
why developing countries are concerned about the brain drain, and why good students prefer to be at schools with other good students. Once we start thinking about externalities, it seems unlikely that they are the same at all levels of schooling. One externality often mentioned is that educated people generate good ideas that enter society's pool of knowledge. This externality might well flow largely from the most educated members of society. If so, differences in secondary and higher education would be more important than differences in primary education.

I should note that an important semantic issue arises here. When Klenow and Rodríguez calibrate their model, they do so using the private return to schooling. In their view, externalities from human capital are outside the realm of the neoclassical model, if such externalities are important, that calls into doubt the neoclassical revival. A broader view of the neoclassical growth model, however, is that it emphasizes capital accumulation as the key to growth, and it can potentially allow for such externalities. Certainly, that is the view I have taken when defending the neoclassical model as a useful theory of international differences in living standards.

All of the issues I have raised point to a single conclusion. We don't know as much as we need to know to calibrate the contribution of human capital to the production process. In particular, we don't know with much precision the share of income that accrues to human capital, we don't know what weight to attach to various levels of education when measuring the total stock of human capital, and we don't know the social return to a year of schooling. In the end, therefore, I am not convinced by this paper's central conclusion that human and physical capital are incapable of explaining the bulk of international differences in income. Put simply, this conclusion is based on more assumptions about unknown parameters than I am willing to swallow.

Comment

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1. Introduction

The neoclassical growth revival is associated with the hypothesis that most of the variation in levels of per capita income across countries is associated with variation in capital and skills, and most of the variation

in growth rates of per capita income across countries is associated with variation in the growth rate of capital and skills. Klenow and Rodríguez associate this view primarily with two papers—Mankiw, Romer, and Weil (1992) (MRW) and Young (1995)—but Mankiw (1995) summarizes this work and provides the clearest statement of the neoclassical growth revival.

Klenow and Rodríguez question both tenets of the neoclassical growth revival and argue that “the dominant cause of the large international dispersion in levels and growth rates of output per worker” is differences in (multifactor) productivity. I do not find much to disagree with in this conclusion, except that it perhaps overemphasizes the importance of productivity. Indeed, Hall and I have argued elsewhere (Hall and Jones, 1996) that levels of productivity vary greatly across countries. We use a slightly different methodology—for example, we follow Solow (1957) in not making an assumption about the exact functional form of the production function—and reach the same conclusion. Part of the reason why the United States has a higher GDP per worker than Ethiopia is that the United States invests more in physical capital and its workers have a much higher level of education. However, a key part of the explanation is also that the United States uses its inputs much more productively than does Ethiopia.

My comments are divided into two parts. The first part attempts to clarify some of the points made by Klenow and Rodríguez. In the process, I will argue that it is not obvious from the MRW evidence that there should have been a neoclassical revival in the first place. The second part steps back to consider where we are in our understanding of the differences in levels and growth rates of output per worker across countries

2. Should There Have Been a Neoclassical Revival?

My first comment is related to interpreting the results in Klenow and Rodríguez’s Table 1, where they decompose differences in output per worker across countries into the contribution from productivity, A , and the contribution from inputs, X . Their summary statistics are based on the coefficients obtained from regressing $\log A$ on $\log y$ and $\log X$ on $\log y$, where y denotes GDP per worker. They interpret these coefficients as conditional expectations: if GDP per worker is higher by one percent, what is our best guess as to how much higher productivity and inputs are?

These coefficients can be motivated in a different way. For example, one might be concerned with how much of the variation in output per

worker across countries is due to variation in productivity vs. variation in inputs. Since $y = AX$, the variance of $\log y$, denoted σ_y^2 , can be written as the sum of the variances of $\log A$ and $\log X$ and their covariance:

$$\sigma_y^2 = \sigma_A^2 + \sigma_X^2 + 2\sigma_{AX}$$

The problem with doing the variance decomposition this way is that one encounters covariance terms.

