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Health and Economic Growth: Reconciling the Micro and Macro Evidence*

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Abstract

There are two prominent methods to assess the effects of health on economic growth. The first is based on the estimated returns on health by means of Mincer wage regressions that are aggregated to derive the macroeconomic effects of population health. The second approach is based on the estimation of a generalized aggregate production function that decomposes human capital into its different components, in particular, population health. While the overwhelming majority of studies based on both methods indicates a positive effect of health on economic growth, the size of the effect is still subject to intensive debate. We show that, after appropriately controlling for potential spillover effects of population health at the aggregate level, the point estimate of the macroeconomic effect of health is remarkably close to the one found by aggregating the microeconomic effects. Specifically, an increase in the adult survival rate of 10 percentage points is associated with a 9.1 percent increase in labor productivity. This effect is sizable and adds a strong argument for investments in population health over and above the direct welfare benefits of good health.

JEL-classification: I15, I25, J11, O11, O15.

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1 Introduction

Health is an important form of human capital. It enhances workers' productivity by increasing their physical capacities, such as strength and endurance, as well as their mental capacities, such as cognitive functioning and reasoning ability. There are many plausible pathways through which health improvements influence the pace of income growth. For example, better health directly increases labor market participation and worker productivity (Strauss and Thomas, 1998; Schultz, 1999, 2002, 2005; Bloom and Canning, 2000; Bloom, Canning, and Graham, 2003); a longer lifespan due to better health increases the incentives to save and invest in human capital (Bloom, Canning, and Sevilla, 2003; Bloom, Canning, Mansfield, and Moore, 2007; Bloom, Canning, and Moore, 2014; Cervellati and Sunde, 2013; Prettnner, 2013); better health, particularly of women, tends to reduce fertility and spur a demographic and economic transition from a state of high mortality, high fertility, and stagnating incomes toward low mortality, low fertility, and sustained income growth (Galor and Weil, 2000; Galor, 2005, 2011; Cervellati and Sunde, 2005, 2011; Bloom, Kuhn, and Prettnner, 2015).

Accordingly, we expect a positive association between health and productivity at the macroeconomic level. One way to assess the size of the macroeconomic effect is adding up the microeconomic effects of health to infer the implications for aggregate income. For example, Fogel (1994, 1997) argues that a large part of British economic growth during 1780—1980 (about 0.33 percent a year) was due to increases in effective labor inputs that resulted from workers' better nutrition and improved health. More recently, Shastry and Weil (2003) and Weil (2007) model output using an aggregate production function that includes the stock of health as measured by the adult survival rate. This calibration exercise suggests that health is a vitally important form of human capital and deserves a central attention in the development process.

A second way to assess the macroeconomic effects of health relies on growth regressions (Barro, 1991; Islam, 1995; Durlauf, Johnson, and Temple, 2005). Strong cross-country correlations between measures of aggregate health and per capita income are well established (World Bank, 1993; Preston, 1975). The causal channel from high income to health that is part of the explanation of this finding is already well-known. Higher incomes promote access to many of the goods and services that promote health and longevity, such as a nutritious diet, safe water and sanitation, and good health care. However, the standard view on causality mainly running from income to health has been challenged in recent years by research highlighting the causal link running the other way, from health to income (see, for example, Lorentzen, McMillan, and Wacziarg, 2008; Suhrcke and Urban, 2010; Aghion, Howitt, and Murtin, 2011; Cervellati and Sunde, 2011; Bloom, Canning, and Fink, 2014; Gehring and Prettnner, 2017).

The aim of this paper is to compare the size of the microeconomic estimates of the

effect of health on wages with the macroeconomic estimates of the effect of health on worker productivity. We therefore estimate a production function model of economic growth, keeping our specification as close as possible to that of Weil (2007) to permit direct comparison between our estimates and his calibration. Estimating an aggregate production function using cross-country data is difficult, because reverse causality, omitted variable bias, and measurement error in the explanatory variables may lead to inconsistencies in parameter estimates. We try to take all three of these issues accurately into account.

According to Weil (2007), an increase in adult survival rates of 0.1 or ten percentage points translates into a 6.7 percent increase in labor productivity. Consequently, health differentials account for about 9.9 percent of the variation in output per worker across countries. According to our estimates, an increase in adult survival rates of 0.1 is associated with a 9.1 percent increase in labor productivity. The 95 percent confidence interval ranges from 0.47 to 17.7 percent and, thus, includes the estimate derived by Weil (2007). Therefore, our macro results are consistent with the micro results of Weil (2007), thus, reconciling the micro-based and the macro-based approach of estimating the effect of health on income growth. Since we include physical capital and education in our empirical framework, the resulting estimate is a measure of the direct productivity benefits of health. Therefore, it excludes any effect that operates through a longer expected life span on investment/saving or education and through a reduction in the fertility rate that spurs a takeoff toward sustained growth.

The central policy implication of our results is that public health measures might be an important lever for fostering economic development. These types of investment could include vaccination programs, antibiotic distribution programs, and iodine supplementation schemes, which lead to large improvements in health outcomes at relatively low cost (Commission on Macroeconomics and Health, 2001; World Bank, 1993; Field, Robles, and Torero, 2009; Luca, Iversen, Lubet, Mitgang, Onarheim, Prettnner, and Bloom, 2014). Since health is an important form of human capital and as such is a productive asset, this adds a strong argument for extra investment in health over and above the direct welfare benefits that good health brings.

As far as the related literature is concerned, a common early empirical approach toward studying the effect of health on economic growth was to focus on data for a cross section of countries and to regress the growth rate of income per capita on the initial level of health (typically measured by life expectancy), with controls for the initial level of income and for other factors believed to influence steady-state income levels (see, for example, Barro, 1991, 1997). Nearly all studies that have examined economic growth in this way have found evidence of a positive, significant, and sizable influence of initial life expectancy (or some related health indicator) on the subsequent pace of economic growth (see, for example, Barro, 1991, 1997; Barro and Sala-i-Martin, 2004; Easterly and Levine, 1997; Sachs and Warner, 1995, 1997; Bhargava, Jamison, Lawrence, and Murray,

2001; Bloom, Canning, and Sevilla, 2004). Notwithstanding that these studies differed substantially in terms of country samples, time frames, control variables, functional forms, data definitions and configurations, and estimation techniques, the estimated effects of life expectancy on subsequent economic growth have been comparable across studies. While, in general, the results of these types of empirical growth equations are not very robust with respect to different specifications, Levine and Renelt (1992), Sala-i-Martin (1997a,b), and Sala-i-Martin, Doppelhofer, and Miller (2004) find that initial health is positively associated with subsequent growth in almost all of the permutations of different subsets of explanatory variables that are analyzed in these studies. This makes initial life expectancy one of the most robust predictors of subsequent economic growth.

The more recent literature has focused on analyzing the effects of health on economic growth by means of dynamic panel data regressions in the vein of Islam (1995) with the lagged dependent variable as one of the regressors to control for the convergence processes.¹ Furthermore, these studies typically employ an exogenous instrument for health to identify the causal channel running from better health to income growth (see, for example, Lorentzen, McMillan, and Wacziarg, 2008; Aghion, Howitt, and Murtin, 2011; Cervellati and Sunde, 2011; Bloom, Canning, and Fink, 2014). One of the few studies that find no evidence for a causal positive effect of health improvements on economic growth is the one by Acemoglu and Johnson (2007). They argue that an increase in life expectancy leads to a rise in population growth, which, in turn, increases capital dilution in the neoclassical growth model and therefore even *reduces* income growth during the convergence process. Using the global epidemiological revolution as an instrument for life expectancy, their empirical results are in line with the theoretical implications. Subsequently, however, Aghion, Howitt, and Murtin (2011) and Bloom, Canning, and Fink (2014) showed that this result is entirely driven by the omission of initial life expectancy from the regression. In addition, Cervellati and Sunde (2011) argue that the results of Acemoglu and Johnson (2007) can only hold for less developed countries that did not yet experience their demographic transition. In these countries, an increase in life expectancy indeed raises population growth and reduces income growth. However, for countries after the demographic transition, fertility declines with mortality and, thus, population growth does not increase when health improves. Hence, the capital dilution effect cannot be effective in these countries. Splitting the sample into pre- and post demographic transition countries, Cervellati and Sunde (2011) find that the effect of life expectancy on growth is significantly positive for post-demographic transition countries and insignificantly negative for the pre-demographic transition countries.

