elite colleges than students from advantaged families (see Dale and Krueger, 1998).

Another finding from the U.S. that may have some bearing on Sweden concerns adult education and training. Studies of job training programs utilizing randomized design have typically found modest payoffs for disadvantaged adult males, and larger payoffs for disadvantaged women (see, e.g., LaLonde, 1995).[6] Evidence on formal adult education is less

[6]Evidence on training effects for Sweden is consistent with the U.S. experience; see, for example, Forslund and Krueger (1997).

extensive, but also suggests normal rates of return to adults who return to school after being displaced. For example, Jacobson, LaLonde and Sullivan (1997) study the experience of workers in Pennsylvania and Washington who lost a job that they held for three or more years, and then entered a community college. Typically, workers completed 8 months to a year of education. They found that the trainees' earnings increased by 2-5 percent more than other workers who did not enter a community college, but the payoff was substantially higher for those who prepared for jobs in certain technical fields and the health professions.

It is unclear whether this evidence of a higher return to human capital for disadvantaged youth applies outside the U.S. But in all regions of the world, Psacharopolous (1995) concludes that there is a higher return to primary schooling than to secondary or tertiary schooling, which also suggests disadvantaged children benefit most from additional human capital investments.

1.3 Theoretical reasons for a higher return for investments in disadvantaged groups

If one tentatively accepts the finding that the return to investments in human capital is higher for less advantaged individuals, what might explain such a phenomenon? Card (1994) and Lang (1993) present models in which individuals from lower income households have higher discount rates. Since individuals select their level of schooling by equating the payoff to schooling to the discount rate, individuals from low-income households naturally have higher returns to schooling in these models.

We would propose a complementary explanation, which can also encompass the related facts about the return to human capital for disadvantaged groups mentioned above. In particular, recognize that children acquire human capital from many sources, such as from parents, teachers and classmates. To some extent, the human capital from these sources may be substitutable. If an individual from a high-income family, for example, receives poor reading instruction at school, the family may compensate by providing tutoring. Low-income families have less scope to substitute home resources for schooling resources, and have home environments that are less conducive to learning, which might explain why pre-school programs are successful for these students. It might also be the case that the educational production function is concave, so students who are at the low end of the ability distribution because of their endowments benefit more from additional human capital than students at the high end.

Inherently, both these explanations rely on some form of imperfect capital markets because, if families were not constrained, they would invest in human capital until the point at which the marginal benefit equals the (universal and constant) marginal cost. But there are reasons to doubt that the supply of funds for investing in human capital is infinitely elastic at the market rate for all families. Many authors have noted that future human capital cannot be used to collateralize loans because of moral hazard problems. Perhaps more importantly, poorly endowed families may underestimate the value of education -- after all, education is purchased to improve information, and those with a low level of education may be particularly susceptible to making suboptimal decisions.

1.4 Social versus Private Returns to Education

Thus far, the discussion has focused on the private return to education. The social return can be higher or lower than the private return. The social return can be higher because of externalities from education, which could occur, for example, if higher education leads to technological progress that is not captured in the private return to that education, or if more education produces positive externalities, such as a reduction in crime and welfare participation, or more informed political decisions. The former is more likely if human capital is expanded at higher levels of education while the latter is more likely if it is expanded at lower levels of education. It is also possible that the social return to education is less than the private return. For example, Spence (1973) and Machlup (1970) note that education could just be a credential, which does not raise individuals' productivities. It is also possible that in developing countries, where higher education has been associated with a greater incidence of unemployment (e.g., Blaug, Layard and Woodhall, 1969) and the return to physical capital may exceed the return to human capital (e.g., Harberger, 1965), increased levels of education may reduce total output.

Most of the micro human capital literature focuses on the private rather than social return to education, but the finding of little ability bias in the Mincerian wage equation casts doubt on at least some forms of credentialling arguments. The possibility of externalities to education motivates much of the macro growth literature, to which we now turn.

2. Macro growth equations

> *Now, if education produces not only differences in individual capacities but also new knowledge resulting in continuous technological, managerial and organizational improvements, the growth in national product due to these improvements can reasonably be regarded as an additional contribution of education.*
>
> *Fritz Machlup, 1970*

If, as Griliches (1977) observed, the micro human capital earnings function spawned "a vast river of econometric studies threatening to engulf us all," then it could be argued that the new macro growth literature has generated a Tsunami of cross-country regression studies threatening to wash us all away. The literature is voluminous. We do not attempt an exhaustive review here. Instead, we summarize the main findings and explore the impact of several econometric issues.

The macro growth literature yields three principally different conclusions from the micro literature. First, the initial stock of human capital matters, not the change in human capital. Second, secondary and post-secondary education matter more for growth than primary education. Third, female education has an insignificant and sometimes negative effect on economic growth.

2.1 From the Mincer Model to the Macro Growth Model

Consider a Mincerian wage equation for each country j and time period t:

(1') $\ln W_{ijt} = \beta_{0jt} + \beta_{1jt}S_{ijt} + \varepsilon_{ijt}$, where we have suppressed the experience term for convenience. This equation can be aggregated across individuals each year by taking the means of each of the variables, yielding what Heckman and Klenow (1997) call the "Macro-Mincer" wage equation:

(2) $\ln Y^g_{jt} = \beta_{0jt} + \beta_{1jt}S_{jt} + \varepsilon_{jt}$,

where Y^g_{jt} denotes the geometric mean wage and S_{jt} is mean education. Heckman and Klenow (1997) compare the coefficient on education in a cross-country log GDP equation to the coefficient on education from micro Mincer models. Once they control for life expectancy to proxy for technology differences across countries, they find that the macro and micro regressions yield similar estimates of the effect of education on income.[7] They conclude from this and other evidence that the "macro versus micro evidence for human capital externalities is not robust."

The macro Mincer equation can be differenced between year t and t-1. Differencing the equation removes the effect of permanent additive differences in technology and endowments. If the return to schooling is constant over time, we have:

(3) $\Delta\ln Y^g_j = \beta'_0 + \beta_{1j}\,\Delta S_j + \Delta\varepsilon'_{jt}$,

where Δ signifies the change in the variable from t-1 to t, β'_0 is the mean change in the intercepts, β_{1j} the return to education, and $\Delta\varepsilon'_{jt}$ is a composite error that includes the deviation between each country's intercept change and the overall average. Notice that this formulation allows the time-invariant return to schooling to vary across countries. If the return to education does vary across countries, and a constant-coefficient model is estimated, then the error will contain a term that is the difference between each country's return to schooling and the mean return, times the change in schooling.

If the return to schooling varies over time, then the first-differenced macro Mincer model can be written:

(4) $\Delta\ln Y^g_j = \beta'_0 + \beta_{1jt}\,\Delta S_j + \delta S_{jt-1} + \Delta\varepsilon'_{jt}$,

[7]When they omit life expectancy, however, education has a much larger effect in the macro regression than micro regression. Whether longer life expectancy is a valid proxy for technology differences, or a result of higher income, is an open question (see Smith, 1998).

where δ is the change in the return to schooling ($\Delta\beta_{1j}$). This tells us that if the return to schooling has increased (decreased) secularly over time, the initial level of education will enter positively (negatively) into equation (4). An implicit assumption in the macro growth literature therefore is that the return to education is either unchanged, or changed endogenously by the stock of human capital.[8]

The typical macro growth model estimated in the literature is motivated by the convergence literature. This leads to interest in estimating parameters of an underlying model such as $\Delta y_j = \alpha_j - \beta(y_{jt-1}-y^*_j) + \mu_j$, where Δy_j denotes the annualized change in log GDP per capita in country j between t-1 and t, α_j denotes country j's steady-state growth rate, y_{jt-1} is the log of initial GDP per-capita, y^*_j is steady-state log GDP per capita, and β measures the speed of convergence to steady-state income. The intuition for this equation is straightforward: countries that are below their steady-state income level should grow quickly, and those that are above it should grow slowly. A prototypical estimating equation is:

(5) $\Delta y_j = \beta_0 + \beta_1 y_{jt-1} + \beta_2 S_{j,t-1} + \beta_3 Z_{j,t-1} + \varepsilon_j$

where y_j is the change in log GDP per capita from year t-1 to t, S_{t-1} is average years of schooling in the population in the initial year, y_{t-1} is the log of initial GDP per capita, and Z_{t-1} includes variables such as inflation, capital, or the "rule of law index."[9] Sometimes equation (5) also includes an interaction between years of schooling and initial log GDP, to allow the rate of convergence to vary across countries with different education levels. Also note that schooling is sometimes specified in logarithmic units in equation (5). Barro and Sala-i-Martin (1995), Benhabib and Spiegel (1994), and others conclude that the change in schooling has an insignificant effect if it is included in a GDP growth equation, even though this variable is predicted to matter in the Mincer model and in some endogenous economic growth models (e.g., Lucas, 1988). Equation (5) is typically estimated with data for a cross-section or pooled sample of countries spanning a 5, 10, or 20 year period.

[8]Psacharopolous (1994; Table 6) finds that, for the average country, the Mincerian return to education fell by 1.7 points over a 12-year period. By contrast, O'Neill (1995) finds that between 1967 and 1985 the return to education measured in terms of its contribution to GDP rose by 58 percent in developed countries and by 64 percent in less developed countries.

[9]Henceforth we use the terms GDP per capita and GDP interchangeably.