The regression coefficients reported by Klenow and Rodríguez are estimates of

$$b_A = \frac{\sigma_A^2 + \sigma_{AX}}{\sigma_y^2},$$

$$b_X = \frac{\sigma_X^2 + \sigma_{AX}}{\sigma_y^2}.$$

Therefore, b_A and b_X are a way of summarizing the variance decomposition, where the covariances are split evenly among the factors

This is a useful way to summarize the variance decomposition, particularly when more than two factors are considered. However, it can sometimes be slightly misleading. For example, in the conference version of their paper Klenow and Rodríguez reported a value of b_A of -0.03 for their replication of the MRW results. According to the formula just given, this is possible if the variance of $\log A$ is offset by a negative covariance between $\log A$ and $\log X$. In fact, this is what they reported. This implies, however, that the A 's differ substantially across countries.

A related point is made by considering the original MRW results. They report that neoclassical inputs can explain a large fraction of the variation in output per worker across countries, the R^2 from their regression of $\log y$ on the components of $\log X$ is 0.78. While this is high, this means that the variation in the residual is equal to 0.22 times the variation in $\log y$. This residual can be interpreted as the logarithm of productivity, and therefore, in the original MRW results, the standard deviation of $\log A$ is equal to $(0.22)^{0.5} = 0.47$ times the standard deviation of $\log y$. In other words, even MRW find large differences in productivity across countries. We are left with the question: Is even the MRW evidence really consistent with the tenets of the neoclassical revival?

My second specific comment is related to the results reported in Klenow and Rodríguez's Table 4. This table contains their version of growth accounting, based on correlations of growth rates of output per

worker with growth rates in factor intensities. They infer from this exercise that most of the variation in growth rates across countries is the result of total factor productivity (TFP) growth rather than growth in the inputs.

I like the basic approach here, but I'd like to suggest an alternative. If all countries were in their steady states, then, as is well known, all growth would be attributable to TFP growth. In this sense, the KR methodology is in some ways set up to deliver their result. An alternative way of gauging the relative importance of TFP growth in inputs that gets around this problem is to ask how much of the exceptional growth in output per worker is due to exceptional productivity growth and exceptional capital deepening.

This exercise is carried out here using the data from Young (1995), as reported by Klenow and Rodríguez. Specifically, I assume that the world's technological frontier is growing at an annual rate of 1%. This number is chosen for convenience; it is easy to see how other numbers would affect the results. Then the growth rate of output per worker can be written as the sum of the growth rate of the world technological frontier, plus the exceptional growth in A and the exceptional deepening (growth in X). According to the results shown here in Table 1, roughly half of the exceptional (i.e. faster than the world frontier) growth in the East Asian economies is due to exceptional growth in A and roughly half is due to exceptional growth in the intensity of the inputs. These results, together with the basic finding (documented in Table 2 of Klenow and Rodríguez) that between 40% and 60% of the variation in levels is due to variation in A , lead me to conclude that roughly half of the variation in both levels and growth rates of output per worker is due to productivity. I think this is worth emphasizing, because oftentimes readers want an all-or-nothing answer, and they may be tempted to conclude from Klenow and Rodríguez's paper that "everything is productivity." A better answer, I think, is that both traditional inputs and productivity play large and important roles.

Table 1 ACCOUNTING FOR MIRACLES

Country	$g_{Y/L}$	Growth of world frontier (%)	Exceptional A growth, $g_A - 1\%$ (%)	Exceptional deepening, g_X (%)
Hong Kong	4.7	1	2.7	1.0
Singapore	4.2	1	-0.7	3.9
S. Korea	4.9	1	1.5	2.4
Taiwan	4.8	1	2.5	1.3

3. *Our Current State of Knowledge*

I want to end my comment with a discussion of the current state of our knowledge about economic growth. One of the important goals of the growth literature is to provide a formal characterization of the dynamic production possibilities of the economy, that is, to provide a model that specifies an aggregate production function $Y = F(K, H, L, A, \dots)$ as well as how the inputs into production evolve over time.

Different papers in the growth literature characterize this dynamic PPF differently. For example, Solow's (1957) paper was concerned with figuring out the "shape" of the aggregate production function $F(\cdot)$, since Solow (1956) recognized that the dynamics of the neoclassical growth model hinged on this function. MRW can also be read as trying to pin down the shape of the aggregate production function within the class of Cobb–Douglas production functions.