The remainder of this paper is structured as follows. In Section 2, we derive the

¹Since, by construction, the fixed effects in such regressions are correlated with the error term, GMM methods are usually employed for estimation (see, for example, Arellano and Bond, 1991; Blundell and Bond, 1998; Judson and Owen, 1999; Bun and Kiviet, 2003).

theoretical effect of health on per capita income by relying on a human capital augmented aggregate production function. In Section 3, we use these results to derive an empirical specification that enables us to estimate the part of income changes that are due to changes in the health stock of the population. Section 4 contains a description of the data and in Section 5, we present our main results. Finally, in Section 6, we draw our conclusions and discuss the policy implications of our results.

2 Production Function

Assume that time $t = 1, 2, \dots$ evolves discretely and consider an aggregate production function of the form

$$Y_t = A_t K_t^\alpha H_t^{1-\alpha}, \quad (1)$$

where Y_t denotes aggregate output; A_t represents total factor productivity (TFP); K_t is the physical capital stock; H_t describes the aggregate human capital stock; and α constitutes the elasticity of final output with respect to physical capital input. Following Lucas (1988), human capital can be decomposed into the number of workers, denoted by L_t , and their average human capital ν_t , such that $H_t = L_t \nu_t$. Furthermore, individual human capital follows a generalized Mincerian approach along the lines of Hall and Jones (1999), Bils and Klenow (2000) and Weil (2007). Hence, we assume that average human capital ν_t follows an exponential function

$$\nu_t = \exp(\phi_h h_t + \phi_s s_t + \phi_{a,1} a_t + \phi_{a,2} a_t^2), \quad (2)$$

where h_t denotes population health; s_t refers to mean educational attainment; a_t describes average experience; and ϕ_j for $j \in \{1, 2, 3, 4\}$ refers to the corresponding semi-elasticities.²

Combining all elements and expressing output in per worker units yields

$$y_t = A_t k_t^\alpha [\exp(\phi_h h_t + \phi_s s_t + \phi_{a,1} a_t + \phi_{a,2} a_t^2)]^{1-\alpha} \quad (3)$$

with $y_t = Y_t/L_t$ and $k_t = K_t/L_t$, respectively. Alternatively, output can also be expressed in per capita terms as

$$\tilde{y}_t = \frac{Y_t}{N_t} = \frac{L_t}{N_t} A_t k_t^\alpha [\exp(\phi_h h_t + \phi_s s_t + \phi_{a,1} a_t + \phi_{a,2} a_t^2)]^{1-\alpha}, \quad (4)$$

where N_t refers to the total population size.

On a competitive labor market, one unit of composite labor ν_t earns the wage w_t that

²Conceptually, h_t , s_t , and a_t need not represent all aspects of health, educational attainment, and experience but those that are relevant for the production of final output.

is equal to its marginal product

$$w_t = \frac{\partial y_t}{\partial \nu_t} = (1 - \alpha) \frac{y_t}{\nu_t}. \quad (5)$$

Accordingly, a worker j with ν_j units of human capital earns a wage of

$$w_{j,t} = w_t \cdot \nu_{j,t} = w_t \cdot \exp(\phi_h h_{j,t} + \phi_s s_{j,t} + \phi_{a,1} a_{j,t} + \phi_{a,2} a_{j,t}^2). \quad (6)$$

This notation normalizes the effective labor input of a hypothetical worker without any health capital, education or experience to unity. Meanwhile, workers with better health, higher education or more experience are equivalent in productivity terms to a larger number of such baseline workers. Logarithmic wages at the individual level thus take the well-known Mincerian form

$$\ln(w_{j,t}) = \ln(w_t) + \ln(\nu_{j,t}) = \ln(w_t) + \phi_h h_{j,t} + \phi_s s_{j,t} + \phi_{a,1} a_{j,t} + \phi_{a,2} a_{j,t}^2. \quad (7)$$

Hence, the aggregate production function in (1) with our measure for human capital in (2) is consistent with wage equations found at the microeconomic level. However, estimating the effect of experience in macroeconomic models is difficult because worker experience varies strongly across individuals but only little across countries (Bloom, Canning, and Sevilla, 2004). In particular, a higher average age and, thus, experience in countries with higher life expectancy tend to be offset by longer schooling and later entry into the workforce. For this reason, we estimate models with and without $a_{j,t}$ and $a_{j,t}^2$ in the empirical section to assess the robustness of the results with respect to the inclusion of experience at the macroeconomic level.

We need to take one additional aspect into consideration. The Mincerian wage form implies that the aggregate human capital stock in the economy is given by

$$H_t = \sum_j \nu_j = \sum_j \exp(\phi_h h_{j,t} + \phi_s s_{j,t} + \phi_{a,1} a_{j,t} + \phi_{a,2} a_{j,t}^2). \quad (8)$$

Accordingly, aggregating human capital requires raising individuals' educational attainment and health to the exponential power. This complication in the aggregation process vanishes if human capital and, thus, wages follow a lognormal distribution. In this case, the arithmetic mean, which represents the average wage, corresponds to the median wage,

which equals the mean of log wages plus one-half of the variance of log wages, σ_t^2 . Hence,

$$\begin{aligned}\ln(H_t) &= \ln\left(\sum_j \nu_{j,t}\right) = \frac{\left[\sum_j \ln(\nu_{j,t})\right]}{L_t} + \frac{\sigma_t^2}{2} \\ &= \sum_j \left(\phi_h h_{j,t} + \phi_s s_{j,t} + \phi_{a,1} a_{j,t} + \phi_{a,2} a_{j,t}^2\right) + \frac{\sigma_t^2}{2} \\ &= \phi_h h_t + \phi_s s_t + \phi_{a,1} a_t + \phi_{a,2} a_t^2 + \frac{\sigma_t^2}{2}.\end{aligned}\tag{9}$$

Intuitively, s_t measures average education and h_t measures average health. Additional marginal investment in education (e.g., one year of schooling) raises a worker's productivity and wages by $100 \cdot \phi_s$ percent. The absolute size of this effect is larger for highly educated, high-wage earners than for poorly educated, low-wage workers. An extra year of education for a highly educated worker also represents a greater investment, because it is more costly to produce in the sense that the worker must forgo a higher wage, while undertaking the extra schooling. Analogously, a marginally better health status (e.g., an increase in the adult survival rate by one percentage point) raises a worker's productivity and wages by $100 \cdot \phi_h$ percent.

3 Empirical Framework

Suppose the production function in (3) applies to $i = 1, \dots, \mathcal{I}$ different countries. Taking the logarithm and using the result from (9), the log of production per worker is given by

$$\ln(y_{i,t}) = \ln(A_{i,t}) + \alpha \ln(k_{i,t}) + (1 - \alpha) \left(\phi_h h_{i,t} + \phi_s s_{i,t} + \phi_{a,1} a_{i,t} + \phi_{a,2} a_{i,t}^2 + \frac{\sigma_{i,t}^2}{2} \right). \tag{10}$$

The use of rates of return to calibrate the coefficient on education, ϕ_s , suggests a parameter value of 0.09 to 0.1 based on an average rate of return taken from the cross-section of studies reported in Bils and Klenow (2000), which are based on the work of Psacharopoulos (1994) and more recent estimates by Psacharopoulos and Patrinos (2004). With respect to the elasticity of output with respect to capital, α , investigators generally agree on values of around 0.3 to 0.4 (see, e.g., Hall and Jones, 1999).