The first-differenced macro-Mincer equation (3) differs from the macro growth equation in several respects. First, the macro growth models use the change in log GDP per capita as the dependent variable, rather than the change in the mean of log earnings. If income has a log normal distribution with a constant variance over time, and if labor's share is also constant, then aggregating GDP in this way would not matter.[10] Second, and probably more importantly, the macro growth literature typically omits the change in schooling. Third, because the macro models are motivated by issues of convergence they include the initial level of GDP, capital, and correlates for steady-state income. Indeed, a primary motivation for including human capital variables at all in these equations is to control for y*.

There are at least six ways to interpret the coefficient on the initial level of schooling in equation (5).[11] First, schooling may be a proxy for steady-state income. Countries with more schooling would be expected to have a higher steady-state income, so conditional on GDP in the initial year, we would expect more educated countries to grow more ($\beta_2>0$). If this were the case, more schooling would not change the steady-state growth rate, although it would raise steady-state income. Second, schooling could change the steady-state growth rate by enabling the work force to develop, implement and adopt new technologies (see Nelson and Phelps, 1966 Welch, 1970 and Romer, 1990), again leading to the prediction $\beta_2>0$. Third, countries with low initial stocks of human capital could have greater opportunities to grow by implementing technology developed abroad. In this case, one would expect $\beta_2<0$. Fourth, a positive or negative coefficient on initial schooling may simply reflect an exogenous change in the return to schooling, as shown in equation (4). Fifth, anticipated increases in future economic growth could cause schooling to rise (i.e., reverse causality), as argued by Bils and Klenow (1998). Sixth, the schooling variable may "pick up" the effect of the change in education, which is omitted from the equation. Sorting through these explanations is difficult. Indeed, Topel (1998) argues that "little can be learned" from macro growth equations because

[10]Heckman and Klenow (1997) also point out that half the variance of log income will be added to the GDP equation if income is log normal. See Heckman and Klenow (1997) for cross-sectional evidence.

[11]The first three of these interpretations are adapted from Topel (1998).

either a positive or negative coefficient on initial human capital is "consistent with the idea that human capital is a boon to growth and development."

2.2 Basic Results and Effect of Measurement Error

Table 2 replicates and extends the "growth accounting" and "endogenous growth" regressions in Benhabib and Spiegel's influential paper.[12] Their analysis is based on Kyriacou's (1991) measure of average years of schooling for the work force in 1965 and 1985, Summers and Heston's GDP and labor force data, and a measure of physical capital derived from investment flows. Following Benhabib and Spiegel, the regression in column (1) relates the annualized growth rate of GDP to the log change in years of schooling. From this model, Benhabib and Spiegel conclude, "Our findings shed some doubt on the traditional role given to human capital in the development process as a separate factor of production." Instead, they conclude that the stock of schooling matters for growth (see column 2 and 5) by enabling countries to adopt and innovate technology faster.

Topel argues that Benhabib and Spiegel's finding of an insignificant and wrong-signed effect of schooling changes on GDP growth is due to their log specification of education. The log-log specification follows if one assumes that schooling enters an aggregate Cobb-Douglas production function linearly. Given the success of the Mincer model, however, we would agree it is more natural to specify human capital as an exponential function of schooling in a Cobb-Douglas production function, so the change in years of schooling would enter the growth equation linearly. In any event, the logarithmic specification of schooling does not fully explain the perverse effect of educational improvements on growth in Benhabib and Spiegel's analysis.[13] Results of estimating a linear education specification in column 4 still

[12]We were not able to exactly replicate Benhabib and Spiegel's results because we use a revised version of Summers and Heston's GDP data. Nonetheless, our estimates are very close to theirs. For example, Benhabib and Spiegel report coefficients of -.059 for the change in log education and .545 for the change in log capital when they estimate the model in column 1 of Table 1; our estimates are -.072 and .523. Some of the other coefficients differ because of scaling; for comparability with later results, we divided the dependent variable and variables measured in changes by 20.

[13]The log specification is part of the explanation, however, because if the model in column (3) is estimated without the initial level of schooling, the change in log schooling has a negative and statistically significant effect, whereas the change in the level of schooling has a positive and statistically significant effect if it is included as a regressor in this model instead.

show a statistically insignificant (though positive) effect of the linear change in schooling on economic growth.

Columns 3 and 6 show that controlling for capital is key to Benhabib and Spiegel's finding of an insignificant effect of the change in schooling variable. When physical capital is excluded from the growth equation, the change in schooling has a statistically significant and positive effect in either the linear or log schooling specification. Why does controlling for capital have such a large effect on education? As shown below, it appears that the insignificant effect of the change in education is a result of the extraordinarily low signal in the education change variable. Indeed, conditional on the other variables that Benhabib and Spiegel hold constant (especially capital), the change in schooling conveys virtually no signal.

Notice also that the coefficient on capital is high in Table 2, around .50 with a t-ratio close to 10. In a competitive, Cobb-Douglas economy, the coefficient on capital growth in a GDP growth regression should equal capital's share of national income. Gollin (1998) estimates that labor's share ranges from .65 to .80 in most countries, after allocating labor's portion of self-employment and proprietors' income. Consequently, capital's share is probably no higher than .20 to .35. Since measured capital is derived from investment flows, and GDP is a direct function of investment, errors in the investment data will mechanically bias the coefficient on the growth in capital upwards; this might explain why capital has such a large and significant coefficient in the growth equations. If the coefficient on capital growth in column (5) of Table 2 is constrained to equal .20 or .35 -- a plausible range for capital's share -- the coefficient on the schooling change rises to .09 or .06, and becomes statistically significant.

2.2.1 The Extent of Measurement Error in International Education Data

We disregard errors that arise because years of schooling are an imperfect measure of human capital, and focus instead on the more tractable problem of estimating the extent of measurement error in cross-country data on average years of schooling. Benhabib and Spiegel's measure of average years of schooling for the work force was derived by Kyriacou

(1991) as follows. First, survey-based estimates of average years of schooling for 42 countries in the mid 1970s were regressed on the countries' primary, secondary and tertiary school enrollment rates. Coefficient estimates from this model were then used to predict years of schooling from enrollment rates for countries in other years. This method is likely to generate substantial noise since the fitted regression may not hold for all countries and time periods, and enrollment rates are frequently mismeasured. Changes in education derived from this measure are likely to be particularly noisy. Benhabib and Spiegel use Kyriacou's education data for 1965, as well as the change between 1965 and 1985.

The widely-used Barro and Lee (1993) data set is an alternative source of education data. For 40 percent of country-year cells, Barro and Lee measure average years of schooling by survey and census-based estimates reported by UNESCO. The remaining observations were derived from historical enrollment flow data using a "perpetual inventory method." The Barro-Lee measure is undoubtedly an advance over existing international measures of educational attainment, but errors in measurement are inevitable because the UNESCO enrollment rates are of doubtful quality in many countries (see Behrman and Rosensweig, 1994). Additionally, students educated abroad are miscounted in the flow data, which is probably a larger problem for higher education. More fundamentally, secondary and tertiary schooling is defined differently across countries, so the data for secondary and higher schooling are likely to be noisier than overall schooling. Notice also that because errors cumulate over time in Barro and Lee's stock-flow calculations, the errors in education will be positively correlated over time.

Even developed countries' data are sometimes measured with error in the available data sets. For example, as illustrated in Figure 2, the Barro-Lee data set indicates that average educational attainment declined by 0.2 years in Sweden between 1980 and 1990. This finding conflicts with other Swedish data, which show rising educational attainment and enrollment in this period. Between 1980 and 1990, for example, the Swedish Level of Living Survey (LNU) indicates that the average number of years of education for those age 18 to 75 increased by just over one year. The different education trends (as well as different mean education levels) displayed in Figure 2 may reflect the fact that 8.7 percent of Swedes reported completing a major part of their education abroad in the 1991 LNU survey, as well as the recent emphasis on raising education levels of adults in Sweden.

As is well known, if an explanatory variable is measured with additive white noise errors, then the coefficient on this variable will be attenuated toward zero in a bivariate regression, with the attenuation factor, R, asymptotically equal to the ratio of the variance of the correctly-measured variable to the variance of the observed variable (see, e.g., Griliches, 1986). A similar result holds in a multiple regression (with correctly-measured covariates), only now the variances are conditional on the other variables in the model. To estimate attenuation bias due to measurement error, write a nation's measured years of schooling, S_j, as it's true schooling, S_j^*, plus a measurement error denoted e_j, so $S_j = S_j^* + e_j$. It is convenient to start with the assumption that the measurement errors are "classical"; that is, errors that are uncorrelated with S^*, other variables in the growth equation, and the equation error term. Now let S^1 and S^2 denote two imperfect measures of average years of schooling for each country, with measurement errors e^1 and e^2 respectively. If the measurement errors in the two education variables (e^1 and e^2) are uncorrelated with each other, then the expected value of the covariance between the two education variables equals the variance of the correctly measured education variable. Moreover, the fraction of the observed variability in S^1 due to measurement error can be estimated as $R_1 = \mathrm{cov}(S^1,S^2)/\mathrm{var}(S^1)$. The statistic R_1 is often referred to as the reliability ratio of S^1, and has probability limit equal to $\mathrm{var}(S^*)/\{\mathrm{var}(S^*)+\mathrm{var}(e^1)\}$. Assuming constant variances, the reliability of the data expressed in changes will be lower than the cross-sectional reliability if the serial correlation of the true variable is higher than the serial correlation of the measurement errors.[14] The reliability ratio for changes in ΔS^1 can be estimated simply by: $R_{\Delta S1} = \mathrm{cov}(\Delta S^1,\Delta S^2)/\mathrm{var}(\Delta S^1)$. Note that if the errors in S^1 and S^2 are positively correlated, the estimated reliability ratios will be biased upward.