With hindsight, estimating the parameters of the aggregate production function econometrically appears to be impossible. The required identifying assumption is that one can separate shifts of the production function from movements along the production function. In practice, I do not see how this can be done. For example, to use instrumental variables, one needs to find a variable that changes the capital stock without changing the efficiency with which the capital is used. In practice, anything that stimulates capital is likely to stimulate productivity as well, particularly if unmeasured utilization is a problem.

For this reason, I now have less confidence in modeling the aggregate production function as $Y = K^{1/3}H^{1/3}(AL)^{1/3}$, as originally suggested by MRW. In addition, Klenow and Rodríguez claim that MRW are using the wrong measure of human capital in their regression. Had they used Klenow and Rodríguez's data, presumably they would have estimated *different* exponents for the production function. Therefore, I see no evidence that the aggregate production function should take this form. Instead, one has to use other evidence such as the absence of trends in factor shares to try to restrict the shape of the production function.

An important contribution of Bils and Klenow (1996) is to argue that the Mincerian wage regressions in the labor literature provide a useful piece of evidence to match in modeling human capital. Like the absence of trends in factor shares, this independent evidence tells us something about the way educational attainment affects output, and it seems to me that this is a superior way of characterizing the human capital portion of the dynamic production possibilities of the economy.

Based on these papers and on other work in the growth literature, one

way to describe the dynamic production possibilities frontier of economies is

$$\begin{aligned}
 Y &= AK^\alpha(hL)^{1-\alpha}, & \alpha &= \frac{1}{3}, \\
 \dot{K} &= Y - C - \delta K, \\
 h &= e^{\gamma S}, \\
 A &= ?
 \end{aligned}$$

The first equation is an aggregate production function in which there is one type of labor in the economy with a skill level h determined by educational attainment. I've simplified the production function relative to Klenow and Rodríguez and MRW. In fact, workers with different skills are probably not perfect substitutes, and the evidence on changes in relative wages suggests that maybe technical change is skill-biased. However, at this point in time, I don't think there is a clear candidate production function with these properties, and growth economists are typically using aggregate measures of human capital that do not distinguish the skill levels of different workers anyway.

I've left the modeling of A unspecified. For the growth of A , Romer (1990), Grossman and Helpman (1991), and others have provided us with very nice theories.¹ About the levels of A we know much less. To the extent that one associates productivity with ideas, models of technology diffusion and transfer are relevant. However, it seems likely that differences in productivity reflect more than simply differences in technology; for example, Hall and Jones (1996) find that the U.S. level of productivity ranks 11th out of 133 countries.

A suggestive stylized fact documented both by Hall and Jones and by Klenow and Rodríguez is that productivity and inputs are highly correlated across countries. Poor countries have low levels of education, investment, and productivity. Rich countries have high levels of inputs and productivity. This correlation suggests that common driving forces may be at work. Hall and I document that measures of the extent to which the government policies and institutions of the economy discourage diversion and rent-seeking can explain much of this correlation and provide a clear empirical explanation for why some countries are so much richer and more productive than others.

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¹ I, of course, prefer the versions of these models in which growth rates are not increasing in the size of the economy, as in Jones (1995)

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Discussion

Robert King began the discussion by asking whether it is feasible to create a more comprehensive human-capital measure using measures of educational attainment available from the United Nations, then aggregating using a reference wage rate. In particular, he noted that similar work has been done in the past by Anne Krueger, but that the data are probably of higher quality now.

Francesco Caselli observed that panel-data techniques developed to deal with omitted-variable bias may be used to identify differences in technology across countries. In his own work he has been able to estimate TFP directly instead of having to rely on the error term for its identification. His finding is that the direct estimates of TFP are highly variable across countries, supporting the conclusions of Klenow and Rodríguez.

Susanto Basu pointed out that the paper largely ignores issues of capital mismeasurement. He cited work by Jong-Wha Lee and others that attributes most of the growth benefits of openness to imports of capital goods. This result suggests that embodiment of technological advances in new capital is important to a degree that may not be captured very well in the Summers–Heston data.

John Haltiwanger expressed two reservations. First, he cited a paper by Baily, Hulten, and Campbell which used plant-level data to decompose aggregate productivity growth into its sources. Those authors