Heckman and Klenow (1997) and Krueger and Lindahl (2001) take a similar approach deriving a macroeconomic equation showing the effects of aggregate schooling based on aggregating up the Mincer wage equation. The major difference is that, in their formulation, the effect of the education level on output is simply ϕ_s , whereas in our approach the effect of schooling is $(1 - \alpha)\phi_s$. This difference arises because they take the cross-country differences and changes in the intercepts in (7) to be random and assign them to the error term in the regression. With our production function, increases in schooling increase the

aggregate level of human capital and labor equivalent inputs in the economy, and depress the wage paid per equivalent worker.

Equation (10) describes aggregate production as an identity that could be estimated directly if all right-hand side variables were available. However, in practice, the level of total factor productivity in country i at time t , $\ln(A_{i,t})$, is not observed. There are several approaches for addressing this problem. We follow Bloom, Canning, and Sevilla (2003) and model total factor productivity as a diffusion process across countries which allows for the possibility of long-run differences in TFP even after the diffusion is complete. Specifically, let

$$\Delta \ln(A_{it}) = \lambda [\ln(A_{i,t}^*) - \ln(A_{i,t-1})] + \varepsilon_{i,t}, \quad (11)$$

where $\varepsilon_{i,t}$ constitutes an idiosyncratic shock. Each country possesses a period-specific upper bound given by $\ln(A_{i,t}^*)$. A country's total factor productivity adjusts towards this bound at rate λ . We assume that this upper bound depends both on country characteristics $x_{i,t}$ and on the worldwide technology frontier μ_t . Moreover, schooling in previous periods may facilitate the diffusion and adoption of existing technologies (Nelson and Phelps, 1966) or spur novel innovation (Romer, 1990; Aghion and Howitt, 1992). Neglecting one of these channels might bias the empirical estimates as indicated by the results of Sunde and Vischer (2015). Hence, lagged schooling, $s_{i,t-1}$, constitutes another determinant of potential TFP (see also Cuaresma, Lutz, and Sanderson, 2014). Since technological gaps are not directly observed, we follow Baumol (1986) and use lagged income per worker as a proxy (see also Fagerberg, 1994; Dowrick and Rogers, 2002). Hence, growth of total factor productivity reads

$$\Delta \ln(A_{it}) = \lambda [\mu_t + x'_{i,t}\Theta + \rho s_{i,t-1} - \ln(y_{i,t-1})] + \varepsilon_{i,t}. \quad (12)$$

Alternatively, a richer model derives lagged total factor productivity $\ln(A_{i,t-1})$ directly from the production function so that

$$\begin{aligned} \Delta \ln(A_{it}) = & \lambda [\mu_t + x'_{i,t}\Theta + \rho s_{i,t-1} - \ln(y_{i,t-1}) + \alpha \ln(k_{i,t-1})] \\ & + \lambda(1 - \alpha) \left(\phi_h h_{i,t-1} + \phi_s s_{i,t-1} + \phi_{a,1} a_{i,t-1} + \phi_{a,2} a_{i,t-1}^2 + \frac{\sigma_{i,t-1}^2}{2} \right) + \varepsilon_{i,t}. \end{aligned} \quad (13)$$

This slightly more comprehensive modeling approach, however, suffers from the disadvantage that the inclusion of additional highly correlated explanatory variables drives up standard error estimates without providing additional insights regarding the parameters of interest. We provide estimates for both models and show that they are qualitatively and quantitatively similar.

Related research has suggested several variables, $x_{i,t}$, that may affect long-run total

factor productivity. For example, Hall and Jones (1999) argue that institutions and “social infrastructure” affect productivity, while Gallup, Sachs, and Mellinger (1999) emphasize the role of geography. Our empirical work experiments with a range of potential variables.

First-differencing (10) and inserting (12) provides the empirical estimation equation

$$\begin{aligned} \Delta \ln(y_t) = & \alpha \Delta \ln(k_t) + (1 - \alpha) \left(\phi_h \Delta h_t + \phi_s \Delta s_t + \phi_{a,1} \Delta a_t + \phi_{a,2} \Delta a_t^2 + \frac{\Delta \sigma_t^2}{2} \right) \\ & + \lambda [\mu_t + x'_{i,t} \Theta + \rho s_{i,t-1} - \ln(y_{i,t-1})] + \varepsilon_{i,t}. \end{aligned} \quad (14)$$

De la Fuente and Domenech (2001) and Bloom, Canning, and Sevilla (2003) use this approach to model TFP diffusion in cross-country production function studies. It is formally equivalent to the autoregressive model of TFP that Griliches and Mairesse (1998) and Blundell and Bond (2000) use in their studies of the production function using firm-level data.

According to the specification in (14), growth in output can be decomposed into three components. The first is growth of the input factors capital, health, schooling, and experience. The second is a catch-up term capturing that some part of the gap to technological leaders is closed at every time period so that the country converges at the rate λ to its upper bound of TFP. The third component is an idiosyncratic shock to the country’s total factor productivity, $\varepsilon_{i,t}$.³

Equation (14) denotes a model of conditional convergence, where the speed of convergence, λ , describes the rate at which gaps in total factor productivity are closing. Therefore, this model stands in sharp contrast to models such as those of Mankiw, Romer, and Weil (1992) and Islam (1995), which take TFP differentials across countries to be fixed. The speed of convergence in these models depends on the time capital stocks take to reach their steady-state levels given fixed investment rates. By including the growth rates of factor inputs directly in equation (14), we can identify the catch-up term – the effect of the gap between actual and steady-state output given current input levels – as the impact of a TFP gap.

In the special case of no technological diffusion ($\lambda = 0$), the lagged level terms in (14) disappear. Hence, our approach encompasses the estimation of a production function in first differences as advocated by Krueger and Lindahl (2001) and Pritchett (2001). Moreover, we can test if this restriction holds. Taking first differences nets out any fixed effects on TFP. Therefore, a test of $\lambda = 0$ tests the null of a fixed effects model, with persistent differentials in TFP, against the alternative that TFP differentials narrow over time because of technological diffusion. Furthermore, our model also encompasses the special case in which there is technological diffusion but the steady-state level of TFP is

³We could allow the shock to grow during each period to have a common component across countries, e.g., worldwide oil or interest rate shocks. However, such a shock would be collinear to changes in the worldwide productivity frontier captured by the time effects and would thus not affect any of our results.

the same in every country. We can test this by examining whether the country-specific variables $x_{i,t}$ have zero coefficients.

In estimating equation (14), we face the possibility that contemporaneous growth rates of factor inputs are endogenous and responsive to the current TFP shock $\varepsilon_{i,t}$. For health and education inputs which are the object of interest, we overcome this problem by exploiting the demographic structure in following an instrumental variables approach. Concretely, changes in the health status and the educational attainment of the working-age population in our model are determined by in-flows from young-age cohorts at the lower end and out-flows from old-age cohorts at the upper end of the working-age population. Hence, one can use lagged levels for the respective age cohort that is to enter or leave the working-age population in the next period as instrument for the contemporaneous growth rate of the factor input. This instrument is plausibly exogenous given our approximation of TFP growth rates which controls for productivity gains that are due to past changes in input factors, past technology shocks, and convergence to the technological frontier. This approach is compatible with lagged TFP levels and expected TFP growth – the catch-up term in equation (14) – affecting previous input decisions (for example, Bils and Klenow, 2000, suggest that schooling decisions depend on expected economic growth). The argument that lagged input levels are uncorrelated with future shocks to TFP is the rationale for estimating equation (14) rather than the level relationship in equation (10). For this argument to be valid, shocks to TFP (the error term in our regressions) must not be predictable.