[14]Formally, $R_{\Delta S1} = \mathrm{var}(S^*)/\{\mathrm{var}(S^*) + \mathrm{var}(e)(1-r_e)/(1-\rho_{S^*})\}$, where r_e is the serial correlation of the errors and ρ_{S^*} is the serial correlation of true schooling.

We can calculate the reliability of the Barro-Lee and Kyriacou data if we treat the two variables as independent estimates of educational attainment. It is probably the case, however, that the measurement errors in the two data sources are positively correlated because, to a large extent, they both rely on the same mismeasured enrollment data.[15] Consequently, the reliability ratios derived from comparing these two measures probably provides an upper bound on the reliability of the data series.

Panel A of Table 3 presents estimates of the reliability ratio of the Kyriacou and Barro-Lee education data. (Appendix Table A.1 reports the correlation and covariance matrices for the measures.) The reliability ratios were derived by regressing one measure of years of schooling on the other.[16] The cross-sectional data have considerable signal, with the reliability ratio ranging from .77 to .85 in the Barro-Lee data, and exceeding .96 in the Kyriacou data. The reliability ratios fall by 10 to 30 percent if we condition on the log of 1965 GDP per capita, which is a common covariate. *More disconcerting, when the data are measured in changes over the 20 year period, the reliability ratio for the data used by Benhabib and Spiegel falls to less than 20 percent.* By way of comparison, note that Ashenfelter and Krueger (1994) find that the reliability of self-reported years of education is .90 in micro data on workers, and that the reliability of self-reported differences in education between identical twins is .57.

These results suggest that if there were no other controls, the estimated effect of schooling changes in Benhabib and Spiegel's results would be biased downward by 80 percent. But the bias is likely to be even greater because their regressions include additional explanatory variables that "soak up" some of the true changes in schooling. A regression of

[15]Another complication is that the Kyriacou data pertain to the education of the work force, whereas the Barro-Lee data pertain to the entire population age 25 and older. If the regression slope relating true education of workers to the true education of the population is one, the reliability ratios reported in the text are unbiased. Although we do not know true education of workers and the population, in the Barro-Lee data set a regression of the average years of schooling of men (who are more likely to work) on the average education of the population yields a slope of .99, suggesting that workers and the population may have close to a unit slope.

[16]Barro and Lee (1993) compare their education measure with alternative series by reporting correlation coefficients. For example, they report a correlation of .89 with Kyriacou's education data and .93 with Psacharopolous's. Our cross-sectional correlations are not very different. They do not report correlations for changes in education.

the change in Kyriacou's education measure on the covariates in column (4) of Table 2 yields an R^2 of 23 percent. If the covariates are correlated with the signal in education changes and not the noise, then there is *no* variability in true schooling changes left over in the measured schooling changes conditional on the other variables in the model.[17] Instead of rejecting the traditional Mincerian role of education on growth, a reasonable interpretation is that Benhabib and Spiegel's results shed no light on the role of education changes on growth.

The Barro and Lee data convey more signal than Kyriacou's data when expressed in changes. Indeed, nearly 60 percent of the variability in observed changes in years of education in the Barro-Lee data represent true changes. This makes the Barro-Lee data preferable to use to estimate the effect of educational improvements. Despite the greater reliability of the Barro-Lee data, there is still little signal left over in these data conditional on the other variables in the model in column 4 of Table 2; a regression of the change in the Barro-Lee schooling measure on the change in capital, change in population, and initial schooling yields an R^2 of .28. Consequently, conditional on these variables about 40 percent of the remaining variability in schooling changes in the Barro-Lee data is true signal.

As mentioned, we suspect the estimated reliability ratios are biased upward because the errors in the Kyriacou and Barro-Lee data are probably positively correlated. To derive a measure of education with independent errors, we calculated average years of schooling from the World Values Survey (WVS) for 34 countries. The WVS contains micro data from household surveys that were conducted in nearly 40 countries in 1990 or 1991. The survey was designed to be comparable across countries. In each country, individuals were asked to report the age at which they left school. With an assumption of school start age, we can calculate the average number of years that individuals spent in school. We also calculated average years of secondary and higher schooling by counting years of schooling obtained after 8 years of schooling as secondary and higher schooling. Notice that these measures will not be error free either. Errors could arise, for example, because individuals could have repeated

[17]Formally, the reliability ratio conditional on the other variables in the model is $(R_{S1}-R^2)/(1-R^2)$, where R^2 is the multiple coefficient of determination from a regression of measured schooling change variable on the other variables in the model.

grades, because we have made an erroneous assumption about school start age or the beginning of secondary schooling, or because of sampling errors. But the errors in this measure should be independent of the errors in Kyriacou's and Barro and Lee's data. The appendix provides additional details of our calculations with the WVS.

Panel B of Table 3 reports the reliability ratios for the Barro-Lee data and WVS data for 1990. The reliability ratio of .90 for the Barro-Lee data in 1990 is slightly higher than the estimate for 1985 based Kyriacou's data, but within one standard error. Thus, it appears that correlation between the errors in Kyriacou's and Barro-Lee's data is not a serious problem. Nonetheless, another advantage of the WVS data is that they can be used to calculate upper secondary schooling using a comparable (if imperfect) definition. As one might expect given differences in the definition of secondary schooling in the UNESCO data, the reliability of the secondary and higher schooling (.72) is lower than the reliability of all years of schooling.

Lastly, it should be noted that the measurement errors in schooling are positively serially correlated in the Barro-Lee data. This can be seen from the fact that the correlation between the 1965 and 1985 schooling levels across countries is .97 in the Barro-Lee data, while less than 90 percent of the variations in the cross-sectional data across countries appear to represent true signal. If the reliability ratios reported in Table 3 are correct, the only way the time-series correlation could be so high is if the errors are serially correlated. Nonetheless, since the serial correlation of true schooling is even higher than the serial correlation of the errors, the reliability of the first-differenced education data is lower than the reliability of the cross-sectional data; that is, more variation due to true education than noise cancels out in the first differenced data. This is apparent from the fact that the reliability ratio is much lower when the data are expressed in changes than in levels.

2.3 Additional Growth Models

Measurement errors aside, one could question whether physical capital should be included in a GDP growth equation because it is potentially an endogenous variable. Fast growing countries have more access to investment (see Blömstrom, Lipsey and Zejan, 1993).

Additionally, considerations of the low signal in schooling changes conditional on capital growth, and the mechanical correlation between measured capital and GDP (since capital is typically derived from investment), lead us to prefer parsimonious models that omit capital. Barro (1997) also excludes capital, so there is some precedent for a parsimonious specification in the growth literature. We first report models without controlling for capital, and then focus on the effect of capital in long-difference models in Section 2.5.

Table 4 reports "stylized" macro growth models without controlling for physical capital for samples spanning 5, 10 or 20 year periods. The dependent variable is the annualized change in the log of real GDP per capita per year based on Summers and Heston's (1991) Penn World Tables, Mark 5:6. Results are generally similar if GDP per worker is used instead. We use GDP per capita because it reflects labor force participation decisions and because it has been the focus of much of the previous literature. The schooling variable is Barro and Lee's measure of average years of schooling for the population age 25 and older. When the change in average schooling is included as a regressor in these models, we divide it by the number of years in the time span so the coefficients are comparable across columns, and comparable to Table 2. The equations were estimated by OLS, but the standard errors reported in the table allow for a country-specific component in the error term. We exclude other variables (such as the fertility rate and rule of law index) that are sometimes included in macro growth models to focus on education, and because those other variables are probably influenced themselves by education. Perhaps more importantly, measurement error problems are exacerbated by including covariates. For example, the correlation between the log fertility rate and education is -.85 in the Barro-Lee data set, which implies that the relative signal of average schooling falls to only one third if fertility is held constant.[18]

Our findings parallel Topel's. The change in schooling has little effect on GDP growth when the growth equation is estimated with high frequency changes (i.e., 5 years). However, increases in average years of schooling have a positive and statistically significant effect on

[18] We arrive at this estimate by assuming that R_S is .81 in the Barro-Lee cross-country schooling data. Using the formula in footnote 16, conditional on the log fertility rate the reliability of schooling is $(.81-.85^2)/(1-.85^2) = .32$.

economic growth over periods of 10 or 20 years. As discussed below, the magnitude of the coefficient estimates on both the change and initial level of schooling are large, probably too large to represent the causal effects of schooling.

The finding that the time span matters so much for the change in education also suggests that measurement error in schooling plays a major role in these estimates. Over short time periods, there is little change in a nation's true schooling level, so the transitory component of measurement error in schooling would be large relative to variability in the true change. Over longer periods, true education levels are more likely to change, increasing the signal relative to the noise in measured changes.