Finally, by including fixed effects in a comprehensive specification, we also allow for the possibility of country-specific growth trends driven by unobserved heterogeneity that exhibits a persistent effect on TFP. However, we refrain from using dynamic panel GMM methods which produced estimates with large standard errors in which no variables were statistically significant. In addition, we take the view that over the five-year intervals all the inputs are potentially correlated with contemporaneous productivity shocks. Therefore, all our regressors must be instrumented by lagged values, as opposed to firm-level studies, in which current inputs are treated as exogenous. Both these factors imply a loss of precision in the estimates and make inferences based on a fixed effects approach difficult.

4 Data

We construct an unbalanced panel of 116 countries observed every five years from 1960 to 2010. Data on real output and physical capital, both in per worker units, are obtained from the Penn World Tables by Feenstra, Inklaar, and Timmer (2015).

Health inputs are proxied by the adult survival rate derived from United Nations (2017). This variable measures the probability of surviving from age 15 to 60 in percent. Conceptually, this measure may be more closely related to adult health and worker

productivity than life expectancy, a measure that is highly sensitive to infant mortality rates. However, adult survival rates act only as a proxy for the health of the workforce, because they measure mortality rates rather than morbidity. Our main reason for using adult survival rates is that it allows us to compare our results directly with those of Shastri and Weil (2003) and Weil (2007).

Following the Mincerian approach, educational input is proxied by years of schooling in the working-age population. To this end, we exploit measures on average years of primary, secondary, tertiary, and total schooling from Barro and Lee (2013) for the population above age 15. We combine age-specific years of schooling with population shares to construct average years of schooling for the working-age population which we define from age 15 to 60 to match our health measure.

We construct aggregate experience as the median age of the population obtained from United Nations (2017) net of an intercept of six years which corresponds to early childhood. Moreover, we deduct compulsory schooling years, collected from statistical yearbooks of the UNESCO (1963–1997) and UNESCO (2017) to account for differences in the age of workforce entry across countries. This correction is necessary because countries with a higher life expectancy and, thus, with an older population tend to have later workforce entry due to longer schooling. Since experience enters the regression framework in differences, this measure takes up variation from changes in median age and compulsory schooling following educational reforms.⁴

In order to control for the effect of wage inequality, we use the disposable income Gini coefficient after taxation and transfers by Solt (2016). These data provide standardized Gini coefficients that are comparable across countries and over time.

Finally, we include some country-specific variables that may affect the long-run level of TFP. These contain an indicator for the quality of economic institutions by Gwartney, Lawson, and Hall (2017); a measure for the value added of the agricultural sector by the World Bank (2017) to control for structural change; the percentage of land area in the tropics by Gallup, Sachs, and Mellinger (1999) to control for geographical factors that may affect productivity and trading opportunities; and a set of regional dummies.⁵

⁴For certain countries, the statistical yearbooks report values for specific regions. Moreover, the educational systems of some countries allow for different categorizations so that alternative figures are conceivable. We correct for these fluctuations and code flatter, i.e., less varying, values in the case of doubt. This procedure tends to render the measure for experience less informative and, thus, increases the corresponding standard errors. Table A1 in the Appendix contains a complete list of coding decisions.

⁵We also experimented with further indicators for landlocked countries (Gallup, Sachs, and Mellinger, 1999) as well as for ethnic fractionalization and polarization by Alesina, Devleeschauwer, Easterly, Kurlat, and Wacziarg (2003) and Reynal-Querol and Montalvo (2005). However, given the set of other controls, these variables did not explain much of the remaining variation.

5 Results

5.1 Baseline Results

Table 1 presents the baseline estimation results with average years of secondary schooling as the proxy for education and controls for worker experience.⁶ Column (1) contains the coefficient estimates for the empirical specification in Equation (14) without controls and without accounting for income inequality. All of the coefficients have the expected sign. Lagged per capita GDP is negative and significantly different from zero at the one percent level. This implies conditional convergence as predicted by the neoclassical growth literature (Solow, 1956; Cass, 1965; Diamond, 1965) and as it is established empirically.⁷ Changes in the capital stock are positively related to economic growth, which is again in line with the economic growth literature. As far as the variables of interest are concerned, the coefficient estimates of changes in health and of changes in education are positive and significant at the one percent level. This suggests that, indeed, health and education are both important dimensions of human capital. The quadratic relationship between growth of per capita income and changes in experience as suggested by a Mincerian specification is indicated by the positive sign of the coefficient for experience together with the negative sign on the coefficient for experience squared. However, only experience squared is (marginally) significant in this specification.

In Column (2), we add the control variables with respect to the quality of economic institutions, the value added of the agricultural sector, the percentage of land area in the tropics, and the set of regional dummies. While adding the control variables raises the explanatory power of the model by means of an increased R^2 , the qualitative results of the regression do not change. From a quantitative point of view, the coefficient estimates for changes in the capital stock, changes in health, and changes in education become lower. In Column (3), we include country-fixed effects instead of the regional dummies. Again, this leaves the qualitative effects intact. While the magnitude of the coefficient estimates for health and education rises, the coefficient for the change of physical capital decreases. In Column (4), we add the Gini coefficient to the specification in Column (2) to control for inequality which enters in a negative but insignificant way. Column (5) presents the results for the model with lagged controls which derives lagged total factor productivity directly from the production function according to Equation (13). In both of these modifications, the qualitative results remain the same as in all the other specifications.

Table 2 compares the results of the specification in Column (2) to the literature. According to Weil (2007) an increase in adult survival rates of 0.1 – or ten percentage

⁶Table A2 in the Appendix reports the estimates for a specification without worker experience.

⁷See, for example, Barro (1991, 1997), Sala-i-Martin (1997b), and Sala-i-Martin, Doppelhofer, and Miller (2004) in cross-section regressions and Islam (1995), Caselli, Esquivel, and Lefort (1996), and Brückner (2013) in panel data settings. For interesting surveys and critical remarks on the literature, see Durlauf, Johnson, and Temple (2005) and Eberhardt and Teal (2011).

Table 1: Return of Health and Education on Productivity (Secondary Schooling)

	No Controls	Adding Controls	Fixed Effects	Adding Gini	Lagged Controls	IV $\Delta(h_{i,t})$	IV $\Delta(s_{i,t})$	IV $\Delta(h_{i,t}), \Delta(s_{i,t})$
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
$\ln(y_{i,t-1})$	-0.045*** (0.0097)	-0.15*** (0.017)	-0.34*** (0.052)	-0.18*** (0.023)	-0.20*** (0.022)	-0.14*** (0.020)	-0.14*** (0.017)	-0.14*** (0.020)
$\Delta \ln(k_{i,t})$	0.52*** (0.054)	0.35*** (0.063)	0.29*** (0.10)	0.23*** (0.067)	0.48*** (0.075)	0.34*** (0.061)	0.34*** (0.064)	0.35*** (0.064)
$\Delta(h_{i,t})$	1.12*** (0.30)	0.59** (0.29)	0.64** (0.27)	0.74** (0.35)	0.68** (0.28)	0.91 (0.94)	0.61** (0.28)	0.80 (0.90)
$\Delta(s_{i,t})$	0.099*** (0.032)	0.075** (0.031)	0.091** (0.046)	0.063** (0.027)	0.063** (0.029)	0.078*** (0.030)	0.049 (0.096)	0.046 (0.093)
$\Delta(a_{i,t})$	0.011 (0.0085)	0.0075 (0.0088)	-0.0100 (0.0095)	0.0089 (0.010)	0.0090 (0.0085)	0.0070 (0.0084)	0.0078 (0.0085)	0.0073 (0.0084)
$\Delta(a_{i,t}^2)$	-0.00069* (0.00036)	-0.00057 (0.00036)	0.00024 (0.00036)	-0.00058 (0.00039)	-0.00064* (0.00033)	-0.00056* (0.00034)	-0.00057 (0.00035)	-0.00055 (0.00034)
$\Delta(\sigma_{i,t}^2)$				-0.60 (0.45)				
R^2	0.29	0.38	0.38	—	0.41	0.38	0.38	0.37
First stage F						26.7	30.2	17.8
Countries	116	116	116	109	116	116	116	116
Observations	613	613	613	461	613	613	613	613
Controls		✓	✓	✓	✓	✓	✓	✓

Notes: All specifications include time effects. Asterisks indicate significance levels: * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

points – raises labor productivity by 6.7 percent. In comparison, our estimates indicate that an increase in adult survival rates of 0.1 translates into a 9.1 percent increase in labor productivity. The 95 percent confidence interval of our estimate ranges from 0.47 to 17.7 percent and hence includes the estimate derived by Weil (2007). Consequently, our macro results are consistent with the micro results of Weil (2007), which reconciles the micro-based and the macro-based approach of estimating the effect of health on income growth. Note, in this context, that the estimates in Table 1 have to be divided by $(1 - \alpha)$ to reflect the coefficient estimate for ϕ_h in Equation (14).

Our coefficient estimate on changes in physical capital (α) attains a value of 0.35. This is in line with empirical estimates on the elasticity of output with respect to physical capital of around 0.3 to 0.4 (Hall and Jones, 1999). Moreover, dividing our estimate on education by $(1 - \alpha)$ yields a return to secondary schooling (ϕ_s) of 11.4%. This effect is consistent with the estimates obtained by Psacharopoulos and Patrinos (2004) who report the return on secondary schooling to be 13.1%. Finally, the signs of our coefficient estimates on the lagged dependent variable (γ) and our experience measures ($\phi_{a,1}$ and $\phi_{a,2}$) are in accordance with previous findings on conditional convergence and a positive but diminishing return to experience, respectively.

Table 2: Comparison between our Estimates and the Literature

Variable	Point Estimate	Confidence Interval	Target
γ	< 0		< 0
α	0.35	(0.23-0.47)	0.3-0.4
ϕ_h	9.1%	(0.47%-17.72%)	6.7%
ϕ_s	11.4%	(2.6%-20.26%)	13.1%
$\phi_{a,1}$	> 0		> 0
$\phi_{a,2}$	< 0		< 0

5.2 Instrumental Variables Approach

In order to address concerns with respect to endogeneity of health and education in our empirical model, we present instrumental variables regressions for the two human capital variables health and education in Columns (6) to (8) of Table 1. To this end, we exploit changes in the demographic structure of the working-age population. Specifically, changes in health and education are explained by in- and outflows of young- and old-age cohorts at the lower and upper end of the working-age population. Hence, we can use lagged levels for the youngest and oldest age cohorts to predict contemporaneous changes in aggregate health and education of the working-age population. Given the strong persistence in demographic patterns, these in- and outflows are plausibly exogenous in an empirical specification that controls for past levels of per capita output, fixed and time effects. Moreover, migration is less of a concern for these age groups in contrast to prime-age workers. In Column (6), we apply this identification strategy to instrument changes in health. In particular, contemporaneous changes in health are instrumented by the health status of the cohort aged 10–14 in the last period, measured in terms of infant mortality at time of birth. In Column (7), contemporaneous changes in education are instrumented by average years of secondary schooling of the cohort aged 55–59 years in the last period. Finally, Column (8) reports results for a specification where both health and education are being instrumented. While the sign of the coefficients stays stable as compared to the other regressions, the instrumented variables become insignificant. However, since the size of the estimated coefficients does not change considerably, this seems to be of less concern. The first-stage regression does not indicate the problem of weak instruments because the F -statistic is well above 10.

5.3 Robustness Checks

As a robustness check, Table 3 contains the estimation results when using average years of schooling (instead of average years of secondary schooling) as the proxy for education. The different columns contain the counterparts of the estimation results presented in Table 1. We observe again in all specifications the expected signs of the coefficients except in

Table 3: Return of Health and Education on Productivity (Total Schooling)

	No Controls	Adding Controls	Fixed Effects	Adding Gini	Lagged Controls	IV $\Delta(h_{i,t})$	IV $\Delta(s_{i,t})$	IV $\Delta(h_{i,t}), \Delta(s_{i,t})$
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
$\ln(y_{i,t-1})$	-0.051*** (0.0095)	-0.15*** (0.017)	-0.33*** (0.052)	-0.18*** (0.024)	-0.20*** (0.022)	-0.14*** (0.020)	-0.14*** (0.016)	-0.14*** (0.020)
$\Delta \ln(k_{i,t})$	0.51*** (0.052)	0.35*** (0.063)	0.27** (0.11)	0.23*** (0.066)	0.48*** (0.075)	0.35*** (0.063)	0.35*** (0.068)	0.35*** (0.068)
$\Delta(h_{i,t})$	1.22*** (0.30)	0.63** (0.30)	0.66** (0.27)	0.76** (0.35)	0.72** (0.28)	0.97 (1.13)	0.60** (0.30)	0.56 (1.18)
$\Delta(s_{i,t})$	0.057** (0.024)	0.038 (0.023)	0.035 (0.029)	0.020 (0.022)	0.035 (0.023)	0.043* (0.023)	-0.019 (0.087)	-0.019 (0.088)
$\Delta(a_{i,t})$	0.0063 (0.0089)	0.0061 (0.0088)	-0.010 (0.0098)	0.0082 (0.010)	0.0077 (0.0086)	0.0051 (0.0087)	0.0083 (0.0082)	0.0084 (0.0085)
$\Delta(a_{i,t}^2)$	-0.00052 (0.00036)	-0.00051 (0.00036)	0.00028 (0.00037)	-0.00054 (0.00039)	-0.00057* (0.00032)	-0.00048 (0.00034)	-0.00055* (0.00033)	-0.00056* (0.00033)
$\Delta(\sigma_{i,t}^2)$				-0.63 (0.45)				
R^2	0.29	0.38	0.38	—	0.41	0.37	0.36	0.36
First stage F						19.6	25.3	10.0
Countries	116	116	116	109	116	116	116	116
Observations	613	613	613	461	613	613	613	613
Controls		✓	✓	✓	✓	✓	✓	✓

Notes: All specifications include time effects. Asterisks indicate significance levels: * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

Column (3), where the sign of experience becomes negative and the sign of experience squared becomes positive, though the coefficients are not significantly different from zero.

In case of using average years of schooling, changes in education loses its significance. The reason for this might be that primary education does not change much in most countries such that the variation in this variable is less informative than in average years of secondary education. Since we are primarily interested in the contribution of the changes of health on income, this is, however, no concern for the purpose of our paper.

6 Discussion

A great deal of the literature on economic growth has been devoted to studying the impact of education on aggregate economic performance and comparing the results with the rate of return to education identified by the Mincer (1974) wage equation. We believe that ours is the first study to compare the estimates of the macroeconomic effect of health on output with the microeconomic estimates of the effect of health on wages that are nowadays available.