Measurement error bias appears to be greater over the 5 and 10 year horizons, but it is still substantial over 20 years. Since the change in schooling and initial level of GDP are essentially uncorrelated, the coefficient on the 20-year change in schooling in column 8 is biased downward by a factor of $1\text{-}R_{\Delta S}$, which is around 40 percent according to Table 3. Thus, adjusting for measurement error would lead the coefficient on the change in education to increase from .18 to .30 = .18/(1-.4). This is an enormous return to investment in schooling, equal to three or four times the private return to schooling estimated within most countries. Moreover, even if labor only captures two-thirds of the rise in GDP associated with an increase in human capital, as is often assumed, the net payoff to labor based on this coefficient is at least double the conventional return to schooling.

Like Benhabib and Spiegel, Barro and Sala-i-Martin (1995) conclude that contemporaneous changes in schooling do not contribute to economic growth, although they note that measurement error in schooling could bias their results. There are four reasons to suspect that measurement error has a particularly acute effect on their estimates. First, Barro and Sala-i-Martin analyze a mixed sample that combines changes over both 5-year (1985-90) and 10-year (1965-75 and 1975-85) periods; examining changes over such short periods tends to exacerbate the downward bias due to measurement errors. Second, they examine changes in average years of secondary and higher schooling. As was shown in Table 3, the reliability of secondary and higher schooling is lower than the reliability of all years of schooling, and the changes are likely to be less reliable as well. Third, they include separate variables for changes in male and female years of secondary and higher schooling. These two variables are highly correlated (r=.85), which would exacerbate measurement error problems if the signal in the variables is more highly correlated than the noise. If average years of secondary and higher schooling for men and women combined, or years of secondary and higher schooling for either men or women, is used instead of all years of schooling in the 10-year change model in column 6 of Table 4, the change in education has a sizable, statistically significant effect. Fourth, they estimate a restricted Seemingly Unrelated Regression (SUR) system, which exacerbates measurement error bias because asymptotically this estimator is equivalent to a weighted average of an OLS and fixed-effects estimator, and it is well known that a fixed-effects estimator can exacerbate measurement error bias.

Because Barro (1997) stresses male, secondary and higher education as a key determinant of growth, we have also explored the sensitivity of our results to using different measures of education, namely primary versus higher education, and male versus female education. When we test for different effects of years of primary and secondary and higher schooling in the model in column 6 of Table 4, we cannot reject that all years of schooling have the same effect on GDP growth (p-value equals .40 for initial levels and .12 for changes). We also find insignificant differences between primary and secondary schooling if we just use male schooling. We do find significant differences if we further disaggregate schooling levels by gender, however. The initial level of primary schooling has a positive effect for women and a negative effect for men, the initial level of secondary school has a negative effect for women and a positive effect for men, the change in primary schooling has a positive effect for women and a negative effect for men, and the change in secondary schooling has a negative effect for women and a positive effect for men. Because schooling levels are highly correlated for men and women, one needs to be cautious interpreting regressions that include disaggregated education variables.

2.4 Effect of Measurement Error on Initial Level of Education

The effect of the initial level of education on growth has been widely interpreted as an indication of large externalities from the stock of a nation's human capital on growth. For example, Barro (1997, p. 19) observes, "On impact, an extra year of male upper-level schooling is therefore estimated to raise the growth rate by a substantial 1.2 percentage points per year." Topel (1998), however, argues that "the magnitude of the effect of education on growth is vastly too large to be interpreted as a causal force." Indeed, Topel calculates that the present value of a one percentage point faster growth rate from an additional year of schooling would be about four times the cost, with a 5 percent real discount rate. He concludes that externalities from schooling may exist, but they are unlikely to be so large.

One possibility is that the level of schooling is spuriously reflecting the effect of the change in schooling on growth, which could account for its large impact on growth. Countries with higher initial levels of schooling also tended to have larger increases in schooling over the next 10 or 20 years in Barro and Lee's data, which is remarkable given that measurement error in schooling induces a negative covariance between the change and initial level of schooling. We initially suspected that the initial level of schooling spuriously picks up the effect of schooling increases, either because schooling changes are excluded from the growth equation or because the included variable is noisy. In Krueger and Lindahl (1998; section 2.4), however, we show that this is most unlikely. In particular, we show that if education is measured equally reliably each period, and if first and second period education are included in the growth regression, then the sum of the two coefficients on the education variables will be biased toward zero. Since a test of whether the initial level of education influences economic growth *conditional on the change in education* boils down to whether the sum of the coefficients on current and lagged education is positive, measurement error in education would tend to produce a bias against finding that the initial level of education influences growth.

2.5 Controlling for Physical Capital

The level and growth rate of capital are natural control variables to include in the GDP growth regressions. First, initial log GDP can be substituted for capital in a Solow growth model only if capital's share is constant over time and across countries (e.g., a Cobb-Douglas production function). Second, and more importantly for our purposes, capital-skill complementarity would imply that some of the increased output attributed to higher education in Table 4 should be attributed to increased capital (see, e.g., Goldin and Katz, 1997). As mentioned earlier, however, systematic correlation between measurement errors in capital and GDP, as well as endogeneity of capital, are reasons to be wary about including the growth of capital in a GDP equation. Nonetheless, here we examine the robustness of our results to controlling for physical capital.

Column (1) of Table 5 reports an estimate of the same 20-year growth model as in column 9 of Table 4, augmented to include the growth of capital per worker. We use Klenow and Rodriguez-Clare's (1997) capital data because they appear to have more signal than Benhabib and Spiegel's capital data.[19] The coefficient on the change in education falls by more than 50 percent when capital growth is included, although it remains barely statistically significant at the .10 level. In column (2) we add the initial log capital per worker, and in column (3) exclude the initial log GDP from the column (2) specification. Including initial log capital drives the coefficient on the change in schooling to close to zero. Notice also that the log of initial capital per worker has little effect in column (3).[20] The growth of capital per worker has an enormous effect on GDP growth, greatly exceeding capital's share in most countries. This finding is consistent with the errors in capital being systematically related to GDP, since both are functions of investment. To explore the sensitivity of the results, in column (4) we constrain the coefficient on the growth in capital to equal 0.35, which is on the

[19]A regression of Benhabib and Spiegel's change in log capital on the corresponding variable from Klenow and Rodriguez-Clare yields a regression coefficient (and standard error) of .95 (.065). The reverse regression yields a coefficient of .69 (.05). These estimates could be biased toward one because of correlted measurement errors in the two variables, since both depend on investment.

[20]If the change in log capital per work is dropped from the model in column (3), then initial log capital per worker does have a statistically significant, negative effect, and the schooling coefficients are similar to those in column 9 of Table 4.

high end of the distribution of non-labor's share around the world. These results indicate that both the change and initial level of schooling are associated with economic growth. Moreover, as Heckman and Klenow (1997) find in cross-sectional data, the coefficient on the change in education is similar to microeconometric estimates.

As mentioned earlier, including capital could exacerbate the measurement error in schooling. Indeed, we find that the reliability of Barro-Lee's 20-year change in schooling data falls from .58 to .46 once we condition on the change in capital, suggesting that the coefficient on the change in schooling in Table 5 should be roughly doubled. In column (5), to try to overcome measurement error we estimate the growth equation by instrumental variables, using Kyriacou's schooling data as excluded instruments for the change and level of schooling. This is the same estimation strategy previously used by Pritchett (1998), but we employ different schooling data as instruments, and use a different measure of capital. Unfortunately, because there is so little signal in education conditional on capital, the IV results yield a huge standard error (.167) for the effect of the change in education. Pritchett similarly finds a large standard errors from his IV estimates, although his point estimates are negative.[21]

We draw four main lessons from this investigation of the role of capital. First, the change in capital has an enormous effect in a GDP growth equation, probably because of a mechanical relationship between the errors in measuring capital and GDP or reverse causality. Second, the impact of both the level and change in schooling on economic growth is sensitive to whether the change in capital is included in the growth equation <u>and</u> allowed to have a coefficient that greatly exceeds capital's share. Third, controlling for capital exacerbates measurement error problems in schooling. Instrumental variables estimates designed to correct for measurement error in schooling yield such a large standard error on the change in schooling that the results are consistent with schooling changes having no effect on growth or a large effect on growth. Fourth, when the coefficient on capital growth is constrained to equal a plausible value, changes in years of schooling are positively related to economic

[21]Aside from the different data sources, the difference between our IV results and Pritchett's appears to result from his use of log schooling changes. If we use log schooling changes, we also find negative point estimates.

growth. Unless measurement error problems in schooling and capital can be overcome, we do not think the cross-country growth equations that control for capital growth will be very informative insofar as the benefit of education is concerned.

In all, we think the results in this section fairly consistently point to an association between GDP growth and contemporaneous education changes, once measurement error in education is taken into account. Although this relationship could come about for spurious reasons (e.g., fast growing countries could choose to spend more of their resources on education), the growth equations do not reject a "traditional role" for human capital.

3. Robustness of the Initial Education Effect on Growth

> *[I]t is not possible to draw a simple straight line relating secondary education to economic growth."*
>
> *W. Arthur Lewis, 1964*

The macro growth equations impose the restriction that all countries have the same relationship between growth and initial education, and that the relationship is linear. The first assumption is particularly worrisome because the micro evidence clearly indicates that the return to schooling varies considerably across countries, and even across regions within countries. For example, institutional factors that compress the wage structure in some countries result in lower returns to schooling in those countries (see, e.g., the essays in Freeman and Katz, 1995). Moreover, one might expect externalities from education to be greater in countries where the private return is depressed below the world market level. Thus, we first allow the effect of the stock of education on growth to vary by country. Next, we examine the effect of the linearity assumption. Both of these extensions to the standard growth specification suggest that the constrained specification estimated in the literature should be viewed with caution.