We estimate that an increase in adult survival rates of 0.1 (or by 10 percent) increases

labor productivity by about 9.1 percent, with the 95 percent confidence interval ranging from 0.47 to 17.7 percent. Our result is therefore somewhat higher than, but consistent with, the calibrated value of around 6.7 percent. This supports the conclusion of Weil (2007), based on calibration, that health plays a large role in explaining cross-country differences in the level of income per worker. So long as macroeconomic estimates do not reject the hypothesis that the productivity effects calibrated on the basis of wage regressions are correct, we have no evidence of substantial externalities, allowing us to use calibration based on microeconomic data as a reasonable guide to the magnitude of the macroeconomic effects.

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Appendix

Table A1: Coding Description

This table describes coding choices countries in which compulsory schooling laws differ by schooling type or target group, and countries which experienced longer spells of turbulence and civil war. This list contains all countries for which information on compulsory schooling was available and, thus, even those that do not enter the estimation sample.

Albania: UNESCO yearbooks report four plus an additional three years of compulsory schooling for 1963 and 1964. From 1965 to 1967, four plus an additional other four years of compulsory schooling are being reported. We code these as seven and eight years of schooling since “[f]our years’ schooling is compulsory for all children; a second period of three (four) years is compulsory for children in towns and villages where a seven-grade (eight-grade) school is available” (UNESCO, 1963-1968).

Andorra: The educational system of Andorra is split into French and Spanish schools. However, since both schooling system differ in terms of compulsory schooling we follow the convention of the UNESCO and code values as missing until 1977. Afterwards, both schools require a minimum of ten years of schooling so that we code a value of ten.

Angola: “The school system in the Portuguese Overseas Provinces forms part of the general pattern of Portuguese education. It is consequently the same as in metropolitan territory, but not all the levels and types of education provided in Portugal are to be found overseas” (UNESCO, 1964-1967). Therefore, we assume that for the years 1964 to 1967, compulsory schooling amounts to four years as it is the case for Portugal.

Argentina: In 1972, compulsory schooling takes a value of eight years while before and afterwards compulsory schooling is consistently reported with seven years. Since the structure of the educational system did not change in 1972, we code a value of seven years.

Australia: For 1963 and 1964, UNESCO yearbooks report eight to ten years of compulsory schooling, varying by state. We take the figure of New South Wales, the most densely populated state, and thus code nine years. From 1968 onward, the yearbooks report values between nine and eleven years, varying by state or whether kindergarten counts towards primary education. We code ten years of compulsory schooling. This figure is consistent with more recent data published by the World Bank. Moreover, the number reflects average compulsory schooling.

Bahrain: For 1971 and 1972, eight years of compulsory schooling are being reported. However, these change “will be applied in 1973/1974” (UNESCO, 1971). Following the value of the preceding years, we code a value of zero. For the period 1987 to 1993, zero compulsory schooling is reported. This figure contrasts with the high values before and afterwards. We thus code a missing rather than a zero value. Based on the age range, twelve years of compulsory are reported for the period 1995 to 1997. However, the compulsory program only contains six years of primary schooling with a general academic curriculum combined with religious instruction which continues to nine years. Correspondingly, we code nine instead of twelve years for the period 1995 to 1997.

Barbados: There is no compulsory schooling from 1963 to 1967, however, the value for 1966 is missing. We impute this value to be zero. For the years 1995 to 1997, UNESCO yearbooks report twelve years of compulsory schooling instead of eleven in preceding and subsequent periods.

Belgium: In 1985 and 1986, UNESCO yearbooks report eight and years of compulsory schooling. Based on the preceding years and the age range, nine years of compulsory schooling are implausible, however. Therefore, we code eight years in 1985 and 1986.

Benin: After independence in 1960, there was a longer spell of political turbulence. In particular, there were several changes in power at the beginning of the 1970s. According to the UNESCO yearbooks, compulsory schooling amounts to six years until 1970, zero years from 1971 to 1974, and seven years from 1975 onward. Due to the unstable nature of government, the exact role of compulsory schooling and whether it was enforced is unclear. Therefore, we decide to code the years 1971 to 1974 as missing rather than a clean zero.

Brazil: For 1963 and 1964, UNESCO yearbooks report compulsory schooling values of four and five years. From 1965 onward, the level remains consistently at four years. Since Brazil follows the Portuguese educational system, we code a value of four for 1963 and 1964.

Brunei: For the years 1995 to 1997, UNESCO yearbooks report compulsory schooling levels of twelve years. These stand in contrast to nine years of compulsory schooling before and afterwards. Since neither the educational system nor the age range of compulsory schooling changed during this period, we code nine instead of twelve years.

Cameroon: Historically, the educational system consisted of French schools in the Eastern and British schools in the Western part of Cameroon. In 1976, the British system was adopted in the entire country. We use compulsory schooling regulations of the British system throughout all periods. UNESCO yearbooks list eight years of compulsory schooling in 1969 and 1970. Given the subsequent period without any compulsory schooling, it is unlikely that this regulation has been enforced. We thus code a zero value for both, 1969 and 1970.

Canada: Compulsory schooling “[...] figures vary slightly from one Province to another” (UNESCO, 1963). Values range from seven to ten years between 1963 and 1968, and eight to ten years between 1969 and 1994. We take a slightly conservative view and code a value of eight years for 1963-1968 and nine years for 1969-1994.

Cape Verde: “The school system in the Portuguese Overseas Provinces forms part of the general pattern of Portuguese education. It is consequently the same as in metropolitan territory, but not all the levels and types of education provided in Portugal are to be found overseas” (UNESCO, 1964-1967). Therefore, we assume that for the years 1964 to 1967, compulsory schooling amounts to four years as it is the case for Portugal.

Czech Republic and Slovakia: We use compulsory schooling regulations of former Czechoslovakia for both countries prior to 1994.

Egypt: For 1989 to 1991, UNESCO yearbooks report nine years of compulsory education. However, these figures are implausible given five years of primary and three years of lower secondary education. Therefore, we code eight rather nine years. For 1995 and 1996, the yearbooks report five years of compulsory schooling. This figure does not reflect lower secondary education which is also compulsory since the educational reforms in the early 1990s. Hence, we set the corresponding value to eight years instead of five.

Fiji: Between 1975 and 1997, UNESCO yearbooks report zero years of compulsory schooling in contrast to eight years from 1963 to 1974. We follow the convention of the World Bank which codes missing values of compulsory schooling between 1998 and 2015. Thus, we code missing values for the years 1975 to 1997.

Finland: For 1967, 1971 and 1972, UNESCO yearbooks report eight instead of formerly nine years of compulsory schooling. Based on the age range and structure of the educational system, these shifts seem implausible. Thus, we code nine year of compulsory schooling.

Germany: Figures are based on West Germany prior to 1990. We code twelve rather than nine years of compulsory schooling in 1968-1970 and 1973-1988. This coding includes nine years of compulsory schooling plus an additional three years of “[...] part time vocational education” (UNESCO, 1973).

Guinea: In 1971/1972, compulsory schooling increases from eight to twelve years before it dropped again back to eight years in 1973. Throughout this period, the overall structure of the educational system remained unaltered. The only detectable change was the range of compulsory schooling from 7-15 to 7-22 which is implausible in comparison to other countries and also Guinea’s legal age. Therefore, compulsory schooling is coded to remain at eight years instead of twelve.

Table A1: ...Continued

Guinea-Bissau: For 1981 and 1982, UNESCO yearbooks report seven years of compulsory schooling in contrast to six years in preceding and subsequent periods. Since the educational system remained unaltered during these years, this change seems implausible. Hence, we code six rather than seven years.

Guyana: Throughout the period 1963 to 1997, compulsory schooling takes a value of eight years with the exception of 1981 and 1982 (nine years), 1983 (six years) and 1995 to 1997 (ten years). However, the shifts are inconsistent with the relative stability of the educational system between 1980 and 1984, as well as the age range of compulsory schooling from 6 to 14 for the period 1995 to 1997. We thus code eight years over the entire period.

India: In 1971 and 1972, the UNESCO yearbooks report various levels of compulsory schooling. In the years thereafter, only a uniform level of five years is reported. This change is justified by the fact that “[t]his information pertains to the majority of states” (UNESCO, 1975). Therefore, we code also a value of five years for 1971 and 1972.

Indonesia: In 1973 and 1974, UNESCO yearbooks report zero values for compulsory schooling. These figures stand in contrast to six years of compulsory schooling before and thereafter. Moreover, the educational system remained unaltered during this period. Hence, we code a value of six instead of zero years.

Iran: For the years 1966, 1967, 1973 and 1974, UNESCO yearbooks report five years of compulsory schooling in contrast to six years in preceding and intermediate periods. However, these figures seem implausible since the educational remained unaltered during this period. Therefore, we code six rather than five years.

Iraq: UNESCO yearbooks consistently report six years of compulsory schooling. In 1983, however, five years are reported although the educational structure did not change. We code six instead of five years. Moreover, compulsory schooling is missing in 1973 and 1974. Since the educational system remained unaltered, we set the value to six years; the same as in the preceding and following years.

Israel: Between 1981 and 1987, UNESCO yearbooks report nine years of compulsory schooling in contrast to eleven years in preceding and subsequent periods. Moreover, this figure seems implausible given the age range from five to 15. Hence, we code eleven instead of nine years for this period.

Jordan: According to the UNESCO yearbooks, compulsory schooling increased from six to nine years in 1964 based on a widening of the age range. However, this increase is not observed in 1965 where the age range is again six years. Therefore, we code six instead of nine years.

Kiribati and Tuvalu: Until 1976, the islands were a British protectorate under the name Gilbert and Ellice Islands. We thus use compulsory schooling of the former protectorate for both Kiribati and Tuvalu. For the years 1975 to 1980 during which the islands became independent, we code missing instead of the reported zero values. For the years 1985 and 1986, UNESCO yearbooks report five years of schooling in contrast to nine years in the preceding and subsequent periods. Since the educational system remained unaltered during this time, we code nine instead of five years.

Kuwait: For 1982 and 1983, UNESCO yearbooks report four in contrast to eight years in preceding and subsequent periods. Since the educational system remained unaltered during this period, we code eight instead of four years.

Laos: For the years 1990 to 1994, UNESCO yearbooks report eight in contrast to five years of compulsory schooling in preceding and subsequent periods. Since the educational system with five years of compulsory primary schooling remained unaltered during this period, we code five instead of eight years.

Lebanon: Throughout the period 1963 to 1997, compulsory schooling is consistently zero years, except for 1971 where UNESCO yearbooks report a value of twelve. Given the overall trend, this value seems implausible so that we code zero years.

Lesotho: The UNESCO yearbooks report compulsory schooling of eight years for the former British Crown colony Basutoland in 1964 and 1965. However, there was no compulsory schooling for the independent state of Lesotho between 1966 and 1984. Moreover, the yearbooks also report a value of zero for the colony in 1963. We thus set the value for compulsory schooling to zero for 1964 and 1965.

Malawi: For 1963 to 1965, UNESCO yearbooks report eight years of compulsory schooling based on the English schools in the former British colony. From 1966 onward, zero years of schooling are reported. Since Malawi became independent in 1964, eight years of compulsory schooling seem implausible. Hence, we code zero rather eight years.

Malaysia: From 1968 to 1984, UNESCO yearbooks report six years of compulsory schooling for some and zero or missing values for other regions. Since there is no compulsory schooling in the most populous regions, we code zero years from 1968 to 1984.

Malta: In 1986 and 1987, UNESCO yearbooks report twelve years of compulsory schooling. Based on the stable educational system, the age range and subsequent values, these figure seem implausible. We code ten instead of twelve years.

Mauritius: For the years 1981 to 1983, UNESCO yearbooks report eight years of compulsory schooling in contrast to seven years in preceding and subsequent years. Since the educational system remained unaltered during this period, we code seven rather than eight years. Between 1987 and 1994, figures for compulsory schooling drop to a level of zero. However, these values seem implausible since the educational system did not change in this period either. We code missing instead of zero values.

Monaco: For 1973 and 1974, UNESCO yearbooks report eleven years of compulsory schooling in contrast to ten years before and afterwards. Since the educational system remained unaltered during this period, we code ten rather than eleven years.

Mozambique: “The school system in the Portuguese Overseas Provinces forms part of the general pattern of Portuguese education. It is consequently the same as in metropolitan territory, but not all the levels and types of education provided in Portugal are to be found overseas” (UNESCO, 1964-1967). Therefore, we assume that for the years 1964 to 1967, compulsory schooling amounts to four years as it is the case for Portugal.

Nauru: For the years 1963 to 1970, UNESCO yearbooks report nine years of compulsory schooling for European and ten years for Nauruan schools. We code a value of ten years.

Nepal: Historically, the Nepalese educational system consisted of English and Sanskrit schools. Until 1967, there was no compulsory schooling for either of these schools. Beginning in 1968, the English school system prescribed five years of schooling while attendance at Sanskrit schools was not compulsory. Following the UNESCO’s convention to document compulsory schooling based on the English system from 1973 onward (UNESCO, 1973), we code five years of compulsory schooling.

New Zealand: For the years 1994 to 1997, UNESCO yearbooks report eleven years of compulsory schooling. However, the educational system consists of six years of primary and four years of lower secondary schooling. For this reason, we code ten rather than eleven years. This coding choice is consistent with preceding and subsequent periods and the stability of the educational system overall.

Niger: For the period 1973 to 1979, the UNESCO yearbooks report compulsory schooling of 12/13 years in contrast to eight years in the preceding and subsequent periods. This substantial change is not reflected in a corresponding transformation of the educational system and only represents shifts in the age range for compulsory schooling. Therefore, this extreme increase seems implausible so that we code compulsory schooling to remain at the level of eight years throughout 1973 to 1979.

Norway: From 1968 to 1970, values of seven and nine years are being reported since “[a] law passed in 1968 extended compulsory education from seven to nine years. This has been applied in most municipalities” (UNESCO, 1968).

Table A1: ...Continued

Philippines: In 1963 and 1964, a missing value of compulsory schooling is reported. However, we decide to code a zero value since “[i]n implementation of Republic Act No. 1124, Department Order No. 1, s.1957, Article 2 states that elementary education shall ultimately be made available for all children between 7 and 13 years” (UNESCO, 1963). Hence, compulsory schooling was not yet implemented in 1963 and 1964.

Poland: Between 1963 and 1970, UNESCO yearbooks report various values of compulsory schooling. We take a conservative view and code 1963 and 1964 with a value of seven years and 1965 to 1970 with a value of eight years.

Republic of Congo: For the period 1973 and 1974, compulsory schooling dropped from an initial value of ten to six years. From 1975 onward, compulsory schooling reverted back to a value of ten years. Throughout this entire time, compulsory schooling age ranges from 6 to 16 years for boys and 6 to 17 years for girls. Therefore, we also code a value of ten years for 1973 and 1974.

Romania: For 1963 and 1964, UNESCO yearbooks report seven or eight years of compulsory schooling. In subsequent years, educational regulations prescribe eight years of compulsory schooling. Based on this stability in the educational system, we set values to eight years for 1963 and 1964.

Saint Lucia: For 1985 and 1986, UNESCO yearbooks report eleven years of compulsory education in contrast to ten years in preceding and subsequent periods. Since the structure of the educational system with seven years of primary and three years of lower secondary schooling did not change during these years, this shifts seems implausible. Hence, we code ten rather than eleven years.