3.1 Heterogeneous Country Education Effects

The specifications previously estimated in the empirical growth literature constrains the initial level of education to have the same effect in each country. A more general model would allow the initial level of education to have a different effect in different countries. Since there is more than one observation per country in the 5- and 10-year growth models, this easily can be accomplished by interacting a set of dummy variables indicating each country with the base year education level for those countries. The average of the country-specific-education slopes provides an informative measure of the effect of initial education on growth for the average country. It is instructive to note that the coefficient on initial education estimated from the restricted, single-coefficient OLS model can be decomposed as a weighted average of the more general country-specific slopes, where the weights are the country-specific contributions to the overall variance in schooling.[22] This result is important because it indicates that the source of variation in the single-coefficient regression and average of the variable coefficients model is the same, but the country-specific slopes are aggregated differently in the two estimates.

Of course, if the assumptions of the constant-coefficient model hold (and the other Gauss-Markov assumptions hold), the OLS weights are the most efficient weights. But if a variable-coefficient model is more appropriate, there is no *a priori* reason to prefer the OLS weights over other weights. Indeed, it is rather odd to weight the country-specific slopes by the OLS weights if the slopes differ across countries. The unweighted-average coefficient is probably a more relevant summary statistic because it represents the expected value of the education coefficient for a random country in the world.

Table 6 summarizes estimates of a variable-coefficient model using 5-year and 10-year changes in GDP. Panel A reports results of regressing GDP growth on average years of schooling for the population age 25 and older, initial GDP and time dummies. Columns 1 and 3 report the constant-coefficient model, whereas columns 2 and 4 report the mean of the country-specific education coefficients. The constant education slope assumption is overwhelmingly rejected by the data for each time period (p-value < 0.0001). Indeed, the R^2 of the equations more than doubles when the education slopes are unconstrained. Of more consequence, the average slope coefficient is negative, though not statistically significant, in the variable-coefficient model. These results cast doubt on the interpretation of initial education in the constrained macro growth equation common in the literature.

Panel B of Table 6 reports results in which average years of secondary and higher schooling for males is used instead of average years of all education for the entire adult population. This variable has been emphasized as a key determinant of economic growth in

[22]This results requires that there are no other covariates; see Krueger and Lindahl, 1998. If country fixed effects are included in the model, the OLS constant coefficient can still be decomposed as a weighted average of the country-specific coefficients even if there are other covariates. But we exclude country fixed effects so that these estimates are comparable to the earlier ones, and because including fixed effects would exacerbate measurement error bias. We would also point out that the average of the country-specific coefficients is still informative when there are covariates, even if the single coefficient estimate can not be decomposed as a simple weighted average of the country-specific coefficients.

Barro's work. Again, however, the results of the constant-coefficient model are qualitatively different than those of the variable-coefficient model. Indeed, for the average country in the sample, a greater initial level of secondary and higher education has a statistically significant, negative association with economic growth over the ensuing 10 years.

The estimates reported in Table 6 exclude the change in education to focus on the effect of the initial stock of education. We have, however, experimented with a variable-coefficient model for the 10-year change in education variable. These estimates were also quite fragile. For example, if we regress annual GDP growth over ten years on a set of interactions between the 10-year change in education and country dummies, the initial log of GDP per capita, and time dummies, the average coefficient for the change in education interactions is negative. But if we also include a set of interactions between initial education and country dummies in this model, the average coefficient on the change in education swells to .18, and the average coefficient on initial education is negative. Since the latter model uses three observations to estimate two parameters for each country, we are reluctant to stress these results.

3.2 Exploring the Linearity Assumption

It is common in the empirical growth literature to assume that initial education has a linear effect on subsequent GDP growth. Although Mincer (1974) provides conditions under which education has a linear relationship with log earnings, these conditions do not necessarily imply that the level of initial education has a linear relationship with income growth. To examine the linearity assumption, we created a set of dummy variables which indicated whether each country's initial average years of education fell in the 0-1 range, 1-2 range, 2-3 range, and so on. We then included these dummies in lieu of linear education in the growth regression in column (4) of Table 4. Figure 3 displays the dummy coefficients (i.e., the unrestricted relationship) and the estimated linear relationship between GDP growth and education. The education-GDP growth relationship appears upward sloping at low levels of education, then plateaus between 4 and 8 years of education, and becomes downward sloping above 8 years of education. The linear model places more weight on countries with a low level of education because they are more numerous, and because they contribute most to the variance of education. An alternative, simpler approach to explore the impact of linearity is to specify GDP growth as a nonlinear function of education. Specifically, we included initial education and its square in the model in column 4 of Table 4. The data seem to prefer the quadratic specification, as the square term is statistically significant. More importantly, the relationship is inverted-U shaped, with a peak at 7.5 years of education. Since the mean education level for OECD countries in 1990 was 8.4 years in Barro and Lee's data, the average OECD country is on the downward-sloping segment of the education-growth profile. We similarly find an inverted-U shaped relationship between education and GDP growth which peaks below the level of education of developed countries when we examine 5 and 20 year changes in GDP, or male upper secondary schooling. Moreover, if we allow GDP to have a quadratic effect, the initial level of education continues to have a nonlinear effect in the models that we estimated.

These results are sensitive to including other covariates in the model, however. First, it should be noted that the effect of the initial level of schooling on growth becomes statistically insignificant once we include a regressor measuring the log of the fertility rate in the model in column 4 of Table 4. Second, Robert Barro has generously provided us with results that indicate that the square of average male secondary and higher schooling is statistically insignificant when he controls for a quadratic in log GDP, the log of the fertility rate, and the other variables in Table 1 of his paper in this volume. When he excludes the fertility rate from the model, however, the squared education term is statistically significant and negative. Since education may influence fertility, and since measurement error in schooling is greatly exacerbated when fertility is held constant, we think it is an open question whether fertility should be included in the growth equation when the goal is to estimate the impact of education on growth.[23]

[23] For completeness, we should mention that we find that the coefficient on the change in schooling remains statistically significant (coefficient = .052; t=2.6) when we add to the model in column 6 of Table 4 explanatory variables measuring squared log GDP, the log fertility rate, inflation, a democracy index and its square, and the growth rate of the terms of trade.

3.3 Estimates for OECD Countries

In view of the sensitivity of the effect of the initial education level on economic growth to the econometric assumptions investigated above, it is worth exploring whether the results hold for the sample of OECD countries. Table 7 presents estimates of the effect of initial education on growth for the subset of OECD countries, measuring GDP growth over 5, 10 or 20 year periods. In each case, the initial level of education had a statistically insignificant and small effect on economic growth. We similarly find that the initial level of secondary and higher education for men has a statistically insignificant effect if it is included in the growth equation instead of the broader schooling measure. These results are not surprising in light of the earlier finding that the average OECD country is on the downward-sloping segment of the education-growth curve.

Together, the results in this section cast doubt on the likelihood that there are large growth externalities from the initial level of education. The pattern of results in the less restrictive (i.e., nonlinear and variable coefficient) specifications cast doubt on the view that the initial level of education exerts a strong influence on growth, especially in high education countries. Most notably, the initial level of education appears to be unrelated to subsequent growth in OECD countries.

4. Conclusion and Policy Implications

And the preservation of the means of knowledge, among the lowest ranks, is of more importance to the public, than all the property of the rich men in the country. It is even of more consequence to the rich themselves, and to their posterity."

John Q. Adams, 1765

The micro and macro literatures both emphasize the role of education for raising income, and income growth. An accumulation of research using individual-level education and income data since the beginning of the 20th century provides robust evidence of a substantial payoff to investment in education, especially for those who traditionally complete low levels of schooling. From the micro evidence, it is unclear whether the social return to schooling exceeds the private return, although available U.S. evidence suggests that positive externalities in the form of reduced crime and reduced welfare participation are more likely to be reaped from investments in disadvantaged than advantaged groups. The macroeconomic evidence of externalities in terms of technological progress from investments in higher education seems to us to be more fragile. Externalities from the initial stock of human capital appear particularly unlikely to apply to OECD countries.

Our findings help resolve an important inconsistency between the micro and macro literatures on education: Contrary to Benhabib and Spiegel's (1994) and Barro and Sala-i-Martin's (1995) conclusions, the cross-country regressions indicate that the change in education is positively associated with economic growth once measurement error in education is accounted for. Griliches (1997) conjectured that the "jarring" finding of no relationship between education changes and GDP growth was due to either measurement error in education or a tendency for more highly educated workers to enter sectors of the economy whose contribution to GDP are systematically under measured. Measurement error in education appears sufficient to account for the insignificant effect of education changes. Indeed, after adjusting for measurement error, the change in average years of schooling has a greater effect in the cross-country regressions than in the within-country micro regressions. Controlling for capital growth reduces the effect of education changes, but the magnitude of the effect in the cross-country data is still at least as great as the micro return to education once measurement error is taken into account.