Sao Tome and Principe: “The school system in the Portuguese Overseas Provinces forms part of the general pattern of Portuguese education. It is consequently the same as in metropolitan territory, but not all the levels and types of education provided in Portugal are to be found overseas” (UNESCO, 1964-1967). Therefore, we assume that for the years 1964 to 1967, compulsory schooling amounts to four years as it is the case for Portugal.

Senegal: UNESCO yearbooks report seven years of compulsory education for 1971 and 1972 and six years for 1973 and 1974. However, compulsory primary education corresponded only to six and five years. Therefore, we code six and five years rather than seven and six.

Singapore: Compulsory schooling has only been introduced in 2003. Hence, we code one missing value as zero before 2003.

South Africa: Between 1963 and 1984, UNESCO yearbooks report seven and nine years of compulsory schooling, varying by state and race. We code seven years of schooling as the corresponding figure for the black population which constitutes approximately 80 percent of the total population.

Sri Lanka: From 1995 to 1997, UNESCO yearbooks report eleven years of compulsory schooling in contrast to ten years beforehand. Based on the age limits which remained unaltered over this period, we code ten instead of eleven years.

St. Vincent and The Grenadines: UNESCO yearbooks report ten years of compulsory schooling for 1968-1974 as well as 1978-1985, and zero years for the periods 1963-1967, 1975-1977 and 1986-1995. Between 1996 and 2004, no values are being reported. The overall structure of the educational system did not change substantially throughout all these periods so that large shifts in compulsory schooling appear implausible. We thus code values for 1963-1967, 1975-1977 and 1986-1995 to be missing rather than zero.

Suriname: UNESCO yearbooks report eleven years of compulsory schooling for the period 1995 to 1997. This figure stands in stark contrast to only six years before and afterwards. Since the educational system with six years of compulsory primary schooling remained unaltered during these years, we code six instead of eleven years.

Swaziland: In the early years until 1965, the educational system consisted of European, African and Eurafrican schools. Since education was compulsory only at European schools, which were abolished from 1966 onward, and not for the other school types, we code a value of zero.

Switzerland: According to the UNESCO yearbooks, compulsory schooling varies between seven and nine years across Swiss cantons from 1963 to 1997. In some cantons, students are additionally required to take up at least two years of “complementary part-time schooling” (UNESCO, 1963). Hence, the reported figures are likely too low. Thus, we follow the convention of UNESCO reports from 1975 to 1981 and code nine years of compulsory schooling throughout the entire period.

Thailand: In 1963 and 1964, UNESCO yearbooks report between four and seven years of compulsory schooling. Based on the age range and subsequent values, we code both observations as seven.

Tonga: For the years 1995 to 1997, UNESCO yearbooks report eight years of compulsory schooling. However, this figure seems implausible in comparison to six years in preceding and subsequent periods. Moreover, the educational system remained unaltered during these years. Hence, we code six rather than eight years. For 2012 to 2015, the World Bank report eight and then 15 years of compulsory schooling. These figures are implausible since only primary education which requires six years of schooling is compulsory in Tonga. Therefore, we code also six years of compulsory schooling for the period 2012 to 2015.

Trinidad and Tobago: In 1973 and 1974, compulsory schooling is reported to possess a value of ten years. Before 1973 and after 1974, this figure corresponds to seven years. Since only primary schooling is compulsory with a standard duration of seven years given entry ages for primary and secondary schooling, we code a value of seven for 1973 and 1974.

Turkey: Between 1965 and 1967, eight years of compulsory schooling are being reported. However, only five years of primary schooling were compulsory. In line with preceding and subsequent periods, we thus code a compulsory schooling of five years.

Tunisia: From 1968 to 1981, UNESCO yearbooks report six years of compulsory schooling. For 1982 and 1983, no values are reported. From 1984 onward, compulsory schooling is documented with a value of zero until 1992. The yearbooks show eleven years of compulsory schooling for 1993/1994, and nine years from 1995 onward. The educational system consists of six years of primary schooling, three years of lower secondary schooling and a further four years of upper secondary schooling. This structure is maintained throughout the entire period 1981 to 1995. Since zero values are implausible, we code them as missing. For the years 1993 and 1994, we set compulsory schooling to nine instead of eleven years.

United States: For the years 1963 to 1997, UNESCO yearbooks present values ranging from ten to twelve years for the U.S. Minimum compulsory schooling corresponds to ten years, formally from age 6 to 16. Some states require students to remain in school until coming of age implying two further years. However, there are also exemption regulations for religious groups and homeschooling. We take a conservative view and set the compulsory schooling thus to the minimum value of ten years which is fulfilled by all states.

Vanuatu: Historically, the educational system consists of English and French schools. Compulsory schooling years refer to regulations with respect to English schools.

Yemen: Figures are based on compulsory schooling of the former Arab Republic of Yemen and the Republic of Yemen.

Zambia: For the years 1963 to 1966, UNESCO yearbooks report compulsory schooling of eight years with zero years from 1967 onward. Since “[e]ducation is compulsory in certain areas only” (UNESCO, 1963-1966), we code the years 1963 to 1966 as zero.

Azerbaijan, Armenia, Belarus, Estonia, Georgia, Kazakhstan, Kyrgyzstan, Latvia, Lithuania, Moldova, Russia, Tajikistan, Turkmenistan, Ukraine, Uzbekistan: Prior to 1992, we code compulsory schooling according to the values of the former Soviet Union. Between 1963 and 1966, UNESCO yearbooks report eight and nine years of compulsory schooling. Since primary schooling comprises only eight grades, we code eight rather than nine years.

Bosnia and Herzegovina, Croatia, Macedonia, Montenegro, Serbia, Slovenia: Prior to 1993, we code compulsory schooling according to values of former Yugoslavia. Figures of Serbia and Montenegro are taken from the Federal Republic of Yugoslavia for the years 1993 to 1997.

Table A2: Robustness: Omitting Experience (Secondary Schooling)

	No Controls	Adding Controls	Fixed Effects	Adding Gini	Lagged Controls	IV $\Delta(h_{i,t})$	IV $\Delta(s_{i,t})$	IV $\Delta(h_{i,t}), \Delta(s_{i,t})$
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
$\ln(y_{i,t-1})$	-0.044*** (0.0091)	-0.15*** (0.016)	-0.34*** (0.048)	-0.18*** (0.020)	-0.21*** (0.021)	-0.14*** (0.018)	-0.14*** (0.017)	-0.14*** (0.018)
$\Delta \ln(k_{i,t})$	0.51*** (0.051)	0.34*** (0.061)	0.32*** (0.092)	0.23*** (0.066)	0.47*** (0.068)	0.34*** (0.062)	0.33*** (0.064)	0.34*** (0.066)
$\Delta(h_{i,t})$	0.76** (0.33)	0.31 (0.30)	0.13 (0.29)	0.32 (0.35)	0.38 (0.29)	1.13 (0.96)	0.36 (0.30)	0.99 (0.92)
$\Delta(s_{i,t})$	0.10*** (0.031)	0.079** (0.031)	0.10** (0.043)	0.073** (0.029)	0.068** (0.028)	0.083*** (0.029)	0.053 (0.10)	0.041 (0.096)
$\Delta(\sigma_{i,t}^2)$				-0.61 (0.42)				
R^2	0.28	0.38	0.39	—	0.40	0.36	0.37	0.36
First stage F						25.2	29.7	17.3
Countries	120	120	120	113	120	120	120	120
Observations	661	661	661	500	661	661	661	661
Controls		✓	✓	✓	✓	✓	✓	✓

Notes: All specifications include time effects. Asterisks indicate significance levels: * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.