The large return to schooling changes found in the cross-country models suggests that reverse causality or omitted variables create problems at the country level of analysis, or that increases in average educational attainment generate nationwide externalities. Although the microeconometric evidence in several countries suggests that within countries the causal effect of education on earnings can be estimated reasonably well by taking education as exogenous, it does not follow that cross-country differences in education can be taken as a cause of income as opposed to a result of current income or anticipated income growth. Moreover, countries that improve their educational systems are likely to concurrently change other policies that enhance growth, producing a different source of omitted-variable bias in cross-country analyses. Education, in the eloquent description of Harbison and Myers (1965), "is both the seed and the flower of economic development." It is difficult to separate the causal effect of education from the positive income demand for education in cross-country data. For this reason, Mankiw (1997) describes the presumed exogeneity of school enrollment as the "weak link" in the empirical growth literature. In our opinion, this link is unlikely to be strengthened unless the cross-country literature can identify natural experiments in schooling attainment similar to those that have been exploited in the microeconometric literature, and unless measurement errors in the cross-country data are explicitly taken into account in the econometric modelling.

For policy makers, the obvious prescription to enhance growth is that, on the margin, funds should be invested in the components of the education system that generate the highest social returns. But the micro and macro evidence suggest that the returns to investing in different educational levels are likely to differ across countries, depending on the country's state of development, distribution of income, and structure of the education system. There are unlikely to be universal answers. In the United States, there is much support for the view that investments in young, disadvantaged children have the highest returns, and that it is very difficult to improve the economic circumstances of adolescent high school dropouts with short-term job training (e.g., Heckman, 1998). This view implicitly underlies the recent increased support for Head Start and smaller primary school classes, and the shift in JTPA funds away from job training for out-of-school youth. But the U.S. may be unique.

The optimal education policy for Sweden may be quite different than for the U.S. Heckman (1998) argues that investment in very young children in America pays a high return because “early learning begets later learning.” In the U.S., 22 percent of children under age 6 live in families that fall below the poverty line, and an incredible 59 percent of children under 6 who live with single mothers are in poverty (U.S. Census Bureau, 1998). High rates of childhood poverty, coupled with a patchwork system of childcare arrangements, may lead to particularly high payoffs to investments in young children in the U.S. Moreover, the lagging development of many young American children, and high existing subsidies to colleges (see Winston and Yen, 1995), may reduce the return on investments at older ages. Sweden has a much more equal distribution of income, and a more extensive and universal system of childcare. As a consequence, Sweden may be in a situation where investments in education for older students pay a higher return than investments in programs for very young children. But one must also be concerned that the U.S. evidence reflects the fact that there are critical stages of development during childhood that condition the payoff to investments at various ages, and that these stages in large part determine the payoff to investing in certain age groups irrespective of economic and social circumstances.

Another overriding factor in Sweden involves the compression of the wage structure, which depresses the private return to acquiring skills compared to the U.S. and most countries of the world. Edin and Topel (1997) find that college enrollment in Sweden is quite responsive to the private payoff to education prevailing at the time students make their enrollment decisions. Although Sweden has a high level of post-secondary educational attainment by world standards, it is nonetheless likely that the level of educational attainment is distorted by the depressed private payoff to education and skills. This consideration may militate in favor of a policy of increasing education at higher levels in Sweden. How this is best accomplished is unclear, however. The current thrust of subsidizing dislocated workers to return to school has benefits and costs. For example, older workers will enter the workforce more quickly than, say, pre-school children, so the gestation period for investments in older workers' human capital is shorter. On the other hand, the U.S. experience has been one of rather ordinary returns to investments in education for dislocated workers. Moreover, in Sweden some observers are concerned that subsidizing unemployed workers to return to school may create a disincentive in which some workers intentionally delay completing their education, find a job only to become unemployed, and then spend a long period in school while collecting unemployment insurance benefits.

We recognize that our conclusion leaves policy makers in something of a quandary. On the margin, should they attempt to expand education for the least or most able, youngest or oldest, to enhance growth? While the macro growth literature may lead policymakers to try to expand higher education, and the U.S. micro-level research on education and training may support a policy of investments in disadvantaged, pre-school children, we think a prudent approach for a country like Sweden would be to pursue a diverse strategy of raising human capital on several fronts.

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Data Appendix

The second wave of the World Values Survey (WVS) was conducted in 42 countries between 1990 and 1993. The sampled countries represented almost 70% of the world population, including several countries where micro data normally are unavailable. The survey was designed by the World Values Study Group (1994), and conducted by local survey organizations (mainly Gallup) in each of the surveyed countries. In most countries, a national random sample of adults (over age 18) was surveyed. For 12 of the countries in our sample (Belgium, Brazil, Canada, China, India, Italy, Netherlands, Portugal, Spain, Switzerland, West Germany and U.K.), sampling weights were available to make the survey representative of the country's population; the other samples are self-weighting. A feature of the survey is that the questionnaire was designed to be similar in all countries to facilitate comparisons across countries. There are, however, drawbacks to using the WVS for our purposes. The primary purpose of the WVS was to compare values and norms across different societies. Although questions about education were included, they appear to have been a lower priority than the normative questions. We were able to derive comparable data from the WVS on mean years of schooling for 34 countries.

Mean years of schooling is calculated from question V356 in the WVS, which asked, "At what age did you or will you complete your formal education, either at school or at an institution of higher education? Please exclude apprenticeships." The variable is typically bottom coded at 12 years of age and top coded at 21 years of age. Although there are some benefits of formulating the question this way, for our purposes it also creates some problems. First, we do not know the age at which respondents started their education. For this reason we have used data from UNESCO (1967) on the typical school starting age in each country.

Second, the top and bottom coding is potentially a problem. For almost one third of the countries (Austria, Brazil, Denmark, India, Norway, Poland, South Korea, Sweden, Switzerland and Turkey), however, a question was asked concerning formal educational attainment. Since, as mentioned above, one of the benefits with the WVS is that the same questions are asked in all the countries, we used this variable only to solve the bottom and top coding problem.[24] We have coded illiterate/no schooling as 0 years of schooling and incomplete primary schooling as 3 years. In the two countries where graduate studies is a separate category, we have set this to 19. For the countries in which the educational attainment variable does not exist, we set years of schooling for those in the bottom-coded category equal to the midpoint of 0 and the bottom coded years of schooling.25 Similarly, we set years of schooling for the highest category equal to the midpoint of 18 and the top coded years of schooling.

Appendix Table A2 reports the weighted mean years of schooling derived from the WVS. The weights for these calculations were the sampling weights reported in the WVS. The sample size for each country is also reported.

[24] For South Korea and Switzerland, however, we exclusively used this variable to derive years of schooling because the question about school leaving age is not asked in these countries For Turkey, school leaving age is only coded as three possible ages, so we use both the educational attainment and school leaving age variable to derive years of schooling.

[25] For East and West Germany the bottom code was 14, and for Finland it was 15. Because school start age was 7 in Finland and East Germany, and 6 in West Germany, we set years of schooling equal to 6 in West Germany and Finland and 5 in East Germany for those who were bottom coded.

Table A1: Correlation and Covariance Matrices for Barro-Lee and Kyriacou Years of Schooling Data

A. Correlation Matrix

	S^{BL}_{65}	S^{BL}_{85}	S^{K}_{65}	S^{K}_{85}	ΔS^{BL}	ΔS^{K}
S^{BL}_{65}	1.00					
S^{BL}_{85}	0.97	1.00				
S^{K}_{65}	0.91	0.92	1.00			
S^{K}_{85}	0.81	0.86	0.88	1.00		
ΔS^{BL}	0.23	0.46	0.36	0.51	1.00	
ΔS^{K}	-0.12	-0.03	-0.17	0.33	0.34	1.00

B. Covariance Matrix

	S^{BL}_{65}	S^{BL}_{85}	S^{K}_{65}	S^{K}_{85}	ΔS^{BL}	ΔS^{K}
S^{BL}_{65}	6.65					
S^{BL}_{85}	7.07	8.01				
S^{K}_{65}	5.66	6.29	5.88			
S^{K}_{85}	5.27	6.19	5.38	6.41		
ΔS^{BL}	0.42	0.93	0.62	0.92	0.51	
ΔS^{K}	-0.39	-0.10	-0.50	1.02	0.30	1.52

Notes: Sample size is 68. A superscript BL refers to the Barro-Lee data and a superscript K refers to the Kyriacou data. The subscript indicates the year. Unlike the other tables, the change in schooling is not annualized.

Table A2: Schooling Data Derived from the World Values Survey

Country	Mean years of schooling (Std. deviation)		Survey Year	Sample Size
Argentina	10.23	(4.88)	1991	766
Austria	8.69	(4.88)	1990	1,296
Belgium	11.53	(3.29)	1990	2,328
Bulgaria	11.29	(3.83)	1990	877
Brazil	4.04	(3.04)	1991-92	1,154
Canada	12.60	(3.19)	1990	1,483
Czechoslovakia	11.78	(2.86)	1990	1,190
Chile	10.48	(4.37)	1990	1,137
China	10.32	(3.51)	1990	745
Denmark	12.50	(3.66)	1990	862
East Germany	9.12	(3.90)	1990	1,175
Finland	12.61	(3.81)	1990	534
France	11.12	(3.62)	1990	830
Hungary	9.79	(3.57)	1990	895
Iceland	12.16	(3.74)	1990	575
Ireland	10.20	(2.81)	1990	847
India	2.97	(4.48)	1990	1,908
Italy	7.88	(4.90)	1990	1,616
Japan	12.29	(2.85)	1990	855
Mexico	8.44	(5.47)	1990	835
Netherlands	11.89	(3.65)	1990	876
Norway	13.43	(4.46)	1990	1,063
Poland	10.11	(3.56)	1989	803
Portugal	6.12	(4.79)	1990	823
Romania	10.50	(4.36)	1993	933
Russia	12.35	(3.67)	1991	1,551
Spain	8.61	(4.49)	1990	2,991
South Korea	12.00	(3.58)	1990	1,040
Sweden	12.79	(3.40)	1990	848
Switzerland	8.63	(2.61)	1988-89	1,154
Turkey	6.13	(4.65)	1990-91	805
U.K.(excl. N.I.)	11.20	(2.50)	1990	1,288
USA	13.26	(2.96)	1990	1,477
West Germany	9.78	(3.34)	1990	1,770

Table 1: OLS and IV Estimates of the Return to Education with Instruments Based on Natural Experiments{PRIVATE }

{PRIVATE } Study	Sample, Identification Strategy, and Instruments	Description	Schooling Coefficients OLS	IV	Hausman Test (p-value)
1. Angrist and Krueger (1991)	1970 and 1980 Census Data. Men. Instruments are quarter of birth interacted with year of birth. Controls include quadratic in age and indicators for race, marital status, urban residence.	1920-29 cohort in 1970	0.070 (0.000)	0.101 (0.033)	0.348
		1930-39 cohort in 1980	0.063 (0.000)	0.060 (0.030)	0.920
		1940-49 cohort in 1980	0.052 (0.000)	0.078 (0.030)	0.386
2. Kane and Rouse (1993)	NLS Class of 1972, Women. Instruments are tuition at 2 and 4-year state colleges and distance to nearest college. Controls include race, part-time status, experience. Schooling measured in units of college credit equivalents.	Models without test score or parental education	0.080 (0.005)	0.091 (0.033)	0.736
		Models with test scores and parental education	0.063 (0.005)	0.094 (0.042)	0.457
3. Card (1995b)	NLS Young Men (1966 Cohort) Instrument is an indicator for a nearby 4-year college in 1966, or the interaction of this var. with parental education. Controls include race, experience (treated as education), region, and parental education.	Models that use college proximity as instrument (1976 earnings)	0.073 (0.004)	0.132 (0.049)	0.227
		Models that use college proximity X family background as instrument	--	0.097 (0.048)	0.616
4. Conneely and Uusitalo (1997)	Finnish men who served in the army in 1982, and were working full time in civilian jobs in 1994. Administrative earnings and education data. Instrument is dummy for living in university town in 1980. Controls include quadratic in experience and parental education and earnings.	Models that exclude parental education and earnings	0.085 (0.001)	0.110 (0.024)	0.297
		Models that include parental education and earnings	0.083 (0.001)	0.098 (0.035)	0.668
5. Maluccio (1997)	Bicol Multipurpose Survey (rural Philippines). Male and female wage earners age 20-44 in 1994, whose families were interviewed in 1978. Instruments are distance to nearest high school and indicator for local private high school. Controls include quadratic in age and indicators for gender and residence in a rural community.	Models that do not control for selection of employment status or location	0.073 (0.011)	0.145 (0.041)	0.068
		Models with selection correction for location and employment status	0.063 (0.006)	0.113 (0.033)	0.123
6. Harmon and Walker (1995)	British Family Expenditure Survey 1978-86. Men. Instruments are indicators for changes in the minimum school leaving age in 1947 and 1973. Controls include quadratic in age, survey year, and region.		0.061 (0.001)	0.153 (0.015)	0.000

Notes: Rows 1-6 are adapted from Card (1998); rows 7-10 are authors' summaries. The estimates and standard errors in row 7 are divided by 4 to approximate the yearly returns to schooling. Hausman tests of the equality of OLS and IV estimates are based on authors' calculations; test in row 10 is only approximate because the models are not identical.

Table 1: Continued

{PRIVATE } Author	Sample Identification Strategy and Instruments		Schooling Coefficients OLS	IV	Hausman Test (p-value)
7. Ichino and Winter-Ebmer (1998)	German Socioeconomic Panel 1986. Men. Instrument is indicator for cohort born 1930-35 and/or whether father served in World War II. Controls include a quadratic in age, unemployment rate at age 14 and indicators for fathers education, socioeconomic status and self-employed status. Returns were calculated based on assumption of 4 years of high school.	Models that use cohort 1930-35 as instrument	0.072 (0.008)	0.148 (0.211)	0.721
		Models that use father in World War II as instrument	__	0.182 (0.070)	0.113
		Models that use cohort 1930-35 and father in World War II as instruments	__	0.177 (0.070)	0.131
	Austrian Microcensus 1983. Men born after 1946. Instrument is indicator for cohort born 1930-35. Controls include age and unemployment rate at age 14.	Models that use cohort 1930-35 as instrument.	0.130 (0.004)	0.237 (0.086)	0.211
8. Lemieux and Card (1998)	1971 and 1981 Canadian Census. 1973 Job Mobility survey. Men, World War II veterans from Quebec (French speaking) and Ontario (English speaking). Instruments are potential eligibility for World War II educational assistance program or an interaction between this and fathers education. Controls include quadratic in potential experience and dummy for Quebec (row 1 and 2) or quadratic in actual experience, dummy for Quebec, served in World War II and fathers education (row 3).	1971 Canadian Census. Models that use potential program eligibility as instrument.	0.070 (0.002)	0.141 (0.048)	0.139
		1981 Canadian Census. Models that use potential program eligibility as instrument.	0.062 (0.001)	0.055 (0.016)	0.661
		1973 Job Mobility Survey. Models that use potential program eligibility interacted with fathers education as instrument.	0.065 (0.003)	0.140 (0.091)	0.410
9. Butcher and Case (1994)	U.S. PSID. White women age 24-65 in 1985. Instruments are indicators for the presence of sisters, or sisters indicator and quadratic in number of siblings. Controls include a cubic in age, indicators for Catholic, oldest child, poor household and parental education.	Models that use indicator for presence of sister as instrument	0.091 (0.007)	0.184 (0.113)	0.410
		Models that use indicator for presence of sister and quadratic in number of siblings as instruments	--	0.182 (0.055)	0.095
10. Duflo (1998)	1995 Intercensal Survey of Indonesia. Men born between 1950-72. Instruments are interactions between indicators for age in 1974 and some measure of the program intensity in region born, capturing the effect of a large scale governmental primary school program. Controls include indicators for year and region of birth and indicators for year of birth, interacted with no. of children and with enrollment rate in 1971.	Models that use number of schools per child built in 1973-78 as a measure of program intensity.	0.062 (0.001)	0.097 (0.034)	0.303

Table 2: Replication and Extension of Benhabib and Spiegel (1994){PRIVATE }
Dependent Variable: Annualized Change in Log GDP, 1965-85

{PRIVATE } Variable	Log Schooling (1)	(2)	(3)	Linear Schooling (4)	(5)	(6)
Δ Log S	-.072 (.058)	.178 (.112)	.614 (.162)	---	---	---
Log S_{65}	---	.010 (.004)	.026 (.005)	---	---	---
ΔS	---	---	---	.012 (.023)	.039 (.024)	.151 (.034)
S_{65}	---	---	---	---	.003 (.001)	.004 (.001)
Log Y_{65}	-.009 (.002)	-.012 (.002)	-.015 (.003)	-.008 (.002)	-.014 (.002)	-.014 (.004)
Δ Log Capital	.523 (.048)	.461 (.052)	---	.521 (.051)	.465 (.052)	---
Δ Log Work Force	.175 (.164)	.232 (.160)	---	.110 (.160)	.335 (.167)	---
R^2	.694	.720	.291	.688	.726	.271

Notes: All change variables were divided by 20, including the dependent variable. Sample size is 78 countries. Standard errors are in parentheses. All equations also include an intercept. S_{65} is Kyriacou's measure of schooling in 1965; Δ Log S is the change in log schooling between 1965 and 1985, divided by 20; and Y_{65} is GDP per capita in 1965. Mean of dependent variable is .039; standard deviation of dependent variable is .020.

Table 3. Reliability of Various Measures of Years of Schooling

A. Estimated Reliability Ratios for Barro-Lee and Kyriacou Data

	Reliability of Barro-Lee Data	Reliability of Kyriacou Data
Average years of Schooling, 1965	.851 (.049)	.964 (.055)
Average years of Schooling, 1985	.773 (.055)	.966 (.069)
Change in years of Schooling, 1965-85	.577 (.199)	.195 (.067)

B. Estimated Reliability Ratios for Barro-Lee and World Values Survey Data

	Reliability of Barro-Lee Data	Reliablity of WVS Data
Average years of Schooling, 1990	.903 (.115)	.727 (.093)
Average years of Secondary and Higher Schooling, 1990	.719 (.167)	.512 (.119)

Notes: The estimated reliability ratios are the slope coefficients from a bivariate regression of one measure of schooling on the other. For example, the .851 entry in the first row is the slope coefficient from a regression in which the dependent variable is Kyriacou's schooling variable and the independent variable is Barro-Lee's schooling variable. The .964 ratio in the second column is estimated from the reverse regression. In panel B, the reliability ratios are estimated by comparing the Barro-Lee and WVS data. In the WVS data set, secondary and higher schooling is defined as years of schooling attained *after 8 years of schooling.*

Sample size for panel A is 68 countries. Sample size for panel B is 34 countries. Standard errors are reported in parentheses.

Table 4: The Effect of Schooling on Economic Growth
Dependent Variable: Annualized Change in Log GDP per Capita

	5-year changes			10-year changes			20-year changes		
	(1)	(2)	(4)	(5)	(6)	(6)	(7)	(8)	(9)
S_{t-1}	.004 (.001)	---	.004 (.001)	.003 (.001)	---	.004 (.001)	.005 (.001)	---	.005 (.001)
ΔS	---	.031 (.015)	.039 (.014)	---	.075 (.026)	.086 (.024)	---	.184 (.057)	.182 (.051)
Log Y_{t-1}	-.005 (.003)	.004 (.002)	-.006 (.003)	-.003 (.003)	.004 (.001)	-.005 (.003)	-.010 (.003)	-.001 (.002)	-.013 (.003)
R^2	.197	.161	.207	.242	.229	.284	.184	.103	.281
N	607	607	607	292	292	292	97	97	97

Notes: First six columns include time dummies. Equations were estimated by OLS. The standard errors in the first six columns allow for correlated errors for the same country in different time periods. Maximum number of countries is 110. Columns 1-3 consist of changes for 1960-65, 1965-70, 1970-75, 1975-80, 1980-85, 1985-90. Columns 4-6 consist of changes for 1960-70, 1970-80, 1980-90. Columns 7-9 consist of changes for 1965-85. Log Y_{t-1} and S_{t-1} are the log GDP per capita and level of schooling in the initial year of each period. ΔS is the change in schooling between t-1 and t divided by the number of years in the period. Data are from Summers and Heston and Barro and Lee. Mean (and standard deviation) of annualized per capita GDP growth is .021 (.033) for columns 1-3, .022 (.026) for columns 4-6, and .022 (.020) for columns 7-9.

Table 5: The Effect of Schooling and Capital on Economic Growth
Dependent Variable: Annualized Change in Log GDP per Capita, 1965-85

	OLS				IV
	(1)	**(2)**	**(3)**	**(4)**	**(5)**
ΔS	.066 (.039)	.017 (.032)	.015 (.042)	.083 (.043)	.069 (.167)
S_{65}	.004 (.001)	.0013 (.0008)	.0005 (.0010)	.002 (.001)	-.001 (.002)
Log Y_{65}	-.009 (.003)	-.026 (.003)	---	---	---
Δ Log Capital per Worker	.598 (.062)	.795 (.058)	.648 (.073)	.35*	.597 (.119)
Log Capital per Worker 1960	---	.016 (.002)	.002 (.002)	-.002 (.002)	.001 (.004)
R^2	.63	.76	.58	.12	.55
Sample Size	92	92	92	92	66

Notes: Change variables have been divided by the number of years spanned by the change (20 years for schooling and log GDP, 25 years for capital). Schooling data used in the regressions are from Barro and Lee. The instrumental variables model in column 6 uses Kyriacou's schooling data as excluded instruments for the level and change in Barro-Lee's schooling variables. Capital data are from Klenow and Rodriguez-Clare (1997), and pertain to 1960-85. The coefficient on the change in log capital in column 4 is constrained to equal .35, which is roughly capital's share.

Table 6: Mean Estimates from a Random coefficient Specification
Dependent Variable: Annualized Change Log GDP per Capita

	5-Year Changes		10-Year Changes	
	Constant Coefficient (1)	Mean Variable-Coefficient Estimate (2)	Constant Coefficient (3)	Mean Variable-Coefficient Estimate (4)
		A. All Years of Schooling		
Coefficient Estimate for S_{t-1}	.0040 (.0007)	-.0033 (.0036)	.0033 (.0008)	-.0064 (.0059)
p-value	---	.000	---	.000
R^2	.197	.481	.242	.690
		B. Male Secondary and Higher Schooling		
Coefficient Estimate for Male Secondary+ S_{t-1}	.0088 (.0017)	-.0196 (.0114)	.0081 (.0020)	-.0353 (.0179)
p-value	---	.0000	---	.0000
R^2	.190	.469	.242	.658

Notes: All regressions control for initial Log GDP per capita and time dummies. The number of countries is 110 for the 5-year change equations and 108 for the 10-year change models. The p-value is for test of equality of country-specific education coefficients in the variable coefficient model. Sample size is 607 in columns 1-2 and 292 in columns 3-4.

Table 7: The Effect of Schooling on Economic Growth in the OECD

Dependent Variable: Annualized Change in Log GDP per Capita, Various Time Periods

Variable	5-year Growth (1)	10-year Growth (2)	20-year Growth (3)
Initial Schooling	-.000	-.000	.000
	(.001)	(.001)	(.001)
Initial Log GDP	-.015	-.015	-.011
	(.008)	(.006)	(.005)
R^2	.43	.55	.35
Sample Size	138	69	23

Notes: The dependent variable has been divided by the number of years spanned by the change. Columns 1 and 2 also includes time dummies.

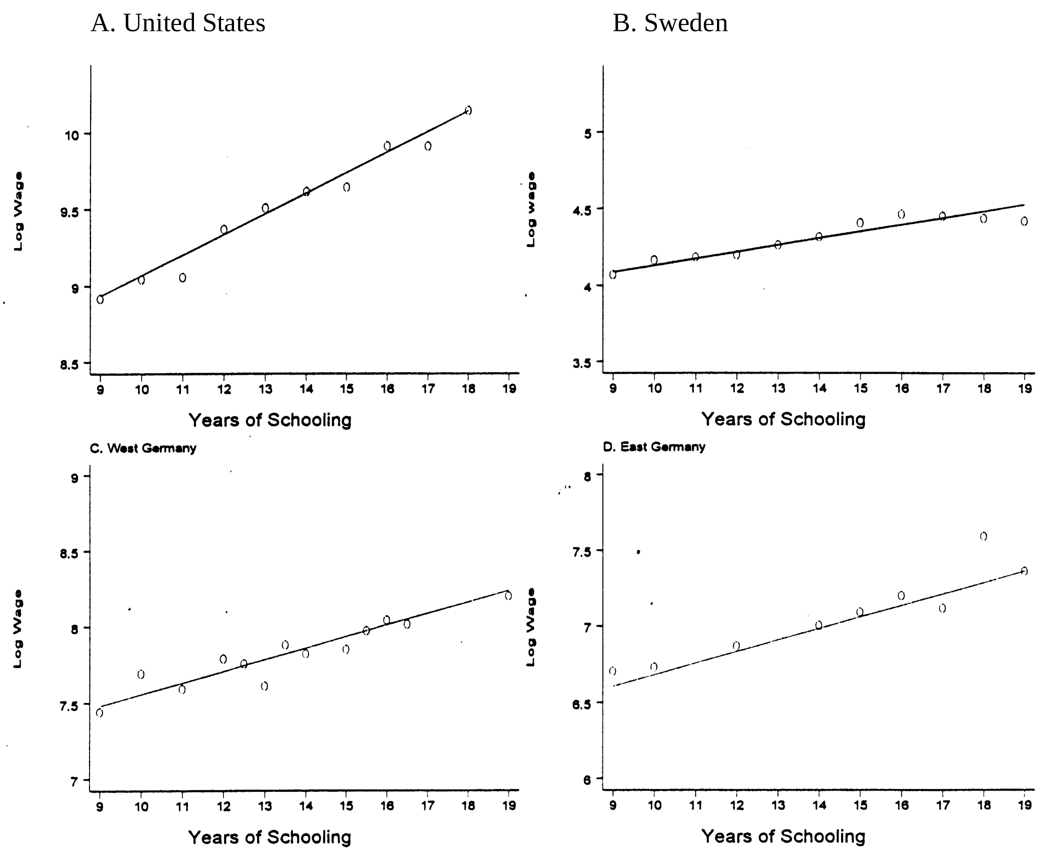
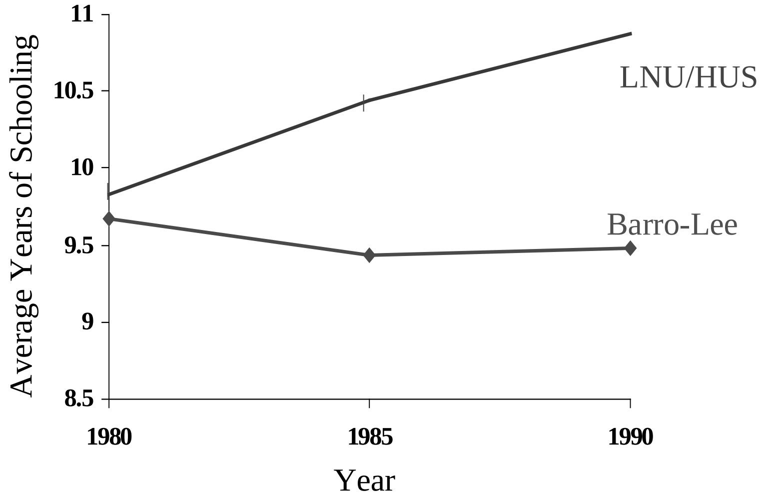


Figure 1: Unrestricted Schooling-Log Wage Relationship and Mincer Earnings Specification for the United States, Sweden, West Germany and East Germany

Figure 2: Average Years of Schooling in Sweden, Barro-Lee versus Other Survey Data



Notes: Barro-Lee data are for population age 15 and older; 1980 and 1991 survey data are from Swedish Level of Living Survey (LNU), and 1984 survey data are from Household Market and Nonmarket Survey (HUS). Both LNU and HUS pertain to population age 18-75.

Figure 3
Linear and Unrestricted Schooling-GDP Growth Relationship, and Change in Unrestricted Relationship